REPORT

OF THE

FOURTH MEETING

OF THE

BRITISH ASSOCIATION.

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BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE;

HELD AT EDINBURGH IN 1834.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1835.

PRINTED BY RICHARD TAYLOR,
RED LION COURT, FLEET STREET.

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OBJECTS AND RULES OF THE ASSOCIATION.

OBJECTS.

The Association contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind, which impede its progress.

RULES.

MEMBERS.

All Persons who have attended the first Meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its Rules.

The Fellows and Members of Chartered Literary and Philosophical Societies publishing Transactions, in the British Empire, shall be entitled, in like manner, to become Members of the Association.

The Office-Bearers and Members of the Councils, or managing Committees, of Philosophical Institutions shall be entitled, in like manner, to become Members of the Association.

All Members of a Philosophical Institution recommended by its Council or Managing Committee, shall be entitled, in like manner, to become Members of the Association.

Persons not belonging to such Institutions shall be elected by the General Committee or Council, to become Members of the Association, subject to the approval of a General Meeting.

SUBSCRIPTIONS.

The amount of the Annual Subscription shall be One Pound, to be paid in advance upon admission; and the amount of the composition in lieu thereof, Five Pounds.

Subscriptions shall be received by the Treasurer or Secretaries.

If the annual subscription of any member shall have been in arrear for two years, and shall not be paid on proper notice, he shall cease to be a member; but it shall be in the power of the Committee or Council to reinstate him, on payment of arrears, within one year.

RULES OF THE ASSOCIATION.

MEETINGS.

The Association shall meet annually, for one week, or longer. The place of each Meeting shall be appointed by the General Committee at the previous Meeting; and the Arrangements for it shall be entrusted to the Officers of the Association.

GENERAL COMMITTEE.

The General Committee shall sit during the time of the Meeting, or longer, to transact the Business of the Association. It shall consist of all Members present, who have communicated any scientific Paper to a Philosophical Society, which Paper has been printed in its Transactions, or with its concurrence.

Members of Philosophical Institutions, being Members of this Association, who may be sent as Deputies to any Meeting of the Association, shall be Members of the Committee for that Meeting, the number being limited to two from each Institution.

COMMITTEES OF SCIENCE.

The General Committee shall appoint, at each Meeting, Committees, consisting severally of the Members most conversant with the several branches of Science, to advise together for the advancement thereof.

The Committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next Meeting. They shall engage their own Members, or others, to undertake such investigations; and where the object admits of being assisted by the exertions of scientific bodies, they shall state the particulars in which it might be desirable for the General Committee to solicit the co-operation of such bodies.

The Committees shall procure Reports on the state and progress of particular Sciences, to be drawn up from time to time by competent persons, for the information of the Annual Meetings.

LOCAL COMMITTEES.

Local Committees shall be appointed, where necessary, by the General Committee, or by the Officers of the Association, to assist in promoting its objects.

Committees shall have the power of adding to their numbers those Members of the Association whose assistance they may desire.

OFFICERS.

A President, two Vice-Presidents, two or more Secretaries, and a Treasurer, shall be annually appointed by the General Committee.

COUNCIL.

In the intervals of the Meetings the affairs of the Association shall be managed by a Council, appointed by the General Committee.

PAPERS AND COMMUNICATIONS.

The General Committee shall appoint at each Meeting a Sub-Committee, to examine the papers which have been read, and the register of communications; to report what ought to be published, and to recommend the manner of publication. The Author of any paper or communication shall be at liberty to reserve his right of property therein.

ACCOUNTS.

The Accounts of the Association shall be audited annually, by Auditors appointed by the Meeting.

TREASURER.

John Taylor, Esq., 14, Chatham Place, London.

LOCAL TREASURERS.

Dr. Daubeny, Oxford.
Charles Forbes, Esq. Edinburgh.
Jonathan Gray, Esq., York.
Prof. Henslow, Cambridge.
William Hutton, Esq., Newcastle-on-Tyne.
Dr. Orpen, Dublin.
Dr. Prichard, Bristol.
George Parsons, Esq., Birmingham.
Rev. John J. Tayler, Manchester.
Samuel Turner, Esq., Liverpool.
H. Woolcombe, Esq., Plymouth.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

TREASURER'S ACCOUNT from 18th June 1833 to 31st July 1834.

RECEIPTS.

	£.	s.	d.
Balance of Account audited at Cambridge	498	4	3
Compositions from 140 Members	679	0	0
Annual Subscriptions 1833, from 716 Members	716	0	0
Ditto 1834, 10 do.	10	0	0
Arrears of ditto 1832, 11 do.	11	0	0
Received on Account of the Sale of 1st volume of Reports	314	7	4
Received for Lithographic Signatures sold	150	4	0
Dividend on 1000*l.*, 3 per cent. Consols, six months, to July 1833 ..... £15 0 0			
Ditto on 1700*l.*, ditto 12 months, to July 1834 ..... 51 0 0	66	0	0
	£2354	15	7

PAYMENTS.

	£.	s.	d.
Expenses at the Cambridge Meeting	266	5	2
Disbursements by Local Treasurers	38	10	11
Purchase of £700 in the 3 per cent. Consols	622	2	6
Paid John Murray for Printing, &c., the 1st volume of Reports	321	0	0
Paid Richard Taylor for printing the 2nd volume of Reports, *on Account*,	250	0	0
Sundry Printing	10	13	0
Expenses on Printing, and Lithographic Signatures, with Report of Proceedings at Cambridge	414	7	8
Salaries to Assistant Secretary and Accountant	120	0	0
Paid the "Committee for the Discussion of Tide Observations"	20	0	0
Balance in the Banker's hands £218 7 6			
Ditto Treasurer's 7 0 4			
Ditto Local Treasurers' 66 8 6	291	16	4
	£2354	15	7

G. B. GREENOUGH,
FRANCIS BAILY, } *Auditors.*

JOHN TAYLOR, *Treasurer.*

FOURTH REPORT.

PROCEEDINGS OF THE MEETING.

1834.

THE British Association resumed its sittings on Monday the 8th of September, 1834, in the city of Edinburgh. The Meeting was attended by a greater number of members than had assembled on any former occasion*; but by means of the arrangements adopted by the Secretaries and local Committee, its proceedings were conducted with order and facility. All the public accommodations which this magnificent capital possesses were opened to the Association: the members received their tickets in the gallery of the Royal Institution; the general Committee sat in the meeting-room of the Royal Society; the Sections were distributed through the class-rooms of the University; the meetings of the entire body were held in the public Assembly Rooms and in the hall of the College Library.

GENERAL MEETINGS.

On Monday evening, at eight o'clock, the first General Meeting was held in the great Assembly Room. The President of the preceding year (the Rev. Professor Sedgwick) addressed some remarks to the meeting on the progress of the Association; he congratulated them on the increased strength in which they had assembled, in a place endeared to the feelings of every lover of science, by so many delightful and elevating recollections, especially by the recollection of the great men whom it had fostered or to whom it had given birth. Among the persons congregating together from different countries to this great philosophical union it had been his good fortune to encounter on his road thither M. Arago the Perpetual Secretary of the

* The number of tickets issued was 1298.

French Institute, a name which the meeting well knew was not inferior in scientific reputation to any in Europe. To meet with such men, to breathe the same atmosphere with them, to partake the same sentiments, to enjoy their conversation, and to gain, he hoped, their friendship, these were among the highest privileges which such unions bestowed.

If he were to be asked what the power is which this Association peculiarly applies to the advancement of science, he would answer,—the power of combination: how feeble is man for any purpose when he stands alone, how strong when united with his fellow-men! It might be true, perhaps, that the greatest philosophical works have been achieved in privacy; but it is no less true that those works would never have been accomplished if their authors had not mingled with men of similar pursuits, and availed themselves of their assistance. To such a commerce of ideas they have often been indebted for the germs of their apparently insulated discoveries, and without such mutual aid they would seldom have been able to carry their investigations to any valuable conclusion. Even in the highest departments of philosophical reasoning, when a question of fact arises, when a point of experiment is reached, the greatest masters of analysis are obliged to call in the cooperation of other labourers, and to wait for the observations of experimental men.

The manner in which the power of combination is brought into action by these meetings might, in some measure, be collected from the results which had sprung out of the proceedings of the last meeting. A discussion, for instance, had then taken place on the subject of the aurora borealis, and measures were adopted for promoting the investigation of the circumstances connected with that remarkable phænomenon. Soon afterwards a beautiful arch appeared across the heavens; it was simultaneously observed by different members of the Association at distant points; and thus elements were furnished for a computation of its height. Again, observations of great value had long since been made at the Royal Observatory of Greenwich by Bradley and Maskelyne: these had lain till now unreduced, like unwrought ore, or raw materials for a valuable manufacture not worked up; and they might still have continued useless and lost to science but for the application to Government resolved upon at the last meeting of the Association, the success of which had been announced in the volume of Reports which had since been printed. The Professor next referred to the progress which by the same agency is making in the observing of the tides and the discussion of tide observations, and to the experiments on the effects of long-continued heat, which are going on in the

iron-furnaces of Yorkshire. He concluded by recommending to the meeting a strict adherence to the principle of excluding from their discussions every subject of a nature not strictly scientific, and expressed the satisfaction with which he resigned his office into the hands of one who had already been placed at the head of science in Scotland, and who had added to laurels gained in fighting the battles of his country the glory of having kindled up the light of philosophy even at the antipodes.

The President (Sir Thomas Brisbane) assured the meeting of the strong desire which animated individuals of every rank in the city of Edinburgh and its neighbourhood to give the warmest welcome to the Association, and to uphold, by its reception, the national character for hospitality. He announced that the Principal and Professors of the University had given the free use of their class-rooms and of other apartments in the College, which would be found admirably adapted for the Sectional meetings of the Association; and he added that other public bodies had not been backward in making similar offers, and in contributing whatever lay in their power towards its accommodation.

The senior Secretary (Mr. Robison) stated the course of proceeding which it was intended to adopt in conducting the business of the present meeting. The principal variation to be made from the course pursued in former years consisted in devoting the entire morning to the meetings of the Sections and their Committees; and transacting the detail of scientific business solely at the morning meetings. In the evening the Chair would be taken at eight o'clock; the officers of each Committee would give a short summary of the proceedings which had taken place in their respective Sections, and these statements would be followed by any communications of a more popular character that might be selected for the evening meetings. In addition to what had been already said of the liberal conduct of public bodies in Edinburgh, he was bound to mention the peculiar obligation under which the Association lay to the proprietors of the building in which they were then assembled, who had not only granted the gratuitous use of their apartments, but had expended a large sum of money in preparing and decorating them for the meeting.

The junior Secretary (Professor Forbes) then delivered the following address:

"It having been suggested that the general view of the progress of the affairs of the Association, so ably executed last year by Mr. Whewell, should annually be continued by the Secretary for the time being, I have undertaken this portion of the duties

which devolve upon the Secretaries for Edinburgh, at the desire of my learned colleague Mr. Robison, who, on the other hand, has engaged briefly to state the nature and motives of the practical arrangements for the present meeting, of which he has had the kindness to superintend by far the most laborious part.

I felt anxious that such a periodical report as I have mentioned should be continued, because of the necessarily fluctuating state of our Body, and the small number of persons who, by circumstances, have been enabled to attend all the meetings, and to become acquainted with the actual operation of a somewhat complicated machine; and I was ready to undertake that duty, because I hoped that I might be able, by an appeal to facts, in the *first* place, to put in a clear point of view, what has not perhaps been enough insisted on, and has therefore been very generally misunderstood,—the perfectly *unique* character of this Association, and the high aims to which its efforts are directed; and, in the *second* place, to demonstrate that these aims and objects are in the due course of attainment; that the members, and especially the *projectors* of this institution, are fulfilling the pledges, of no common character, which they gave to the public, and this more especially in relation to the proceedings of the past year.

"The character of the Association, I have said, may be considered as *unique*. It is not to be confounded with those numerous and flourishing institutions which have sprung up, especially of late years, for the simple diffusion of scientific truths. Such *diffusion* does not even, properly speaking, include any attempt at *extension* or accumulation: if in many cases it does promote such extension, it is indirectly, and beyond a doubt has sometimes had the opposite tendency. The intellectual wealth of mankind is no more increased by this operation, than is the weight of the precious metals under the hand of the gold-beater. A greater display may indeed be attained, and a more commodious application to the useful and the elegant purposes of life; but for actual increase of the stock which may hereafter be fashioned with ease and expedition by the hands of a thousand artificers, we must recur to the miner toiling in his solitary nook, and to the labourer who painfully extracts some precious grains from the bed of the torrent. It is the furtherance of this species of productive energy that the British Association claims for its capital object. The diffusion of a taste for science amongst its numerous members is no doubt also one of the most necessary and most desirable consequences of the principles upon which it is founded; but it is not the basis of these principles. To teach those who have never pursued natural knowledge but

as an occasional amusement,—to feel that for them a field lies open which tomorrow they may call their own,—to lend them such aid as may promote the success of their exertions, by removing the preliminary difficulties, and pointing out the existing boundary betwixt the known and the unknown,—to stimulate these exertions and those of others who have already become, to a certain degree, familiarized with the labours and with the results of intellectual toil, by enabling them to mix with the veterans in each department who have gained, and who still continue to gain, the highest rewards which the investigation of nature confers,—who will point out the methods which they pursued, the disappointments which they met, and the difficulties which they surmounted, thus affording at once the gratification which every generous mind feels in personal communication with those who have signalized themselves by intellectual achievement, and the instruction and encouragement for the pursuit of a similar course which words, and words alone, can impart,—*these* we hold out as amongst the first and the most valuable objects proposed to be attained by the institution of this Association.

"No doubt societies for the promotion of natural knowledge have been in existence for near two centuries, and these have done much to the due advancement of science itself, as well as the promotion of a more general taste for its cultivation. They were admirably adapted to the period of their institution, when the difficulties of ordinary communication, and the want of scientific journals, made the Royal Society of London the great centre of philosophical information,—when new experiments were there first repeated,—when new theories were there first discussed,—and when its *Transactions*, and those of the other academies of Europe—fraught with the literary treasures which Hooke and Wren, and Boyle and Leibnitz, and the Bernouillis, loved to display, and which Newton alone loved to conceal—were the couriers which published to Europe the intelligence of the successive intellectual victories of that mighty age. Rarely even then, however, and latterly still less, did these societies attempt to guide in any specific direction the investigations of their members, or to form any school of science for the initiation of fresh inquirers. The formation of such schools of disciples who voluntarily combined under some philosopher of eminence, partly did away with the necessity of this on the Continent; whilst the total want of anything similar in our own country, and the less specific objects of those honorary rewards which from time to time have been given by learned societies in all countries, and which have occasionally drawn forth all the powers of some mas-

ter mind to the solution of a specific difficulty proposed as a prize question, necessarily produced a greater want of systematic cooperation amongst scientific men in Britain than is to be found in several countries not her political superiors.

"The migratory scientific associations of Germany and Switzerland—to which we gratefully acknowledge that our British one owes its rise,—embrace only one class of the objects to which we have above alluded as characterizing this Body. Their aim was simply to promote the intercourse of scientific men, and to diffuse a taste for the prosecution of science. Their existence is not permanent,—they execute no functions but for the moments during which their members are once a-year assembled,—they regard not the past, and have no cares for the future,—they merely receive and consider the communications which the zeal of individual members places in their way. Such was at first proposed to be the character of the Body this day assembled, in imitation of the foreign meetings; but a more extensive design was subsequently adopted, and it was determined to establish a permanent society, of which these annual reunions should simply be the meetings, but which, by methods and by influence peculiarly its own, should continue to operate during the intervals of these public assemblies, and should aspire to give an impulse to every part of the scientific system, to mature scientific enterprise, and to direct the labours requisite for discovery*.

"If we now turn from the aims to the *acts* of the Association, we shall find gratifying proof that these designs were not chimerical, and that the primary machinery devised for effecting them was wanting neither in efficiency nor in permanence. The first and most signal proof which we can cite, is the production of those Reports on the Progress of Science, which appeared to be one of the most important objects of such an institution, and one which, beyond all dispute, no existing society could have attempted. To call upon persons whose time was in all cases more or less valuable, for such a devotion of it as was required for a systematic and precise detail of the recent progress of the sciences which they respectively cultivated, was to make a demand, the boldness of which cannot perhaps well be appreciated but by those who have had experience in the labour of bringing together the substance of detached, though often profound, papers in the extensive range of scientific periodicals and academical collections. Yet so obvious

* The author here described the share which some of the founders of the Association had respectively taken in planning and conducting the institution.

was the utility of the proposed undertaking, that, in the very infancy of the Association, there were found several distinguished individuals, and chiefly from the University of Cambridge, who had not even been present at the first meeting, but who volunteered to undertake some of the most valuable of those reports which appeared in the first volume of the Proceedings of the Association. As Mr. Whewell enumerated these in his last year's address, I will not further allude to them. It ought, however, specially to be observed, that these reports differ entirely from the short systematic treatises on scientific subjects with which the press teems. They are not primarily intended for the general reader—they are not meant for the purpose of popularizing technical subjects; their main object is so to classify existing discoveries as to lead the individual who is prepared to grapple with its difficulties, to start with the most complete and accurate knowledge of what has already been done in any particular science,—not intended itself to contain that knowledge, but merely to serve the purpose of a *catalogue raisonnée*, by means of a lucid analysis and arrangement, at the same time (and here is the great necessity of securing the cooperation of persons distinguished in the several departments,) that the report should point out the most important questions which remain for solution, whether by direct experiment or by mathematical investigation.

"The second volume of Reports has amply justified the expectations with which it was hailed; and whilst the first was chiefly occupied with reports upon great and leading divisions of science, we have here several happy specimens of a still greater division of labour, by the discussion within moderate limits of some particular provinces. Thus, Mr. Taylor has treated of one particular and most interesting question in Geology, the formation of Mineral Veins,—one of the most important, in a theoretical point of view, which could have been stated, and which, from its intimate connexion with commercial speculation, might have been expected in a country like ours to have been more specifically treated of than it has been. It strictly belongs to the dynamics of the science, to which, since the time of Hutton, but little attention has been paid until very recently. By the exertions, however, of Mr. Carne, of Dr. Boase, and Mr. Henwood of Cornwall, whose researches are to form one point of discussion in the Geological Section at the present meeting, the question of the origin of mineral veins, though probably by no means decided, has been brought prominently forward.

"That electric agency was concerned in the disposition of metalliferous veins can scarcely be doubted; and the connexion

between electricity and magnetism, now so fully established,—the connexion between metalliferous veins and lines of elevation, and between the latter and the isodynamical lines of terrestrial magnetic intensity, as suggested by Professor Necker of Geneva,—point out a bond of union between this subject and that of terrestrial magnetism, on which we have a report by Mr. Christie, where the very interesting direct observations of Mr. Fox of Falmouth, on the electro-magnetic action of mineral veins, are particularly noticed. Mr. Christie's theory of the diurnal variation of the needle, which he is desirous should be submitted to the test of a laboratory experiment, is likewise intimately connected with the actual constitution of our globe*. The whole subject of Terrestrial Magnetism is one of the most interesting and progressive of the experimental sciences. The determination of the *direction* of the magnetic energy by means of two spherical coordinates, termed the variation and the dip, and the measure of the *intensity* of that force, are the great objects of immediate research, as forming a basis of theory. The existence of four points on the earth's surface, to which the needle tends, has long been known; and the position of two of these (in Northern Asia and America,) has recently been elucidated by the persevering efforts of Professor Hansteen and Commander Ross. The precise numerical determination of the elements just alluded to acquires a deep and peculiar interest from the multiplied variations which they undergo. Not only are these elements subject to abrupt and capricious changes, which Baron Humboldt has termed *magnetic storms*; but gradual and progressive variations are undergone at different hours of the day, at different seasons of the year, and throughout longer periods, which may even perhaps bear a comparison with the sublime cycles of Astronomy.

"Natural History forms a more prominent subject in this volume than in the last, though the reports of Professor Lindley 'on the principal questions at present debated in the Philosophy of Botany'; and of Dr. Charles Henry 'on the Philosophy of the Nervous System', refer only to particular departments of widely extended subjects, which are again to be resumed in more general reports, undertaken for the present meeting,—that by Mr. Bentham, on Systematic Botany, and by Dr. Clarke of Cambridge, on Physiology in general. We cannot but remark with pleasure, that one of the points for inquiry particularly insisted on by Professor Lindley, that of the influence of the chemical nature of soils, and of the excretions of plants, was taken up at an early period of the existence of the Association, by one

* Report, p. 122–3.

of its most zealous supporters, Dr. Daubeny; and that, in reference to the review by Dr. Henry of the labours of European physiologists, we may quote, as a national honour, the discoveries of our distinguished associate Sir Charles Bell.

"On the general connexion and occasional apparent opposition of theory and practice, I would refer to some very pertinent remarks in the address of Mr. Whewell at the last meeting. The importance of carrying on both simultaneously and independently, and of looking to our increased knowledge of both as the only sure means of ultimately reconciling discrepancies, has been manifested by the desire of the Council of the Association to procure two distinct reports on the Theory and Practice of Hydraulics, which have been drawn up with remarkable perspicuity, and within a small compass, by Mr. Challis and Mr. Rennie. Both of these gentlemen have shown their zeal in the objects of the Association by promising to continue their valuable labours. Mr. Rennie, on that part of his subject which relates to the motion of fluids in open channels, and Mr. Challis on some of those exceedingly interesting branches of theory altogether modern, which physically, as well as in their mathematical methods, have the closest analogy to that case of the motion of fluids treated of in the present volume, namely, the theory of Sound, and the intimate constitution of Liquids. When, in addition to these reports, we shall have received that undertaken by Mr. Whewell upon the mathematical theory of Magnetism, Electricity, and Heat, we shall undoubtedly possess the most complete outline extant of a department of knowledge entirely of recent date.

"In the science of Hydraulics, indeed, some progress in theory has accompanied the increase of practical information, at least since the time of Newton; but in the other strictly *practical* report of the present volume, that of Mr. Barlow on the very interesting subject of the strength of materials, little or nothing has been done of much theoretical importance since the days of Galileo. Circumstances, which it would be easy to point out, prevent our setting out, except in rare cases, from unimpeachable data; but several very interesting conclusions of general application are derivable from well-conducted experiments, and the Association may claim some credit for having brought into general notice the ingenious investigations of Mr. Hodgkinson of Manchester, more particularly alluded to in this paper.

"One report, and that the longest which has been printed by the Association, remains to be mentioned. It is by Mr. Peacock on the present state of Mathematics. When we consider the vast extent of the subject, and the extremely limited number of persons, even in the whole of Europe, capable of

undertaking it, we must consider the production of a work of so much labour as the present, which, as yet, is incomplete, but which the author has promised to resume, as the best trophy to which we can refer in proof of the entire efficiency of the Association according to its original plan,—as a proof of the ability and the indefatigable industry which it has enlisted in its service,—as a proof that its aim is not the dissemination of superficial literature, stamped with the effigy of science, and lowered for the demand of the indolent and the careless,—but that it is intended to refine the precious metal until it reaches a state of chemical purity, not to alloy and coin it for the purposes of a promiscuous and debased currency. Mr. Peacock undertook his report in the early days of the Association, when its friends were yet few and its success dubious; its execution has been delayed by the extent of the subject and labour of the task. The report on the Differential and Integral Calculus, which was intended to form the basis of it, is delayed, and the present one is devoted to a discussion chiefly of algebraic methods, and a close examination of the metaphysical principles upon which this interpretation of analysis is founded. The author has thus been led to extend the views which, in his recent systematic treatise, he had developed in regard to the signs of affection of algebraic quantities, including those of imaginary quantities, of discontinuous functions, and the interpretations of zero and infinity. The author has then treated of Series, as regards their fitness for giving directly conclusive results, particularly when such series are *divergent*, leaving to the other part of the report a detail of the progress in the application of series, which is more practical than metaphysical. The author then treats historically of the elementary works in use on Algebra and Trigonometry; and devotes the last part of the report, consisting of above fifty pages, to the Theory of Equations, in which he has minutely analysed some of the most remarkable papers on this abstruse subject. Altogether this report (especially when completed,) cannot fail to fulfill, in a striking manner, the two great objects of such works: *first*, to supply those engaged in collateral branches of science with the means of referring to and obtaining the information they may require upon methods which perhaps are of daily utility in physico-mathematical inquiries, but with which, from the vast extent of the science of pure mathematics, the shortness of human life prevents the possibility of a complete and systematic acquaintance, unless it be made the special object of study; and, in the *second* place, to point out, where chasms of reasoning occur, what mathematical methods are impregnable, and what rest upon a still dubious basis,

in a metaphysical point of view, several of which are very specifically treated of in Mr. Peacock's report. It is much to be desired that nothing may longer postpone the conclusion of a work which cannot fail to reflect honour upon the Association.

"Were those annual Reports the only fruits of the labours of this Society, there would be no reason to complain. But yet more specific results of its impulsive action on science may be quoted. The questions suggested by the reporters and others, and recommended for investigation, have met with ready attention from several individuals capable of satisfactorily treating them. Professor Airy has himself investigated, from direct observation, the mass of Jupiter, suggested as a desideratum in his report on Astronomy; and since the last meeting of the Association, has confirmed his first results by new observations, which give almost the same mass by the observed elongations of the satellites, as had been deduced from the perturbations of the small planets by Jupiter. Hourly observations of the Thermometer in the South of England have, in two instances, been commenced; and we are assured that the same desirable object is about to be attained by the zeal of the Committee in India, where the Association has established a flourishing colony. A series of the best observations for ascertaining the law which regulates the fall of rain at different heights, conducted at the suggestion of the Physical Section, by Messrs. Phillips and Gray of York, have been ably discussed by the former gentleman, in last year's report, and have since been continued. A regular system of Auroral observation, extending from the Shetland Isles to the Land's End, has been established under the superintendence of a special committee, and specimens of the results have been published. Observations on the supposed influence of the Aurora on the Magnetic Needle have likewise been pursued in consequence of this proceeding. The conditions of Terrestrial Magnetism in Ireland have been experimentally investigated by Professor Lloyd. An important inquiry into the law of Isomorphism has been undertaken by a special committee, which has likewise reported progress; and an elaborate synopsis of the whole Fossil Organic Remains found in Britain is in progress, under the hands of Professor Phillips. Many specific inquiries are besides going forward, under particular individuals, to whom they were confided; whilst it is not to be doubted that numberless persons, many of them perhaps new to the world of science, are at this moment pursuing investigations recommended in general terms, in one or other of the publications of the Society.

"To others the Association has not scrupled to commit a portion of the funds at their disposal, for the purpose of pursuing

objects which required an outlay not to be expected from individuals. Among the most important of these is the collection of the Numerical Constants of Nature and Art, which are of perpetual recurrence in physical inquiries, and which has been confided to the superintendence of Mr. Babbage. When objects of still more peculiar national importance presented themselves, the Association has fulfilled its pledge of stimulating Government to the aid of science. Five hundred pounds have been advanced by the Lords of the Treasury towards the reductions of the Greenwich Observations, at the instance of the Association; and more recently the observations recommended by the Committee on Tides have been undertaken, by order of the Lords of the Admiralty, at above 500 stations on the coast of Britain.

"Individuals, as we have said, have been stimulated by the influence of the Association; but so may nations and great bodies of men. Its published proceedings have found their way into every quarter, and are tending to produce corresponding efforts in distant lands. Our reports on science have produced some very interesting counterparts in the literary town of Geneva. America has taken the lead in several departments of experiment recommended by the Association; and the instructions for conducting uniform systems of observation have been reprinted and circulated in the New World. We must likewise consider it as an especial proof of the influence and importance of the Association, that a Report on the Progress of American Geology has been undertaken and executed by Professor Rogers of Philadelphia*. Similar contributions from some other foreign countries have been promised, which will extend the utility of the Association, by making us acquainted with the more characteristic state of science in the various parts of Europe. Nor can we fail, on the present occasion, to consider as a most auspicious promise of the future success of the Association, that the distinguished Secretary of the Institute of France has not only honoured this meeting by his presence, but has promised to interest that powerful body on behalf of the important objects contemplated by the Association, which its cooperation might effectually secure. The formation of a Statistical Section at Cam-

* Some strictures having been made in America on the extracts from this report which appeared in the *Edinburgh New Philosophical Journal*, it may be proper to remark that, in requesting from Professor Rogers a general outline of what is known of the geology of the United States, the Association could not expect that all parts of such a sketch should be verified by the personal observation of the author; it is also due to him to state that his report is published under the unavoidable disadvantage of not having been revised by the author in passing through the press.—*Editor.*

bridge was the prelude to the establishment of a flourishing society, which acknowledges itself the offspring of this Institution, and which promises, by a procedure similar to that introduced by the Association, to advance materially the greatly neglected subject of British statistics.

"Gentlemen, I shall be satisfied if, in the preceding hasty review, I shall have given you some direct and tangible proof of the working of a system, the excellence of which may best be appreciated by such statements. Did it come within the scope of these observations, (which it does not,) I could quote examples, equally specific, of the powerful moral influence of the Association. Yet, in conclusion, I will call upon you to remark, because I believe that it comes home to the breast of every one who has habitually attended these our annual reunions, what a spirit is infused into otherwise isolated and perhaps ineffective exertions, when many minds, conversant with one class of objects, and aiming at one great end, unite in friendly and intellectual converse. There is an impulse there which no system of cold calculation can estimate. There is a bond in the sense of community of purpose, which is the cement of society. There has been, we fear, a general but most erroneous impression abroad, that philosophers are incapable of enjoying, and stoically superior to, the ordinary sociabilities of life,—that scientific ardour dwells only in the mind of the solitary, and gives place to narrow-minded jealousy, when another attempts to share the prize. If, in a few cases, such allegations have not been without a colouring of truth, it is to meetings like these that we should look for a cure which no mere reasoning can effect. The most striking feature of these meetings has ever been, the pervading sense which has thrown a peculiar character over them, of the one great and exalted object which united so many distinct and unconnected individuals,—which not less has drawn into this great assembly, the single and unaided labourer in the cause of science, from the solitudes of the country, or the still greater intellectual solitude of some noisy and commercial city, and the phalanx of scholars who have shared the advantages, and sustained the reputation, of the great academical foundations of the country.

"True it is that, looking merely to the moral influence of the Association, some there are whose zeal for the promotion of science places them above the necessity of such an external stimulant. But we must not legislate for individual and such rare cases. Those who have once trod the higher walks of science, need perhaps no inducements to revisit these sublime elevations. The footway may be sharp and narrow, surrounded with preci-

pices and occasionally enveloped in mists,—but they have there breathed that pure and elastic air which descends not to lower regions,—and through the cloudy openings they have caught rich and extensive views, showing at once the configuration and the bearing of the country, which less daring spirits must painfully and partially explore. Such men are independent of any reward but that which the exertion itself bestows; yet, let it not be called an ignoble motive, if the traveller, embarked on the discovery of a new, and hitherto untrodden, path, which leads to the point to which he aspires, feels fresh vigour infused into his frame, by the consciousness that, in the valley beneath, a thousand eyes are watching his progress, and that a shout of applause, unheard except perhaps in imagination by him, will announce the arrival of the adventurer at the summit of the alpine chain.

"We look forward without anxiety to the future fate of the Association. So long as it continues to be guided by the same principles as heretofore, it cannot fail to confer a substantial benefit upon the science of Britain. We have enough of energy in action to communicate to the many the knowledge of the few, but it is to prevent the stagnation of the stream at the fountain-head, which should be our especial object. True it is that but a few are able or disposed to devote themselves unreservedly to those great enterprises which require the whole man; yet, though it is morally impossible that any others should undertake the highest generalizations to which we have just alluded, a division of labour is as practicable in intellectual as in mechanical science. If one designing mind direct the whole, distinct labourers may be engaged, unknowing each other's tasks, yet happy in the consciousness of being more usefully and more honourably employed than in imperfectly attempting the execution of works which they might individually complete. The exquisite piece of mechanism which, in the form of a watch, issues from the manufactories at Paris or Geneva, has its various elements of its wheels and pinions, its balance and fusee, collected from the detached cottages of the peasantry of the Jura.

"To combine individual effort, to render parts capable of combination into a whole, to economize time, and thus virtually to lengthen the lives of those whose exertions are valuable in the cause of science, may be considered as humble, yet surely most important, contributions to its advancement. We shall have little reason to regret the want of a National Institute, whose existence is the just subject of pride to our continental neighbours, so long as individual exertion can supply the stimulus which even the sunshine of wealth and patronage has sometimes failed to excite."

The Members reassembled at the same time and place on the evenings of Tuesday, Wednesday, Thursday, and Friday. At these meetings lectures on various subjects of science were delivered by Dr. Robinson, Dr. Lardner, Dr. Buckland, Professor Sedgwick, and Mr. Whewell. On each evening the Chairmen or Secretaries of the Sectional Committees reported the proceedings which had taken place during the morning in their respective sections. In concluding the last report of the transactions of the Medical section, Dr. Abercrombie said, "The whole business of the section, Sir, has been conducted in the most satisfactory manner, and a great variety of important communications has been laid before it; but considering these as not adapted in general for a mixed audience, I have alluded to them in very few words; having, therefore, intruded but little upon your time, I trust you will indulge me with your attention while I express in the name of the medical profession of Edinburgh the high satisfaction we have received from the meeting which is now drawing to a close, especially by having been brought into personal intercourse, and, I trust, personal friendship with so many distinguished individuals whose names have been long familiar to us as holding the highest rank in science. From their combined exertions we expect the most valuable results to every department of human knowledge. I am none of those who anticipate from the researches of physical science anything adverse to the highest interests of man as a moral being. On the contrary, I am convinced that those who have made the greatest advances in true science will be the first to acknowledge their own insignificance when viewed in relation to that incomprehensible One who guides the planet in its course, and maintains the complicated movements of ten thousand suns and ten thousand systems in undeviating harmony. Infidelity and irreligion, I am satisfied, are the offspring of ignorance united to presumption; and the boldest researches of physical science, if conducted in the spirit of true philosophy, must lead us but to new discoveries of the power, and wisdom, and harmony, and beauty which pervade all the works of Him, who is eternal."

The last General Meeting was held on Saturday the 13th of September, at 3 P.M., in the hall of the College Library. The General Secretary reported the Proceedings of the General Committee, the time and place appointed for the next meeting, the names of the Officers and Council who had been elected, the objects and extent of the votes of money for promoting experiments and investigations, the number and nature of the reports solicited from men eminent in science, and the recommendations of special subjects for research.

The thanks of the Association were then voted to the following bodies and individuals:—

On the motion of the Rev. Dr. Buckland, seconded by the Rev. Dr. Lloyd,—To the University of Edinburgh, its Patrons and Officers, for the ample accommodation afforded to the meeting in the College;

On the motion of Lord Greenock, seconded by Prof. Forbes,—To the Royal College of Physicians and the public bodies who have given or offered the use of their premises to the Association;

On the motion of the Rev. Baden Powell, seconded by G. B. Greenough, Esq.,—To the proprietors of the Assembly-rooms, who have given the gratuitous use of their spacious premises, and fitted them up in a splendid manner for the reception of the Association;

On the motion of R. I. Murchison, Esq., seconded by the Rev. Prof. Sedgwick,—To the Highland and Agricultural Society of Scotland.

Mr. Murchison stated that this Society was eminently entitled to the thanks of the Association for their liberal and zealous endeavours to promote the geology of their country. Intimately persuaded that the agricultural and mineral resources of Scotland would be improved by an increased knowledge of its subsoil and rocks, the Highland Society had advanced premiums for the completion of a geological map of Scotland, and had exerted themselves to obtain possession, through His Majesty's Government, of the valuable unpublished documents of Dr. MacCulloch, which, without these spirited efforts, might long have lain dormant.

On the motion of the Rev. Wm. Whewell, seconded by Prof. Hamilton, thanks were voted—To the President (Sir Thomas Brisbane,) and the Vice-Presidents (Sir David Brewster, K. H., and the Rev. Dr. Robinson);

On the motion of the Rev. W. V. Harcourt, seconded by Prof. Phillips,—To John Robison, Esq., and Prof. Forbes, Secretaries for the Edinburgh meeting;

On the motion of John Taylor, Esq., seconded by John Robison, Esq.,—To the Treasurer and the Committee of Reception,

Mr. Charles Forbes, } *Treasurer,*

Dr. Christison,
Dr. Borthewick,
Mr. Cay,
Mr. Craigie,
Mr. Burt, } *Committee of Reception,*

and other gentlemen who gave their valuable aid in making arrangements for the meeting;

On the motion of Prof. Sedgwick, seconded by Lord Brougham, —To M. Arago and other distinguished foreigners who have honoured the meeting with their presence on this occasion;

The Lord Chancellor said, "I rise to second the motion conveying thanks to these most illustrious men. The high honour of being called upon to perform this duty I owe, not, certainly, to any service I have done to this Association, because this your last day of meeting is (owing to an accident of a domestic nature, which retarded my journey,) the first of my appearing here. I owe it to the circumstance of having the honour, the very undeserved honour (but yet one of the proudest of my life), to be a member of the National Institute of France, and the friend of the distinguished philosopher whose name is mentioned in this motion. Gentlemen, allow me to say that I look upon this as one of the most important and unquestionable of all the benefits this Association is calculated to bestow—that it brings together men of science from every quarter of the world. The benefits of this are great to science; but they are great, also, to society; for in proportion as men know one another, they are the more disposed to cultivate habits of friendly intercourse, especially if their intimacy subsists on grounds so mutual as science: for they who devote themselves to science are of no country; over them the angry blast and tempest of war rages innocuous; the pursuits in which they unite are naturally favourable to that greatest of all objects which human rulers ought to have in mind, I mean the maintenance of peace and goodwill among men. It has sometimes been remarked, that war is a game at which, if the people were wise, governments would not often play; and it has also been said of men, that the longer they live the more clearly they see that life is too short to be spent in personal quarrels; it is the same with nations: the world is growing too wise and experienced to bear war. As there is no duty more sacred and imperative on the part of governments than to promote, by every means, that peace which ought to bind the great family of mankind together in all its departments and institutions, so I hold, that whatever brings men into contact on such mutual ground as science tends to facilitate the task of rulers, and makes it easy to keep at peace with neighbouring states. I beg leave, therefore, both on scientific principles and also on the principles of universal philanthropy, most heartily to second the motion."

On the motion of the Rev. Dr. Robinson, seconded by Sir Charles Lemon, thanks were voted to the Rev. William Vernon Harcourt for his continued and unremitting exertions as General Secretary.

The President, in closing the meeting, said, that it had been his good fortune to attend all the former meetings of the Association at York, Oxford, and Cambridge, and he was rejoiced to think that Scotland had not fallen short in the reception which it had given them on the present occasion; he had himself shared in the benefit of those hospitable feelings with which the Association had been welcomed, having had the honour that day of receiving with several distinguished individuals the freedom of the City of Edinburgh*. "The eminent foreigners," he added, "who have attended the meeting have all expressed their desire to assist in promoting the objects of the Association, and I have been requested by my illustrious friend M. Arago, whom I have had the happiness of knowing for nineteen years, to assure them of his own willingness and that of the Institute of France to cooperate with them in every thing in which mutual assistance might be serviceable to the advancement of science." He then adjourned the meeting to the 10th of August, 1835, at Dublin.

GENERAL COMMITTEE.

The General Committee met in the apartments of the Royal Society on Monday the 8th of September, and came to the following Resolutions:

Rules.—That in Rule 2, respecting privileges of admission, for the words 'Fellows and Members of Royal and Chartered Societies', be substituted the words 'Fellows and Members of Chartered Literary and Philosophical Societies publishing Transactions'.

That grants of pecuniary aid for scientific purposes from the funds of the Association shall expire at the meeting following that at which they were granted, unless they shall have been acted upon, or a continuation of them ordered by the General Committee.

Committee of Recommendations.—That Mr. W. Baily, Sir D. Brewster, Dr. Brown, Rev. Dr. Buckland, Rev. G. Peacock, Professor Forbes, Professor Hamilton, Professor Roget, Dr. Turner, Rev. W. Whewell, Dr. Richardson, and the Rev. W. V. Harcourt be a Committee to report in what manner the funds of the Society may be best appropriated to the promotion of sci-

* At an extraordinary meeting of the Town Council, held on Saturday, the 13th of September, diplomas of the freedom of the City of Edinburgh were presented by the Lord Provost to the following Members of the Association: Sir Thomas Brisbane, M. Arago, Professor Moll, Dr. Dalton, and Dr. Brown.

ence, and what reports on the state and progress of science it is desirable to obtain, directing their attention especially to the recommendations of the Committees of Science.

That this Committee have power to add to their number the names of any Members of the Association whose assistance they may desire.

That this Committee be directed to communicate with M. Arago, and to report whether there are any scientific objects which may be advanced by the cooperation of the Association with the Institute of France.

That the Committees for Science be requested to communicate to the Committee of Recommendations any suggestions they may think useful respecting particular scientific objects which might be advanced by the appropriation of the funds of the Association, and particular departments of science on the state and progress of which reports are wanted.

Committees of Science.—That the Members of the Committees of Science established at Cambridge be considered as the Sectional Committees of this meeting, with power to add to their number from the Members of the General Committee.

Two Secretaries were appointed to each Committee.

Corresponding Members.—That MM. Arago, Quételet, Œrsted, and De la Rive be elected Corresponding Members of the Association.

That the Council be empowered to add to the list of corresponding members the names of foreigners eminent in science, and desirous to cooperate in the objects of the Association*.

The Committee met from day to day for the election of Members.

On Saturday, the Committee of Recommendations having made their report, after receiving from the Treasurer an account of the state of the funds, the General Committee adopted the recommendations and allowed the grants of money therein contained.

Letters of invitation to the Association having been received from the Bristol Institution, the Literary and Philosophical Society of Liverpool, the Royal Dublin Society, the Royal Irish Academy, the Geological Society of Ireland, the University of Dublin, —It was resolved, That the next meeting of the Association be held in Dublin, and that the thanks of the Association be returned to the scientific institutions from which invitations have been received.

* In consequence of this resolution Baron Humboldt, Professor Berzelius, Professor Schumacher, Professor Agassiz, and Professor Moll were elected Corresponding Members of the British Association.

That the Council be instructed to make such arrangements that the Sections may be enabled to meet for scientific business on the morning of the second Monday in August.

That the salary of the Assistant General Secretary be increased to the sum of 200*l.* per annum, to enable him to attend at the places of meeting for the purpose of making arrangements previous to the assembly of the Association, and to bear such expenses as the Council may think proper to indicate.

Officers and Council elected.

President elect.—Rev. Dr. Lloyd, Prov. of Trin. Coll. Dublin.

Vice-Presidents elect.—Lord Oxmantown. Rev. W. Whewell.

Secretaries for Dublin.—Professor Hamilton. Professor Lloyd.

Treasurer.—John Taylor.

General Secretary.—Rev. W. V. Harcourt.

Assistant General Secretary.—Professor Phillips.

Council.—Professor Airy. Rev. Dr. Buckland. Dr. Brown. G. Bentham. William Clift. Professor Christie. J. E. Drinkwater. Geo. B. Greenough. Dr. Hodgkin. John W. Lubbock. G. Rennie. Rev. G. Peacock. Dr. Roget. William Yarrell. *Ex officio,*—The Trustees (Professor Babbage, R. I. Murchison, John Taylor,) and the Officers of the Association.

Secretaries.—Dr. E. Turner. Rev. J. Yates.

Members of Committees of Sciences elected.

I. *Mathematics and General Physics.*

Chairman.—Rev. W. Whewell.

Deputy Chairmen.—Rev. Dr. Lloyd. Rev. Dr. Robinson.

Secretaries.—Professor Forbes. Professor Lloyd.

Committee.—M. Arago. F. Baily. Sir David Brewster. Sir Thomas Brisbane. Rev. J. Bowstead. E. J. Cooper. Lieutenant Drummond. Rev. R. Greswell. Professor Hamilton. Thomas Henderson. William Hopkins. Dr. Jackson. Dr. Knight. Rev. Dr. Lardner. Professor Moll. Rev. R. Murphy. Lieutenant Murphy. Rev. G. Peacock. Rev. Dr. Pearson. Professor Powell. John Ramage. G. Rennie. Rev. Dr. Robinson. John Robison. Professor Stevelly. Professor Thomson. Professor Wallace. W. L. Wharton. Charles Wheatstone.

II. *Chemistry and Mineralogy.*

Chairman.—Dr. Hope.

Deputy-Chairmen.—Dr. Dalton. Dr. Thomas Thomson.

Secretaries.—Mr. Johnston. Dr. Christison.

Committee.—Dr. Daubeny. Dr. Turner. Dr. Lloyd. Rev. W. V. Harcourt. Thos. J. Pearsall. William Hatfeild. Professor Traill. Dr. William Gregory. Dr. Thomas Clark. Professor Graham. Arthur Connell. Luke Howard. Charles Tennant. Charles Mackintosh. William West. Richard Phillips.

III. *Geology and Geography.*

Chairman.—Professor Jameson.

Deputy-Chairmen.—Lord Greenock. G. B. Greenough.

Secretaries.—Professor Phillips. T. Jameson Torrie. Rev. J. Yates.

Committee.—Rev. Dr. Buckland. Dr. Boase. J. Bryce. Major Clerke. Rev. Professor Sedgwick. Colonel Silvertop. H. T. Witham. William Smith. John Taylor. W. C. Trevelyan. R. I. Murchison. William Hutton. Charles Lyell. L. Horner. J. B. Pentland. R. J. Griffith. William Copland. Dr. Hibbert. R. Stevenson. Lieut. Murphy. William Clift. Sir Thomas Dick Lauder. Sir George Mackenzie. Rev. Dr. Fleming. Dr. Traill. Captain Maconochie. Henry Woolcombe. S. P. Pratt. M. Agassiz. William Nicol. Rev. W. Turner.

IV. *Natural History.*

Chairman.—Professor Graham.

Deputy-Chairman.—Sir William Jardine, Bart.

Secretaries.—William Yarrell. Professor Burnett.

Committee.—G. A. Walker Arnott. M. Agassiz. Rev. Dr. Adam. C. Babington. Dr. Robert Brown. W. Christy. Dr. Coldstream. Allan Cunningham. John Curtis. David Don. P. B. Duncan. Dr. R. Dickson. Dr. Daubeny. Rev. L. W. P. Garnons. Dr. Greville. B. D. Greene, Boston, U.S. Professor Henslow. Professor Hooker. Rev. Dr. Hincks. Professor Jameson. Rev. L. Jenyns. —— Mackay. Dr. Richardson. Commander Ross. J. F. Royle. P. J. Selby. Colonel Sykes. W. Spence. Richard Taylor. Professor Treviranus. William Thompson. Dr. Wasse. James Wilson.

V. *Anatomy and Medicine.*

Chairman.—Dr. Abercrombie.
Deputy-Chairmen.—Sir Charles Bell. Professor Clark.
Secretaries.—Dr. Roget. Dr. William Thomson.
Committee.—Dr. Alison. Dr. Arnott. Sir G. Ballingall. S. D. Broughton. Dr. J. Campbell. Professor Clark. William Clift. Dr. Davidson. Dr. Hodgkin. Dr. Holme. Dr. Home. Dr. Maclagan. Dr. Roget. James Russell. Dr. Thomson. Dr. A. T. Thomson. Dr. Wm. Thomson. Professor Treviranus. Dr. Turner. Dr. Yelloly.

VI. *Statistics.*

Chairman.—Sir Charles Lemon, Bart.
Deputy-Chairmen.—Colonel Sykes. Benjamin Heywood.
Secretaries.—Dr. Cleland. C. Hope Maclean.
Committee.—Howard Elphinstone. Rev. E. Stanley. J. E. Drinkwater. Rev. W. Whewell. The Earl Fitzwilliam. Sir John Sinclair, Bart. Sir Thomas Acland, Bart. John Kennedy. Captain Churchill. R. I. Murchison. John Whishaw. Dr. Chalmers. L. Horner. John Marshall. Neil Malcolm. Francis Clark. Major Shadwell Clerke. George William Wood. Right Hon. Lord Jeffrey. John Gordon. Sir Henry Jardine. Right Hon. Holt Mackenzie. Rev. Dr. Henry Duncan. Dr. Brunton. Rev. Peter Chalmers.

COMMITTEE OF RECOMMENDATIONS.

Meetings of this Committee were held on Thursday, Friday, and Saturday, for the purpose of conferring with M. Arago, and considering and revising the recommendations to be submitted to the General Committee.

M. Arago, having been requested to state his views as to any points on which it appeared to him that it might be useful for the British Association to cooperate with the Institute of France, noticed in particular the great advantage which might be expected to accrue to magnetical science from the establishment of observatories furnished with adequate instruments, and under the superintendence of a competent observer, throughout the extensive possessions of the British empire, and dwelt upon the necessity of arranging magnetical observations upon a uniform and well-approved plan. He also spoke of the value of more extensive and systematic observations on the tem-

perature of the earth and springs at small and great depths from the surface, and mentioned some of the sources of error in such researches, and the means of obviating them*.

The Committee came to the following Resolutions:

That it be represented to the Government of this country that the British Association conceive it would be of great service to science, if magnetical and meteorological observatories were established in several parts of the earth, furnished with proper instruments well constructed on uniform principles, and if provision were made for careful and continued observations at those places;—that in Great Britain and its colonies there are points favourable for such observations; and that it is the more desirable that the British nation should take a part in carrying them on, since a system of similar observations, as the Association is informed, has begun to be established in France and its dependencies.

That Mr. Baily, Mr. D. Gilbert, Mr. Lubbock, and the Rev. G. Peacock be a Committee to make the required representation to the Government, and to solicit the cooperation of the French Institute.

That the East India Company be requested to further the same objects, especially at their establishment at Madras.

That notice be given that any persons who may be able to obtain the temperature of the air, water, and rock, in mines and borings of known depth, or the indications of thermometers sunk to different depths, in different kinds of soil and in different parts of the earth, are requested to make known their names and the places where they have this opportunity, in order that they may receive instructions for making such observations, and communicating the results to the Association.

That Mr. Taylor, Prof. Forbes, Prof. Powell, Mr. R. Fox, Mr. Lubbock, Dr. Dalton, Rev. Dr. Robinson, Prof. Christie, Prof. Lloyd, and Prof. Phillips be a Committee, with power to give instructions and to make arrangements on the subject of the thermometrical observations recommended in the above resolution, and that 100*l.* be placed at their disposal for these objects†.

That M. Arago be respectfully requested, with the least possible delay, to publish, and to have reduced, his valuable and extensive collection of magnetical observations made at the observatory at Paris.

* On the subject of Artesian wells, see the *Annuaire* for 1835.

† The Committee has taken steps to have instruments constructed suitable for the experiments in mines, wells, &c., and to have sufficient instructions conveyed to persons who will undertake the researches required at selected points in various parts of the country.

M. Arago expressed his readiness to comply with the request of the Committee as soon as it should become practicable; and stated that the immense collection referred to (amounting to more than 100,000 careful observations, and relating to nearly all parts of magnetical science,) had been some time since destined for publication, but that the printing of them had been postponed in consequence of an application which had come from England for the cooperation of France in furnishing data for the improvement of the theory of the tides. When Mr. Lubbock applied to the Bureau des Longitudes, through M. Poisson, for the loan of the manuscript observations on the tides at Brest, it was decided, at the earnest recommendation of M. Arago, that an object in which other nations were thus taking interest should have the preference given to it; that the observations on the tides at Brest should be printed at the expense of the French Government, and that copies should be furnished to those persons in foreign countries who were ready to use them.

The Committee then proceeded to receive and revise the recommendations laid before them by the Committees of Science.

COMMITTEES OF SCIENCE.

The several Committees met daily at 10 A.M., to arrange the business of the Sections and to determine on the recommendations which were to be presented to the Committee appointed to receive them.

Committee for Mathematical and Physical Science.

The Committee reported on the part of the Sub-Committee for discussing observations on the tides (see vol. ii. p. 471.), that the discussion of the tide observations, and the formation of tide tables, was in considerable progress, and would be continued with all practicable expedition. That the sum of 50*l.* only, out of the 200*l.* appropriated by the British Association for this purpose, had at present been paid by the Treasurer; but that it was probable that the whole of the sum so appropriated might be eventually required.

The gentlemen of the Athenæum of Liverpool, on being applied to on the part of the British Association, with great liberality and kindness sent the original manuscript of Mr. Hutchinson's observations to Mr. Dessiou of the Admiralty, who was engaged by the Sub-Committee to discuss them. These observations are now undergoing the requsite calculations.

The Corporation of Liverpool also, at the suggestion of the Sub-Committee, have established two sets of apparatus for the purpose of making tide observations at Liverpool, the one at the Clarence Docks, the other at the Black Rock. Mr. Yates of West Dingle, and Lieut. Drummond, R.N., the director of the Ordnance Survey of the coast in that neighbourhood, have given much valuable aid to the objects of the Sub-Committee.

The Committee reported on the part of the Sub-Committee for superintending the reduction of the Observations of Bradley, Maskelyne, and Pond, on the Sun, Moon, and Planets, made at Greenwich, (see vol. ii. p. 469.), that the reduction of these observations was in progress; that the Royal Society had contributed a copy of Maskelyne's Observations for that purpose;—That the Syndicate of the University Press at Cambridge had given the paper and press-work for the printed forms requisite for the calculations;—That one calculator had been employed since the beginning of March, in which interval the transits have been taken out of Bradley and Maskelyne; the means of the wires have also been deduced, some advances made towards completing the imperfect transits, and considerable progress in preparing the apparent right ascensions of the fundamental stars;—that a temporary stop was put to a portion of the work, in consequence of a very severe illness with which Professor Airy was attacked during the last summer, but that the work was again proceeding with as much expedition as possible.

The Committee reported the following Recommendations:

1. That it is desirable that the Constant of Lunar Nutation should be deduced from observations made with the mural circle at Greenwich.

2. That it is expedient that the sum of 100*l.* be appropriated to the purpose above mentioned by the British Association; and that Sir Thomas Brisbane, Rev. Dr. Robinson, and Mr. Baily be requested to superintend the deductions.

3. That it is desirable that the Standard Scale made some years ago by Mr. Troughton for the town of Aberdeen should be compared with the Standard Scale recently made for the Royal Astronomical Society; and that application should be made by the British Association to the magistrates of that town for the loan of the same for the purpose above mentioned.

4. That Mr. Baily be requested to make the requisite comparisons, provided the loan of the Scale can be obtained*.

5. That the Difference of Meridians between the Observatories of Greenwich, Cambridge, Oxford, Edinburgh, Dublin, and Ar-

* The scale has been received, and is under examination by Mr. Baily.

magh should be determined by means of chronometers, or by signals, or by both methods, and that application be made to Government for their assistance in accomplishing this object. That the Astronomer Royal, Dr. Robinson, Prof. Airy, Prof. Rigaud, Prof. Henderson, Prof. Hamilton, Sir Thomas Brisbane and Lieut. Drummond be requested to carry this recommendation into effect.

6. That Mr. W. Gray, jun., and Prof. Phillips be requested to continue their experiments on the Quantities of Rain falling on the top of York Minster and other adjacent stations.

7. That Mr. Peacock be requested to continue his Report on certain branches of Analysis for the next meeting.

8. That Mr. Whewell be requested to execute for the next meeting the Reports on the Mathematical Theories of Heat, Electricity, and Magnetism.

9. That Mr. Challis be requested to proceed with his Report on the Mathematical Theory of the Motion of Fluids.

10. That Mr. Rennie be requested to proceed with his Report on Practical Hydraulics.

11. That Mr. Willis be requested to prepare his Report upon Acoustics for the next meeting.

Committee for Chemical and Mineralogical Science.

The Committee reported that they had received statements of the progress of the experiments on the specific gravity of certain gases, and on the effects of long-continued heat on mineral and organic substances, for which sums of money had been appropriated at a former meeting, and recommended the continuance of those appropriations.

They reported also the following Recommendations:

1. That Mr. Graham be requested to submit to further investigation the amount of security to be derived from the Safety Lamp.

2. That Mr. Graham and Dr. Williams be requested to investigate further the phænomena of Low Combustion.

3. That a sum of 10*l.* be placed at the disposal of Mr. Johnston to defray the expense of preparing a specimen of Chemical Constants, in conformity with the suggestions of Mr. Babbage.

4. That Dr. Dalton, Dr. Hope, Dr. T. Thompson, Mr. Whewell, Dr. Turner, Prof. Miller, Dr. Gregory, Dr. Christison, Mr. R. Phillips, Mr. Graham, Prof. Johnston, Dr. Faraday, Prof. Daniell, Dr. Clark, Prof. Cumming, and Dr. Prout be appointed a Committee, to report to the next Meeting their opinion on the adoption of an uniform set of Chemical Symbols; with power to add to their number. Dr. Turner to be Secretary.

5. That Dr. Roget be requested to report on the progress of

Electro-chemistry and Electro-magnetism, so far as regards the experimental part of the subject.

6. The Committee recommended the researches commenced by Sir David Brewster into the Optical properties of Minerals to the attention of chemists.

Committee for Geology and Geography.

The Committee reported the following Recommendations:

1. That Mr. Stevenson be requested to complete the Report of the relative level of land and sea, and on the waste and extension of the land, which he has presented to this Meeting.

2. That with a view to perfect our knowledge of the Fossil Ichthyology of the British islands, a sum not exceeding 105*l.* be paid by the Treasurer to Dr. Buckland, Prof. Sedgwick, and Mr. Murchison, to be applied for the purpose of assisting M. Agassiz in carrying on his Ichthyological work.

3. That the recommendations relating to the veins and sections of Flintshire, the heaves of Cornwall, the quantity of mud and silt, the experiments of Mr. G. Watt, and the desiderata noticed by Mr. John Taylor and Mr. Conybeare, and the sixth and tenth queries, be repeated.

4. That a sum not exceeding 20*l.* be placed at the disposal of Mr. J. Yates and Mr. G. Rennie, for the purpose of the experiments on the quantity of mud and silt in rivers.

5. That evidence should be collected as to the direction and probable sources from which drifted blocks and pebbles, referrible to rocks not existing in the neighbourhood where they now occur, whether in insulated masses, or in beds of superficial gravel, may have been derived *.

6. That evidence should be collected as to the form and direction of hills or ridges of superficial gravel, and the sources whence the materials of such gravel hills may have been transported to their present place.

7. That observations should be made on the direction and depth of grooves and hollows, such as are often found on the faces of hard rocks and beneath superficial deposits of drifted clay and gravel not referrible to the action of any existing currents.

8. The Committee further reported that it appeared to them that the advancement of various branches of science is greatly retarded by the want of an accurate map of the whole of the British Islands:—that it be recommended to the Council to consider of the propriety of representing this opinion to His Ma-

* The Assistant-Secretary has forwarded to persons known to possess information on these subjects a circular, of which copies may be had on application to him.

jesty's Government, with a view of expediting the completion of the still unfinished or unpublished parts of the Ordnance Survey*.

* In consequence of this recommendation the following Memorial, upon the state and progress of the Ordnance Survey of Great Britain, was presented to the Chancellor of the Exchequer on the part of the Association, by a deputation from the Council:

MEMORIAL.

"The Trigonometrical Survey of Great Britain, conducted by men of high scientific attainments, commenced its operations in 1798, with a view to the construction of a general map, and in 1805 the first sheets of that work were published. Of one hundred and eight sheets required to form the whole map of England sixty-five only have yet been published, at which rate of progress thirty years would elapse before the survey could reach the banks of the Tweed. Now, although from the exertions recently made in this department, the rate of publication has been accelerated, yet, on reference to the highest authorities on this subject, no prospect is held out, even upon the present improved system, that the desired result can be attained in less than ten years, after which the entire map of Scotland will remain to be constructed.

"Your memorialists conceive that this simple statement of the condition and future prospects of the Survey might in itself be a sufficient reason to induce Parliament to increase the grant allotted to this branch of public service. But to place the evil complained of in a clear light, they venture to submit to you the following considerations.

"Urgent calls for the acceleration of this Map are made by many proprietors of land and mines both in the North of England and in Scotland, who contend that in the construction of rail-roads, canals, or other public works, that portion of the kingdom is subjected to great expenses and difficulties from the want of it. In forming the Western rail-road from London to Bristol an outlay of several thousand pounds in surveying has been saved by the possession of those portions of the Map which are published, whilst the correctness of the physical features laid down upon them has enabled the engineer at once to select his line of operations, and thus to gain at least a year of time in the commencement of the work. Similar results have been obtained in Ireland, in forming the Ulster canal, in consequence of the publication of the Ordnance Map of that country. Another important benefit will be conferred upon the public by the completion of this Map, in the correction of the coast surveys, determining the precise position of headlands and form of bays; a point of considerable moment in the northern parts of this maritime country, where the outline of the coast is broken and dangerous. In illustration of this it may be mentioned, that in the progress of the *yet unpublished* parts of this Survey, errors of position in the most accredited charts of this coast have been detected to an extent in one instance of eleven miles!

"Your memorialists particularly invite attention to the fact, that although a very large portion of the expense relating to the *Scottish survey* has been incurred, not only in establishing the great triangulation, but also in minutely and accurately surveying a large portion of the South-west of Scotland, the materials so collected are now, they believe, laid by in the archives of the Map-office, without the prospect of their being made available for many years; whilst it must be observed that the knowledge thus locked up relates to one of those tracts of the empire where its diffusion would prove of singular advantage. Upon this head, indeed, it can be shown that the delay is not only a negative but a positive evil, in as much as, but for the conviction that many years could not elapse between the execution of this Survey and its publication, the inhabitants themselves would have endeavoured to improve the maps.

"In this backward state of a national geographical survey, Great Britain

Committee for Natural History.

The Committee reported the following Recommendations:

It was resolved,

1. That Mr. James Wilson be requested to report on the present state of our knowledge of the geographical distribution of Insects, particularly *Coleoptera.*

2. That Dr. Richardson be requested to prepare a Report on the state of our knowledge of the Zoology of North America.

stands almost alone among the civilized nations of Europe, whilst it is obvious that in no country can the perfection of its maps be more imperiously called for. The trigonometrical survey of Austria is completed as respects the Tyrol, the Eastern Alps, Bohemia, and Austria Proper.

"Prussia has nearly completed her survey.

"France, though possessing the elaborate maps of Cassini, has still deemed it essential to institute a new survey of her whole dominions, which is now going on in so vigorous a manner, that though only commenced in the year 1828, there is every reason to suppose that the whole will be finished long before the British survey (at its present rate of progress) will have been completed.

"Bavaria holds forth an example highly worthy of imitation. Her survey, commencing in 1819, has made such rapid progress that out of one hundred sheets to illustrate her territories sixty-three have been already published, and the whole work will be terminated in six years, and this too upon a scale of *three* inches to a mile.

"Now in none of these countries is there the hope that such expenditure of public money can be repaid, whilst in England and Scotland there are many districts where the *sale* of the Trigonometrical Survey *will go far towards repaying* the cost of production.

"Though deeply sensible of the advantages which must accrue to *physical science* from the diffusion of these maps, seeing that the published portions of them have already enabled the geologist to develope with precision the mineral structure of large tracts of England, your memorialists solely avoid dwelling upon this important point because the subject requires more explanation than can be well condensed into a short memorial.

"Anxious for the progress of science, and its application to national uses in every portion of the United Kingdom, your memorialists have had their attention the more powerfully attracted to the languid condition of the Ordnance Survey of Great Britain, by the contrast which it presents to the active manner in which the survey of Ireland is now conducted; for whilst they rejoice that this important object is there so munificently supported as to admit of the rapid publication of a map constructed upon a scale of *six* inches to a mile, they must at the same time deplore, in regard to some of the most valuable tracts of England and Scotland, that a survey upon a scale of only *one* inch to a mile is making such feeble progress.

"Your memorialists therefore trust that His Majesty's Government will suggest to Parliament the propriety of an adequate grant for the acceleration of a work in which so many public interests are involved, and they feel confident that enlightened men of all political parties will unite in the support of such a truly useful and national measure.

"By order of the Council of the British Association
for the Advancement of Science,

"May 28, 1835. (Signed) "ROD. I. MURCHISON,
"*Chairman.*"

3. That Dr. Greene and Dr. Hooker be requested to prepare a Report on the state of our knowledge of the Botany of North America.

4. That the Zoological Queries introduced in last year's Recommendations be continued, except the 6th and 7th.

5. That the Botanical Inquiries be continued.

6. As a full and arranged Catalogue of the works on Natural History (including Memoirs, &c., in Journals and Transactions,) would greatly facilitate the study of that branch of science, it is recommended that at the next meeting of the Association a Committee be appointed for devising the means of forming and publishing such a catalogue; and that in the mean time, to aid the labours of that Committee, gentlemen who have devoted themselves to the study of particular departments of natural history be earnestly requested to send in to the Assistant-Secretary lists of the works, memoirs, &c., relating to such departments.

Medical Committee.

The Medical Committee reported that the sum of 25*l.* was placed at the disposal of Dr. Marshall Hall and Mr. Broughton for the investigation of the subject of the Sensibilities of the Nerves of the Brain; that these gentlemen have presented a report, which has been read and highly approved; that their experiments are not yet complete, but they do not ask for any further grant for the prosecution of them. (*Report received.*)

They further reported that a sum of 25*l.* was placed at the disposal of Dr. Roupell and Dr. Hodgkin for prosecuting an inquiry into the effects of poisons on the animal œconomy; that an interim report has been read from these gentlemen who are prosecuting the inquiry, and that they do not at present ask for any further grant.

The Committee recommended, as an important object of inquiry, the anatomical relations of the absorbent and venous systems in the different classes of animals, to be illustrated by injected preparations and graphic representations.

The Committee, considering the contradictory results obtained by the distinguished anatomists who have prosecuted this subject of investigation, recommended that two Sub-Committees be appointed for prosecuting the inquiry, the one to sit at Edinburgh and the other in London.

The Edinburgh Sub-Committee to consist of Dr. Allen Thompson, Dr. Alison, Dr. Fletcher, Dr. Sharpus, Dr. Hardyside, Dr. Reid, Mr. Mackenzie and Mr. Dick; and the London Sub-Committee to consist of Dr. Hodgkin, Dr. Roget, Dr. Clark of Cambridge, Mr. Bracey Clark, Mr. Clift and Mr. Broughton, with

power to add to their numbers. They further recommended that a sum not exceeding 25*l.* should be placed at the disposal of each Sub-Committee for assisting the prosecution of such researches.

The Committee recommended the prosecution of inquiries on the pathology of the Nervous System; on the successive motions of the different parts of the heart, and the sounds which accompany them. Three Committees were named for the prosecution of these researches in London, Edinburgh, and Dublin.

The Committee recommended the appointment of Medical Sub-Committees, to communicate with the Statistical Committee of the Association, or with the Statistical Society in London, relative to a registration of deaths, comprising particulars of a medical nature, with the view that if any legislative measure should hereafter be adopted as to registration, such suggestions may be offered by the Association as may seem best fitted to attain the requisite information for this desirable object. Two Committees were named, one in London and the other in Edinburgh. London: Drs. Yelloly, Bright, Roget, Bisset Hawkins, and Clark, 6, George Street, Hanover Square. Edinburgh: Drs. Abercrombie, Traill, Christison, W. Thomson, and Alison*.

The Committee recommended that Dr. Christison be requested to draw up a Report on the circumstances in vegetation which influence the medicinal efficacy of plants.

Statistical Committee.

The Committee recommended that a Sub-Committee should be formed, who should associate with themselves certain gentlemen connected with the conduct and publication of the new Statistical Account of Scotland, to be named by that body for the purpose of drawing up a set of queries by which more minute information on statistical subjects than that hitherto received may be obtained, and that the Committee be authorized to defray the expense which may attend the printing of the queries.

That Mr. Taylor be requested to draw up a series of questions upon the condition and habits of the mining population of Cornwall and Wales, with a view to obtain a complete account of the statistics of that class.

The Committee reported that in pursuance of a recommendation of the Association, Professor Jones had applied for leave of access to the archives of the East India Company, and that that body, with its accustomed liberality, had afforded him every facility in prosecuting his researches.

* These Committees have been for some time in operation.

APPROPRIATION OF FUNDS,

At the instance of the Committee of Recommendations: £.

For the prosecution of Thermometrical Observations at various depths from the surface, under the direction of a Committee named for that purpose . . . 100

On the recommendation of the Committee for Mathematical and Physical Science:

For determining the Constant of Lunar Nutation from the Greenwich Observations 100

For discussing Observations of the Tides in order to improve the Tide Tables (*vote of last year continued and enlarged*) 250

For the construction of a telescopic lens of rock salt, (*vote of last year enlarged*) 80

On the recommendation of the Committee for Chemical and Mineralogical Science:

For the execution of a specimen of chemical constants on the plan of Professor Babbage 10

For experiments on the effects of long-continued heat on mineral and organic bodies, (*vote of last year continued*) 50

For determining the specific gravity of hydrogen and other gases (*vote of last year continued*) 50

On the recommendation of the Geological and Geographical Committee:

For advancing our knowledge of British fossil Ichthyology 105

For experiments on the quantity of mud transported by rivers 20

On the recommendation of the Committee of Anatomy and Physiology:

For experimental investigations on the effects of poisons on the animal economy, (*vote of last year continued*) 25

For investigating the relations of the absorbent and venous systems 50

For defraying certain expenses incurred in the execution of thermometrical observations at Plymouth by the late Mr. G. Harvey 20

£860

SECTIONAL MEETINGS.

The Sections assembled daily at eleven A.M., in the Class Rooms of the College, to hear the communications in different departments of science prepared to be laid before them by the secretaries of their respective committees.

The following is a list of the communications which were made to the meeting, divided into four classes: 1st, Reports on the state and progress of science, drawn up at the request of the Association; 2nd, Accounts of researches undertaken at the request of the Association; 3rd, Notices in answer to queries and recommendations proceeding from the Association; 4th, Miscellaneous communications.

I. *Reports on the State and Progress of Science, drawn up at the request of the Association.*

On the Geology of North America, Part I. By Professor Rogers.

On the State of our Knowledge of the Laws of Contagion. By Dr. Henry.

On Animal Physiology. By Dr. Clark, Professor of Anatomy, Cambridge.

On the recent Progress and present State of Zoology. By the Rev. L. Jenyns.

On the Theory of Capillary Attraction. By the Rev. James Challis.

On the Progress and present State of the Science of Physical Optics. By the Rev. H. Lloyd, Professor of Nat. Phil. Dublin.

On the Progress of Hydraulics considered as a Branch of Engineering: Part II. By George Rennie.

II. *Accounts of Researches undertaken at the request of the Association.*

Remarks on the relative Level of Land and Sea, &c. By Robert Stevenson, Engineer.

Results of a Second Series of twelve months' observations on the Quantities of Rain falling at different elevations above the ground. By William Gray, jun., and Professor Phillips.

Account of the institution of Experiments on the effects of long-continued Heat. By the Rev. W. V. Harcourt.

Account of researches in Crystallography. By Professor Miller.

Account of the progress of experiments on the nature of the Secretions from the Roots of Vegetables. By Dr. Daubeny, Professor of Chemistry and Botany, Oxford.

Notice of the progress made in the comparative analysis of Iron in the different stages of its manufacture. By Professor Johnston.

Notice of the progress made in determining the specific gravities of Oxygen, Hydrogen, and Carbonic Acid. By Dr. Dalton.

Account of researches on the effects of Poison on the animal œconomy. By Dr. Roupell and Dr. Hodgkin.

Account of researches on the Sensibilities of the Nerves of the Brain. By Dr. Marshall Hall and S. D. Broughton.

Account of the performance of a Chronometer with a Glass Balance-spring. By E. J. Dent.

Notice of the performance of an Instrument for ascertaining the quantities of mud transported by Rivers. By George Rennie.

III. *Notices in reply to Queries and Recommendations of the Association.*

On the electrical condition of Metalliferous Veins. By R. W. Fox.

On the peculiar circumstances attending certain Coal Districts in the midland counties of England. By R. I. Murchison.

On the direction &c. of Non-metalliferous Fissures. By Professor Phillips.

On the Limestone of Closeburn. By C. G. S. Menteath.

On the Beds inclosing the Hæmatite of Dalton. By Professor Sedgwick.

On the supposed Metamorphosis of Crustacea. By J. O. Westwood.

On the progress made in inquiries relative to the Secretions from the Roots of Vegetables. By Dr. Dunbar.

On the nature and quantity of the Gases given off from Thermal Springs. By Dr. Daubeny.

On the purity and specific gravity of Mercury, Dr. Thompson remarked that he considered the mercury as imported into this country to be pure, and the specific gravity assigned to it by Cavendish to be correct, as it agrees with recent determinations by Mr. Crichton, from experiments continued through a whole winter.

On products collected in Chimneys of Furnaces. By Mr. Lowe.

IV. *Miscellaneous Communications.*

Abercrombie, Dr. On the study of Mental Philosophy as a part of Medical Literature.
Adam, Rev. W. On a Sextant furnished with a Spirit Level, to be used at sea or land when the horizon is invisible.
Addams, R. On a phænomenon of Sound.
Adie, J. On the Expansion of Stone.
Agassiz. On the Fossil Fishes of Scotland.
—— On the recent genus *Salmo*.
Aitken, Dr. On the Motions of Blood in Mammalia.
Alison, Dr. On the Vital Powers of Arteries leading to inflamed parts.
Andrews, T. On certain Caves in Rathlin, &c.
Arago. Remarks on the methods of conducting experimental researches in Magnetism, especially for the detection of minute variations of Intensity and Direction.
—— Proposal of submitting M. Poisson's conclusions regarding the Change of Density near the Surface of Fluids to an experimental test, by the observation of the angle of the complete polarization of light at these surfaces.
—— On the hypothesis of Transversal Vibrations in Physical Optics, and the claims of Dr. Thomas Young as the first to propose it.
Arnott, G. W. On *Cocculus Indicus*.
Auldjo, J. Notice of a work of M. Rotindo on the Statistics of Naples.
Badnall. On Friction on Railways.
Bell, Sir Charles. Discourse on the Nervous System.
Blackadder. Notice of a Fossil Fish from Glammis.
Boase, Dr. Statement of his views on the question of the Stratification of certain primary Rocks.
—— On Fissures and Veins.
Boujou, Dr. Sur les rapports reciproques de la Médecine et la Philosophie.
Breen, Hugh. On a property of Numbers.
Brewster, Sir David. On Colours in the spaces of the Rainbow.
—— Experiments on the effects of Reflexion from the surfaces of Crystals when those surfaces have been altered by solution.
—— On a large specimen of Amber from Ava.
—— On the Optical Characters of Minerals.
—— On the Structure of Feathers.
Brisbane, Sir Thomas. Notice of a fact observed in registering the Fall of Rain.

Brisbane, Sir Thomas. Notice of Sand from New South Wales for the manufacture of Glass.
—— Notice of an Ephemeris of Halley's Comet by Mr. Rumker.
Brown, Dr. On the Plurality of Embryos in *Coniferæ*.
Brown, Capt. On *Pecten aspersus*.
Brunel. On the Construction of Arches without centering.
Bryce, W. J. On certain Caves in the North of Ireland.
Buckland, Rev. Dr. A Lecture on several remarkable Fossil Fishes and Reptiles, delivered at an Evening Meeting of the Association.
—— Notice of a fossil Marine Plant from the Red Sandstone near Liverpool.
Bushnan, Dr. On the detection of Worms in the Human Veins.
Challis, Rev. James. Theoretical explanations of some facts relating to the composition of the Colours of the Spectrum.
Christie, Professor. Description of a Meteorological Phænomenon.
Christison, Professor. Action of Water on Lead.
Clark, Dr. On the use of the Hot Air Blast in Iron-furnaces.
Clarke, Dr. (*deceased.*) On the Ventilation of Hospitals.
Cleland, Dr. On the Statistics of Glasgow.
Dalyell, J. G. On the Propagation of Scottish Zoophytes.
Dick, David. On the cementing the internal surfaces of Object-glasses.
—— On a new Suspension Railway.
Dick, William. On the use of the Omentum.
—— On the Elastic Tissue of animals.
—— Observations on the Tongue of the Chamæleon.
Drake. On the Change of Colour in the Elder.
Drinkwater, J. E. On the Origin of the Statistical Society of London.
Dunn, John. Description of a new Clinometer.
Fitzwilliam, Earl. On the details desirable in Statistical Reports relating to Agriculture.
Forbes, Professor. On a new Sympiesometer.
Graham, Professor. On Hydrated Salts.
Grant. On Tables of Insurance.
Graves, J. T. On Exponential Functions.
Gilbertson, William. On Marine Shells of existing species at various elevations near Preston.
Gordon, Alex. On the construction and uses of Polyzonal Lenses.
Greenock, Lord. On certain Coal Tracts in Scotland.

Greenock, Lord. Notice of the section of Trap and Sandstone in the Castle Hill, Edinburgh.

Greenough, G. B. On the Stratification of certain primary Rocks.

Gregory, Dr. W. Notice of various Organic Products.

—— Abstract of Reichenbach's discoveries.

Hailstone, Rev. J. On minute Oscillations of the Barometer.

Hall, Colonel. Account of excursions in Quito.

Hall, Elias. Exhibition of a model of the Geology of Derbyshire.

Hamilton, Professor. On Conjugate Functions.

—— On a General Method in Dynamics.

Harlan, Dr. Notice of some Organic Remains of the United States.

Hibbert, Dr. On the ossiferous beds in the Basins of the Forth, Clyde, and Tay.

Hodgkinson, E. Experimental Researches on Collision.

Howard, Luke. On the Quantities of Rain at different elevations.

Jameson, Professor. Notice concerning the Fossil Fishes of Scotland, and the geological age of the formations in which they occur.

Jardine, Sir William. Account of Fishes collected in Sutherlandshire.

Johnston, Professor. On Oxichloride of Antimony.

Jordan, T. B. On a construction of the Magnetic Needle.

Kemp, K. T. On the Liquefaction of the Gases.

Knight, Dr. On the Organic Remains in the Flints of Petershead, &c.

—— On a method of rendering visible the Vibrations of heated Metals.

Lardner, Rev. Dr. A Lecture on Professor Babbage's Calculating Machine, delivered at an Evening Meeting of the Association.

Lloyd, Prof. On a method of observing the Magnetic Needle.

Lowe, George. Exhibition of certain products obtained in Gas Works, &c.

Lyell, Charles. On the relative Level of the Land and Sea on the shores of Scandinavia.

—— On the Characters of Stratification in the discussion on Primary Rocks.

MacConnochie, Captain. Notice of a work by M. Guerry, Sur laS tatistique morale de la France.

MacDonnell, Dr. On the Pulse, and the variation of its quickness from various causes.

MacGillivray, W. On the Natural History of the Transition Ranges of Scotland.

—— Exhibition of drawings of the Vertebrate Animals of Great Britain and Ireland.

Maclaren, Charles. On the Geology of the Pentlands.

Milne, David. On the Geology of Berwickshire.

Murchison, R. I. On the Transition Formations of the Welsh Border.

Murphy, Rev. R. Notice of some recent electrical Experiments, by Mr. Snow Harris, on the retention of Electricity on the surfaces of bodies in vacuo.

Murphy, Lieut. Notice of the progress made in the Ordnance Survey of Ireland.

Murray. On Rates of Mortality.

Murray, J. On the cultivation of *Phormium tenax* in Scotland.

—— On the Chamæleon.

—— On the Ascent of the Sap.

Nicol, W. On the structure of Fossil Wood.

Pentland, J. B. On a peculiar configuration of the Skull in a race of men formerly existing in Peru.

Phillips, Professor. On a method of causing the centre of gravity of a Dipping-needle to coincide with its axis of motion.

—— On the Stratification of Primary Rocks, (in discussion on that subject.)

Powell, Professor. On the Repulsion produced by Heat.

—— On the Achromatism of the Eye.

—— On the Dispersion of Light.

Quetelet. In a letter to Mr. Whewell, M. Quetelet states his belief that he has succeeded in reducing the examination of the Law of Population to the discussion of mathematical formulæ, and requests that his views may be tested by a comparison of the calculated results with those furnished by observations in England, the United States, and elsewhere.

Ramage, John. On the construction of large reflecting Telescopes.

Reid, Dr. On the Connexion of Muscles with Nerves.

Rennie, G. Notice of the successful performance of an Instrument to measure the quantity of Mud in the water of Rivers.

Robinson, Rev. Dr. A Discourse on Halley's Comet, delivered at an Evening Meeting of the Association.

—— On the Visibility of the Moon in total eclipses.

—— On the Situation of the Edinburgh Observatory.

Royle, J. F. On the Character of the Vegetation of the Himalaya Mountains.
Russell, J. On the Resistance to Floating Bodies.
Sang, Edward. On the Geometry of Lines of the third order.
—— On Vibrating Wires.
—— On a property of successive Integer Numbers.
Saull, W. D. Drawing of the Incisors and Canine Teeth of the Hippopotamus, from a gravel-pit near Huntingdon.
Saumarez, Richard. On Light and Colours.
Saxton, Joseph. On an Instrument for measuring minute Variations of Temperature in Metal Rods, &c.
Secretary to the Society of the Sons of the Clergy in Scotland. Notices relating to a Statistical Survey of Scotland.
Sedgwick, Rev. Professor. On the Stratification of certain Primary Rocks, (in reply to Dr. Boase's views.)
—— A Review of the Geological Proceedings of the Meeting at Edinburgh, delivered at an Evening Meeting of the Association.
Selby, P. J. Notice of Birds collected in Sutherland.
—— On the Postorbital Glands in Natatorial Birds.
Sharpey, Dr. On the Vascular System of the Porpoise.
Smith, William. Observations on the Waste and Extension of Land on the East Coast of England.
Stanley, Rev. E. Notice regarding Statistical Returns for Parishes.
Statistical Society of Manchester, by Mr. Heywood. Statistical Returns relating to Manchester.
Stevelly, Professor. On some branches of Meteorological Science.
—— On a Vernier to be adapted to a scale of unequal parts.
Sykes, Lieut.-Col. On Mean Temperatures in India.
Syme, Professor. On removing portions of Joints.
Taylor, John. On the Directions of Mineral Veins in different countries.
Thomson, Dr. Allen. On the Structure of the Human Fœtus and that of Mammalia at early periods of development.
—— On the external Gills of the Young of the Skate, and on the Gills of some Reptilia.
—— On the Change of Colour observable in the Cuttle-fish.
Thomson, Dr. A. T. On Iodides.
Thomson, Dr. T. Notice of a Fossil Plant (probably marine) from the Glasgow Coalfield.
Thomson, Dr. W. On black Discoloration of the Lungs.
Toorn, M. Van der. On the Water in Sulphate of Zinc.

Tough, Rev. Mr. On a Glass Celestial Sphere.
Traill, Professor. On the Laryngeal Sac of the Reindeer.
—— On the Geology of the Orkneys.
—— On the Fossil Fishes of the Orkneys.
Trevelyan, A. On the application of Vapour of Alcohol to the purpose of a chemical Lamp Furnace. (See *Phil. Mag.* 1834.)
Trevelyan, W. C. On Fossil Wood from Faroe.
—— On the Geographical distribution of Plants in Faroe.
Turner, Dr. E. On Atomic Weights; that they are not representable by whole numbers.
West, William. On the presence of Sulphur in Bar Iron.
Whewell, Rev. W. A Lecture on certain Phænomena of the Tides, delivered at an Evening Meeting of the Association.
—— Suggestions regarding Sir J. Herschel's explanation of Dispersion according to the Undulatory Theory.
Williams, Dr. C. On the State of Knowledge regarding Sound.
—— On the Phænomena of Low Combustion.
Wilson, J. On the Coleopterous Insects of Sutherland.
Yates, Rev. J. On some facts regarding the Stratification of Primary Rocks.

REPORTS

ON

THE STATE OF SCIENCE.

Report on the Geology of North America, Part I., by HENRY D. ROGERS, *F.G.S.*

IN obedience to the request of the British Association, expressed to me at the last Annual Meeting, I beg leave to offer the following Report on the present state of our knowledge of the geology of North America.

The magnitude of the region, our remoteness from the fountains of science in Europe, and likewise some peculiarities in the geological structure of the country, have operated hitherto to make our efforts in exploring its formations tardy and uncertain. But the friendly interest expressed by the British geologists in our labours is calculated to cheer and quicken our progress.

It will be seen to be among not the least important of the good results of this Association, that it can invigorate by its ample spirit the youthful science of a distant but kindred continent.

The plan and object of this Report make it necessary to offer an introductory section on the general physical geography of the country. In no section of the globe will a more obvious and marked connexion be seen between the geographical features of the surface and the geology. Such a description is indispensable indeed, for certain geographical boundaries will be found the best, in fact almost the only, guide we possess at present for judging of the probable range and extent of certain formations over many extensive districts not yet explored.

Physical Geography.—Omitting the minor irregularities, and confining our survey to the great masses which compose the continent of North America, its structure will be seen to exhibit great simplicity and regularity. From the Atlantic to the Pacific Ocean, and from the Arctic Sea to the Gulf of Mexico, the whole area seems naturally divided into two great plains,

bounded by two broad ranges, or rather belts, of mountains. One plain, the least considerable by far, occupies the space between the Atlantic and the Appalachian or Alleghany Mountains, and extends from Long Island, or more properly from the eastern coast of Massachusetts, to the Gulf of Mexico, losing itself at its south-western termination in the plain of the Mississippi: this last is a portion of the second great plain, which we may style the central basin of the continent, and occupies much the largest portion of the whole surface of North America. In breadth it spreads from the Alleghanies to the Rocky Mountains, and expands from the Gulf of Mexico, widening as it extends northward, until it reaches the Arctic Sea and Hudson's Bay. Over the whole of this great area occur no mountain chains, nor any elevations beyond a few long ranges of hills. It is made up of a few very wide and regular slopes, one from the Appalachians, westward to the Mississippi; another, more extensive and very uniform, from the Rocky Mountains eastward to the same; and a third from the sources of the Mississippi and the great lakes northward to the Arctic Sea. The most striking feature of this region is the amazing uniformity of the whole surface, rising by a perfectly regular and very gentle ascent from the Gulf of Mexico to the head waters of the Mississippi, and the lakes reaching in that space an elevation of not more than 700 or 800 feet, and rising again in a similar manner from the banks of the Mississippi westward to the very foot of the Rocky Mountains. From the Alleghanies to the Mississippi the surface is more broken into hills, and embraces the most fertile territory of the United States. Three or four hundred miles west of the Mississippi a barren desert commences, extending to the Rocky Mountains, covering a breadth of between four and five hundred miles, from the Missouri in lat. 46°, the whole way into Mexico. The territory from the sources of the Mississippi, north, is little known except to fur traders and the Indians, but is always described as low, level, and abounding in lakes.

Of the two chief mountain belts which range through the continent, both nearly parallel to the adjacent coasts, the Alleghany, or Appalachian, is by far the least considerable. This system of mountains separates the central plain or basin of the Mississippi from the plain next the Atlantic, though its ridges do not in strictness divide the rivers which severally water the two slopes. The northern and southern terminations of these mountains are not well defined; they commence, however, in Maine, traverse New England nearly from north to south, deviate from the sea and enter New York, cross Pennsylvania in a broad belt, inflecting first to the west and then again to the south, and from

thence assume a more decidedly south-western course, penetrating deeper into the continent as they traverse Virginia, the two Carolinas, and Georgia, into Alabama. Throughout this range, especially in the middle and southern portions, they are marked by great uniformity of structure, an obvious feature being the great length and parallelism of the chains, and the uniform level outline of their summits. Their total length is about 1200 miles, and the zone they cover about 100 miles broad, two thirds of which is computed to be occupied by the included valleys. They are not lofty, rarely exceeding 3000 feet, and in magnitude and grandeur yield immeasurably to the Rocky or Chippewayan Mountains which traverse the opposite side of the continent.

This last system of mountains, the Andes of North America, skirts the continent on the side of the Pacific in a broad belt from the Isthmus of Panama almost to the Arctic Sea, its extreme northern limit, as defined by Captain Franklin, being far north on the Mackenzie's River. The chains within this zone are many of them very lofty, their average direction, until they enter Mexico, being nearly north and south. Within the United States territory they rise abruptly from the sandy plain before described, in longitude about 32½° west from Washington; and from that meridian nearly the whole way to the ocean the region is mountainous, with elevated sandy plains, and volcanic tracts resembling those of Mexico. The summits of many of the Chippewayan chains are far above the limit of perpetual snow, the highest points being about 12,000 feet above the sea.

When we regard the grandeur of the dimensions exhibited in these several divisions of North America, the extreme regularity prevailing over great distances both in the plains and systems of mountains, and the straightness and parallelism of these to its long coasts, we are prepared to look for a proportionately wide range and uniformity in its geological features. To comprehend the relations of our formations to each other, and the true extent of the portion of our geology at present partially developed, the exhibition of which is in fact the main end and object of this Report, a further description, rather more in detail, of our geography is here requisite.

Let us first contemplate that long and comparatively narrow plain defined above, which lies between the Atlantic Ocean and the chains of the Alleghany mountains. This tract, which in the New England States is very narrow, comprising the mere coast and islands, expands in its course southward, the mountains in Carolina being more than 200 miles from the sea. It is divided longitudinally nearly through its whole length by a well marked

geographical and geological boundary, commencing on the coast of Massachusetts and running to Alabama. The boundary meant is the eastern edge of a well exposed range of primary rocks, which, from New Jersey as far south as North Carolina, forms a nearly definite limit to the flowing up of the tide in the Atlantic rivers. Between it and the ocean the country is throughout low, flat, and sandy, while westward the rest of the plain rises in gradually swelling undulations to the base of the blue ridge or eastern chain of the Alleghanies. The rivers descend from the mountains over this western portion of the tract, precipitate themselves over the rocky boundary mentioned, either in falls or long rapids, and emerge into the tide level to assume at once a totally new character. South of North Carolina this line of primary rocks leaves the tide and retires much nearer to the mountains, though it still preserves its general features, separating the rolling and picturesque region of the older rocks from the tertiary plains next the ocean; and though the tide does not any longer lave its base, as in Virginia, Maryland and Pennsylvania, it still produces rapids and cataracts in the southern rivers which cross it. Ranging for so very great a distance with a remarkable uniformity of outline and height, on an average between 200 and 300 feet above the tide, it constitutes as admirable a geographical limit as it does a commercial one. Nearly all the chief cities of the Atlantic States have arisen upon this boundary, from the obvious motive of seeking the head of navigation; a striking example of the influence of geological causes in distributing population and deciding the political relations of an extensive country. Below this boundary the aspect of the region is low and monotonous, the general average elevation of the plain probably not exceeding 100 feet. Its general width through the Middle and Southern States is from 100 to 150 miles. As the tide enters this tract so extensively, flowing, except in the more southern States, entirely across it, a series of very abundant alluvial deposits occurs, distributed throughout. The surface is everywhere scooped down from the general level to that of the tide by a multiplicity of valleys and ravines, the larger of which receive innumerable inlets and creeks, while the smaller contain marshes and alluvial meadows. The whole aspect of the barrier of primary rocks forming the western limits of this plain forcibly suggests the idea that at a rather lower level they once formed the Atlantic shore, and that they exposed a long line of cliffs and hills of gneiss to the fury of the ocean: a survey of the plain just described as strongly suggests the idea that all of it has been lifted from beneath the waves by a submarine force, and its surface

cut into the valleys and troughs which it presents by the retreat of the upheaved waters. The submarine origin of all this tract will be made apparent in treating of its geology; but in reference to its valleys, it may be well to remark that it has no doubt been torn by more than one denuding wave, in as much as the great current which has evidently rushed over other portions of the continent has also passed across this tract, and strewed it as we see with diluvium. How many such denudations of the strata have operated to form the present broad valleys of its enormous rivers, or how much of the excavation has been due to the continued action of the rivers themselves, we have, so far at least, no sufficient data to form a decision.

The level region here spoken of I propose calling, for convenience, the Atlantic *Plain* of the United States, while the territory between it and the mountains may be fitly entitled the Atlantic *Slope*.

The extensive denudation of the surface of this plain will be found highly favourable to the accurate development of its geology. It is from this and the accessible nature of its rivers that we already know more of its strata, and especially of its organic remains, than we do of any other district of the country. Its horizontal strata are in many places admirably exposed in the vertical banks of the rivers, often through many miles' extent; and the mass of appropriate fossils thus procured, as will be seen from this Report, is already far from insignificant. This plain, widening in its range to the south-west, bends round the southern termination of the Alleghanies in Alabama, and expands itself into the great central plain or valley of the Mississippi. The tract in question embraces the greater portion of the newer secondary and tertiary formations hitherto investigated upon this continent, though, notwithstanding the great area it covers from Long Island to Florida, it may yet be found to constitute but a small section of the whole range of those deposits, when we shall, on some future day, have explored in detail the vast plains beyond the Mississippi.

The ledge of primary rocks, bounding the tertiary and cretaceous secondary deposits of the Atlantic coast, may be delineated by commencing at the city of New York, and tracing a line marked out by the falls in nearly all the rivers from that point to the Mississippi. It is thus marked in the falls of the Passaic at Patterson, in the Raritan near New Brunswick, in the Millstone near Princeton, in the Delaware at Trenton, the Schuylkill near Philadelphia, the Brandywine near Wilmington, the Patapsco near Baltimore, the Potomac at Georgetown, the Rappahanock near Fredericksburg, James River at Richmond,

Munford Falls on the Roanoke, the Neuse at Smithfield, Cape Fear River at Averysboro, the Pedee near Rockingham, the Wateree near Cambden, the Congaree at Columbia, the Falls at the junction of the Saluda and Broad Rivers, the Savanna at Augusta, the Oconee at Milledgeville, the Ockmulgee at Macon, Flint River at Fort Lawrence, the Chattahooche at Fort Mitchell, &c., deviating thence north-west through the state of Mississippi. Towards the southern termination of this rocky ledge, in Alabama for instance, it does not consist, as it generally does elsewhere, of gneiss, but is formed of the ancient sandstone and limestone of the Alleghanies. It everywhere, however, appears as a natural line of division, of great length and uniformity, separating two tracts of very dissimilar geological age and features. The upper tract, which I have called the Atlantic slope, possesses a very variable width; it is narrow in New York and the New England States, where the mountains approach the coast, and narrow also in Alabama, where they approach the plains occupied by the cretaceous rocks of the south, but is much expanded in Virginia and the Carolinas. Here it has a breadth of about 200 miles, ascending from the tide in an undulating hilly surface, to a mean elevation of perhaps 500 or 600 feet near the mountains. As it approaches these, its hills swell into bolder dimensions until we gain the foot of the blue ridge or first chain of the Alleghanies. It consists almost exclusively of the older sedimentary and stratified primary rocks. This fine hill tract exhibits a marked uniformity in the direction of its ridges and valleys, running very generally north-west and south-east, or parallel with the mountains. The ridges, though not high, are long, and the fertile intervening valleys very extensive. It embraces a variety of fine soils, and an immense water power in its rivers and running streams.

Geology of the United States.—I propose to treat of our formations in the order of the latest first, commencing the survey of each group in the districts where it is best known. I shall therefore, in this first part of my Report, describe whatever is known of our recent, tertiary, and cretaceous formations, and shall reserve an account of the rest of the secondary and all the primary rocks for the next annual meeting of the Association. By the delay I hope to be able to add materially to the accuracy of the geological map, and it will enable me to present some of the results of the geological surveys now set on foot by the States of Maryland and Tenessee, together with whatever else may in the mean while be brought to light.

The tertiary and cretaceous groups yet known to us in North America are confined almost exclusively to the Atlantic plain

of the United States, and to the southern part of the great central valley, or basin of the Mississippi. The lines along which these formations have been traced in the valley of the west are few and far apart, so that our present survey is chiefly confined to the tide-water plain along the Atlantic.

The same line, which was before sketched as forming the boundary of the Atlantic plain, will be observed, in tracing it through the states of New Jersey, Pennsylvania, Delaware, Maryland, Virginia, and North and South Carolina, to coincide almost exactly with the western limit of the tertiary and secondary formations here to be discussed. From Long Island, south, this barrier of primary rocks presents everywhere a remarkably abrupt and well defined line of separation between these newer deposits and the rocks of older origin. North of Long Island, on the main land of Connecticut, Rhode Island, and Massachusetts, the precise position of this line is not so readily traceable. Along the coast of the two first states little or nothing of the newer formations is seen; and, if we except the small portions stated by Hitchcock as occurring in the valley of the Connecticut river, and on the eastern peninsula of Massachusetts near Cape Cod, they have not been noticed on the continent east of New York. The islands of Nantucket, Martha's Vineyard, and Long Island are all, however, embraced within the area of the upper strata about to be described.

The acknowledged difficulty of defining the exact æra to which the newest deposits belong, is sensibly felt in treating of those of the United States. The amount of strata within this area which have had their origin in the class of geological causes at present in action, is, no doubt, very considerable. Indeed, geologists are accustomed to allude to the changes wrought by the Mississippi and Niagara as among the most striking within the recent period anywhere to be met with. Nevertheless, it seems very possible that a large portion of the alluvial matter which borders the mouths of the rivers and coast, may have been formed before the earth, or this continent at least, was tenanted by man. The evidence upon this point will be given presently. The first class of phænomena to be examined are those which are unquestionably recent.

Of *volcanic action* we have no traces east of the Mississippi. The earthquakes which convulse the equatorial and southern sections of the continent rarely reach the United States; and when felt, they come with such greatly diminished force as to be hardly sensible. The forces now in action are, therefore, exclusively aqueous. These, however, prevail over very extensive areas, as will be seen on adverting to the size and num-

ber of the rivers, the magnitude of the coast, and the enormous lakes where freshwater deposits are probably accumulating on a scale of great extent.

Alluvial Deposits.—From the mouth of St. Croix River to Florida Point, the length of the Atlantic coast is about 1800 miles; and along the Gulf of Mexico, from Florida Point to Sabine River, the boundary of the United States coast, the distance is 1100 miles more. The first section receives the rivers which descend the Atlantic slope. The several basins drained by these rivers, according to the view given by Darby, are forty-two in number, and the total area drained is 252,900 square miles.

The smaller river basins in the vicinity of the delta of the Mississippi, from Sabine River to the western slope of Florida inclusive, are, excluding the great basin of the Mississippi, sixteen, with an area of 144,240 square miles. The area drained by the Mississippi and all its tributaries is computed at 1,099,000 square miles. I do not extend the survey to the many large rivers which enter the gulf west of the Sabine. The quantity of sediment conveyed to the ocean from so wide an area must be very enormous; and, as a proof, we behold either an alluvial delta or a bar at the mouth of almost every river. The entire line of sea-coast, from the Sabine to the mouth of the Pearl, presents an uninterrupted marsh 400 miles long, and from 30 to 50, or even 70 miles wide, the production solely of the Mississippi and the rivers adjacent. From the mouth of the Pearl eastward, the sandy pine tract reaches the gulf, and extends, with little interruption, along the whole sea-coast of the Mississippi, Alabama, and great part of Florida. Along this part of the gulf, and along the Atlantic from the point of Florida to New Jersey, though many extensive marshes occur upon the coast, the shore is more generally sandy. At the mouths, however, of nearly all the rivers, low, marshy, alluvial tracts are to be seen. Low down, towards their mouths, these rivers run through extensive flats or meadows, most of which are at present elevated above the highest spring-tides, though it is possible that many of them, during unusually heavy storms or great freshets, may be liable to be partially submerged. These meadows are often several miles in width, and bordered on each side by abrupt banks, consisting of the solid strata of the country, so that they have all the aspect of having been, at a former period, permanently beneath the tides, which, on this supposition, penetrated their valleys in the shape of extensive bays and estuaries.

The river meadows are never covered by the coating of

diluvial sand and gravel which conceals all the other formations of the country; a circumstance which will enable us to distinguish between them and another group of more ancient alluvial deposits to be described further on.

Recent Changes in the Mississippi.—I am informed by Mr. Tanner, the geographer, that a striking example of the manner in which this river ordinarily varies its channels was witnessed about two years ago, at the mouth of the Red River. A remarkable bend at that place, known as one of the longest and most circuitous loops in the Mississippi, was cut off by the simple expedient of digging a very short trench across the narrow neck which the stream was daily scooping away. In 24 hours steam-boats passed through the new channel, and it immediately became the outlet of the Red River, which before entered the Mississippi by the lower side of the bend, but now discharges itself along the upper. By this change the river has been shortened 20 miles.

When it is recollected that in freshets the current of the Mississippi descends at the rate of five and even six miles an hour, and at low water at the rate of two miles, it will at once be seen how great a load of sedimentary matter it can annually sweep down into its delta, and how rapidly this must augment both in height and superficial area. As an example of the rate at which it is growing, the Old Balize, a post erected by the French about the year 1724, at the very mouth of the river, is now two miles above it. There was not at that time the smallest appearance of the island, on which, 42 years after, Ulloa caused barracks to be erected for the pilots, and which is now known as the New Balize.

The distance from the mouth of the river, at which the chief deposit of sediment usually takes place, is about two miles. When these shoals accumulate sufficiently, they form small islands, which soon unite and reach the continent; and thus the delta increases. So enormous has been the growth of such deposits, not only opposite the mouths of the Mississippi, but around the whole northern shore of the gulf, that nearly the entire coast of Louisiana is inaccessible, from the shallowness of the water, except immediately through the channels of the rivers.

An almost universal feature in the entrances of the rivers of the Atlantic is the bar obstructing their mouths. That of the principal entrance of the Mississippi had, in 1722, about 25 feet of water upon it; Ulloa, in 1767, found 20 feet at the highest flood; and in 1826 the depth was only 16 feet.

Above these obstructions the rivers are generally much

deeper; the Mississippi, at New Orleans, being above 100 feet deep, which depth it preserves to the mouth of the Missouri. Mobile Bay is crossed by a bar, having only 10 feet of water, and the bar of the Altamaba of Georgia has 14 feet, which is, perhaps, about the average depth to be found at the entrance of most of the southern rivers of the Atlantic coast.

Alluvial Terraces.—Besides the alluvial flats which border so many of the rivers at an elevation of only a few feet above the tide, and which may have been formed during the present relative level of the land and sea, there are plains of another class, which often occupy the sides of the valleys in terraces more remote from the rivers. This common feature on many of the rivers of the United States, I mention not only from my own observation, but on the authority of various works, as *Stoddard's Sketches*, *Drake's Picture of Cincinnati*, *Darby's Louisiana*, and Professor Hitchcock's *Report on the Geology of Massachusetts*; some of them mentioning two, three, or even more of these river terraces. The latter author thus describes them on the Connecticut river in Massachusetts: "If we start from the edge of the stream at low water, and ascend a bank of 10 or 15 feet high, we shall come upon an alluvial meadow, which is frequently overflowed, and is consequently receiving yearly deposits: this may be regarded as the lowest terrace. Crossing this, we ascend the escarpment of a second terrace, 30 or 40 feet in height, which may be seen at intervals on the same level on all sides of the meadow. This second terrace is rarely very wide in any place, and seems to be only the remnant of a meadow, once much more extensive, which has been worn away. Ascending from this 40 or 50 feet up another escarpment, we reach the plain that forms the bottom of the great valley of the continent: this constitutes the upper terrace." He adds, that terraces, more or less distinct, exist on almost every stream of considerable size in the State, wherever the banks are low enough to admit of alluvial flats. Professor Hitchcock imputes these terraced valleys to the sudden bursting of the barriers of a lake or pond through which the stream flowed, or the sudden removal of an obstruction in the river, by which it cut a new channel into the soft soil above the obstruction. I would beg leave to suggest, however, whether, in the case of so many successive terraces, such an explanation is not rendered improbable, from the difficulty of imagining so many debacles taking place in succession upon the same river. The circumstance that nearly all our river valleys which have the structure described, occur in districts where the rivers could never have been crossed by ridges of

rock—no relics of such barriers being seen, for example, among the horizontal formations of the Atlantic plain—is, I think, conclusive evidence that we must seek for some other cause.

That the cause which has given the delta of the Mississippi its present elevation was the uplifting agency of forces from within the earth, we shall see additional evidence for admitting, when I treat presently of some of the newest of our fossiliferous deposits. In the present infancy of geological research in the United States, we are not prepared to venture any views upon the age to which the terraces in question belong. It is very possible that they may be finally referred to several distinct periods. Many of them are covered by the general capping of diluvium, which renders it very likely that the date of some of them is earlier than the recent period. In the absence of organic remains, it is wisest to leave the discussion of the age of these formations open until a larger stock of information has been gathered concerning them.

Of the Coast Islands, and their probable Origin.—Having, in the previous section, given some account of a few of the causes now in action on this continent, as a specimen of the kind of phænomena which in this country present themselves on a scale of peculiar magnitude, I shall proceed to a feature in our geology closely connected with the foregoing class of operations, implying the agency of almost the very same powers, and, if I mistake not, taking us into a period very little, if at all, earlier than that of the river deltas and alluvium just described. There is to be seen lying a little off from the main shore, along the chief extent of the Atlantic coast, an interesting range of shoals and islands, all running parallel with the shore, and distinguished by the same uniform features. These long, narrow, and low islands of sand range from Long Island to Florida, and around nearly the whole northern sweep of the Gulf of Mexico. They are rarely more than a mile or two wide, sometimes 20 or 30 miles long, and, on an average, about 12 feet high. The geology of Anastasia Island, on the coast of Florida, is a representation of many others, though it must be confessed we know extremely little respecting them.

Anastasia Island, opposite St. Augustine, upon the eastern coast of Florida, is, according to Mr. Dietz (*Jour. of the Acad. of Nat. Sci. Philadelphia*), about 10 or 12 miles long, $1\frac{1}{2}$ broad, and has not more than 10 or 12 feet of elevation above the level of the ocean. It lies parallel to the shore, at a distance of from 2 to 3 miles. The greater part of the northern portion, and perhaps the whole of the island, is composed of horizontal layers of a semi-indurated rock, consisting wholly of fragments

of shells, belonging, as far as examined, almost, though not exclusively, to species inhabiting the adjoining coast. The mass is divided, by thin seams of some foreign matter, into layers from 1 to 18 inches thick, and is so soft before exposure to the air, that it is easily cut by a tool into slabs of any required size, and in this form is extensively used for building. Near the surface the fragments of the shells, generally speaking, are the smallest; but they occur of various sizes, and frequently in the same layer the shells are entire. Much of this rock, especially the more comminuted kind, exhibits not unfrequently a confused crystallization; this process having gone so far as to present the fragments in an almost obliterated state. The coarse varieties are composed of some fragments evidently thus altered, and of others which have not yet lost their colouring matter. The shells belong principally to the genus *Arca*; they are *A. pexata*, *A. ponderosa*, *A. incongrua*, *A. transversa*; also *Lutraria canaliculata*, all of Say; besides a *Mactra*, a *Donax*, a *Crepidula*, a *Lucina*, and another species of *Arca*, which is probably either extinct upon our coast, or extremely rare. *Natica*, *Oliva*, and *Nassa trivillata*, of Say, are also mentioned. Mr. Dietz attributes the formation of this island to the agitation of the tides and winds, conceiving the shells to be driven first towards the shore, and deposited afterwards at their present distance from the beach by the retiring tide. But such an explanation seems not altogether satisfactory, for I cannot learn that this heaping-up of shells from beneath the water is anywhere noticed upon our sea-islands at present. The winds do indeed drive the sands from the beach, and the shoals which are laid bare at low water, upon them, but mingled with hardly any shells, while the rock of Anastasia Island is made up of shells exclusively. Such agitation would seem incompatible with the accumulation of so homogeneous a mass, which is found to contain neither pebbles, sand, nor other transported matter of any sort. My own present conviction regarding these coast-islands is, That they are all the portions of a range of shoals or bars formed along the line of junction of the turbid waters from our rivers, and the great in-setting currents connected with the gulf-stream;—that since the existence of the gulf-stream and the present drainage of the Atlantic plain, this growth of sediment opposite the coast has been going on;—that in the more tranquil places upon these bars, vast colonies of shell-fish planted themselves;—and that the whole line of shoals has been lifted, with part of the adjacent continent, by the force of an earthquake or earthquakes, to their present small elevation above the waves. Traces of more

than one such up-heave of the continent during the tertiary period, may possibly be found hereafter, when the various systems of plains and terraces along the rivers and the coast shall have been more investigated.

There can be no doubt that most of the islands opposite the coast of the Middle States, New Jersey for instance, are hourly on the increase. They consist, like the opposite main shore, of marsh as a substratum, which is seen to receive a covering of sand blown in from the sea side whenever the tides and gales are favourable. Thus, the side of these islands next the sea is sandy and on the increase, while that adjacent to the continent is marshy, and in many cases appears to be wearing down under the action of the rapid current which sweeps through the intervening sound or strait. As a proof of the daily growth of some of these islands, or *beaches*, as they are called, Cranberry Inlet is now closed up, though it still bears the name "Inlet," as may be seen upon any map of the Jersey coast.

It is impossible, therefore, to refer them all to the period which produced Anastasia Island, and the islands and coast in its neighbourhood, though, regarding the manner of their formation, there can be no doubt that the same combination of causes, winds and currents, operated in producing them all. These causes, as I have already shown, are active, in the present day, in effecting similar deposits along the delta of the Mississippi; nor do I perceive any good reason why we should not admit the agency of the same in remote tertiary periods. Our rivers, since the appearance of the carboniferous formations, at least, must have been always very large, and have formed vast deposits of sediment in the sea; and there is every reason to suppose that the gulf-stream, which has evidently much to do in shaping these deposits, has existed since an early period of our coast formations. The true age of that great ocean current can only be decided when we know more thoroughly the geology of the isthmus separating North from South America. In the mean time we may safely apply the actions which are daily witnessed upon our coast, to formations so very little older, as that of Anastasia Island.

Raised Estuary Formations of the Gulf of Mexico.—A very extensive bed of shells, bordering on the Gulf of Mexico, seems to claim a position somewhere in the group of formations now before us. It appears to hold a place on the confines, as it were, of the tertiary and the recent formations. It is thus described by Mr. Conrad: "An interesting deposit borders the Gulf of Mexico, and is probably several hundred miles in extent. It consists entirely of two species of shells, *Cyrena Ca-*

rolinensis, and *Rangia cyrenoides* of Des Moulins (*Clathrodon cuneatus*, Gray); the former, however, is rare, the deposit consisting almost entirely of the latter shell. In the vicinity of Mobile, which is built on a sandy flat, very little elevated above the tide, the beds in question are superficial, although covered by a vegetable mould bearing a forest of gigantic pines. When one of the trees is prostrated by the wind, the decomposing shells are seen adhering to the roots, but beneath they are entire, and nearly as hard, when dry, as the recent species. It is remarkable that they occur in beds with scarcely any admixture of sand or earth, and they are consequently found extremely useful in repairing roads, and paving the streets of the city. They are dug from the surface of the soil, both on the main shore and the islands of the bay. These deposits border the bays of the Gulf of Mexico between Mobile and New Orleans, and they occur in the vicinity of Franklin, Louisiana. The Chandeleur Isles, between Mobile Bay and the delta of the Mississippi, consist of deposits of these shells covered by a fertile soil. The *Rangia* lives in vast numbers in the extensive flats below Mobile, burrowing three or four inches beneath the surface of the sand, in which numerous depressions indicate where they are to be found." According to Mr. Conrad, the *Rangia* was first seen in a sub-fossil state in the newer Pleiocene, at the mouth of the Potomac, where, however, it is rare. Though it there occurs in a deposit of marine shells, the sea appears not to be the usual resort of the species; and it is only in the brackish water in the bays and estuaries that it is abundant. He is therefore inclined to regard the few found in marine deposits as coming from some neighbouring estuary. As it abounds in the recent state in the present sheltered sounds which fringe the Gulf of Mexico, the presumption is very strong that the fossil beds above described are colonies which, previous to the change of level of the land, flourished in precisely similar situations. This would account satisfactorily for the narrow and very long belts in which they run, skirting round the bays and the coast above its present marshes, from Pensacola, in Florida, to near Franklin, in Louisiana.

Diluvial Action over North America.—Almost the whole surface of North America, as far as examined, may be said to be covered with an investment of earth, pebbles, and boulders, obviously of diluvial origin. The thickness of this deposit varies, though its average depth may be said to be from ten to twenty feet. All that low and level tract described as the Atlantic plain, and also the lower sections of the great valley of the Mississippi, appear to be the districts where it conceals the under-

lying strata to the greatest depth. Over the whole of this extensive territory it covers the horizontal strata of the tertiary and cretaceous deposits, and obscures them so effectually that, except in the cliffs, along the rivers, and in the sides of the ravines and valleys, these formations are rarely or never exposed. If we begin our examination of this great mass of detritus upon the Atlantic coast, we there find it to consist of fine sand and gravel, in which form it abounds over the peninsula of Jersey, Maryland, Virginia, and North Carolina, and all the states along the Atlantic to the Mississippi. This soil along the seaboard may very possibly, if we judge from its consisting so entirely of pure finely comminuted sand, have been reclaimed from the ocean since the general distribution of diluvial matter over the continent. But even upon this view, it is to be regarded as the result of diluvial action. The pebbles are of a kind, in fact, which could only come from the interior, above the range of rocks bordering the tide. They do not belong to the tertiary and cretaceous strata of the Atlantic plain, but to the older rocks of the Atlantic slope and the mountains. As we advance inward from the coast, the mass of diluvial matter becomes less sandy and coarser, the soil somewhat less barren, and the vegetation more diversified, though still consisting principally of pine. Over the upper portion of the Atlantic plain, or nearest its rocky boundary, the mass contains the gravel in a much coarser state, mingled with clay sufficiently pure for bricks. Rolled blocks and boulders of no inconsiderable size occur, especially in the valleys of the rivers, when within ten or twelve miles of the boundary mentioned. For many miles from the coast there is rarely anything but the diluvium. In the central districts of the tract the fossiliferous strata appear beneath it, though near the upper limits of this tract these often disappear again, and the region immediately eastward of the rocky boundary presents the diluvium covering another class of deposits very different from the tertiary and secondary beds which underlie it near the sea.

The deposit along the east of the rocky boundary, or, in other words, at the head of tide in the Middle States, is not diluvium, as from the absence of fossils many might at first imagine. At many places, as Bordentown on the Delaware, the deep cut of the Chesapeake and Delaware Canal, Baltimore, &c., the mingled mass of ordinary diluvium reposes upon very regularly stratified beds of dark blue clay, containing decayed trees, lignite, and amber; the whole mass precisely such in appearance and contents as to lead to the conviction that it is more probably an al-

luvial mass deposited in front of the ancient rocky coast, than a portion of the detritus left by diluvial action.

Proceeding now from the Atlantic plain towards the mountains, the diluvial matter is more irregularly distributed, in consequence of the undulations of the surface. It may be seen in greatest quantity in the valleys of the rivers, the boulders which cover their beds and sides being almost invariably traceable to formations which lie at some miles' distance to the *north-west and north*. This distribution of the diluvium from the north and north-west is *not confined to the rivers whose valleys run in those directions*, but belongs, it is believed, to at least all the middle and northern latitudes of the continent. It is seen west of the Alleghanies, throughout the region of the Ohio and Mississippi, as well as extensively over the Atlantic slope and the tertiary Atlantic plain. Bigsby and the travellers to the north have already shown it to prevail in the latitudes north of the United States.

The very extensive valley which crosses Pennsylvania, Maryland and Virginia, lying immediately east of the blue ridge, though it consists principally of transition limestone and greywacke slate, is strewed also with innumerable blocks and boulders of the same sandstone which composes most of the blue ridge, and appears, so far as yet examined, to be newer, together with fragments from the hills between the valley and the Atlantic. Opposite to the passes or breaks in this first range of mountains, the quantity of such transported matter on the south-east of them is particularly great; and many of the first ridges of the chain are covered to an unknown depth upon their flanks and even their summits by the diluvial matter in a comminuted state. As an instance, the mountain which bounds this valley in Pennsylvania, running west from the Susquehannah through Cumberland county, and called there the North Mountain, is covered with a mass of little else than sand, such as could not be derived from the limestone tract to the south-east, but just such as would be formed from the disintegration of the sandstones of the Alleghanies.

It is stated by Hayden, in his Geological Essays, that in Washington city itself, which is south of the first primary ridge, and about fifty miles south-east of the mountains, there is a small area covered with rolled masses of sandstone, some of which would weigh from 200 to 500 pounds, and containing perfect impressions of shells resembling *Terebratula*. Now, no fossiliferous formations occur until we pass beyond the blue ridge, and the blocks must have come from the north-east or north, at least sixty miles. I have myself seen fragments of

similar boulders in the neighbourhood of Columbia, on the Susquehanna, containing several species of *Producta* and *Terebratula*, which could only have come from a like region within the mountains of Pennsylvania, a distance perhaps of fifty miles at least. Drake, in his *Picture of Cincinnati*, mentions large masses of granite in that part of Ohio, resting upon the ordinary finer diluvium. The nearest granite on the north is at least one hundred leagues distant; while no primary rocks occur south or east within even a much greater limit. We are reminded here of the great detached blocks which strew the plains of northern Europe, and the explanation suggested that they have been carried there by floating upon ice. They occur, promiscuously dispersed over a great extent of country in Ohio, Kentucky, and Indiana, and are in no way connected with the present river valleys.

I may mention as an interesting fact, corroborating the opinion of the northerly origin of the current here advanced, that Mr. Conrad, who has explored the State of Alabama, was never once able to perceive a boulder upon its surface.

Besides the fossiliferous deposits of very recent date, described by Mr. Conrad, around the Gulf of Mexico, many of our rivers adjacent to the sea present extensive beds of shells, of another class, but probably referrible to the same origin and the same period of elevation. They consist of the common *Ostrea virginica*, almost exclusively, with a very few of the recent univalves of the coast, all of these being shells peculiar to the bays and estuaries of the rivers, and the shallow sounds on the inner side of the Sea Islands and shoals along the coast. The position in which these beds of shells are invariably seen is upon the low level plains adjacent to the tide creeks of our rivers, where they appear to have dwelt in colonies in the sheltered bays at a time when these plains were at a small depth beneath the water, and to have been lifted with them by, perhaps, the last shock which has changed the level of the coast. These shells, in a sub-fossil state, occur in Cumberland County, New Jersey, on the bank of Stow Creek, at Egg Harbour, on the Severn, at Euston, in Maryland; again, upon the York river in Virginia, and indeed upon many others of the southern rivers. They occur at the mouth of the Potomac, resting upon the beds of marine shells, which were originally described in the *Journal of the Academy of Natural Sciences* by Mr. Conrad, and considered by him as referrible to the newest of our fossiliferous formations. In the same locality these beds of fossil *Ostrea virginica* are seen to be covered by the diluvium, so that there can be no question of their origin having been during the latest stage, as it were, of

the tertiary period, and not connected, as imagined by the vulgar, with human agency. The usual position of these beds of *Ostrea* is near the rivers, at a small elevation from the tide. They seem to hold also nearly the same elevation along the coast of New Jersey and elsewhere.

Ancient Alluvium.—The above subdivision of our strata is adopted for the sake of treating, under an independent head, a group of beds of no inconsiderable extent in the United States, and which, in their phænomena, seem to cast important light upon the former revolutions of the Atlantic side of the Continent. They point to a period when this coast had a very different configuration, and denote in a striking manner one of the revolutions which have impressed upon the tract included between the sea and the mountains the peculiar features which it now bears. The formation I allude to immediately underlies, wherever it occurs, the general investment of diluvium.

It has produced, hitherto, very few organic remains of the description proper to enable us to judge of its relative place in the series; but as the few shells occasionally found in it belong to species now inhabiting our Atlantic waters, and as, from all its other characters, it has evidently been formed under different circumstances from our other tertiary beds, and at a period apparently much more recent than any of the rest with which it can be compared, I am induced to place it thus apart, and to give it provisionally, from its obvious origin, the convenient and not too theoretical name of 'ancient alluvium.'

This deposit is the same which has usually, in this country, gone under the name of plastic clay formation,—a title sufficiently inappropriate, even were it to express correctly its true place in the tertiary series, and now particularly ineligible, when, in place of being one of the lowest tertiary deposits, it will be seen, from the evidence I shall present, to be one of the very uppermost. The beds I am speaking of consist generally of numerous alternating deposits of gravel, sand, various coloured tenacious clays, often black and ferruginous conglomerates, iron ore, and lignite. They occur exposed in the deeper sections of our canals and rail-roads, and in the banks of some of the rivers, where they usually reach from the water's edge to an elevation of sixty, seventy, or more feet. They extend along the upper edge of the Atlantic plain, ranging along the eastern base of the rocky Atlantic slope, in a belt several miles wide, and appearing at intervals, where the rivers have cut through them, from the coast of Massachusetts as far at least, it is believed, as the Mississippi. Professor Hitchcock, speaking of these beds in the valley of the Connecticut river,

describes them under the name of the *most recent tertiary*, which I have stated to be my own view. But he makes a distinction between these and other similar beds in Martha's Vineyard and elsewhere, which he calls plastic clay. The first, he says, are horizontal layers of white siliceous sand and blue plastic clay, almost entirely destitute of any organic remains. These beds constitute most of the level and elevated terraces along the valley of this and most of the other rivers of New England: the height of the plains above the water is from fifty to one hundred feet. Along the Connecticut, in some places, the clay beds alone compose the cliff, and are from forty to more than seventy feet thick. They repose beneath fifteen or twenty feet of diluvial matter. Their position, and all their features, here and everywhere else, indicate a general *uplift* of the strata along the whole line of the primary boundary when that boundary formed the coast, and a consequent emergence of these beds from about the water level, where they seem to have grown as marshy deltas, accumulated along the ancient mouths of the rivers. On this supposition, the mouths of the Atlantic rivers were at the points where they now form their falls, and break through the boundary of the older rocks; and it is singular enough that all the conspicuous deposits of these clays, imbedding the trunks of trees and lignite, are just opposite, or near to, the same points. At Gay's Head, on the coast of Martha's Vineyard, are alternating sands and clays, which I refer to this formation, rising in the cliff to a height of between 150 and 200 feet. The clays contain a bed of lignite, which is, in some places, five feet thick. It alternates with the clays, especially the blue, and is often intimately mixed with them, forming a comminuted dark mass, resembling peat. Woody fibre is often distinguishable in it, and the whole has the appearance of a deposit of peat, through which logs are interspersed. The principal beds lie not far from the middle of the cliff, and have a dip of from 40° to 50° north. In this lignite bed are found impressions of dicotyledonous leaves, apparently *Ulmus*, *Salix*, &c., trees at present growing in the country. Associated with these beds of clay, however, occur several variations of sand; and what at first seems startling enough, one bed described as a green sand, containing remains of Crabs, casts of shells, Alcyonites, &c., evidently referrible to the cretaceous formation of New Jersey, and also interstratified with the same osseous conglomerate, from which were procured the teeth of a Crocodile and several bones, some of them very large, being nine inches thick, and as much in length. The worn and mutilated state of these remains, and the mixture in which they are found, prove forcibly that the bed

is derived from the violent disintegration of a much more ancient formation than that in which it occurs, namely, of a cretaceous deposit, like that of New Jersey, which may possibly underlie this island and Long Island also, they being exactly in its range. The dip and contortion of the strata at Gay's Head lend considerable probability to the foregoing explanation of the origin of this bed of detritus from the greensand. In other quarters, the ancient alluvial beds which I am discussing are usually nearly horizontal, or when they incline, it is with a gentle dip towards the ocean; but in the strata at Martha's Vineyard the dip is abrupt and in the contrary direction, being to the north. These circumstances, taken in conjunction with the fact, that a precisely similar deposit of detritus from the greensand formation covers the northern edge of that group of beds in many spots in New Jersey, where I have seen it not far east of the beds of so-called plastic clay and lignite,—as, for example, between New Egypt and Bordentown,—make me venture to put forward the suggestion that the cretaceous formations of our coast have probably extended further to the north-east than at present, occupying what is now Long Island Sound, and its prolongation eastward. Viewing the island of Martha's Vineyard and Long Island as remnants of a more extensive ancient tract in structure, like the peninsula of New Jersey, we can readily account, I think, for all the above phænomena, together with some others which they present.

According to Hitchcock, similar strata of the tertiary clays, which I have called ancient alluvium, underlie the diluvial covering in both Nantucket and Long Island. They are conspicuously exposed in New Jersey, in the sections of the rail-road near Amboy, and again very strikingly on the Delaware, near Bordentown. Here they have all the features which they display at Martha's Vineyard, with the exception that they are nearly horizontal and less brightly variegated in colour. Lignite, containing pyrites, dicotyledonous wood, and amber, abound in the dark tenacious clay or ancient peat, which has here a thickness of many feet. The following description of this formation in New Jersey and the States south of it will serve to show its extensive range and important character.

In ascending the Raritan it is traced on the south-east shore to within three miles of Brunswick. Approaching Bordentown by the rail-road it is conspicuously exposed for several miles in nearly all the deep cuttings. At Bordentown the banks of the Delaware consist of its various beds of brilliant sands and dark and white clays for more than two miles. At Philadelphia it occurs, but at a lower level, remains of trees having been

found forty-five or fifty feet beneath the city. It is seen in the sections along the Delaware and Chesapeake Canal, where its black tenacious vegetable clay and its sands precisely resemble those above: also in the sections of the Newcastle and Frenchtown rail-road. Around the harbour of Baltimore these deposits occur on a large scale. In excavations made at Baltimore abundant remains of trees and their fruits, particularly the black walnut, have been found at the depth of forty-five or fifty feet. In Virginia, along the same line, as at Richmond for example, similar facts are well known. Near Baltimore, in sinking a well in the Star Fort at Fort M'Henry, two miles below the granite ridge, or supposed ancient coast, the workmen came upon a mass of carbonized wood in a boggy marsh fifty feet below the surface. In digging a well in the same Star Fort (perhaps the same well), a tooth of the *Mastodon* was found at the depth of nearly sixty feet. At a point on the Chesapeake Bay, about twenty miles below Baltimore, called Cape Sable, very extensive beds of these clays occur, abounding in lignite, pyrites, and amber. The uppermost stratum is sand, very ferruginous, often sixty or seventy feet thick, then a stratum of lignite three to four feet. Below this a bed of sand, intermixed with enormous quantities of pyrites, nests of this mineral occurring from a foot to a foot and half in thickness, and of fifteen or twenty square feet in surface. Next follows a bed of earthy lignite, from five to twelve feet deep, containing an abundance of pyritous wood, with fragments of bituminous wood thirty feet long. In this stratum of lignite have been also found specimens of a curious comb or nest, the work of an insect. These are from one to three inches in length: each cell has several minute holes. The substance is a resinous matter, resembling amber in properties, and the whole nidus is generally attached around a stem or carbonized twig.

The next stratum is an argillaceous sandstone two to five feet thick, uneven on its surface, while the beds above are all nearly horizontal. Below is a bed of whitish grey clay four feet, and beneath all a bed of sand. The enormous accumulation of carbonized trees in this place, now eighty miles in a direct line from the sea, and at least fifteen from the supposed ancient coast or boundary of primary rocks, points very clearly to the existence, at some ancient date, of an extensive delta here. Whether these beds at Cape Sable may hereafter be found continuous with those around Baltimore in which the Mastodon's tooth was found, time will ascertain, but as yet we have no data precise enough from which to infer the probable place of these beds in the series.

Their geographical position is between the supposed ancient openings of the Susquehanna and Potomac rivers. No one who is familiar with the annual floods of these rivers, and has seen the burden of wood and trees which the former tears up in its passage through the mountains, and discharges each spring into the Chesapeake Bay, can doubt that the very same rivers have probably been employed in olden time in forming these very tracts of sand, clay, and lignite. There are now in the upper part of the bay large flats, which consist solely of sand and drifted timber, the annual scourings of the Susquehanna; and if we conceive these tracts to become converted into marshes and swamps, as might readily happen, we have all the circumstances, and in the same district, which would be requisite to produce from these recent deposits beds perfectly similar to the more ancient ones just described.

Whether those ancient alluvial deposits from Martha's Vineyard to the Chesapeake are all of one date of formation, and what indeed their precise age is, are matters demanding much future research to determine. I have called these beds alluvial, but by no means venture to suppose them all the results of accumulation in deltas, strictly so called. Our rivers may have had basins or estuaries through the tracts in question, throughout which, as well as upon the coast, these beds may have collected. The details of this formation further south are not in a sufficiently authentic shape to be presented; we know, however, that similar beds of clays, sands, and lignites occur largely upon most of the southern rivers, and upon the Mississippi, on a scale which is truly gigantic.

I am inclined to consider as a portion of the same formation an extensive group of variegated clays and sands which spread themselves very widely over the States of Georgia and South Carolina. Like the others before treated of, these contain few fossils. They are seen to repose in some places upon the cretaceous rocks, as those in New Jersey do; in some places upon Eocene; and they are also found below the diluvium.

These beds have been already referred by Vanuxem to ancient alluvial origin. He thus describes them:

"The ancient alluvial is chiefly composed of red earth. This earth is pretty uniform in its character, consisting of sand, with a minute portion of clay, coloured by red oxide of iron. Its inferior parts often contain pebbles, sometimes coarse nodules or geodes of iron, resting almost invariably on the white or variegated clays, or upon those masses which contain littoral shells. Though not often met with beyond North Carolina, it is extremely abundant in all the States south of it. It appears to

occupy the highest elevations above the secondary and tertiary classes, and consequently could not have been formed by our existing rivers. It is entirely unmixed with the tertiary, and destitute of the fossils which characterize the latter; it must therefore be considered as distinct from it, at the same time that it is unlike the modern alluvial, whose origin is clearly attributable to the overflow and inundation of our rivers."

This red earth is precisely similar to the mixture of sand and clay which we may witness discharged from some of the turbid rivers of the Mississippi in the present day. It is seen covering tertiaries at Augusta in Georgia, Columbia in South Carolina, &c. The ancient alluvial beds, resembling in all respects those at Bordentown, are seen along the Santee canal in Georgia, where they repose on secondary beds. In excavating this canal, not only the dark vegetable clay before described, but much lignite and the remains of a *Mastodon* were found, marking the agreement of the deposit in all respects with the corresponding beds at Baltimore and elsewhere. . .

Fossil Mammalia of the United States.—The extinct species of the higher orders of animals found fossil in the United States are *Mastodon giganteum*, *Elephas primigenius*, another Elephant (a tooth only being known, differing considerably from the tooth of either the living or fossil species), *Megatherium*, *Megalonyx*, *Bos bombifrons*, *Bos Pallasii*, *Bos latifrons*, *Cervus americanus*, or fossil Elk of Wistar, and *Walrus*.

Of living species also found fossil, we may enumerate the Horse, the Buffalo, and three or four species of Deer. The situations in which these have been found have been either very recent undisturbed alluvial bogs, or a slightly disturbed marshy deposit like Big Bone Lick, neither of them covered by the general diluvium; thirdly, boggy beds containing lignite referrible to an ancient alluvium, covered by diluvial sand and gravel; and lastly, the floors of caves, buried to a very small depth with earth not described.

The largest collections of bone remains occur in boggy grounds called Licks, affording salt, in quest of which the herbivorous animals, wild and domestic, enter the marshy spot and are sometimes mired. The most noted of these deposits is Big Bone Lick in Kentucky, occupying the bottom of a boggy valley kept wet by a number of salt springs, which rise over a surface of several acres. The spot is thus described by Mr. Cooper: "The substratum of the country is a fossiliferous limestone. At the Lick the valley is filled up to the depth of not less than thirty feet with unconsolidated beds of earth of various kinds. The uppermost of these is a light yellow clay, which apparently is

no more than the soil brought down from the high grounds by rains and land floods. In this yellow earth are found, along the water courses at various depths, the bones of Buffalos (Bison) and other modern animals, many broken, but often quite entire. Beneath this is another thinner layer of a different soil, bearing the appearance of having been formerly the bottom of a marsh. It is more gravelly, darker coloured, softer, and contains remains of reedy plants, smaller than the cane so abundant in some parts of Kentucky, with shells of freshwater Mollusca. In this layer, and sometimes partially imbedded in a stratum of blue clay, very compact and tenacious, are deposited the bones of extinct species." Mr. Cooper has been at the pains to compute, from the teeth and other parts known to have been removed from Big Bone Lick, the number of individuals requisite to furnish the specimens already carried off:

Mastodon maximus	100	individuals,
Elephas primigenius . . .	20	—
Megalonyx Jeffersonii . .	1	—
Bos bombifrons	2	—
Bos Pallasii	1	—
Cervus americanus	2	—

and it is probable that some still remain behind.

It is possible that the Horse ought to be added to this list of animals once indigenous to America. During the early settlement of the country, the great bones were either lying on the surface of the ground, or so near it as to be obtained with very little labour.

The next most important kind of locality in which such remains are often found, is simply a soft bog or meadow, where most of the finest specimens known in this country have been obtained. As an example of the common condition in which the Mastodon is found, I may describe the situation of one disinterred in 1824 near the sea-coast of New Jersey, three miles from Longbranch. "The proprietor of the farm, walking over a reclaimed marsh, observed something projecting through the turf, which he struck with his foot, and found to be a grinder tooth. Two other teeth, some pieces of the skull, the spine, the humeral, and other bones were afterwards found. The soil around was a soft dark peat, full of vegetable fibres. Though the skull and many other bones had been removed before Messrs. Cooper, Dekay, and Van Ransaeller, examined the spot, they were able to behold the vertebral column with all the joints, the ribs articulated to them, resting in their natural position, about eight or ten inches below the surface. The scapulæ both rested upon the heads of the humeri, and these, as in life, in a

vertical position upon the bones of the fore arm. The right fore arm inclined a little backwards, and the foot immediately below was a little in advance of the other, in the attitude of walking. Ten inches below the surface was the sacrum, with the pelvis united though decayed. The femora were close by, but lay in a position nearly horizontal, the right less than the left, and both at right angles with the spine. Both tibiæ, each with its fibula, stood nearly erect in their natural place beneath the femora, and below them were the bones of the hinder feet in their places: no caudal vertebræ were seen. The marsh had been drained for three years, and the surface had in consequence been lowered about two feet, producing, it has been conjectured, the dislocated attitude of the thigh-bones. Beneath the peaty bed a sandy stratum was seen, and all the feet were noticed to be standing upon the top of this floor of the bog."

I have already described the nature of the beds in which the antediluvian Mastodon tooth was found at Fort M'Henry near Baltimore; and concerning the bed in which the cane specimens, the Megalonyx, &c., have been buried, I have no information sufficiently satisfactory to offer.

Localities of Fossil Mammalia.—ELEPHAS PRIMIGENIUS: *Big Bone Lick*, Kentucky, the teeth especially in great numbers. *Biggin Swamp*, in South Carolina, teeth eight or nine feet below the surface. (Drayton.) Kentucky has furnished the greatest number of teeth, but South Carolina the largest collection of other parts of the skeleton. (Godman.) *Monmouth County*, New Jersey. (Mitchell.) *Opelousas*, west of the Mississippi, bones and teeth in recent alluvium. (See Durald in *Ann. Phil. Trans.* vol. vi. p. 55., also Darby in Mitchell's translation of Cuvier's *Theory of the Earth.*) *Stone in Carolina*, teeth. (Catesby.) *Queen Anne's County*, Maryland, a grinder, differing considerably from the tooth either of the living or fossil species, in stiff blue clay by the side of a marsh.

MASTODON MAXIMUS: *Big Bone Lick*, Kentucky, in a dark coloured marsh, the upper stratum somewhat gravelly, the substratum a blue tenacious clay, both imbedding bones; over all a light yellow soil, brought apparently from the adjacent high grounds: all the larger bones broken as if by violent action (Cooper).

The remains of Mastodon are found indeed in nearly all the *Western States* in bogs and soft meadows uncovered by any diluvial stratum. *White River*, Indiana, upper jaw and teeth. (Mitchell.) The marshes and bogs near the *Walkill*, west of the Hudson, New York. This vicinity yielded the first and finest skeleton yet procured, viz. the magnificent specimen in the Philadelphia Museum. (Peale.) Also on the *North Holston*,

a branch of the Tennessee river. *Carolina*, bones, &c., in a morass like the rest. (Jefferson's notes on Virginia.)

Again, in *Wythe County*, Virginia, at five feet below the surface, near a salt-lick, a large number of bones, almost an entire skeleton, was found, said to have been accompanied by a mass of triturated branches, leaves, &c., enveloped in a sac, supposed to be the *stomach*, not however correctly. (See Godman's *Nat. History*.) *Chester*, Orange County, New York, in a peat bog, four feet beneath the surface, many fine fragments. (Mitchell.) On the York River some fine members of a skeleton were found, in marsh mud, surrounded by roots of cypress trees. (Madison, *Medical Repository*.) On the *coast of New Jersey*, near Long Branch, in a bog, almost an entire skeleton, in the natural erect posture, the head hardly below the surface. (Cooper's *Annals of the New York Lyceum*.) In *Rockland County*, New York, grinders three feet deep in mud. (Mitchell.) Near *Baltimore*, at Fort M'Henry, in digging a well in the Star Fort, in a stratum of marsh mud, nearly sixty feet below the surface, under a layer of diluvium. (Hayden's *Geol. Essays*.) Remains of Mastodon abound at the Salines (Licks) of *Great Osage River* to as great an extent, it is said, as at Big Bone Lick, or around the Walkill. (Godman.)

Megatherium. Fragments of at least two skeletons in recent marsh, *Skidaway Island*, *Georgia*. (Cooper.)

Megalonyx. A fragment of an arm or thigh-bone, a complete radius, an ulna, three phalangal claw-bones, and some bones of the feet, found about thirty feet below the surface of the floor of a cavern in Green Briar County, Virginia. (Godman.) *Big Bone Lick* has furnished a large humerus, a metacarpal bone, a right lower maxillary bone with four teeth, a detached molar tooth in good preservation, a clavicle, a tibia of the right side. (Cooper.) Megalonyx bones have also been found in *White Cave*, Kentucky.

Bos bombifrons: two heads at *Big Bone Lick*. (Harlan's *Fauna Americana*; Wistar's *Trans. American Phil. Society*.) Bos Pallasii, Dekay: a head, *Big Bone Lick*, also *New Madrid*, on the Mississippi,—closely resembles *Bos moschatus*. Bos latifrons (Harlan): a portion of a skull, ten miles from Big Bone Lick: Cuvier allies it to the *Bos Urus* of Europe.

Cervus americanus (Fossil Elk): two imperfect skulls, *Big Bone Lick* (Cooper). Horse: *Big Bone Lick* (Cooper), *New Jersey* (Mitchell). The existence of the Horse pervious to the occupancy of this country by the Europeans, is not well established by the occurrence of its remains, though the evidence is in favour of the opinion. Walrus: anterior portion of the cranium, fossil, from *Accomac County*, Virginia. Not

known whether it belongs to the living species. This animal has not been seen on the American coast south of lat. 47°. (*Annals of the New York Lyceum*, vol. ii. p. 271.)

It was suggested, first, I believe, by Mr. Vanuxem, that all the bones of the Mammoth and other extinct quadrupeds of this country yet found, have been in either the ancient or modern alluvium. Some have been inclined to attribute them exclusively to the catastrophe which has strewed the surface of this continent with transported blocks and gravel, or have supposed, in other words, that the races perished by that diluvial action which I have before shown to have occurred, after the period of the ancient alluvium, and prior to the recent. Notwithstanding the extreme neglect which has been hitherto evinced in recording the geological situation of the interesting organic remains of the extinct Mammalia of this country, sufficient information has been collected to enable us to reason, I think with some certainty, concerning the date of their disappearance.

It will be observed that we have authentic accounts of the remains of extinct Mammalia under two entirely dissimilar situations. In one case, as in the Mastodon tooth discovered near Baltimore, the fossil occurs in an ancient bog, covered by a thick bed of sand and diluvium. This is one of the deposits which I have called ancient alluvium, and which seems to belong to some æra of the tertiary period, but what precise epoch is at present quite uncertain. Another set, apparently consisting of the very same species, occurs in the most recent class of bogs and marshes, buried to a very slight depth beneath the surface. The latter is the situation in which by far the largest number of Mastodon, Elephant, and other bones have been found. These newer bogs or marshes are in no case seen to be covered by any diluvial matter, but appear, on the contrary, from their low level and their wet state, being often traversed by streams, to have experienced little or no change since the fossil relics were originally entombed in them. In the regions beyond the Alleghanies, most of these remains occur in spots which are called Salt Licks, which are meadows and swampy grounds where the soil on the surface of the ground is impregnated with muriate of soda, from the springs which empty themselves from the muriatiferous sandstones which abound in the Western States. Big Bone Lick, in Kentucky, is an example of one of these. Here have been found not only vast numbers of the fossil bones of the extinct races, but quantities almost as great of the Buffalo, besides many of two or three species of Deer, now, like the Buffalo, indigenous to the country. This, therefore, would appear to have been resorted to not only in modern times by the living races, but more anciently by

animals now extinct, for the salt, and it may be for the food and pleasant coolness produced by the marsh. Our travellers to the western regions, where the Buffalo or Bison now ranges, have daily opportunities of witnessing these animals entrapped and perishing in these licks and swamps; and it seems evident that the Mastodon and Elephant of former times, from their huge size and unwieldy forms, must have been equally exposed to the same fate. Granting such to have been the chief cause which has buried these races, we see at once why such remains are found only in meadows or soft places, why they occur at such small depths, and why in so many cases the head has been seen resting nearly on the surface of the marsh; the cranium universally decayed; and the skeleton either in its natural erect position, or the ponderous bones below, and the ribs and vertebræ above. (See *Annals of the New York Lyceum*, vol. i. p. 145., also *Ossemens Fossiles*, 2nd edit. tom. i. pp. 217, 222.)

The state of perfect preservation in which so many of these bones are found, is another argument that the animals have perished by such a cause, and not by any violent catastrophe. There is at present in the Philadelphia Museum a pair of magnificent tusks of the Mastodon, so little acted on by time, that the beholder almost fancies he sees the marks and scratches on the enamel which it received in the living state. These beautiful remains were found by a countryman in Ohio when digging an ordinary ditch in his meadow, so that it is probable that the rest of the skeleton lies near, and at very little depth. From all the facts before me, I have little hesitation in giving my opinion that the extinct gigantic animals of this continent, the Mastodon, Elephant, Megalonyx, Megatherium, fossil Bos, and fossil Cervus lived down to a comparatively recent period, and that some of them were in existence as long ago as the æra anterior to that which covered the greatest part of this continent with diluvium.

Two interesting conclusions seem here naturally to suggest themselves: first, that the diluvial catastrophe, whatsoever it may have been, could not have introduced any very material change of climate or condition upon the continent, or we should have beheld the races sooner extinguished; and, secondly, that the physical features of the surface were the same or very nearly the same when the Mastodon lived as now; so that his extinction seems neither traceable to violent revolutions, so called, nor to any decided change of climate; which, seeing that no appreciable change of physical geography has taken place since his day, ought to remain the same now as when he formerly stalked through the continent, and perished in the same morasses which at this day entrap and bury our less gigantic living races of animals.

It may seem at variance with what I have here advanced of the recent and tranquil extinction of these animals, that in the enormous accumulation of their relics at Big Bone Lick, the boggy matter should be found partially filled with gravel, and the larger bones universally fractured. However, the small amount of gravel described as mingling with the peaty mass, seems hardly to imply that this spot was visited at this time by any violent action, such as covered the adjoining hills with their boulders and gravel; so that, on the whole, I am most inclined to explain the fractured condition of the jaws, femora, &c., by the constant treading and floundering of the huge animals over the skeletons of their ancestors.

Tertiary Formations.—Many circumstances tend to give peculiar interest to the tertiary geology of America at the present time. The day appears to have come when some of the broad conclusions recently arrived at in Europe may be fitly tested by a comparison with the phænomena of remoter regions, and America would seem to be so much dissociated from Europe, both by its insulated position and different physical structure, that the comparison between them will possess peculiar weight. The great range which characterizes all the deposits of America belongs, no less remarkably, to those of the tertiary age, and affords a very favourable opportunity for ascertaining to what extent formations so recent may be distributed without departing materially from an uniform type, or where they do depart, of determining the causes which influence the variation. The existing animal and vegetable races of this hemisphere differ so widely from those of the Old World, that we are induced to inquire when, or whether at any time, the species on the opposite sides of the Atlantic were more nearly identical. These inquiries, bearing intimately on some of the most important questions of the science, have been recently discussed with great ability by Mr. Lyell. I do not consider that our researches have proceeded to a sufficient length to render them of much weight upon many points in tertiary geology; but I nevertheless venture to remark, that they will be found to afford a striking corroboration of the soundness of the new principle upon which the tertiary formations of Europe have been arranged in chronological order by Lyell. They will perhaps be seen at the same time to suggest some slight changes in the views hitherto entertained respecting the different circumstances under which tertiary and secondary groups are supposed to have been severally formed.

The area within which the tertiary deposits of this country occur, so far as our information at present extends, is that portion of the United States which I have styled the Atlantic

plain, together with an undefined portion of the part of the great central plain of the continent which is connected with the Mississippi.

The northern limit of the tertiaries, so far as at present unequivocally ascertained, is in the south-eastern corner of New Jersey, adjacent to the Delaware Bay. Here it appears to compose the greater part of the counties of Cape May, Cumberland, and the south-west corner of Salem. From that point it is believed to extend almost continuously through the eastern portions of Delaware, Maryland, Virginia, and North Carolina, and in interrupted patches further south through South Carolina, Georgia, Alabama, and Mississippi into Louisiana, and the territory west of the Mississippi river.

The following arrangement will show the range, as far as hitherto discovered, of the tertiary and recent formations of this country.

Synoptical Table of Recent and Tertiary Formations of the United States.

Periods.	Character of Formations.	Localities of the Formations.
RECENT.	Modern alluvium, consisting of sands, clays, and marshes, containing trunks of trees; and occasionally relics of human workmanship.	Deltas of nearly all the rivers, especially the Mississippi.
	Terraced valleys of alluvial origin.	Connecticut, and many other rivers, especially in the New England States.
1.	Loose shell rock, composed of comminuted fragments of shells, exclusively those now found recent on the coast.	Anastasia Island, and the sea islands and beaches parallel to the coasts of Georgia and Florida generally. Probably most of the other sand beaches and islands also, which lie along the coast as far as Long Island.
2.	Raised estuary formation, formed almost entirely of shells of the *Rangia Cyrenoides* in a subfossil state.	Extends along the whole shore of the Gulf of Mexico from Pensacola to Franklin in Louisiana, bends around Mobile Bay, Lake Pontchartrain, and ranges across the delta of the Mississippi, immediately above its marshes, a total distance of nearly 300 miles, and probably much further.

Periods.	Character of Formations.	Localities of the Formations.
3.	Banks of oyster shells, ascribed to the Indians, contain occasionally fragments of older tertiary shells, as Pectens, &c.; matrix generally sandy, like that of the beach.	New Jersey; Choptank River, Maryland; York River, Virginia, &c.
DILUVIAL.	Boulders, pebbles, and sand, derived from the primary and ancient secondary rocks of the interior. No organic remains.	Surface of the United States generally.
ANCIENT ALLUVIAL.	Beds of clay and variegated sands. One bed is a deep black tenacious clay, containing leaves, trunks of trees, lignite, amber, and vegetable products generally. It has all the aspect of having been once a saltwater marsh, similar to that now at the mouth of the Mississippi. No remains but such as are supposed to belong to existing species have hitherto been found in these clays, and they are therefore, for the present, put apart from the true tertiary formations.	Martha's Vineyard; Long Island; greater part of New Jersey from Amboy Bay along the sections of the railroad to Bordentown; Chesapeake and Delaware Canal in Delaware; Telegraph Hill, Baltimore; near the city of Richmond, Virginia; Cape Sable in the Chesapeake Bay, Maryland. There is little doubt that the same appears interruptedly the whole way to the Mississippi.
NEWER PLEIOCENE.	A lead-coloured clay.	Mouth of the Potomac, St. Mary's County, Maryland.
OLDER PLEIOCENE and MEIOCENE.	Alternating beds of sand and clay, the sandy beds often abounding in fossil shells, which are sometimes in a friable and pulverulent state, giving the bed the character of a shell marl. These fossiliferous beds rest almost invariably on a bed of blue clay; sometimes the sands are greenish, but more usually they are yellow with a slight admixture of clay.	Cumberland County, New Jersey; Cantwell's Bridge, Delaware; Chester Town; Easton, and nearly all the eastern shore of Maryland; the whole of Charles, St. Mary's, Calvert, and part of Prince George Counties, Maryland. In *Virginia*, in Lancaster, Gloucester, and all the peninsula between James and York rivers; also nearly all Norfolk, Nansemond, Isle of Wight, Surrey, and Prince George Counties. In *North Carolina*, near the towns of Wilmington, Murfreesboro', and throughout the counties of Craven, Duplin, &c., across the State. In South Carolina, Vances ferry on Santee river seems to be about the termination of the middle tertiary groups of the United States.

Periods.	Character of Formations.	Localities of the Formations.
EOCENE.	A series of whitish and lead-coloured friable limestones, and ferruginous and siliceous sands, all abounding in extinct species of shells and zoophytes. Occurs also very frequently in the form of a fine-grained siliceous rock, abounding in casts and impressions of shells. This is used as a burr stone in Georgia.	Piscataway, near the Potomac River, Maryland; Upper Marlboro', Maryland; Vance's Ferry, South Carolina; and across to Three Runs, on Savanna River; Shell Bluff, Savanna River; Silver Bluff on the same, in Burkis County, Georgia; near Milledgeville in Georgia; Early County, Georgia; Wilcox County, Alabama; Clayborne, Alabama; St. Stephens, Alabama; Munroe, on the Washitaw River, west of the Mississippi. All these localities are on the authority of Mr. Conrad, who has either seen them in person or received eocene fossils from them.

It is necessary, perhaps, that I here explain in what sense I employ the very useful nomenclature of Lyell. I wish it to be understood that I apply the terms Pleiocene, Meiocene, and Eocene to our beds, not under the idea of any strict identity, either in geological character or age, being discoverable between them and the strata which have severally received those names in Europe, but to express simply their own comparative chronological relations, and their connexion with the recent organic races of this country, independently of any direct comparison with formations elsewhere. It is possible, indeed it is very likely, that some of our formations, our newer pleiocene, for example, may exhibit in their organic remains nearly the same proportion of recent species as certain beds in Europe, and yet differ materially from the latter in positive age; for I conceive it is a fair inference, that throughout a certain period of more or less duration, the relations of species, from general physical causes, must be more *stable* in some regions than in others, varying less rapidly, for instance, upon the tranquil shores of the United States, than near the often agitated coasts of volcanic Sicily. If this view be granted, and I think it should not be overlooked in attempting to establish identity of period in distant strata, the tertiary formations of America will furnish an instance in illustration of the importance of the caution recently given us by Mr. Murchison and other eminent geologists of England, that we make out a classification of our rocks from their *own relations*, instead of ranking them, as we have hitherto invariably done, merely as members of European types.

More extended researches in our recent conchology will doubtless inform us, from time to time, of the existence in the living state of some shells which we now regard as occurring only in the extinct and fossil state; and the natural tendency will therefore be, to lessen the apparent antiquity of each formation. It is this prospect of being compelled to modify our arrangement of the tertiary beds, as the researches multiply, that has made me hesitate to fasten upon all of them the terms of the new nomenclature, which they might otherwise claim. I propose, therefore, to designate them for the present by the convenient synonyms of 'newer tertiary' for the newer pleiocene, 'middle tertiary' for the older pleiocene and meiocene together, and 'older tertiary' for the eocene. The newer pleiocene and the eocene certainly exist in well-pronounced characters, and there will be little or no necessity, now at least, to employ their proposed synonyms; but the case is very different with the less defined formations of an intermediate age, and I shall therefore find it of essential assistance to employ, for these, the term 'middle tertiary'. American geologists will be careful not to confound the middle tertiary beds, of which I am speaking, with those which Mr. Conrad has designated by the same name, and which are clearly of eocene date, as both that gentleman (in his researches among the fossils of Clairborne, Alabama,) and Mr. Lea have been prompt to show.

Newer Pleiocene of St. Mary's County, Maryland.—In the tertiary mass now before us, the number of well-characterized shells is such as to enable us to examine their relations to the species now living in the neighbouring ocean, or peculiar to other formations. For our knowledge of the formation at the mouth of the Potomac, we are indebted exclusively to the researches of Mr. Conrad; and it is mainly from his descriptions and on other information which he has been kind enough to impart, that I am enabled to present the following brief account of the deposit. He justly pointed out its very modern character, by showing the identity of nearly all the species with the shells at present living on our coast. Mr. Conrad thus describes the formation:

"About three miles north of the low sandy point which forms the southern extremity of the peninsula, the bank of the Potomac rises to an elevation of about fifteen feet at its highest point: the fossils are visible in this bank to the extent of a quarter of a mile. The inferior stratum is a lead-coloured clay, containing vast numbers of the *Mactra lateralis* of Say, which in many instances appear in nearly vertical veins, as though they had fallen into fissures. The *Pholas costata* is also nu-

merous, and each individual remains in the position in which the living shell is usually buried in the sand or mud; that is, vertical, with the short side pointing downwards: they are so fragile, that they can rarely be taken entire from the matrix. Upon this stratum of clay, in a matrix of sand, lies a bed of the *Ostrea virginica*, in some places a foot in thickness. It is nearly horizontal; in some places at least eight or ten, and in others not more than four feet above high-water mark. The diluvium above exhibits a vein of small pebbles, traversing it horizontally, and at a distance resembling a stratum of shells. Not only are the fossils of this locality the same as existing species, but in some instances they retain their colour; a circumstance common to the later deposits of Europe. The distance from the nearest point on the Atlantic Ocean is about forty-five miles, but it is at least one hundred by the course of the bay. It will be observed, that nearly all the shells are known to inhabit the shores of the United States at the present time: those of them which are now only known in the fossil state are extremely rare, or of minute dimensions."

Mr. Conrad also mentions to me as an indication of the great tranquillity which has attended the deposition of these beds, that the underlying blue clay is everywhere penetrated by the *Pholas costata* in its natural position. The upper bed contains *Ostrea* associated with *Mytilus*, a fragile shell, in a very entire and undisturbed condition. It is not a little curious that the same fellowship of *Ostrea virginica* and *Mytilus recurvus* (*hamatus*, Say,) should subsist at the present day in the Gulf of Mexico, though the latter shell has never been seen in the more northern latitudes of our coast. The *Rangia*, likewise a gulf shell exclusively, occurs also in the same newer pleiocene, so that we seem to have indications of a higher temperature even so late as the newest of our tertiary periods. Several of the species, however, in the foregoing table were long supposed by our conchologists to inhabit, in the present day, only the most southern portions of our Atlantic coast, but the same have been since found as far north as the shores of Rhode Island.

The bed of *Ostrea virginica* reposing upon the fossiliferous blue clay, has already been referred to a somewhat newer date, from the circumstance of its entire identity with the very recent beds of fossil oyster seen on the margins of some of our rivers and bays, in circumstances which prove them to be among the very newest of all the upheaved accumulations of the waters of the coast. Considering these upper remains, therefore, as not quite contemporary with the subjacent and more diversified assemblage of marine shells, we may regard the latter as having

colonized a tract in the bed of the ocean, much deeper than would be compatible with the known habits of the oyster, until the occurrence of an alteration of the level, from what may be termed deep sea, to a shallow estuary; the clay enveloping the lower shells, indicating perhaps the ooze peculiar to the one, the oysters above lying in a sediment equally characteristic of the other, namely, sand. The supposed change of circumstances would be just such as would tend to banish the pelagian shells and to introduce in abundance a race like the oyster, delighting in protected coves and shoals. A second elevation of the region must have taken place to bring both beds to their present position above high tide, and to expose them to receive their covering of diluvial pebbles, which is said to be thick and well characterized. The deposit at the mouth of the Potomac is the only one of its exact period at present discovered, though, from the appearance of successive small *upheaves* at various times, along nearly the whole Atlantic plain, it seems reasonable to look for the occurrence of beds of nearly similar age in other sections of the coast; if, indeed, we have not already found one at Charleston, and on the adjoining coast of South Carolina.

Formations of the older Pleiocene and Meiocene periods.—These deposits which, before the appearance of Mr. Lyell's nomenclature, went under the name of the 'upper marine' formation, from a supposed identity with the beds of that name in certain basins in Europe, constitute by far the most extensively distributed portion of our tertiary beds yet explored. There is even, I think, reason to believe that we have deposits of a wide range which may be separately classed, some in the meiocene, others in the older pleiocene period, though in many cases it is not possible, from the present limited catalogue of their fossils, always to infer with precision their exact comparative age: on this account, and also from the circumstance that their mutal geographical connexion has never been yet properly examined, I prefer, for temporary convenience, to treat them for the present under one head, as the deposits of one great middle tertiary group. These are clearly separated by a well-marked and perhaps wide interval from the more recent newer pleiocene on the one hand, and the more ancient eocene on the other, though there is some ground to believe that they will be found to blend into each other by various shades of approximation,—unless, indeed, future researches may point out among them an additional number of distinguishing fossils. It is very probable, from present indications, that when we have investigated the deposits of these two periods we shall find it requisite to inter-

calate several subordinate new periods between the principal ones already recognised, fulfilling the plan and the predictions of Mr. Lyell.

The proportion of species in the tables which are to follow will show how far these suggestions are pertinent. In the mean while I shall content myself with establishing, from proper evidence, the existence of both the older pleiocene and meiocene in their broader limitations, being assisted by the tables and notes of Mr. Conrad, whose researches in this field, in Maryland and Virginia, constitute the chief of what we at present know touching these formations, and whose expression of concurrence in some of my present general views gives me much satisfaction.

Geographical Range of the older Pleiocene and Meiocene Formations.—Commencing most probably, as I have already stated, in the southern extremity of New Jersey, these tertiary beds show themselves in a wide and, at present, undefined belt continuously through Delaware, Maryland, Virginia, and North Carolina, in the south of which State, and in part of the adjoining State of South Carolina, they only occur in interrupted patches, thinning out and disappearing altogether after reaching the Santee river in South Carolina.

New Jersey.—Hardly anything is known of these formations in this State, beyond what may be inferred from a small collection of shells procured by Mr. Conrad, near Stone Creek, Cumberland county, and by a few specimens of similar fossils received from Cape May and other places along the same shore of the Delaware Bay. In mineral character the middle tertiary beds of New Jersey appear, from the slight examination which they have had, to consist of yellowish siliceous sands, resting upon a lead-coloured clay, the chief receptacle of the fossil shells above enumerated. There is reason to believe that tertiary beds are nearly continuous from Salem to Cape May and Great Egg Harbour, a tract of at least forty miles long by ten or fifteen broad. How much more of the peninsula of New Jersey may consist of tertiary beds, we cannot say, as the surface is deeply covered by diluvium and sea sand.

Delaware.—In this State tertiary formations of the same period certainly exist, though it is a district which has received no attention. At Cantwell's Bridge, fossils have been procured, which Mr. Conrad is inclined to refer to the middle tertiary, though we consider the locality to require further investigation before we can pronounce even thus far.

Maryland.—Formations, the major part at least of which are fairly of the middle tertiary age, occupy nearly the whole

surface of both shores of the Chesapeake Bay south of an irregular line drawn from Kent county to a few miles below the city of Washington on the Potomac. Over this great area, which is nearly one hundred miles long from north to south, and more than fifty wide, the tertiary beds are seen under nearly uniform characters in almost every spot where the rivers or ravines have exposed sections. The upper layers are usually arenaceous and repose very generally upon a more argillaceous stratum, often developed as an almost pure lead-coloured clay. Both deposits are highly fossiliferous, and when seen on the side of a river, present, sometimes, little else than a bank of shells and zoophytes, often in a state of fine preservation.

Some of the most conspicuous localities are on the Chester river, which is about the northern boundary; also at Easton and Cambridge, all on the eastern shore; and again on the western shore of the bay, especially in St. Mary's county, where many of the fossils of this formation were first discovered. Mr. Conrad describes the fossiliferous mass as extending in the precipitous banks of St. Mary's river nearly a mile. This bed, he says, contains many extinct species; it furnishes a large number of genera, with very few species of each, while the individuals are in the greatest abundance. The bank is elevated perhaps thirty feet in the highest point above tide, and there the stratum of shells rises fifteen feet above the river. Siliceous masses with imbedded shells are numerous, and are used for the foundations of buildings. The inferior stratum of these banks is clay, which appears to contain the same species of shells as the sand above it. On the eastern shore of Maryland in the banks of the Choptank, not far from Easton, Mr. Conrad observed the following section.

	Feet.
1. Diluvium.	
2. Sand, with *Pecten Madisonius* and *Balanus Proteus* almost exclusively	2
3. *Cytherea marylandica, Corbula* and *Pecten Madisonius*, in sand	7
4. *Cytherea marylandica* in vast numbers, in sand, with *Crassatella marylandica* in abundance	4
5. Blue clay, with *Perna maxillata.*	

Virginia.—Tertiary deposits, apparently of the same middle group, occupy, it is believed, nearly all Virginia east of a few miles below the primary boundary, and are seen to put on all the varieties observable in Maryland, being continuous and identical with those just described as belonging to that region. The average breadth of the deposit in Virginia may be stated, therefore, at about sixty miles, and its length the whole extent of the State, from the Potomac south to the State of North Ca-

rolina. Throughout this great area it has scarcely received any geological examination, our only accurate information being that procured by Conrad in his researches among the fossils in Suffolk county, and again at York town, and some recent examinations made by my brother, W. B. Rogers, along the James and York rivers and the peninsula embraced between them. The general distribution of the formation, however, is well known, because the fossiliferous parts of the deposit are sought over nearly the whole region for the fertilizing action of their carbonate of lime and shells upon the soil, in consequence of which the whole deposit bears the name of marl in all the States in which it occurs. I select for description the beds upon the James and York rivers, as being best known to us, and probably characteristic of the deposit generally.

On the James river, along the cliffs in the counties of James city and Warwick, the fossiliferous strata are finely exposed. My brother, Professor Rogers, thus describes the locality: "By far the most striking exhibition of the tertiary strata which I have yet seen is on the bank of James river, from a little above King's Mill upwards. The bank has an average height of sixty feet, and from the water-line to a few feet from the top is occupied by shells: in some places huge blocks of the deposit have fallen down, exposing the specimens in a very perfect state. The mass in which the shells are imbedded is usually a stratum of sand, sometimes covered by, but mostly resting upon, blueish clay, which also includes the same fossils. The sand, as in Maryland, is mostly yellowish, though it has often a green hue, like that called *turtia* by the French. It is sometimes indurated into a rough concreted mass by the cementing action of the carbonate of lime of the shells. An interesting fact is, the occurrence among it of the green grains so characteristic of the secondary greensand of New Jersey."

My brother states, that after examining at least thirty distinct localities, he has found the greensand an invariable ingredient in all, some having as much as thirty per cent. of this mineral. At Burwell's Mill the stratum over the shells for five feet is an *olive* and red clay, containing from thirty to forty per cent. of the greensand, from which it receives its colour, olive or green, precisely as certain beds of similar clay in the secondary tracts of New Jersey acquire the same tint.

A section upon the side of a mill-pond recently drained near Williamsburgh, about midway between the York and James rivers, affords the following arrangement: 1st, reddish sand, about eight feet, containing near the bottom a stratum two feet thick of shells, chiefly *Venus* and *Arca idonea*, very large;

2nd, blue marl, full of *Venus*, twelve inches; 3rd, blueish green marl, four feet thick, having at the bottom a mass of *Pecten*, and below this a crowded layer of *Perna*, all perfectly horizontal.

On the York river the stratification, though it does not exhibit so lofty a precipice filled with shells as before described on the James river, presents the clay strata very beautifully. Immediately overlying the shells is a continuous bed of clay, many miles long, in some places forming a vertical wall, ten or fifteen feet high, and as smooth as masonry. It is of a blue colour, and divided by thin layers of sand, perfectly horizontal, into portions about eight to twelve inches thick, so that the appearance is very much like that of a wall. The incumbent clay in some places thins out, and changes colour to a reddish brown, which makes it scarcely distinguishable from the diluvium above; sometimes it is subdivided into two strata, separated by sand and gravel. This clay is a very common deposit throughout the tertiary marl region, sometimes beneath, sometimes above the shells, and often both below and above, and also containing shells. In appearance it resembles a clay which is a member of the secondary greensand formation of New Jersey. From York town, six miles up the river, is the following interesting section: 1st. Near York, a curious rock, containing shells, often in minute fragments, being somewhat like masses of the crag of England. Here the strata are not horizontal; but in a ravine below the town they dip on opposite sides towards the ravine at an angle of more than thirty degrees. This shell rock is an indurated calcareous sand, formed of shells, not partially decomposed, but comminuted by attrition. It had obviously been subjected, as Mr. Conrad observes, to a violent action of the waters at a period anterior to the tranquil deposition of the perfect shells it contains. 2nd. From York town to about three miles up the river, the principal stratum consists of shells, overlaid by the above-mentioned blue clay, separated near its western end into two strata. 3rd. At some distance higher up, the shell rock again takes the place both of the stratum of shells and the overlying blue clay. 4th. Beyond this again, and still on the same level, the blue clay is seen resting once more on the unbroken shells. The appearance of the wall of clay in these places is very curious; it is as smooth as if cut with a spade, and resembles the wall of a fortress. On the Nansemond river, in the immediate neighbourhood of Suffolk, very nearly the same series of strata is seen as described upon the York and James rivers. Yellowish sand reposes most generally upon the blue clay, both beds containing a profusion of shells, and rising from the river

some fifteen or twenty feet. This would appear to be the prevailing order, not only in all the portion of Virginia here described but throughout the middle tertiary region, from whatever part of it accounts have reached us, whether from Maryland or North Carolina.

North Carolina.—The middle tertiary beds are prolonged through this State in a belt, the east and west boundaries of which are not at present ascertained, but which appears to contract both in width and thickness as we proceed south.

Professor Mitchell, of the University of North Carolina, mentions it as ranging through the following localities: Near the northern boundary of the State it appears on the Meherrin river at Murfreesboro'; further south, on the banks of the Roanoke at Scotland Neck; again on the banks of Fishing Creek near Infield, in Halifax county, and on the banks of the Tar river near Greenville, in Pitt county, and also a little below the falls of the Tar; in several places in Craven county, and on the banks of the Neuse, below Newbern. It appears also in Duplin county, and on the banks of Cape Fear river, at Walker's Bluffs, and eight miles above Elizabeth. Walker's Bluff, like all the other considerable bluffs in this State, is on the western or right bank of the river as we descend. It extends about three fourths of a mile along the river, and then recedes and loses itself in the general plain of the country above. The stratum of shells is from five to twelve or fifteen feet in thickness, and its upper surface seventy-five feet above the mean level of the water in the river. The tide flows a few miles above it. Beneath the shells are alternating and irregular strata of sand and blue tenacious clay, the latter predominating. Above the shells the surface rises as we recede from the river, until it gains a height of about one hundred feet, which is not far from the average level of the surface of this portion of the State above the sea. In Duplin and many parts of the south-eastern corner of the State, as along Cape Fear River, near Wilmington, this formation rests immediately upon the upper zoophitic limestone of our southern cretaceous rocks. It is here in fact a mere capping, having a thickness of not more than a very few feet, but still abounding in characteristic fossils.

South Carolina.—Mr. Conrad has the following observation in allusion to the southern extremity of these beds, which I have here termed middle tertiary: "The formation has not been found south of Vances Ferry, on the Santee river, in South Carolina; nor do I believe it occurs in Georgia, Alabama, or Mississippi. I never myself observed it in any part of South Carolina, though I explored the country between Charleston and

the Eutaw springs, which is wholly secondary. The deposit therefore at Vances Ferry is probably very limited in extent, and extremely superficial, capping the cretaceous rocks in the same manner as at Wilmington.

"The pleiocene probably occurs on the Santee river, near the junction of the Congaree and Wateree rivers, as Mr. Say describes two species of *Arca*, evidently pleiocene fossils, from a locality near the junction of these two rivers."

Such, then, are the principal localities at present known of the middle tertiary formations in their apparently continuous range from the Delaware to the Santee, over a tract perhaps eighty miles in breadth from the coast. In external character, mineral contents, and organic remains, the sedimentary deposits over this great tract exhibit a most marked uniformity, and were it not that the principle has been furnished us whereby, through a comparison of recent and extinct fossils, the relative antiquity of each locality may be determined, at least approximately, we should, without a doubt, regard the whole tract as one simultaneous formation. I look confidently, however, for both older pleiocene and meiocene proportions among the species, and shall not be surprised if we discover, ultimately, almost every intermediate ratio. For this intimate association of the two periods there would seem to exist a natural and obvious cause. The whole of our tertiary, and even cretaceous groups, are all deposits effected under the same general physical exposures, all accumulations upon the same coast, bearing traces of no convulsions, and therefore interrupted by no hiatus. These formations occupy one extensive plain, where the stratification is amazingly horizontal, which is crossed by no ridges, and therefore subdivided into no basins; so that the whole may be considered as having resulted from a set of causes continuing in activity throughout a long period.

Having procured a table of all our middle tertiary fossils at present known to us, and with it an enumeration of the species, recent and extinct, from the more important localities which have been explored, I am enabled to attempt a determination of the relative age of these beds, from the numerical relations of the shells.

The total number of species from our tertiary beds, excluding the eocene and the newer pleiocene, is about 195; of these, nearly forty are known as recent shells, inhabiting principally our own coast. This presents us with a proportion of rather more than twenty-one in one hundred, or about the ratio of the living species in the meiocene formations of Europe.

From New Jersey to North Carolina there is every reason to

suppose that the greater part of the tertiary tract will furnish even a less proportion of living species than *one fifth*, while the tertiary beds of North Carolina have contributed a group of shells of which nearly *two thirds* are of recent species. The latter territory would therefore most probably come within the pleiocene epoch, while the former districts are pretty clearly of the American meiocene. It is an interesting fact, however, that our meiocene shells, if we can at present call them such, resemble most the species of the European older pleiocene.

The following brief details embrace the results of a comparison of the respective fossils of each principal locality of our middle strata, according to present data.

New Jersey.—To begin with the locality in New Jersey, it will be shown that we can at present enumerate only thirteen species whose relations are established. Of these, twelve are extinct, and one is supposed to be recent.

What is curious in this small list is its containing so small a proportion of species recent on our coast, though the deposit evidently does not belong to our older tertiary, or eocene. The species are either the same as those found in the meiocene or middle tertiary of Maryland, or where they differ they are mostly analogous. It is certainly not fair to reason from such very limited data as are furnished us by this small list of fossils. New additions to our present rather small catalogue of recent shells may materially lessen the proportion of the species regarded as extinct: anticipating this, I feel the less hesitation in separating the beds of New Jersey from the eocene period. I consider it, nevertheless, possible that some of the middle tertiary formations of this country may ultimately exhibit very nearly eocene proportions, while the character of a majority of their fossils may mark them to be decidedly meiocene in their relations.

St. Mary's, Maryland.—This place has furnished about fifty-six species, thirteen of which are recent on our coast, while the remaining forty-three are extinct. The proportion of recent species here is 23 per cent.

Easton, Maryland.—The deposit upon the Choptank river, near Easton, has presented, so far, about twenty-six species, twenty-two of them extinct and four living. Among the extinct species, the *Perna maxillata* is conspicuous, as it always is wherever the deposit shows a large preponderance of species no longer living. The recent species here are about 16 per cent. of the whole, placing the bed, like that of New Jersey, perhaps in the meiocene period.

Suffolk, Virginia.—Here the total number of species procured is forty-five, about thirty-five of which are extinct and ten re-

cent on our coast. It is evident that here the proportion of living species is greater than in most of the preceding localities, the proportion being something like 22 per cent. Should this ratio not materially vary with new discoveries, the deposit must be ranked, like the preceding, with the meiocene period, notwithstanding that its shells are rather the analogues of the European older pleiocene.

York, Virginia.—About forty-four species are known from this spot, thirty-six of which are extinct, and the remainder recent. The living species here are nearly 18 per cent. of the whole, which differs but little from the ratio at Suffolk.

Smithfield, on the James River, Virginia.—The deposit at this place has furnished sixty-four species, fifty-five extinct and nine recent. This affords a proportion of about 14 per cent., which, if it be taken as the true expression of the relations of the species, would place the locality in the meiocene, and perhaps in an older division of the period than would belong to some of the preceding deposits.

North Carolina.—Though the fossil shells of this State have been very little examined, the present list indicates a group of beds decidedly more modern than any detailed above. Of thirty-seven species at present known, twenty-four, or nearly two thirds, are recent: should this proportion remain nearly the same, after the catalogue has been duly augmented, we must rank some at least of the interesting deposits of North Carolina in our older pleiocene, placing them most probably late in the period. A certain modern aspect about these shells lends countenance to this prediction.

I shall terminate this account of our middle tertiary beds with a list of the fossil shells of this period, which are common to the strata of both America and Europe. They are:

1. Lucina divaricata, *Lam.*
2. Cerithium melanioideum, *Sow.* (In the London clay.)
3. Ostrea virginiana, *Gmel.*
4. Dentalium dentalis, *Linn.* (D. alternatum, *Say.*)
5. Venus rustica? *Sow.* (Isocardia fraterna, *Say.*)
6. Pectunculus subovatus, *Say.* (P. variabilis, *Sow.*)

Older Tertiary, or Eocene.—The first notice of eocene deposits occurring in the United States, as characterized by organic remains, was published by Mr. Conrad in the *Journal of the Academy of Natural Sciences* in 1830, from observations he had made in the vicinity of Fort Washington, in Maryland: he also stated that such beds occurred at Vances Ferry, on the Santee river, where it is since ascertained that they are covered by a superficial deposit of the fossils of the pleiocene period. One characteristic fossil of the eocene of Claiborne (*Ostrea sellæformis,*

Conrad,) occurs at the Eutaw springs and at Nelson's Ferry on the Santee river, but it lies in a white limestone, associated with very different fossils from those which accompany this *Ostrea* at Claiborne. This limestone is doubtless analogous to that on which the tertiary of Claiborne is based, but its true character is given by Dr. Morton, in his *Synopsis*, now in the press. Eocene deposits commence in Maryland, and extending in a south-west direction, crop out at intervals in the States of Virginia (?) and North and South Carolina, and are always of very inconsiderable breadth. They meet the Savannah river at Shell Bluff, fifteen miles below Augusta, and appear at Silver Bluff and other places, occupying a space of about forty miles, following the course of the same river. According to Mr. Vanuxem, Shell Bluff is about "seventy feet high, formed of various beds of impure carbonate of lime, of comminuted shells, and having at its upper part the *Ostrea gigantea*? in a bed nearly six feet in thickness."

The eocene formation appears on the Oconee, below Milledgeville, judging from a few fossils which have been sent from that vicinity. The matrix is calcareous, whitish, and very friable. We know nothing of its appearance on Ocmulgee and Flint rivers, but it has been observed in various parts of Early county, and it occurs at Fort Gaines on the Chattahooche, where it constitutes a bluff from 150 to 200 feet in height, which has a close resemblance to that at Claiborne. Its extent on the river is about one mile.

In Georgia it is common to find the fossiliferous beds of the eocene developed as a pure siliceous rock or Buhr stone. The calcareous and other matter originally in the rock has all disappeared and been replaced by silica, preserving, however, the casts of shells so perfectly that they may often be readily recognised.

The eocene next appears in Wilcox county, Alabama, in the state of a hard dark-coloured sandstone, containing the characteristic shells, which are not mineralized at all, but are chalky and imperfect. This formation only extends eight or nine miles along the Alabama river. Claiborne Bluff is about one mile in length: a similar bluff, of equal extent, occurs three miles below, and about three or four miles south of this the deposit terminates in a bluff of less elevation. Here the upper bed is characterized by *Scutella Lyelli* (Conrad), the stratum being about three feet in thickness, with a matrix of angular quartzose sand, tinged by oxide of iron. Nearly the whole country in the vicinity of Claiborne is secondary, the eocene having been traced only about one mile east of the village, in the banks of a small

creek. The ridge dividing the waters of the Alabama and Tombeckbe, also secondary, is composed of cretaceous limestone, full of *Nummulites Mantelli* (Morton). St. Stephens, on the Tombeckbe, is situated on a bluff of the same, about one hundred feet in height; but the eocene appears a short distance north of it, separated from the secondary by a strip of alluvial soil. Here, however, the two upper strata only are visible, the superior bed of limestone being but a few feet in thickness, whilst at Claiborne the corresponding one is about forty-five feet thick. The arenaceous stratum is precisely similar to that of Claiborne, but the fossils are not so well preserved, and are chalky and friable. We know of no locality west of this, in Alabama or Mississippi, where the eocene formation occurs; but on the Washita river, near the town of Monroe, it is associated with the strata of the cretaceous group, as Mr. Conrad ascertained by examination of some fossils sent to the American Philosophical Society by Judge Bry. The most abundant fossil beds of the eocene at this place appears to be *Corbula oniscus* (Conrad), a shell very common in the arenaceous strata at Claiborne.

No other information has been received of any other localities of eocene deposits, but doubtless many will be discovered when geology is pursued in a more systematic manner.

The following diagram will explain the order of succession and the thickness of the strata in Claiborne Bluff, and to these are added the two members of the cretaceous group, which occur in the vicinity. Those species are indicated which occur in both formations; they are highly interesting, as they furnish indubitable evidence of the antiquity of these tertiary beds. Among more than two hundred species of shells at Claiborne, there is not one which is identical with a fossil of the pleiocene of this county; *one only is even an analogue*: not one can be referred to any recent species, much less to a native of the coast of the United States. One only, *Lutraria papyrea* (Conrad), is the analogue to a species of our coast, *L. canaliculata* (Say), in its general appearance, but is very remarkable in having the umbones turned in an opposite direction to those of the latter species.

Diagram representing the Strata composing the Bluff at Claiborne.

		Thickness.	Observations.	Range of certain Species.
Claiborne Bluff, Eocene.	1. Diluvium.	20 feet.		
	2. Whitish friable limestone.	45 feet.	Contains casts of a few species occurring in the next stratum. The most characteristic fossil is *Scutella Lyelli.* Some species of *Anthophyllum* also occur.	*Pecten calvatus.* *Scutella Lyelli.*
	3. Ferruginous siliceous sand.	6 feet.	This portion is indurated, and the fossils occur in casts.	
		. . . . . 14 feet.	. . . . . Very friable, and contains about 70 genera and 200 species of shells: among them are *Cardita planicosta, Corbis lamellosa, Pyramidella terebellata,* of the calcaire grossier; apparently no species now existing, and none identical with those of the pleiocene of Maryland.	. . . . . *Cardita planicosta.*
	4. Sand with a calcareous cement.	3 feet.	Concretion of *Ostrea sellæformis.*	
	5. Soft lead-coloured limestone.	. . . . . 70 feet.	. . . . . Contain *O. sellæformis* in abundance, but other fossils are rare; some casts of univalves, a *Pecten, Anthophyllum, Flustra, Turbinolia,* &c. Hardly a trace of those species of the strata Nos. 3 and 6.	. . . . . *Plagiostoma dumosum.* (Rare.)
	6. Friable lead-coloured limestone. Level of the river.	Thickness unknown.	Contains the same class of shells as stratum No. 3; the most characteristic fossil *Cardita planicosta,* a shell very characteristic of the eocene.	*Cardita planicosta.*
	7. Very white friable limestone.	Thickness unknown.	Contains many casts of shells peculiar to itself, and no other fossil of the next deposit than *Gryphæa Vomer.* Characteristic fossil, *Nummulites Mantelli* (Morton).	*Scutella Lyelli.* *Plagiostoma dumosum.* *Pecten calvatus.* *Ostrea sellæformis.* *Ostrea panda.* *Ostrea cretacea.* *Gryphæa Vomer.*
Cretaceous Limestones, near Claiborne.	8. Blueish limestone, alternating with friable limestone, siliceous sand, and marl.	300 feet.	The characteristic fossil is *Exogyra costata.*	*Ostrea panda.* *Ostrea cretacea.* *Gryphæa Vomer.*

"If the deposit at Fort Washington, Maryland, be correctly referred to the *eocene*, it must be a newer member of that formation than Claiborne Bluff, in as much as the species are generally distinct, and no secondary fossil occurs amongst them. The only recent species is *Venus mercenaria* (Lam.); and one of the most characteristic shells, *Ostrea compressirostra* (Say), is found in the middle tertiary on James river, Virginia. Perhaps the deposit at Fort Washington will be found to class itself in a more recent period than the eocene."

The total number of our eocene shells is about 210, nearly all the species being from a single locality, namely, Claiborne, Alabama. Other deposits, as that of St. Stephens on the Tombeckbe, present a large collection of species also, but they have been found not to differ from the species at Claiborne.

It is remarkable enough that the older tertiary or eocene strata of Alabama contain a profusion of specimens of four secondary species, and yet possess not one species common with our meiocene or middle tertiary. This is just the reverse of what occurs among the corresponding formations in Europe, the eocene and meiocene coalescing there by 42 common species in 1238 of eocene, and the cretaceous and eocene strata having nothing identical between them. From this, and the interesting fact that our formations of this period contain not a single known recent species, it seems pretty evident that our southern tertiary strata assume an earlier place in the American eocene period than the beds of the Paris basin occupy in the eocene period of Europe.

A fact equally as curious and unexpected is, that out of about 210 eocene fossils from Alabama, not more than six are discovered to be common to the same period in Europe. They are,

1. Corbis lamellosa, *Lam.*
2. Cardita planicosta, *Blainv.*
3. Bulimus terebellatus, *Lam.*
4. Solarium patulum, *Lam.* (S. scrobiculatum, *Conrad.*)
5. ——— canaliculatum, *Lam.* (S. ornatum, *Lea.*)
6. Fistulana elongata, *Desh.*

It is not improbable, however, for reasons formerly advanced, that the number of identical species will augment as our strata and coast are more explored.

Several other species show a resemblance to fossils of the eocene beds of the Paris and London basins, though they are obviously specifically different.

Connected with the foregoing comparisons among our tertiary shells ought to be an inquiry into the number of shells which frequent our coast, and their relations to the living species of

European seas. I have accordingly procured from my friend Mr. Conrad a catalogue of the known marine shells inhabiting our coast from Louisiana to Maine. This I should have been glad to insert, as such a list has not before been made, but for the length to which this Report has grown under my hands. It is important to know, however, that the whole number of marine species, excluding those of the West Indies and the southern region of the Gulf of Mexico, does not much exceed 200. It is possible that by dredging our coast a large accession to the list might accrue, yet it is apparent that the North American border of the Atlantic is not prolific in *Testacea*; and the same seems to have been equally the fact during the several tertiary periods.

Mr. Conrad and Dr. Morton have arranged with care the following useful table of *recent species* common to the European and American coasts of the Atlantic.

1. Purpura Lapillus.
2. Buccinum undatum.
3. Natica canrena.
4. Fusus islandicus.
5. Cyprina islandica.
6. Saxicava rugosa.
7. Lucina divaricata.
8. Pholas crispata.
9. ——— costata.
10. Anomia Ephippium.
11. Solen ensis.
12. Mya arenaria.
13. Mytilus edulis.
14. Modiola papuana.
15. Mactra deaurata.
16. Spirorbis nautiloides.
17. Thracia convexa.
18. Solecurtus fragilis.
19. Glycimeris siliqua.
20. Cardium islandicum.
21. ——— grœnlandicum.
22. Tellina punicea.
23. Venus mercenaria.
24. Pecten islandicus.
25. Strigilla carnaria.
26. Balanus ovularis.
27. ——— elongatus.
28. Anatifera dentata.
29. ——— vitrea.
30. ——— lævis.
31. Teredo navalis.
32. Serpula ———.

The above list is likely to be sensibly augmented as fresh species are discovered.

Here are 32 species in 200 (or one sixth) common to the two sides of the Atlantic, while, as we have seen, in 195 fossils of our middle tertiary there are but 6; and in 210 eocene fossils also only 6 which inhabited both continents during those remoter æras. We shall presently see, that during a still earlier period, that of the secondary cretaceous group, there was but a *single species* in 102 described which had this wide dispersion over both continents. Whether we shall discover a like dissimilarity in the organic remains of yet older formations is a question still to be solved, and it will require much preliminary labour and research.

In concluding this survey of our tertiary formations, I ought

not to omit the curious and important fact, in harmony, I believe, with all the views here advanced, that among the organic remains of these deposits no traces of anything of freshwater or terrestrial origin have ever been discovered.

Steps in the History of the Tertiary Formations of the United States.—The whole of that large tract of the Atlantic plain and the basin of the Mississippi now found to be occupied by the tertiary and cretaceous formations, was originally laid down by M'Clure as alluvial. The first approach to a just knowledge of its geology commenced with the determination of about forty species of fossil shells collected in Maryland by Mr. Finch. Neither of these gentlemen, however, drew any geological inferences from the organic remains they examined. Dr. Van Ransaellar afterwards referred the deposits in question to the age of the upper marine tertiary formation of England. Dr. Morton supported the same opinion, pointing out several species of fossil shells common to both sides of the Atlantic. Afterwards, in 1830, Mr. Conrad visited Maryland, discovered the newer pleiocene at the mouth of the Potomac, which however he did not pronounce to be tertiary, examined the fossils of the formations which I have called middle tertiary or older pleiocene and meiocene, and which he had previously named upper marine, and also those of Fort Washington on the Potomac, which he ventured to suggest were of the age of the London clay. In 1832, after a visit to Suffolk, James river, and York, to collect tertiary shells, Mr. Conrad commenced his work on the fossil shells of the tertiary formations of this country, retaining the term 'upper marine' for the older pleiocene, using the title 'middle tertiary' for what he had shown to belong to the age of the London clay, and which he now shows to be our eocene, and applying the name 'lower tertiary' to a class of beds described as the plastic clay formation, first by Mr. Finch, and afterwards by Hitchcock and Morton, and as subordinate to the secondary by Vanuxem, but which I have recently shown, under the appellation of 'ancient alluvium', to be of much more recent formation. Not long after, Mr. Conrad visited the eastern shore of Maryland, where, on the Choptank, he procured many new fossils, and made some interesting observations upon the beds in which they occur. In 1833 he visited Alabama, where he found the eocene very largely developed. His discoveries among the organic remains of that quarter constitute the largest contribution yet made to our tertiary geology. More recently, my brother and myself have begun the development of the pleiocene and meiocene in Virginia. In 1834, Mr. Lea published his *Contributions to American Geology*, describing about two

hundred shells from the eocene of Alabama, the right of priority to the discovery of many of which, however, Mr. Conrad and he dispute.

Many scattered descriptions of parts of our tertiary field have appeared from time to time in our Journals; but as they have contained little or no scientific geology, I do not deem it necessary here to mention them.

Cretaceous Formations.—The survey just given of our tertiary formations is calculated, I think, to show how greatly formations of the same or nearly the same period, occurring in remote regions, may differ both in mineralogical characters and in organic remains. The peculiarities which distinguish the tertiary rocks of this country from those of Europe are clearly traceable to the general dissimilarity in the physical structure of the two continents, particularly in the almost total absence of volcanic formations in the United States.

This country, for a long series of periods, seems to have suffered less repeated and powerful convulsions than the opposite shores of Europe; so that the same comparative exemption from disturbances is as apparent in our secondary as I have already shown it to be in our tertiary periods. I have already noticed the remarkably small number of species of fossils common to the tertiaries of the two continents, and I doubt if we shall ultimately establish any closer identity in those of the group now before us. While the cretaceous formations of Europe, from Ireland to Russia, are characterized throughout by a numerous class of peculiar fossils, it is not a little singular that so few of the same species should present themselves in the rocks of the corresponding period in America—not more than two perhaps of the 108 which are known. This fact, in conjunction with the no less striking one that we have yet discovered no true chalk in North America, has made me hesitate to apply without some qualification the received European names to these formations. For our information regarding this group, which embraces at present, perhaps, the most advanced portion of our geology, we are mainly indebted to the writings and researches of Dr. Samuel G. Morton, a new edition of whose work having just appeared, I am enabled to present this branch of the subject in its most complete state.

Dr. Morton entitles these newest of our secondary beds the 'cretaceous group', and regards them as divisible into two formations, the lowest of which he calls the ferruginous sand, and the upper the calcareous strata. A very few years ago the group in question was not known to extend beyond the peninsula of New Jersey and a small part of Delaware. Subsequent discoveries,

however, mainly due to Mr. Conrad, have shown it to exist in nearly all the Southern States; and from specimens brought, from time to time, from the interior of the continent, it would appear to occur abundantly on the Missouri far across towards the Rocky Mountains. From observations made by Professor Hitchcock upon the clay and sand strata of Martha's Vineyard, there seems little reason to doubt its existence either beneath that island or somewhere in the vicinity; and it is more than probable, from appearances, that it underlies Long Island. "It is first unequivocally seen in New Jersey, whence it may be traced locally through Delaware, Maryland, Virginia, North and South Carolina, Georgia, Alabama, Mississippi, Tennessee, Louisiana, Arkansas, and Missouri." Dr. Morton remarks that "these various deposits, though seemingly insulated, are doubtless continuous, or nearly so, forming an irregular crescent nearly three thousand miles in extent; and, what is very remarkable, there is not only a generic accordance between the fossil shells scattered through this vast tract, but, in by far the greater number of comparisons I have hitherto been able to make, the same species of fossils are found throughout: thus, the *Ammonites placenta, Baculites ovatus, Gryphæa Vomer, Ostrea falcata, Exogyra,* &c., are found without a shadow of difference from New Jersey to Louisiana, although some species have been found in the latter state that have not been noticed in the former, and *vice versâ.*"

Calcareous Formations.—Beds of limestone and calcareous sandstones form the upper strata of the secondary class throughout the greater part of the marl region, as it is called, of New Jersey. They always occur in thin, horizontal, and rubbly layers, either interstratified with blueish clay, or more commonly resting immediately upon the friable sands and marls of the formation beneath. The more calcareous beds are often highly fossiliferous and partially crystalline, reminding me strongly in their stratification and general appearance of some of the compact and thin oolites of England. Dr. Morton describes these calcareous strata as presenting the following varieties,—

"An extremely friable mass, containing at least 37 per cent. of lime, with a considerable proportion of iron, silex, &c. It appears to be almost entirely composed of disintegrated zoophytes—

"A yellowish or straw-coloured limestone, as hard as the carboniferous varieties, containing numerous organic remains,—

"A granular or subcrystalline limestone, intermediate in structure between the former two, and including similar fossils.

"A white soft limestone, not harder than some coarse chalks, which it much resembles, replete with fossils.

"All these varieties occasionally contain infiltrations of siliceous matter, and considerable masses of chert are sometimes observed in them: they also present some appearances of the green grains so characteristic of the marls adjacent."

These calcareous strata appear to be much less abundantly distributed in New Jersey than the friable sands and marls upon which they rest, for they have hitherto been found only at interrupted intervals along the south-eastern border of the marl region.

Limestone strata, however, seem to compose nearly the whole of the cretaceous group in the Southern States, where they exist on a scale of vast extent and thickness, rising into bold undulating hills, which resemble in their features the surface of the chalk in Europe, and seldom or never repose upon the sands which form their substrata in New Jersey. In Alabama, Mr. Conrad states this formation to constitute nearly the whole bed of the country, the eocene occupying very limited patches in the valleys of some of the rivers. Generally throughout Georgia and the States south and west of it, these limestones are developed as two distinct strata. That which is universally superior in position is a very white friable limestone, containing many casts of shells peculiar to itself, while beneath this is a compact blueish limestone, alternating with friable limestone and with greenish siliceous sand, which is indurated into a rock, and contains fossils and the peculiar green particles of silicate of iron. The thickness of the lower deposit is stated to be about 300 feet on the Alabama river. Its characteristic fossil is the *Exogyra costata*, the same shell which is so remarkably distinctive of the marl beds in the ferruginous sand formation of New Jersey and Delaware.

In some places, as in Wilcox county, Alabama, this lower limestone is seen to rest upon a still inferior bed of a friable greenish sandstone, containing fossils, especially the *Ostrea falcata*, and also presenting, like the limestone above it, some of the green grains everywhere characteristic of these cretaceous formations.

Ferruginous Sands of New Jersey.—These arenaceous strata compose the chief mass of the secondary deposits in New Jersey, being but partially overlaid by the very thin calcareous strata before mentioned. The mineralogical character of this deposit is extremely variable, though the most usual constituents are the following: 1st. Siliceous sand, mostly yellowish and ferruginous, though sometimes of a green colour, answering to the

glauconie sableuse of Brongniart. These sands occasionally occur in indurated strata containing fossils, when they form a rock precisely the same in all respects as that which underlies the limestone in Alabama. 2ndly. The peculiar greenish chloritic grains of the greensand formation of Europe. This mineral exists generally in the shape of small grains of about the size and form, and not unfrequently of the dark plumbago colour, of gunpowder. Sometimes it has a rich warm green, but more commonly an olive grey or dull blue, or even a very dark chocolate colour.

The grains, although they contain about 50 per cent. of silica, are not gritty, can be easily bruised between the teeth, and when moistened some varieties can even be kneaded into a somewhat plastic mass. A pile of this marl, as the granular mineral is called by the inhabitants of New Jersey, after being somewhat exposed to the air, frequently contracts a light grey hue, from the exterior grains becoming coated with a white inflorescence, which, from some observations I have made, is most probably carbonate of lime. The following analysis by Mr. Seybert presents a fair average of the composition of the green grains:—silica 49·83, alumina 6·00, magnesia 1·83, potash 10·12, protoxide of iron 21·53, water 9·80; loss 0·89 = 100 grains. Other analyses show occasionally as much as 5 per cent. of lime.

Mica in minute scales mingles not unfrequently in the less pure varieties of the marl, which often contains more or less blue clay.

Once or twice, in examining a mass of these mineral grains, I have detected numerous minute spicula of selenite. Almost every large heap of the marl exhales a distinct odour, closely resembling sulphur. These mineral grains occur in greater or less proportion in nearly all the strata, both arenaceous and calcareous, of the formation; but what is remarkable, they occur alone, without any admixture of either sand or clay, in a homogeneous deposit, which seems to underlie nearly the whole secondary tract of New Jersey, the stratum averaging ten or twelve feet in thickness.

It is this stratum which is especially called the marl, rather from its highly fertilizing action upon the soil than for any resemblance it has to marl strictly defined. I am not aware that the green chloritic substance has been found composing any extensive separate deposit, in such a state of entire purity, in any other region. I have met with no description of any such stratum out of New Jersey, either in Europe or among the cretaceous masses of our Southern States.

Beds of a dark blue tenacious clay, not unlike the gault of England, occur sometimes associated with these beds of marl,

and sometimes the clay and marl are mingled. Beneath the stratum of pure greensand or marl, a dark ferruginous sandstone, containing many of the same cretaceous fossils which abound in the marl, has occasionally been reached. This, which is the lowest bed of the group, exhibits a striking resemblance to some of the ferruginous sandstone and conglomerate of the lower greensand of England, and serves to indicate how similar in general the chemical and mechanical circumstances appear to have been during the same geological period on both sides of the Atlantic.

Some localities in New Jersey present "beds of a siliceous gravel, the pebbles varying in size from coarse sand to an inch in diameter, and either loose, or cemented by brown oxide and green phosphate of iron; the mass containing sometimes a profusion of fossils." When it occurs, it usually rests above the marl.

The last bed to be described is a sandstone deposit, resting above all the deposits here enumerated. It occurs rarely *in situ*, except as the top stratum on most of the detached ridges and outlying hills, but it is found, mingled with the general diluvium, in worn and broken fragments over nearly all the denuded tracts. It consists of sand and minute pebbles of quartz united by a dark brown ferruginous cement, the whole rock having a very perfect resemblance to the ferruginous conglomerate of the lower greensand at Lockswell Heath in Wiltshire, England. It is destitute, however, of fossiliferous impressions and casts. Sometimes it incloses a sensible quantity of the green grains, which, however, have no effect in modifying its colour.

The sand composing the rock has often the character of a coarse triturated beach sand; this is especially seen in the quarries about four miles east of Burlington, where it occurs in a regular horizontal bed many feet thick.

The diversified deposits of sand, marl, clay, sandstone, gravel, &c., described above, assume a great variety of mineralogical character, resulting from their various conditions of induration, and their almost endless intermixture. The most fossiliferous beds are the marl, and the marly sand which usually reposes immediately upon it. In the marl the organic remains, consisting of shells, zoophytes, and bones of *Reptilia* in great number, appear to have been preserved in a very perfect state from the imperviousness of the greensand to water, which descends with facility through the arenaceous beds above, but is invariably arrested and thrown out along the upper surface of the marl. The water percolating through the overlying marly sands has effected a change upon the fossils, leaving them in this bed

either mere casts, or almost entirely obliterating them. In its descent it is seen to become charged with ferruginous matter, staining the fossils near the upper surface of the marl of a deep brown colour, and coating whatever it overflows with a ferruginous incrustation.

I have nowhere seen a better example of the changes which the infiltration of water can effect upon strata than may be witnessed in these marl deposits of New Jersey, where every variety of dissolving and cementing agency is in hourly operation upon a large scale.

The *mineral contents* of these secondary strata of New Jersey are, iron pyrites in profusion, lenzinite, peculiar spheroidal masses of a dark green colour, carbonate and phosphate of lime occasionally replacing the fossils in the form of casts. Lignite is extremely abundant; it is found in the lower strata of the Chesapeake and Delaware canal, in almost every variety from charred wood to well-characterized jet.

The following appears to be the most usual order of the above-described cretaceous strata in New Jersey:

1. Dark ferruginous sandstone and conglomerate, consisting of limpid quartose sand, cemented by a dark brown ferruginous paste; contains also some of the green grains.

2. Rubbly calcareous stratum.

3. Arenaceous stratum, being chiefly a yellow sand, mingled with a greater or less share of the green grains, or marl, and a small quantity of clay. Sometimes thirty or forty feet thick. Fossils usually in the state of *casts*.

4. Marl. A mass of little else than the chloritic grains, loose and uncemented, 10 or 12 feet thick; full of fossils.

5. A red ferruginous sandstone, full of the impressions and casts of shells;—the particles being limpid quartz sand, and some green grains.

With respect to the basis upon which the greensands of New Jersey rest, nothing is known with certainty. Although a section was made in cutting the Chesapeake and Delaware canal, of nearly one hundred feet deep, the upper part through the beds of the ancient alluvium, and the lower through those of the cretaceous period, no older formation was reached. There seems good reason to believe, however, from the nonappearance of any formations along the Atlantic plain of an age corresponding to the oolite and new red sandstone groups of Europe, that the superior secondary beds repose, wherever they are developed in the States north of Alabama, upon rocks of the primary class. In Alabama, on the other hand, where the primary formations do not extend, the probability is, that they rest upon rocks of

the age of the grauwacke and carboniferous formations, in as much as the two have been seen by Mr. Conrad in the northern part of that State almost in contact.

The whole of the above-described strata of North Jersey might seem to merit the name of the greensand formation of the United States, and I should propose applying this designation to the deposit, in lieu of that of ferruginous sand, which was originally appropriated to it by Dr. Morton, were it not, first, that the greensand being but little developed among the beds of the same periods in the vast formations of the south, the name would not be expressive of the prevailing character of the group, except in the comparatively very limited area of New Jersey; and secondly, that in the present early stage of our discoveries, I am not entirely satisfied as to what are its true relations to the European formations, and therefore hesitate to appropriate to it the title of a formation with which there is little prospect of its ever being shown to be strictly identical either in mineral structure or organic contents.

The following more detailed description of these formations in New Jersey is so well and succinctly given by Dr. Morton in the recent edition of his *Synopsis*, that I shall extract the account almost entire.

"*Ferruginous Sand.*—In New Jersey the tract which has been known by the name of the marl district may be located as follows: Draw two lines, one from Amboy to Trenton, the other from Deal to Salem; let the Atlantic Ocean connect the eastern, and the Delaware river the western points of these lines: this irregular oblong tract incloses nearly the whole marl deposits of New Jersey, so far, at least, as it has hitherto been explored. There is reason, however, to suppose that it occupies a much larger proportion of the peninsula, especially in some places, overlaid by deep deposits of clay and sand, as at Bordentown, White Hill, &c.

"In other localities, the older pleiocene (meiocene) overlies the secondary, as is the case a few miles from Salem.

"The fossils, as will hereafter be shown, are of a very striking character, occasionally grouped in vast numbers, and in other instances almost wholly absent. The genera *Gryphæa*, *Exogyra*, and *Belemnites* are found abundantly throughout."

"*Calcareous Strata.*—The calcareous beds have been traced as far south as Salem, and north to Vincent town, a tract of nearly sixty miles in length, in a direction nearly parallel to the Delaware river, and from seven to ten miles east of it. They are marked throughout by the several varieties of calcareous rock already described, and characterized by abundance of zo-

ophytes and *Echini*, and a few species of shells. These fossils, with a few exceptions, have also been found in the arenaceous bed; but many of the organic remains of the latter are not observed in the limestone strata, which have not yielded any multilocular univalves, unless the doubtful fossil *Belemnites? ambiguus* be of this character: neither do they contain *Terebratulæ* nor *Exogyræ*."

Throughout the marl region of New Jersey, the traces of an extensive denudation of the former surface are everywhere conspicuous; and, what is remarkable, the excavation has extended almost invariably down to the marl stratum, but hardly in any case through it; the consequence of which is, that nearly all the meadows and low grounds, which are very numerous, expose this deposit immediately beneath the surface. These depressions in the surface are always occupied by creeks and streams, many of them receiving the tide, while the rest are only a few feet above it. The uplifting force must therefore have operated very equally over the whole region, as the strata themselves sufficiently evince in their undisturbed features and uniformly horizontal position, wherever they are seen, from Salem to their termination on the shores of Amboy Bay. The Neversink Hills, Mount Holly, and Mullica Hill, are low insulated outlying hills, from 100 to 200 feet elevation, having, like all the ridges in this region, their longer axes parallel with the Delaware river, or in other words, with the longitudinal diameter of the tract. These hills and ridges are almost invariably capped by a thin layer of the superficial ferruginous sandstone or conglomerate, which I have before stated to be the general overlying rock of the marl deposits. The mineralogical nature of this rock, its uniform parallelism to the other secondary beds wherever the surface has not sustained much denudation, its universal occurrence in scattered fragments throughout all the intervening denuded tracts, and the quantity of the green grains in it, are all reasons to induce me to think that this rock is a true member, and the uppermost bed of the New Jersey secondary group. The whole formation expands towards its north-eastern extremity; in approaching which it seems likewise to increase regularly in elevation, attaining its greatest height in the Neversink Hills. As to the various upheaving and denuding actions which have brought this portion of New Jersey to its present configuration, I am not now prepared to speculate, but shall merely in this place remark, that the valleys adjoining the streams in this tract, like the valleys in the tertiary districts further south, are never covered by the diluvium which invests the general surface of the country. They are also of

such size and structure as to preclude the idea that the present puny streams could have had any part in excavating them. They must suggest to every geologist the conclusion that they have been filled by the tide from one escarpment to the other, so that each was a broad bay or short tidal river.

"*Delaware. Ferruginous Sand.*—In this State, the blue and grey friable marls extend in the line of the Chesapeake and Delaware canal, from St. George's almost to the western lock. St. George's and its vicinity afford *Gryphæa* and *Exogyra* in great numbers, with *Ostrea falcata*, and some *Belemnites*. The deep cut of the canal abounds in *Ammonites*, *Baculites*, and *Scaphites*, without any of the fossils previously mentioned. This locality consists of a series of pyritous sands and clays, of which the shells are decomposed, leaving only the casts."

"*Maryland.*—I am informed that the ferruginous sand occurs below Annapolis in this state, at which place it is chiefly characterized by *Alcyonia*. Mr. Conrad obtained at Fort Washington, on the Potomac, a solitary valve of *Exogyra*, indicating the presence of this formation."

"*Virginia.*—A writer in the *American Journal of Science* speaks of the occurrence of *Belemnites* and *Gryphææ* on James river, but gives no locality."

"*North Carolina. Ferruginous Sand.*—This is well developed at Ashwood, on Cape Fear river, where, according to the late Mr. William Bertram, there are several beds of dark-coloured marl containing *Belemnites*, shark's teeth, pyritous lignite, &c. &c. These strata are surmounted by the usual diluvial mass to a depth of ten or twelve feet." At Wilmington, North Carolina, Mr. Conrad found the upper marine formation resting immediately on secondary limestone precisely like that described by Dr. Morton as occurring in New Jersey; it is in thin layers, and reposes directly on a hard rock, which is the equivalent of the ferruginous sand, as it abounds in *Exogyra costata* and other characteristic fossils. The calcareous strata are said by intelligent persons here, to extend sixty miles up Cape Fear river, and from its mouth coastwise as far north as Cape Hatteras.

"*South Carolina.—The ferruginous sand* formation occurs near Effingham's Mill, on Lynch's Creek. The fossils are chiefly *Exogyra costata*. Mar's Bluff, on Pedee river, and Nelson's Ferry on Santee river, afford the *Belemnites americanus*.

"*Calcareous Strata.*—The calcareous strata form an extensive basin to the west of the city of Charleston: this limestone, which is of the newest cretaceous formation, is mostly yellowish white, friable, and replete with fossils, although the number of species hitherto discovered is inconsiderable. Among these

the *Ostrea cretacea* and *Ostrea panda* occur also in the older cretaceous deposits of Alabama."

"*Georgia.*—The ferruginous sand appears to abound near Sandersville in this State, whence I have received a number of specimens of the *Belemnites americanus.*"

"*Alabama.*—This State presents a vast deposit of both strata. Mr. Conrad informs us that the counties of Pickens, Bibb, Greene, Perry, Dallas, Marengo, Wilcox, Downes, Montgomery, and parts of Clarke, Monroe, and Conecute, are chiefly composed of the older cretaceous strata. In Clarke county the newer cretaceous rock predominates.

"One of the localities most prolific of fossils is Prairie Bluff, in Wilcox county. The following diagram will convey an idea of its strata:

Feet 2. Loam.
2. Ferruginous sand, generally indurated, with *Exogyra* and *Gryphæa.*
70. Same deposit, in a friable state, with abundance of *Ostrea falcata.*
River bed.

"The older cretaceous rock constitutes the long and perpendicular bluff at Demopolis, where it has been ascertained by boring to be at least 500 feet thick. The more elevated bluff at Erie is chiefly composed of the same rock, which is here very friable, and well characterized by fine specimens of *Pecten quinquecostatus*, as well as abundance of *Exogyra costata.* A short distance north of Erie, the cretaceous rocks terminate, following the course of the Black Warrior; and at Tuscaloosa the old red sandstone with bituminous coal forms the bed of that river. The Tombeckbe and most of its tributaries run entirely through a region, the substratum of which is the cretaceous group, although it is probable that their sources originate in the carboniferous limestone, which may extend into the north-east section of Mississippi. We learn from travellers that the cretaceous rocks chiefly compose the countries of the Chickasaws and Choctaws, and it is highly probable that nearly the whole State of Mississippi is of the same formation. It is worthy of remark that all the prairies of Alabama and Mississippi have a substratum of the older cretaceous rock. The newer cretaceous strata prevail only in the southern portion of Alabama, are never covered with a prairie soil, and have not been observed north of the central parts of Clarke and Monroe counties.

"*Nummulite Limestone.*—'After crossing the Alabama river at Claiborne,' says Mr. Conrad, 'I travelled over a level alluvial country for two or three miles, when the surface became broken by gravelly hills, covered by a pine forest. Near Suggs-

ville the hills are formed of the nummulite limestone, masses of which are scattered in every direction: it is porous, and contains spheroidal cavities, formed, no doubt, by the decomposition of organic remains, which leave loose casts that are easily washed out by the rains. The most characteristic fossil at this place is *Ostrea panda.*

"These limestone hills occur at intervals to the vicinity of Jackson, on the Tombeckbe: on Basset's Creek one of these hills rises probably to a height of 300 feet above the water level. St. Stephens is on a high bluff of this rock, which, wherever it occurs, forms a very broken or undulating surface. A short distance above the village, the bluff rises nearly perpendicular from the river, and is about 100 feet high. Everywhere in the vicinity this limestone crops out on the summits of the hills, and myriads of *Nummulites Mantelli* are scattered over the surface of the decomposing rock. The *Gryphæa Vomer* is occasionally found among them, and the *Ostrea panda* is abundant; but no other fossils occur excepting what are peculiar to the limestone in question. On the hills the *Pecten Poulsoni* is in abundance. Near low-water mark in the bluff is a stratum of shells, consisting of *Ostrea panda* and *Plagiostoma dumosum,* both equally abundant. The surface of this rock is in many places very hard and of a blueish colour, compact and glittering when fractured, and is convertible into excellent lime.

"Again it is often white and friable, and so much resembles chalk that it is not surprising that it should have been mistaken for the real chalk of commerce, from which it differs, in possessing a coarse and more granulated structure, and in containing a considerable proportion of argillaceous earth."

"*Mississippi.*—This State has an extensive marl tract in the Chickasaw fields, near the borders of Tennessee."

"*Tennessee.*—The south-western portion of Tennessee represents a continuation of the tract just mentioned, which takes a westerly direction across the Mississippi River at the Chickasaw Bluffs."

"*Louisiana.*—Dr. Pitcher, in a recent letter, describes an extensive deposit of ferruginous sand between Alexandria and Natchitoches. Judge Bry has also noticed it near the township of Wachita, on the Wachita River, where it is recognised by *Belemnites, Ammonites,* and *Gryphæa.*"

"*Arkansas.*—Mr. Nuttall long ago found fossils of this formation on the calcareous platform of Red River, above and below the junction of the Kiameska; and Dr. Pitcher, of the

United States army, now at Fort Gibson, has obtained specimens for my use, among which I readily identify the *Gryphæa Vomer*, *Exogyra costata*, &c."

For the sake of exhibiting more fully the conditions of the comparison between the formations of the superior secondary, or cretaceous group of North America, and the equivalent group in Europe, I shall present the following summary of the organic remains hitherto discovered in New Jersey, Delaware, and Alabama.

SAURIA.

Mosasaurus.—Thought to be identical with the Mosasaurus of Europe. New Jersey. (*Morton.*)

Geosaurus.—Teeth and part of a jaw. New Jersey. (*Dekay.*)

Crocodile.—Teeth and other portions, indicating three species, from the marl region. New Jersey.

Saurodon.—(*Hays.*) Portions of a jaw of an extinct animal, the relations of which are not very clearly known. It is thought to be analogous to the Saurians. (See *American Philosophical Transactions.*)

Great Saurian of Honfleur.?—I have recently described two vertebræ from Jersey, and another from Alabama, which I regard as either identical with, or very closely allied to, bones figured by Cuvier from Honfleur, which he considers to approach nearer to the *Plesiosaurus* than to any other genus. (See *Journ. of the Acad. Nat. Sci. of Philadelphia.*)

TESTUDO.

Several bones from the marl deposit in New Jersey. (*Morton.*)

PISCES.

Squalus.—Teeth and vertebræ of several species of shark are abundant in New Jersey and Alabama. (*Morton.*)

Sphyræna.—Some remains of this curious genus of fishes occur in the blue marl of New Jersey. (*Morton.*)

AVES.

A solitary tibia of a bird of the genus *Scolopax* has been found in the green marl in New Jersey. (*Morton.*)

TESTACEA, &c.

The whole number of *Testacea*, *Echinodermata*, and Zoophytes described by Dr. Morton in his *Synopsis of the Organic Remains of the Cretaceous Group of the United States*, is 108 species. Of these, two belong to genera which are new, while one species only, the *Pecten quinquecostatus*, is thought to be common to the strata of both America and Europe.

This latter fact is certainly not a little remarkable, as it goes

to prove, contrary to general opinion, that the organic races of remote regions differed as much during a part of the secondary æra as during the more modern tertiary and recent periods.

It certainly seems difficult to explain, upon a distinction frequently admitted between secondary and tertiary formations,—namely, that the former are deep sea deposits, while the latter have been formed in more confined and local basins,—why the range of the species should have been actually less in the earlier æra than during the more modern dates of the tertiary. So far as relates to the superior secondary formations of the United States, I can perceive no evidence whatever that they were produced in a deeper sea than the tertiary beds which succeeded them. The secondary rocks have fully as much the appearance as the tertiary of having been the bed of a shallow sea, like that which encircles our Atlantic coast with so wide a belt of soundings at the present day. It must be borne in mind that all this portion of North America is, and has been since the period of the coal formation, remarkably exempt from agitation by volcanic causes; so that the Atlantic plain offers no resemblance, in its universally horizontal beds, to the broken, contorted, and denuded strata which diversify the tertiary and secondary scenery of the western regions of Europe. We are not likely ever to discover the modern formations of this country resting among the Alleghanies, as the cretaceous formations of Europe cap the Alps and Apennines. For the same reason we may look in vain over the whole of North America for a structure like that seen in the Weald, or in other well-known disturbed districts along the southern coast of England. So many successive upheavings and submersions as those shores have experienced, betoken the long-continued activity of subterranean forces during a time when the similar actions upon this side of North America were almost dormant.

We are presented with no phænomena along the flat monotonous coast of the United States, like those which lend so high a charm to the geology and scenery of the cliff-lined coast of the English Channel.

So small an amount of disturbing action ought to favour the wide dispersion of the marine inhabitants of this region; and we are therefore not to be astonished at seeing, as we do, many of the New Jersey fossils in Alabama, or at finding, as we have every reason to anticipate, the same group of species in the strata upon the Missouri, 2000 miles west from the cretaceous formations upon the Atlantic.

Similar reasons should lead us to look for a somewhat gradual transition from the secondary to the tertiary series of

fossils; and we do accordingly witness a manifest mingling of the races of the two periods, as the following Table will make apparent.

TABLE showing the Species common to the Eocene and the Upper Cretaceous Strata, and also the Species common to the latter and the Lower Cretaceous Strata, in Alabama.

Formations.	Range of Species.
Older tertiary, or eocene.	Plagiostoma dumosum. Ostrea sellæformis. Pecten calvatus. Scutella Lyelli.
Upper cretaceous limestone.	Plagiostoma dumosum. Ostrea sellæformis. Pecten calvatus. Scutella Lyelli. Ostrea cretacea. Ostrea panda. Gryphæa Vomer.
Lower cretaceous limestone.	Ostrea cretacea. Ostrea panda. Gryphæa Vomer.

After carefully reviewing, in a tabular form, the relations of the organic remains of our upper secondary group, I find that if we adopt for our data the 102 known species of *Testacea* and *Echinodermata* (rejecting the zoophytes), we perceive that 14 species are peculiar to the upper cretaceous formation of Alabama, and that only two or three of its species are found in the marl formation of New Jersey. We discover, however, that a much larger number are common to the New Jersey deposits, and the *lower* limestone formation in Alabama.

Subtracting the above 14 species, in order to make the comparison between the marl and this latter formation, we have of the two classes mentioned 88 species. Out of these 88 species, 39 are peculiar to the marl formation of Jersey and Delaware, 32 to the older calcareous strata, and 17 common to the two. These numbers show a want of identity in the fossils of the two regions worthy of notice. The two deposits, the ferruginous sand or marl of New Jersey, and the inferior calcareous strata of the south, are regarded by Dr. Morton as one formation. Though this opinion may very possibly be correct, to establish it in the present state of our data would be difficult. It is possible, indeed, that

while strata strictly synchronous are forming, as great a difference may prevail between two groups of species inhabiting remote sections of the same coast as is observable in comparing those of our two secondary deposits. But on the Atlantic coast of North America such differences should be less than upon almost any other, from the influence of the gulf-stream, and other causes elsewhere stated.

We are therefore at present at a loss to know how much of this want of identity among the species we should ascribe to disparity of age in the formations; how much to difference in the aqueous climate, and other circumstances controlling organic life.

Until a more extended list of fossils shall have been collected for the comparison, and, above all, until our geologists shall have examined more in detail the phænomena of the stratification and structure of each region, I would recommend that the question of their relative age be not anticipated by the application of a common name, but that this point be left for a season *sub judice*.

I think it not improbable that we shall ultimately regard the upper limestone of our superior secondary group in Alabama as a somewhat newer formation than the inferior calcareous strata of the same state on the arenaceous marl deposit of New Jersey. The occurrence of several of its fossils among the fossils of the overlying eocene seems to indicate that its true position *is near the top of the secondary series.*

Taken in their mineralogical relations, the marls and sands of New Jersey would seem to occupy a place corresponding nearest to the greensand formation of Europe; and the limestone strata of the south may be thought to harmonize imperfectly with the chalk, or a portion, perhaps, more truly with the calcareous strata of Maestricht. Such certainly are their rather obvious analogies mineralogically, but it is doubtful if this ought to decide the question of their relative age. I would not have it understood, therefore, that I view the American upper secondary formations in any other light at present than as the loose equivalents of the great cretaceous group of Europe. I have already mentioned the existence of *but one*, or at furthest two species, to link the organic remains of these strata in the two opposite continents.

Another striking peculiarity, which also marks the want of that resemblance which we might expect, is the absence from these formations of any true chalk deposit. There would appear to be no sufficient evidence of the existence of this remarkable formation in any known region of North America. May

not this be another result of the long dormant state of the volcanic forces in this hemisphere? It has been a received doctrine, I believe, that igneous action has had much to do with giving solubility to so vast a mass of silica and carbonate of lime, which are regarded in the chalk formation as having been produced rather in the state of a chemical precipitate than in that of a mechanical sediment.

The following recapitulation of the leading facts and deductions brought forward in the foregoing survey of our superior secondary formations, will assist in elucidating more clearly the present state of this portion of our geology.

1. The deposits of New Jersey differ from those of the Southern States in being chiefly arenaceous, and in containing an immense quantity of the pure chloritic mineral called greensand.

2. The organic remains hitherto discovered are nearly all, with the exception of one or two species, peculiar to this continent.

3. The existence of great quantities of lignite, of the remains of *Scolopax*, a shore bird, and the position of these beds in New Jersey, contiguous to the primary boundary or ancient coast, all indicate that they were deposited in a comparatively shallow sea, analogous in position to the present extensive line of soundings which skirts the coast.

The obvious shallowness of the portion of the secondary ocean where these beds were formed, may perhaps help to explain the remarkable discordance alluded to between the American and European marine species of this period.

4. The calcareous masses of Alabama, at least the upper beds, are probably different in age from the marls and arenaceous beds of New Jersey.

5. The marl formation of New Jersey is, perhaps, most nearly represented by the European greensands. The limestone deposits of the South, on the other hand, resemble more the upper members of the cretaceous group; for example, the formation of the plateau of Maestricht.

6. Thus far there is no evidence of the existence of true chalk in North America. Genuine flints have not yet been found in any bed.

7. Volcanic forces, during this period, seem to have been nearly dormant, which may perhaps assist in accounting for the absence of the chalk.

8. The want of accordance, both in organic remains and mineral character, between these beds and the cretaceous group of Europe; the difficulty of deciding their identity at present

for the want of a sufficient knowledge of the structure and superposition of our formations; and, above all, the importance of pursuing our geology free from the shackles of a nomenclature originally adapted to another continent,—render it desirable that we reject the terms in use, and appropriate to this group of formations a name which shall be independent of old associations, and yet express their position in the geological series.

Report on the State of our Knowledge of the Laws of Contagion. By WILLIAM HENRY, *M.D., F.R.S., &c., late Physician to the Manchester Royal Infirmary and Fever-Wards.*

THE subject of the following pages may perhaps appear, on first view, not to fall within those boundaries, which have been assigned by the British Association to the field of its labours. I hasten therefore to avow, at the outset, that it is no part of my object to trespass upon the province of practical medicine, or to treat the topic of contagion in any other light, than in that of a purely philosophical question. Under this point of view, the inquiry is open to all, whose education has embraced the principles of chemical and physical science, and who possess a general acquaintance with the laws of the animal œconomy. Much valuable information has indeed been already contributed to the history of contagion by persons of this class; among whom the late John Howard, the enlightened and devoted philanthropist, is an eminent example.

The establishment of sound conclusions on this subject is of the highest importance, not only to individuals and to small communities, but to the interests of whole nations. On such principles alone can wise and salutary measures for obviating the importation, and checking the spread, of contagious maladies, be based; and it is for want of them that legislators and executive governments have enforced regulations, some of which are nugatory and absurd, and others positively mischievous. The quarantine laws of every civilized country call, indeed, loudly for revisal and remodelling; and this can only be effected by mutual agreement between different nations. In their present state, those laws are both inadequate and oppressive. They lay great stress upon observances that are of no value, and overlook others that would be really efficacious. They impose grievous restraints on personal freedom; fetter our commerce; abridge the demand for produce and manufactures; and, by diminishing employment over wide and populous districts, increase the sufferings attendant on poverty, and give rise to *inborn* diseases, even more formidable than those, against which they are intended to act as barriers.

An inquiry into the laws of contagion, it must however be admitted, is beset with many pressing difficulties. Our senses, the great inlets of our knowledge of the material world, give us no insight into the properties of this subtile agent; nor do we derive

any assistance from the most refined instruments, or from the most delicate chemical tests. All that we perceive is a series of events, often faintly marked, the connexion of which with each other, even their order as to priority or sequence, can only be deduced by processes of reasoning, that are open to more than usual sources of fallacy. In no one instance is the effect of an external agent upon living animals universally the same, but modified by peculiarities of structure; by temperament, age, sex, and habit; and above all, by those imperceptible changes to which the nervous system is perpetually liable. Even our mental constitution and habits,—the imagination, the affections, and the passions,—exercise a powerful sway over our susceptibility to contagious diseases; and when such diseases do arise, often direct their course and determine their issues. The phænomena of contagion, moreover, are in many cases extremely complex, being owing to a variety of causes which it is far from easy to analyse, and separately to weigh and appreciate. The omission, too, of a single link in a chain of observations has frequently rendered the whole series valueless, as data for accurate reasoning.

Difficult, however, as the investigation is in itself, it has been rendered still more so by the manner and temper in which it has been conducted. Every kind of error, that has obstructed the progress of philosophy, may be exemplified from writers on this subject. Observers have viewed phænomena with the desire of establishing preconceived opinions. Facts have been described in language so highly coloured, or so mingled with hypotheses, that it is scarcely possible to discover its legitimate meaning. All that favours one side of an argument has been strongly insisted upon, while adverse evidence has been denied its due authority; and the love of truth has been sacrificed to the anxiety to baffle an adversary by ingenious sophistry. Such at least is a faithful picture of the greater part of what has been written on this subject *in the spirit of controversy*, excited, as it has generally been, by intemperate discussions of the quarantine laws. But it would be unjust not to except from this censure a numerous class of writers on contagious diseases, who have united an eminent capacity for observing and reasoning, with perfect singleness of purpose in the pursuit of truth. The names of Lind, Pringle, Cleghorn, Russell, Blane, Haygarth, Willan, Currie, Ferriar, and of many others who might be enumerated, are sufficient pledges for the accuracy of their reports of facts, and for the soundness of their conclusions. It is to authorities of this kind (in many instances confirmed, in a few corrected, by my own observation,) that I am chiefly indebted for the materials of the following pages, to which I have given the form of proposi-

tions or 'general laws'; not that I consider them as entitled to the weight of settled and invariable principles, but as open to be modified and amended by the results of further experience.

Laws of Contagion.

I. The animal body, when the seat of certain morbid actions, is known to elaborate within itself poisons, which are capable of imparting to healthy individuals the same diseased condition, and the power of generating similar poisons. These poisons have been called CONTAGIONS, from *contingo*, whence *contactus*; or INFECTIONS, from *inficio*. A distinction between these terms has been attempted by some writers; but, avoiding etymological discussions, I shall employ them in that general and popular sense, which regards them as synonymous or nearly so.

II. It is consistent with the testimony of the best observers*, that some contagions (chiefly those of typhus, and its congenera,) may *originate* in the animal body when exposed to the action of certain external causes. Among these causes are confinement in overheated, close, and ill ventilated places; scanty or bad food; intemperance; excessive fatigue; long exposure to cold and moisture; and, among mental influences, the whole train of depressing passions and emotions. It was doubted, however, by Mr. Howard† whether any of these causes *singly* be adequate to the production of contagious fever; but, though they certainly operate more powerfully in conjunction, there is no reason to disbelieve their separate efficiency. For, 1. The crowding of numbers together without change of air has been known to occasion low fevers of the most formidable type. Out of 146 persons, shut up during a whole night of sultry weather at Calcutta, in a wretched prison called the Black Hole, (a cube with sides of only 18 feet,) not more than twenty-three survived, of whom several were affected with low fevers of a typhoid character, ending in carbuncular eruptions‡. 2. Half a century has scarcely elapsed since our prisons and hospitals were almost constantly the seats of fevers of the worst character§, generated within their walls; and though banished from thence by an improved system of construction and management, yet similar fevers continue to *originate* in the crowded and squalid habitations of the abject poor. 3. Even among the lower animals, similar effects have been produced by the same causes. During a

* Fordyce, Haygarth, Currie, Clark, Howard, Ferriar, Willan, &c.
† *On Lazarettos*, 4to, p. 231.
‡ See Mr. Holwell's interesting Narrative, *Annual Register*, 1758, p. 278.
§ Well described by Dr. Hunter, *Medical Transactions*, vol. iii. p. 345.

long voyage in a ship, the hold of which was densely crammed with swine and sheep arranged on different sides of the vessel, Dr. Fordyce observed that both those kinds of animals were at different times attacked with contagious fevers, the symptoms varying in the two species, and the disease not spreading from the one species to the other, nor at all affecting the passengers or crew*.

III. Independently of crowding and confinement, contagious fevers do, however, occasionally arise without any immediate prototype. The recollection of every medical practitioner must furnish examples in which simple fevers, arising from cold and other causes, in persons well fed and well clad, have by neglect become contagious in their progress; and if particular examples of this kind are seldom recorded, it is because of the notoriety of the general fact. An instance of typhus fever, thus originating spontaneously, is related to have happened to one of the family of the late Dr. Jenner†.

IV. Diseases which break out in a scattered manner, where the agency of contagion can neither be traced nor even suspected, have been called *sporadic* (from σποράς, *sparsus*). This class therefore includes all disorders that are not produced by contagion; nor by accidents or obvious injuries; nor by any cause affecting numbers of individuals in common.

V. There is an extensive class of acute diseases, which have never yet been proved to arise *sporadically*. These, from the greater distinctness and more uniform succession of their symptoms, have been considered as separate species. They have therefore been termed SPECIFIC DISEASES, and their causes SPECIFIC CONTAGIONS, or SPECIFIC INFECTIONS. Such are siphylis, measles, smallpox, cowpox, hooping-cough, scarlatina, and a few others.

VI. In a great proportion of instances, specific diseases may be traced to communication, either by contact or near proximity, or intermediately, with some person suffering under the same disease. But it frequently happens that the most searching and diligent inquiry fails to trace a specific disease to its source. We are told that not one in twenty cases admitted into the Smallpox Hospital in London could be referred to any immediate original‡. In a few instances, specific diseases have appeared within boundaries which might have been supposed to have perfectly excluded them. In the Penitentiary at Millbank, a prisoner was seized with smallpox, notwithstanding his ap-

* *First Dissertation on Fever*, p. 112.
† Baron's *Life of Jenner*, p. 106.
‡ Dr. Gregory, *Cholera Gazette*, No. 2.

parently perfect insulation*. But in this and all similar cases, the probability is much greater that a specific disease, like smallpox, should have been received from a pre-existing source, than that, contrary to all experience, the poison should have originated afresh. Many instances too are on record, in which the penetration of contagious diseases, into situations supposed to be perfectly isolated, has been traced to intercourse, though forbidden by the strictest rules, and even by menaced punishment. Another mode of conveying infection, beside that of direct communication, which will be pointed out in the sequel (§. XXII. *et seq.*), will account for a great part of the apparent exceptions.

VII. The conclusion that 'contagious diseases of a specific kind never originate spontaneously,' is strengthened by the following facts:—1. They have never been met with in any country, when visited for the first time, after having been previously shut out from intercourse with the civilized world. 2. The historical æras may be fixed, when many of them first invaded the countries where they now prevail, and the line of their march may be distinctly traced out†. 3. Specific diseases have been known to become extinct for a time in certain situations, and their revival has been traced unequivocally to a foreign source. Thus, the smallpox disappeared several times from the island of Minorca, apparently from having already attacked all who were liable to it. In one instance the interval extended to seventeen years; in another, after having been absent for three years, its return was clearly traced to the crew of a ship of war which had arrived from the Levant. Seven similar intermissions of the same malady are recorded to have happened at Boston in New England, in three only of which the channel of its reintroduction could be discovered. But these three instances render it much more probable that the poison, causing the disease, should in the remaining four have been imported anew, than that it should again have been generated. For though it cannot be denied that a poison may be again elaborated, by a concurrence of the same circumstances which originally produced it, yet, in assigning causes, we must be guided by actual observations, and not by possible contingencies‡.

* Fact communicated by Dr. Roget.

† Hawksworth's *Voyages*, vol. iii. page 56. Siphylis was introduced by the crews of Bougainville's vessels into the Sandwich Islands, ii. 232. De Pauw, *Recherches Philosophiques sur les Américains*, tom. i. Robertson's *History of America*, book iv.

‡ The origin of new specific diseases is a topic too extensive to be entered upon here. The reader is referred, therefore, to an excellent essay by Dr. Ferriar, in the first volume of his *Medical Histories and Reflections*; to the various publications of Dr. Jenner, John Hunter, Adams; &c.

VIII. It may be held as a general principle, that no specific poison ever gives rise to any other contagious malady, than that of which it is itself the product. The poison of smallpox never occasions measles; nor that of measles smallpox. It must however be acknowledged, that the sequent disease is seldom an exact *fac simile* of the antecedent, but often differs from it, not only in degree, but in the absence of one or more of the usual phænomena, or in the addition of others not commonly observed. Scarlatina, it is well known, when communicated to numbers from a common source, may affect some severely and others slightly; and the general fever, the eruption, and the affection of the fauces and throat, may exhibit almost infinite varieties. In like manner, the mild and distinct smallpox has often imparted a confluent and dangerous sort; and the reverse. It is needless to multiply examples, because inconstancy of symptoms is observable, not of contagious disorders only, but of all others, whether acute or chronic. Our classifications and nomenclatures of diseases are in fact founded, not on constant and uniform characters, like those establishing the distinctions of natural history, but on general features, which are liable to be qualified by many exceptions, and which present almost infinite varieties of aspect.

IX. Of the nature of those processes, by which a simple fever becomes contagious in its progress, we are totally ignorant. The opinion that a contagious poison is, in any case, generated by a change in the animal fluids analogous to fermentation or to putrefaction, (a change veiled by Sydenham under the phrase *commotio sanguinis*), is inconsistent with general reasoning as well as with observation. The tendency to putrescence in the solids or fluids of the animal body, at temperatures favourable to that process in dead matter, is counteracted by the undefinable principle of LIFE, so long as that principle retains sufficient energy. During a contagious fever, none of those gases are *necessarily* evolved, which are the constant products of animal putrefaction. A person sick of typhus fever, enjoying all the advantages of cleanliness and fresh air, and emitting no sensible odour, may yet impart a fatal infection. An instance is on record, in which a person under such circumstances was accompanied, for about half a mile, in a coach, by four individuals, none of whom perceived the slightest odour, but all caught the infection, and died in consequence*. It may be remarked also, that the odours, which arise from persons labouring under acute specific diseases, are not similar to those of common putrefying matter, but are distinct and pe-

* Fordyce, *Dissertation*, p. 115.

culiar*. When we add to these arguments, that the perversion of a vital process, such as that of secretion, occasions, in at least one decided instance (*rabies canina*), the formation of a poison, by an organ which commonly secerns a bland and harmless fluid, the weight of evidence must be allowed greatly to preponderate in favour of the opinion, that *all morbid animal poisons are the results of vital operations*; and that chemical changes, if concerned at all, are under the control of the vital principle.

X. Among contagious poisons, there are some that exist in a visible and tangible state, generally in that of liquids; others are not at all perceptible by our senses, and are known to us only by their effects. The liquid poisons are efficient, only when applied beneath the cuticle, or to parts where the cuticle is very thin, or to the surfaces of mucous membranes; and if immediately and completely washed off, they inflict no injury. The action of some of those poisons, of siphylis for instance, does not necessarily extend beyond the part to which they are applied. Other poisons, when inserted or *inoculated*, act locally in the first instance, and afterwards give rise to general febrile excitement, which is necessary to the formation of fresh poison in the inoculated part, or in the system. After inoculation for smallpox, the constitutional disturbance is generally well marked; in cowpox, often so faintly as to be scarcely distinguishable; yet even in the latter, some degree of general fever seems to be essential to the perfect state of the pustule†. It is only at this period of full development (called the time of *maturation*) that the fluid contents of the pustule, (which in the cowpox is limpid, in smallpox purulent,) can be depended upon for producing its appropriate effect. Before maturation, the fluid is inert; after that period, it is sometimes effete, and sometimes produces a modified disease‡. All attempts to excite smallpox or cowpox, by inoculating with the blood or with any other animal fluid, have been unsuccessful§.

XI. When the liquid animal poisons are kept in a moist state, at temperatures not exceeding those of a warm atmosphere, they undergo spontaneous changes which materially affect their specific properties. Variolous matter, thus negligently preserved, has been known to produce a train of symptoms resembling those of smallpox, but yet giving no security against the return of

* The odours attending the plague, smallpox, and Asiatic cholera are instances.

† Jenner, *Inquiry*, &c., 4to, 1798, p. 71.

‡ Jenner's *Further Observations*.

§ Darwin's *Zoonomia*, § xxxiii. 2.

that disease*. But the liquid poisons, dried at the lowest temperature adequate to that purpose, may be kept in close vessels unimpaired for an indefinite time, and regain their infectious properties when moistened with very little water. The mixture of them, however, with a large proportion of water, renders them inefficient. Dr. Darwin relates that, in some experiments by Mr. Power, smallpox matter was found to be infectious after diffusion through five times its quantity of water; but that its dilution might be carried so far as to render it inert†. This is precisely analogous to what happens with common poisons, the most virulent of which is disarmed of its noxious power, when sufficiently diluted.

XII. Of the chemical constitution of the liquid contagious poisons we are entirely ignorant; nor is it probable that the knowledge, if we possessed it, would throw any light on their mode of action. We are well acquainted with the composition of many poisons (the prussic and arsenious acids, for example), without at all understanding in what way they act so powerfully upon the animal system.

XIII. Beside the liquid poisons, requiring contact for their operation, there is another class which are independent of that mode of communication, and are transmitted to small distances through the atmosphere. Such are those of scarlatina, measles, hooping-cough, chicken-pox‡, &c. In a few instances diseases imparted by contact are also caught by emanations or effluvia. The smallpox, it is well known, may be propagated in both ways; and the plague, certainly infectious at small distances, has, of late years, been proved to be communicable by inoculation with the matter of the glandular abscesses. Dr. White, after two unsuccessful attempts to inoculate himself, caught the plague by the third, and died in three days; and Dr. Valli, in 1803, fell a victim to a similarly rash experiment§.

* Jenner's *Further Observations*, p. 19. † *Zoonomia, u. s.*

‡ Chicken-pox (*varicella*) is not inoculable. See Thomson's *History of Smallpox*, 8vo, p. 283.

§ See Sir Robert Wilson's *History of the Expedition to Egypt*, p. 257; Wittman's *Travels in Turkey*, pp. 516, 518; and Granville in the *Pamphleteer*, xxv. About the close of the sixteenth century a dispute arose, which has continued almost to the present day, whether the plague be a contagious disease or not. Exclusion from that class has been extended also to typhus, yellow fever, and scarlatina. Indeed smallpox and measles are the only febrile maladies, which are admitted by some of the opponents of contagion to be propagated by a specific poison. All others, affecting numbers at one place and one time, have been by them classed with epidemics. It is needless to reply to the arguments in favour of this doctrine, because they have been already refuted, in a manner that should set the question at rest for ever, by Dr. Roget, in a Report presented to Parliament in 1825. (See *Parliamentary History and Review*,

XIV. There is only one form in which ponderable matter is capable of being transmitted invisibly through the atmosphere, viz. in that of elastic fluids, either permanent at common temperatures, or existing as such within a certain range of temperature and pressure. The former are called *gases*, the latter *vapours*; but the distinction is one of convenience only, and is not marked by any well defined boundaries. Contagious poisons, when diffused through the atmosphere environing an animal body by which they are generated, can exist only in the form of vapour. Like all other vapours they must be governed, as respects their degree of concentration in a given space, chiefly by the existing atmospheric temperature.

XV. Of the chemical constitution of contagious emanations, we are equally ignorant as of that of the liquid poisons. We may conclude, however, that they consist of the commonly known elements of animal matter, and that their diversities depend, as in several well known instances of gaseous compounds, on modifications of the proportions, or even of the molecular arrangement of like proportions, of those elements. Thus, the very same proportions of carbon and hydrogen are known to constitute no less than three elastic fluids, each distinguished by peculiar mechanical and chemical properties. From the little stability of composition of contagious poisons, evinced by their being decomposed by temperatures not above 212° Fahr., as well as, perhaps, by weak chemical agents, it appears that their elements are held together by very feeble affinities.

The notion, which appears to have originated with Kircher, that contagious emanations are at all connected with the diffusion of *animalcula* or *acari* through the atmosphere, is purely hypothetical. It has been defended, with a singular want of sound argument, by Nyander, in a dissertation which Linnæus, with equal want of judgment, has admitted into the fifth volume of the *Amœnitates Academicæ*. All that can be conceded in favour of such an hypothesis, is, that the assigned cause is not impossible; but not a single valid analogy has hitherto been advanced to confirm it. On the contrary, the opinion is at variance with all that is known of the diffusion of volatile contagions.

XVI. We have no decisive evidence, through what channels contagious emanations escape from the animal body. They may issue from the whole of its surface; but it is probable that they transpire chiefly through that fine membrane, lining the air-cells of the lungs, which the phænomena of respiration show to

8vo, published in 1826 by Longman and Co.) It is desirable that this valuable document should be made accessible to medical and general readers, by republication in some less voluminous work.

be permeable, in both directions, by gaseous and vaporous fluids. Through the same membrane, it is probable that contagious emanations are chiefly admitted into the sanguiferous vessels. Certain poisons (prussic acid, for instance,) have been traced by their odour and chemical qualities into the blood*. But as we have no tests of contagious poisons, it must remain conjectural that they also are admitted into the blood-vessels, and circulate with that fluid. Even were that point established, it would remain to be determined whether they act by producing chemical changes, or by at once affecting the nervous expansions, and through them the great nervous centres.

XVII. The theory which has been framed to account for the spread of contagious emanations, is founded on the same principle as that assumed to explain the diffusion of aqueous and other vapours, viz. that a chemical affinity exists between vapours and atmospheric air, producing a kind of *solution* analogous to that of saline bodies in water. But this theory, though ingeniously supported†, is superseded by the more probable views of Dr. Dalton, that in all mixtures of elastic fluids, whether gases or vapours, with each other, chemical affinity has no share in the effect, but that they maintain their state of equilibrium by their respective elasticities alone‡. In our atmosphere, for example, the oxygen and nitrogen gases, which are its constant ingredients, and the carbonic acid and aqueous vapour, which vary a little in their proportions, are diffused through each other by their respective elasticities, according to certain mechanical laws. This is not the fit place for a detail of the evidences, on which Dr. Dalton originally founded his opinion, nor of the additional arguments deducible from the experiments of Mr. Graham§. It is sufficient to remark, that the probabilities are greatly in favour of the new theory, which, by analogy, may be extended to the contagious vapours. These effluvia, it is probable, are also diffused through the atmosphere, not by a process of *solution*, but by the *elasticities inherent in them as vapours*; which elasticities are amenable only to variations of temperature and pressure, and are totally independent of changes in the proportions of the ingredients of the atmosphere.

XVIII. The activity of contagious emanations has been ascertained to be confined within very moderate distances from

* Christison *On Poisons*, 8vo, 1829, p. 561. "Poisons," the same writer observes, "act on the mucous membrane of the pulmonary air-cells, with a rapidity not surpassed by their direct introduction into a vein." p. 22.

† Chiefly by Dr. Haygarth. See his *Inquiry*, and also his *Sketch*.

‡ *Manchester Memoirs*, vol. v. series i.

§ *Transactions of the Royal Society of Edinburgh*, 1832.

their source. As respects the emanations of the plague, this has been attested by several writers. 1. Dr. Russell, the author of an excellent *History of the Plague*, preserved himself from that disease, during a residence of several years at Aleppo, by avoiding a nearer approach to the sick than four or five feet *. Mr. Howard's experience satisfied him that, in a still atmosphere, twelve feet was a perfectly safe distance †. Assalini took no other precaution, than to avoid inhaling the breath of persons under that disease. 2. Smallpox infection was believed by Dr. Haygarth, not only from his own experience but from a series of experiments conducted by Dr. O'Ryan, of Lyons, not to extend beyond half a yard from the patient; and that of typhus to be at least as limited ‡. 3. Scarlatina, when introduced by a new comer into a school, has generally been observed to spread first to those associated in the same class, or otherwise, with the infected person. On these facts is founded the salutary practice of separating the sick from the healthy, on the first appearance of a contagious malady; by which, in numberless instances, its progress has been effectually stopped. It is a happy consequence, also, of the limited extent of the infectious circle, that in a well aired apartment, all those soothing and beneficial ministrations, that do not require a very close approach to the sick, may be performed with little if any danger to the attendants.

XIX. It is impossible, however, to assign, to any species of contagious emanation, distinct and constant boundaries. Even in each particular instance, these limits are necessarily liable to frequent variation. For, 1. The more abundant the production of contagious effluvia, the wider, *cæteris paribus*, will be the area over which they will be diffused. 2. Imperfect ventilation extends the diameter of the infectious circle, and renders the poison efficient at distances where, by due dilution with atmospheric air, it would have been perfectly inert. Even the poisonous gases prepared by chemical processes, it is well known, may, if largely diluted with atmospheric air, be respired for a certain time, without even the slightest injury. By availing ourselves then of the law, which renders a certain state of concentration essential to the activity of volatile contagions, it is easy to obtain complete exemption from their deleterious effects. *Abundant dilution, indeed, effected by well planned and assiduous ventilation, is the most certain, if not the only, means of security against contagious emanations, as they issue from the sick.*

XX. The process of *spontaneous diffusion* is too slow to ac-

* Russell *On the Plague*, 4to, 1791, p. 99.

† *On Lazarettos*, p. 34; and Appendix to that work, p. 31.

‡ *Inquiry*, p. 97; and *Sketch of a Plan to exterminate Smallpox*, p. 237; also *Letter to Dr. Percival*, p. 9.

count of itself for the spread of contagious emanations, and is applicable chiefly to a quiescent condition of the atmosphere. But it is known that contagious poisons may be conveyed by the motion of *masses of air*, which mechanically sweep those effluvia along with them, in a state consistent with their activity *at moderate distances*. Of this it is sufficient to cite the following, out of several similar examples:—1. At the Old Bailey Sessions held in London in May 1750, the poison of jail-fever was wafted by a current of air from a prisoner at the bar, in such a direction as to infect the lord mayor, two of the judges, several of the barristers, and eight of the Middlesex jury, who all died in consequence; but all the London jury, who sat out of the current, escaped*. The black assizes at Exeter and Oxford were distinguished by similar catastrophes. 2. Even in the open atmosphere, infection may be propagated to small distances. Dr. Haygarth relates an instance, the circumstances of which were strictly investigated, in which a child was infected with smallpox, by passing another sick of that disease on the walls of the city of Chester, where they are about a yard and a half broad†. 3. Howard and Russell agree, that, in the open air the contagion of the plague lurks chiefly to leeward; and they ascribe their own exemption from its effects, when examining patients out of doors, to the precaution of always standing to the windward of the sick. It is probable that currents of low degrees of force are more dangerous vehicles of contagion than strong gales or storms, since the latter must not only dilute the poisonous vapours below their point of activity, but rapidly carry them off, so diluted, to a distance.

XXI. There is no reason to believe that the atmosphere of an extensive district, or even of a city or open street, can be mingled with such a proportion of animal contagion, as to become infectious to numbers. The extreme mobility of the particles of air among each other, and the almost unceasing variations of temperature at the earth's surface, occasion constant though sometimes scarcely perceptible currents, which mingle any poisonous vapours, that may be abroad, with the general atmospheric mass. All experience, indeed, as well as general reasoning, is against the *wide* diffusion of animal contagion in an active state. The smallpox, we are assured by Dr. Haygarth, was never known to spread from house to house, even in the most confined parts of the city or suburbs of Chester, provided the rule of non-intercourse with infected families was strictly observed ‡. The plague does not cross the narrowest streets or alleys at Constantinople,

* *Gentleman's Magazine*, 1750.

† *Inquiry*, pp. 97 and 100.

‡ *Inquiry and Sketch of a Plan*, &c.

though not ten feet wide; and the English residents at that city live in perfect security within the walls of the Pera, even while the plague is raging around them*. Nor has it ever been known in a single instance that fever hospitals, which were at first violently opposed, and even indicted at law as dangerous nuisances, have spread infection to a contiguous house. On the contrary, those institutions have often cleared their immediate vicinity from fever, by extinguishing solitary cases, which would otherwise have multiplied rapidly in the midst of poverty and filth. It is due to Dr. Haygarth to state, that in the year 1775, he first recommended the establishment of fever-wards as a practical inference from the law of the limited sphere of contagion, of which his inquiries had furnished many of the best illustrations†. His proposal was soon afterwards sanctioned by Mr. Howard, who had learned, by his own experience, the limited sphere of contagion, and the great advantages of cleanliness and ventilation in suppressing the fevers of jails and hospitals.

XXII. It has been long known that dry porous bodies, when exposed to the atmosphere, increase in weight by absorbing aqueous vapour. In like manner, there can be no doubt that contagious vapours or emanations are absorbed by porous substances, and are again exhaled in an active state. Boyle remarked that "amber, musk, and civet perfume some bodies, though not brought into contact with them, as the same determinate disease is communicable to sound persons, not only by the immediate contact of one who is infected, but without it‡." Contagious emanations, thus imbibed by porous bodies, have received the name of *fomites* §. They are capable of issuing forth with unabated, and, it is even asserted on good authority, augmented activity‖. It is probable, therefore, that they are emitted in a state of increased concentration, the porous body having imbibed those vapours, in preference to the elastic fluids which constitute the atmosphere. The propagation of contagious poisons, in the state of fomites, is illustrated by the following among numberless similar instances:—1. The contagion of the plague of 1665 was conveyed in a box of clothes from London to Eyam, a small village in Derbyshire, out of whose scanty population it carried off two hundred and fifty persons**. 2. Smallpox infection has been transmitted from London to Liverpool, by means of new apparel made in a room where persons were sick of that malady. 3. Dr. Hildebrant introduced the poison of scar-

* Clark's *Collection of Papers*; and Macmichael, *Pamphleteer*, xxv.
† *Letter to Dr. Percival.* ‡ *Boyle's Works*, by Shaw, 4to, vol. i.
§ The plural of *fomes*, fuel. ‖ Cullen, Lind, Campbell, Clark, &c.
** Mead, quoted by Howard *On Lazarettos*, p. 24.

latina into Podolia, a distance of several hundred miles, by a suit of clothes, which he had worn at Vienna while attending persons sick of that disease, and had laid by for several months*. 4. Of the propagation of a fever of the typhoid character by fomites, Sir John Pringle has recorded a striking example. A number of old tents, which had been used as bedding by soldiers sick of low fever, were, on the disembarkation of the troops at Ghent, sent to be repaired. Twenty-three Flemish workmen were employed in the business, out of whom seventeen took the fever and died, though they had no personal communication with the troops†.

XXIII. It has not been ascertained how long fomites may retain their activity; but there is reason to believe that in articles closely packed they may remain unaltered for several years. Sennertus relates an instance in which, after a violent plague at the city of Breslaw, in 1542, the pestilential contagion imbibed by linen cloth which was kept folded up, issued forth fourteen years afterwards in another city, and gave rise to a plague, which caused great devastation‡. In Dr. Parr's *Medical Dictionary* (art. CONTAGION), a fact is stated, which, if well authenticated, would indicate a much longer period for the durability of the contagion of plague.

XXIV. The subject of fomites is well worthy of further investigation. Hitherto we have acquired no information respecting the comparative powers of different porous bodies to absorb contagion. Technical distinctions into "more or less susceptible articles" are, it is true, recognised by the quarantine laws; but they appear to be founded on loose analogies rather than on careful observations. 1. It is extremely probable that *different* porous bodies vary as to their powers of absorbing the same contagious emanation, as we know that they differ in their powers of imbibing a given elastic fluid. 2. In *the same* porous body, it is quite conceivable also, that the power of absorbing different contagions may vary with its states of dryness, temperature, mechanical aggregation, and other circumstances. A light and spongy material will probably be found a more active absorbent of contagion, than the same substance when rendered dense by packing or by manufacturing operations. 3. A low temperature of the porous body will probably cause it to absorb more contagion than an elevated one; the dryness of the solid being supposed equal in both cases. When once impregnated also, an increased temperature will probably act in

* *Dict. de Médecine*, Paris, 1822, art. CONTAGION.

† Pringle *On the Diseases of the Army*, part I. ch. iii.

‡ Quoted by Boyle, *Shaw's Abridgment*, vol. i.

disengaging fomites, just as odours lurk unperceived in a garment till the wearer enters a warm apartment. It is consistent with this opinion, that clothes, which have been in contact with persons suffering under typhus, sometimes infect those who wash them in hot water. 4. The distance from the source of contagious effluvia, at which porous bodies exert their absorbent power, is undetermined. There is probably a distance at which their elasticity may be so increased by dilution, as to be more than equivalent to the absorbent power of the solid. The more highly the atmosphere surrounding the sick is charged with contagious effluvia, the more abundantly, may it be expected, that those effluvia will be absorbed by solids. 5. The colours of porous bodies have been shown, by the experiments of Dr. Stark, to exert a decided influence over their absorption of odours, the dark colours being most efficient. He has suggested, therefore, by a fair analogy, that colour may modify also the absorption of contagious effluvia*.

XXV. In several well authenticated instances, persons conveying fomites with injurious and even fatal effects to others, havr themselves escaped infection. Prisoners discharged in theie usual health from Newgate, at the time when that jail was the seat of a contagious fever, have infected the keepers of shops and public-houses in the neighbourhood†. The same consequences followed also the liberation of debtors from the jail at Gloucester. In the memorable instance, too, already cited, the criminals who, by the fomites lurking in their clothes, spread so fatal a pestilence through the court of assize, were in their ordinary state of health. Previous ablution of their bodies, and the putting on clean and uninfected clothing, would doubtless have prevented that extensive disaster.

XXVI. Of contagious diseases, some attack the same individual repeatedly: such are siphilis, typhus, and the plague. The last-mentioned, however, rarely attacks twice during one season; for out of 4400 cases, Dr. Russell observed reinfection to happen within that interval in 28 only‡. Other contagious maladis, such as smallpox, cowpox, measles, hooping-cough, and scarlatina, especially the first four, occasion some change in the human body, which, in a great majority of instances, secures it during life from a return of the same disorder. Smallpox and cowpox act as safeguards against each other; or when (failing this) the one occurs in a person who has passed through the other, the

* *Philosophical Transactions*, 1832.

† *Proceedings of the Board of Health at Manchester*, p. 89—100. Clark's *Collection of Papers*, p. 10.

‡ Russell *On the Plague*, pp. 190, 305.

second in order of sequence, whether smallpox or cowpox, assumes a modified, and generally a much milder form*. There can be little doubt, however, that those two diseases are essentially the same. We have no evidence that any one specific disease affords a security against any other, which is distinguished from it by marked characters and a different succession of symptoms. Neither smallpox nor cowpox gives a durable protection against measles, hooping-cough, or scarlatina.

XXVII. It is in few instances only that two contagious poisons act together upon the human body, producing simultaneously two distinct maladies. Scarlatina has been known to supervene on typhus; and hence the precaution, in some fever hospitals, of distinct wards for those two diseases. Smallpox and cowpox may coexist†; so also may cowpox and measles‡; but smallpox and measles are incompatible at the same time. Mr. Hunter inoculated for smallpox a child who, as afterwards appeared, had been previously exposed to the infection of measles. The measles appeared and completed its course, before the inoculation took effect, after which the smallpox began, and passed through its usual stages§. Two similar instances are related by Dr. Darwin, in both of which the smallpox, after being suspended by the measles, exhibited an unusually mild character||.

XXVIII. A certain duration of exposure to contagious emanations is essential to their full effect. This is precisely analogous to what happens with respect to noxious gases, which may be breathed in mixture with common air, for a few moments, without injury. On this subject Dr. Haygarth's observations establish the conclusion, that air weakly impregnated with smallpox or typhus contagion, may be breathed for a long time, and air strongly charged with either, for a short time, with equal impunity¶. Medical practitioners who have sustained no injury from visits of ordinary dnration, have been infected after staying unusually long in the apartments of persons suffering under contagious fevers. A very dilute contagion, however, is known to disorder the health, when it does not produce the whole of the morbid phænomena in their usual degree and order of succession.

XXIX. We have no observations sufficiently correct to enable

* For the fact that cowpox is milder after smallpox, see Jenner's Tract, 1798, p. 18.

† Adams *On Morbid Poisons*, p. 398.

‡ Jenner's Tract, 1799, p. 63.

§ Hunter *On the Blood*, &c., Introduction.

|| *Zoonomia*, §. xxxiii.

¶ *Letter to Dr. Percival*, p. 41.

us to pronounce, of any one disease, at what period it begins to be infectious. Dr. Russell could not satisfy himself on this point as to the plague*. The smallpox was believed by Dr. Haygarth not to be attended with contagious effluvia until after the appearance of the eruption, and to diffuse its poison most abundantly when the pustules had reached the period of maturation†. Scarlatina is well known to spread by infection, before the characteristic eruption on the skin shows itself. It is probable that the infectious period is not always the same for the same disease, but bears some proportion to the violence of the fever, and to other circumstances.

XXX. It has not yet been decided respecting any one disease, when it ceases to be infectious. Dr. Russell could not determine when convalescents from the plague ceased to infect others, nor when the fluid contained in the glandular abscesses was no longer dangerous. Persons, recovering from smallpox, infect others so long as the smallest scab is visible on the skin. Convalescents from scarlatina continue to impart that disease for ten days, or longer, after all the symptoms have disappeared, and even after the desquamation of the cuticle‡. Hence, in part, the difficulty of eradicating that malady from any situation where numbers are subject to it. Asiatic cholera (a disease contagious under certain circumstances,) emits the most active poison in its advanced stage, or rather in the state of consecutive fever. The infectious property of the bodies of persons who have died of that disease, though testified by several writers §, requires more accurate investigation. If the affirmative should be established, the effect may still be imputed to a poison *formed* during life, and only *exhaled* after death. Infection from bodies dead of plague is denied by Howard, Desgenettes, and Wittman, and the infectious power of yellow fever is said to terminate with life.

XXXI. It is seldom that the effects of contagious poisons, either liquid or vaporous, manifest themselves immediately after being received into the body. Well authenticated instances, however, are not wanting of the speedy and decided operation of the effluvia of plague, typhus, smallpox, &c., when in a concentrated form. But in a great majority of cases, several days or weeks (in the instance of hydrophobia, even months) have elapsed, before the morbid phænomena have appeared. The period differs for different poisons, and is not always the same for the same poison. It has been called the *latent period of infection, the time of incubation*, &c. The following intervals, though collected from the best sources, are to be considered merely as approximations.

* Russell, p. 304.
† *Inquiry*, p. 53.
‡ Blackburn, pp. 5, 14, 36.
§ Becker *On Cholera*.

The *plague*, according to Dr. Russell, lies dormant about ten days. Among those inhabitants of Aleppo, who shut themselves up after having been previously in the way of being infected, no instance occurred of the appearance of the malady after the ninth or tenth day.

In a number of cases of *smallpox* registered by Dr. Haygarth, the eruptive fever began on some day between the sixth and fourteenth after inoculation. Infection by emanations was not apparent until about two days later*. The latent period of *chickenpox* is, on an average, nine or ten days†. The pustule of *cowpox* is distinguishable about the third day after vaccination, and is perfected about the tenth‡.

The contagion of *measles* lies dormant for ten or fourteen days §. In *scarlatina* the interval does not exceed from two to six days||. No attempts to inoculate either of those diseases have yet succeeded¶.

Typhus makes its approaches in so gradual a manner, that it is scarcely possible to mark distinctly its latent period. The observations of Dr. Haygarth indicate great latitude as to the time during which typhus infection may remain dormant in the system, viz. from less than ten days to even three or four weeks**. The peculiar difficulty, however, of ascertaining the interval, reduces greatly the value of the testimony of that careful observer in this instance.

Asiatic cholera in Prussia, according to Dr. Becker, indicated a latent period of from four to six days. Observations in this country tend to establish a similar interval. Among all the vessels that performed quarantine at Standgate Creek, not one exhibited an original case of cholera after the seventh day††.

XXXII. When a number of persons are exposed, apparently under precisely the same circumstances, to a contagious poison, it seldom happens that all are affected by it. It is to individual peculiarities influencing the state of the body at the time, that we are to look for the causes of these varieties. The circumstances promoting the action of contagion have been classed together under the name of PREDISPOSING CAUSES, which agree generally in lowering the strength of the body, or depressing the energy of the mind. Among these may be reckoned fatigue, want of sleep, extreme cold or heat, crowded or close places, air tainted by putrefying substances, scanty or bad food, or oc-

* *Inquiry.* † Heberden, *Comment.* cap. 96. ‡ Jenner.
§ Heberden, cap. 63. || Blackburn, p. 34
¶ The experiments of Dr. Francis Home on the inoculation of measles, have not, I believe, succeeded in other hands.
** Haygarth's *Letter to Dr. Percival.* †† *Cholera Gazette*, No. 3.

casional long fasts, excessive evacuations, and intemperate indulgences of every sort. The depressing passions of fear, grief, and anxiety are powerful auxiliaries of contagious poisons. So also are religious creeds that lead to gloom or despondency, or that inculcate observances requiring abstinence, or other practices unfavourable to health*. But of all predisposing causes, poverty, with its attendant physical and moral evils, prepares the greatest numbers of victims to contagious diseases, and most widely spreads their destructive ravages.

It may be received, then, as a general conclusion, to be applied to all our reasonings in special instances, that NO ONE MALADY IS INVARIABLY AND UNDER ALL CIRCUMSTANCES CONTAGIOUS; in other words, that A CONTAGIOUS POISON IS SUCH ONLY IN A LIMITED AND QUALIFIED SENSE.

XXXIII. Beside the general causes promoting or counteracting the efficiency of contagious poisons, there are others of limited operation, affecting chiefly certain individuals or classes of men. 1. From peculiarities of structure or constitution not at all understood, some persons enjoy an exemption from particular contagious diseases. Before the preventive powers of cowpox were known, it was not unusual to meet with instances in which persons had entirely escaped the contagion of smallpox, though repeatedly exposed to it, and even after being inoculated with its virus. By diligent and careful inquiry, Dr. Haygarth was led to estimate the proportion of persons who had reached the middle age without taking the smallpox, at one in twenty-three; and if it be admitted that in some instances the exceptions were only apparent, there will still remain a sufficient number to establish the general observation. During the prevalence of typhus fever a similar proportion of persons has been estimated to escape†. 2. Whole tribes and classes of men share in liability to be infected by some diseases, and in the power of resisting others. In hot climates the negro resists certain morbid poisons which the European is unable to withstand. The Bedouin Arabs, we are told, wear with impunity the cast-off clothes of persons who have died of plague, without even attempting to purify them‡; but the soldiers of the French army in Egypt fell victims to the same practice, which all the authority of the General-in-chief could not suppress§. 3. Different periods of life modify the predisposition to infectious diseases. Old persons enjoy an exemption from some contagions, but not

* Instance in Howard *On Lazarettos*, p. 25, and in the *Doctrine of Fatalism*.
† *Letter to Percival*, pp. 32, 33. ‡ Blane's *Medical Logic*, p. 176, note.
§ Larry, *Mémoires*, p. 333.

from others; and infants at the breast show a remarkable insensibility to some contagious maladies.

XXXIV. But of all the circumstances that impart the power of resisting contagion, the most remarkable is the force of habit. In this respect, as in many others, we find a close analogy between ordinary and contagious poisons. Large doses of opium, any one of which would be fatal to an uninitiated person, are habitually swallowed several times daily, by those accustomed to its use. In like manner, medical practitioners and the nurses of the sick breathe, with impunity, contagious emanations to which they are in the daily habit of being exposed. It was remarked by Dr. Ferriar, that the keepers of lodging-houses in Manchester, of the lowest and filthiest kind, from which typhus fever was seldom absent, were untouched by the reeking poison, while the newcomers kept up a constant succession of victims to its effects*. To habit, also, the prisoners, who carried contagious poison in their clothes into a court of justice, owed their own protection.

XXXV. The immunity acquired by habit is not, however, in all cases either permanent or absolute. 1. Medical practitioners and nurses, who have long discontinued their avocations, have again become liable to be infected by febrile contagion†. 2. Persons accustomed to breathe without injury atmospheres impregnated to a certain extent with contagion, yield to the influence of stronger doses. The late Dr. Clark, of Newcastle, though rendered by constant habit proof against typhus contagion of common strength, caught that disease in a severe form by suddenly undrawing the bed-curtains of a patient, and thus subjecting himself to a rush of air more than usually pestilential‡. 3. Persons, who by habit are enabled to resist one kind of infection, do not on that account enjoy a security against others. Of this, beside many other instances, we have a striking illustration in the havoc, which spread so rapidly among the medical practitioners in Prussia, when Asiatic cholera first appeared in that country§.

XXXVI. There is reason to believe that contagious poisons may be received into the system, and may remain in it some time without manifesting their usual consequences, until some accidental cause calls them into full action, and gives birth to the usual train of symptoms. Circumstances of this kind have been called CONCURRING or EXCITING CAUSES. Generally speaking, they are identical with those which, acting upon the body before exposure to contagion, are termed predisposing causes, the enu-

* Ferriar, *Medical Histories*, vol. i. p. 173.

† Haygarth's *Letter*, pp. 41, 44. ‡ Clark's *Collection of Papers*.

§ Dr. Wagner's "*Report of the Cholera in Prussia*," *Bibl. Brit.*, No. 51. p. 179; and Silliman's *American Journal*, vol. xxv. p. 179.

meration of which it is needless to repeat. Dr. Russell observed the plague to "hang ambiguously" for several days about persons. In this state, and even when there was no such evidence of being infected, an overheated bath or a sudden impression of fear, especially when the disease itself was the object, has excited the lurking poison into activity*. The late Dr. Jenner, after having been much exposed to typhus contagion, experienced no ill effect until a long and fatiguing ride on horseback in extremely cold weather proved an exciting cause of that malady, which he then underwent in its usual form†. Dr. Lind relates, that out of a number of sailors, all of whom had been in the way of febrile infection, a part only, who had been permitted to go ashore, and while there had been engaged in a debauch, fell sick of low fevers.

XXXVII. Among causes influencing the spread of contagious diseases, climate has been reckoned, using that term in its enlarged sense, and not merely as applied to geographical position. There can be no doubt that climate modifies the *predisposition* of the human body to receive infections. In addition to this effect, varieties of temperature, one of the principal elements of climate, must necessarily affect the elasticity of vaporous contagions, and consequently their diffusibilities. Certain poisons (those perhaps which appear to have low vaporising points, as smallpox, influenza, and Asiatic cholera,) exert their powers alike in the hottest and coldest regions. Other poisons demand a temperature not below 60° of Fahrenheit's thermometer‡. Such is that of plague; while the yellow fever does not exist at temperatures below 80°, and in North America has been checked in its spread by a single frosty night. But an increase of temperature above a certain point (90°) disarms the contagion of plague of its power§; and typhus (or hospital) fever is unknown in tropical regions‖. Measles and scarlatina also are, in such countries, of very rare occurrence. It is not improbable that the highest temperatures observed in the atmosphere may actually destroy or decompose contagious poisons, as I have endeavoured to prove may be effected, so far as respects those of cowpox and scarlatina, by temperatures not greatly exceeding 100° Fahr.¶.

The influence of weather over the spread of contagion has not been sufficiently examined. So far as respects predisposition, it

* Howard, p. 33; and Russell, p. 303.
† Baron's *Life of Jenner*, p. 106.
‡ Blane, *Med. Log.*, p. 173.
§ Russell, Antes, &c.
‖ Dr. Hunter, *Medical Transactions*, vol. iii. p. 355.
¶ *Phil. Mag. and Ann. of Philos.*, November 1831, and January 1832.

is probably considerable. Its direct effects upon contagious effluvia are perhaps resolvable into temperature alone.

XXXVIII. Such is a general outline of the facts that are known respecting contagion, and of the conclusions to which they lead. No one, however, who has inquired into this subject, can fail to be struck with the imperfections of our knowledge respecting it,—with the paucity of observations sufficiently correct to serve as the foundations of general laws,—and with the number of questions which still remain to be solved*. A long course of diligent attention to phænomena, and a persevering and rigid employment of the inductive logic, will doubtless supply many of these deficiencies. But there is another mode of interrogating nature, hitherto little used in this department of philosophical inquiry, that of EXPERIMENT, which, in the investigations of physiology, has supplied materials for the happiest generalizations. In exploring the nature and laws of contagion, experiment has hitherto done very little; and extensive regions of discovery remain to be entered upon, with the aid of that powerful light. Difficulties and obstacles may be expected in the research, but none that, either in number or amount, would be insuperable by an ardent and inventive mind. Let it be remembered, as an incitement, that the inquiry has a higher object than the gratification of speculative curiosity; that its tendency to the advantage of mankind is direct and unquestionable; and that its success would add another triumph to those, which philosophy has already achieved over physical evil,—evil, no doubt, permitted to exist, among other reasons, that it may be overcome by the vigorous use of those intellectual powers and faculties, with which man is so preeminently endowed.

XXXIX. This view of the subject of contagion would be incomplete, without noticing a class of diseases, which have been ascribed to causes of much more extensive operation, and are generally *contrasted* with those of a contagious nature. They are named ENDEMIC and EPIDEMIC DISEASES†. Both agree in attacking a number of individuals; but the former are more limited than the latter as to the extent of their diffusion, and may often be traced to causes of local operation.

XL. 1. Acute or febrile ENDEMICS prevail, either constantly or periodically, over tracts of country of considerable area; or they may be confined to a province, a district, a city, or street, or a par-

* As these questions arise obviously out of the statements of what is already known, it appears unnecessary to collect them into a series of '*quærenda*.'

† *Endemic*, from εν *in*, and δημος *the people*; *Epidemic*, from επι *upon* or *among*, and the same substantive. The terms, therefore, differ only in the greater comprehensiveness given by the latter preposition.

ticular part of a street; or to a single building, as a house, a jail, or a penitentiary. When spread over an extensive space, several circumstances have been observed to be favourable to their production. Such are, situation with respect to the level of the sea, or that of the surrounding country; the form of the surface, as inclined or flat; the nature of the soil or substrata; the quantity and quality of the water; the state of drainage and cultivation; the vicinity of forests, and of swamps and marshes. From marshy ground exhalations almost constantly ascend, which give rise to fevers of a peculiar type, called *remittents* when they occasionally abate, and *intermittents* when the symptoms are absent for distinct intervals. In no instance has a remittent or intermittent been communicated from one individual to another; but intermittents are apt to pass into remittents, and the latter to assume a continued type, when they become decidedly contagious.

2. Marshy exhalations, or *miasms*, as they may be exclusively called (to distinguish them from animal contagions), are evolved most abundantly in hot weather, from ground which is alternately moist and dry, or barely covered with water; not if entirely or constantly inundated. Either fresh or sea-water is adequate to their production; but the alternation of the two has, in certain situations, rendered miasms particularly virulent*. Marshy ground, however, is not essential; for the half-dried gravelly beds of rivers have been observed to occasion fevers of a severe type†. In a few instances newly broken ground is recorded to have had the same effect‡. In general, miasms occupy low situations, insomuch that no greater an elevation than the upper stories of a house has afforded protection against them. But this is not universal, for they have been known to rise to considerable heights§, though in such instances the form of the ground indicates that they have been carried up inclined planes, by winds blowing from the place of their production. The sphere of the activity of marsh miasms surpasses beyond comparison that of animal contagions, obviously on account of the infinitely greater quantity in which they are generated. The

* Giorgini (Mem. read to the Royal Academy of Sciences in July 1825) gives a frightful picture of the disease called *Malattie di Cattiva*, caused by marshes of this kind at the foot of the Ligurian Apennines.

† Ferguson, *Edinburgh Transactions*, ix. 273.

‡ A remarkable instance is related in one of the latter volumes of Silliman's *American Journal*.

§ According to Monfalcon, (*Hist. des Marais*, Paris, 1824,) to 1400 or 1600 English feet. See also Ferguson, *loc. cit.*

Pontine marshes, covering an area of eight leagues by two, have spread their deleterious exhalations, in certain directions of the wind, to the mouth of the Tiber. In the West Indies miasms have affected the crews of vessels moored 1500 toises (3200 English yards) from the shore (Monfalcon); but this is probably much more than the usual distance.

3. The chemical properties of marsh miasms have been investigated by several writers, but with little other fruit than a catalogue of negative qualities*. Miasms are not the mere products of putrefaction†, and have not necessarily a fetid odour. Experiment has not demonstrated any departure, in the air over marshes, from its usual proportions as to oxygen and azotic gases. Neither carburetted, sulphuretted, or phosphuretted hydrogen, nor ammonia, has been detected in these exhalations. The principle on which their peculiar agency depends, still remains to be determined by experiment.

4. There are several points of analogy between the operation of marsh miasms, and that of contagious poisons, upon the human body. Both require a certain predisposition in the persons exposed to them; and this susceptibility is imparted by the same causes. The power of resisting miasms as well as contagions is acquired by habit, at least to a certain extent. But no continuance of usage ever protects persons, who are constantly exposed to an atmosphere impregnated with exhalations constituting *malaria*, from their pernicious effects. In some marshy countries, the perfect and vigorous human form is never seen; and a race of men inhabit them who are alike destitute of physical and mental energy, and who in middle life exhibit all the signs of old age. Strangers arriving there are doomed to inevitable destruction; and all attempts to extend our geographical knowledge of such regions, however well concerted, have been baffled by the overwhelming power of endemic pestilence.

XLI. Epidemics are much more widely diffused than endemics; so widely, indeed, that they have been imputed to certain conditions of the atmosphere, called *epidemic constitutions of the air*. To this term there can be no objection, provided it involve no hypothesis as to causes. The only legitimate meaning of the word *epidemic* is, *an acute disease prevailing over the whole or a large portion of a community, at seasons not in general*

* The most elaborate and able work which I have seen on the subject, is the *Recherches Historiques, Chimiques, et Médicales sur l'Air Marécageux*, par J. S. E. Julia. 8vo. Paris, 1823.

† It has been suggested (*Foreign Quarterly Review*, No. XXI.) that miasms are the products of plants of the genus *Chara*.

marked by regular intervals, and not traced to local causes. Though the works of writers on epidemics give us no insight into their causes, yet they contain excellent descriptions of the phænomena. Of these the following is a very general outline:

1. Epidemic diseases do not observe any fixed cycles, nor can we at all anticipate the periods of their return. Some epidemics, however, are disposed to prevail most at particular seasons of the year, as in spring and autumn.

2. Epidemics seldom spread suddenly over very extensive regions, but are observed to make a gradual, often a slow, progress from one kingdom to another, from province to province, and even from one locality to another not far remote. The influenza, (a catarrh, accompanied with extreme debility,) which was epidemic in England in 1782, was noticed in the East Indies in October and November 1781; at Moscow in December of the same year; at St. Petersburgh in February 1782; in London it was in full force in May; in France in June and July; and in Italy in July and August. In the months of August and September it prevailed in Portugal and Spain*. The Asiatic cholera, it is well known, made even a much more tardy progress from the East westwards, and did not appear in England until about fourteen years after it was known in British India.

3. On the first appearance of epidemics, they are not always distinguished by those symptoms which mark them in subsequent periods. The plague, for instance, for the few weeks after its first invasion, is frequently unaccompanied by bubos or carbuncles, which are seldom wanting when it has raged long in any place.

4. When diseases of this kind attack any country, they continue to spread until they have reached the period of their most general prevalence, called their *acme*, and then decline. These periods of commencement, acme, and decline, seldom coincide for the same epidemic at different places. Of three localities, for instance, not far remote from each other, the plague, which visited England in 1666, was often observed at the same time to be first showing itself in the one; to be at its height in another; and to be on the wane in the third. The Asiatic cholera exhibited similar irregularities in this and other countries.

5. Epidemic diseases *of the same name* differ materially, both as to degree and to symptoms, at different visitations. The epidemic of one year may be almost universally a mild and tractable disease, and that of another extremely severe and dangerous.

* See a general account of the Influenza, drawn up from the reports of medical practitioners residing in various parts of England, in the *Medical Communications*, vol. i.

6. All the *predisposing causes* enumerated as promoting the spread of contagious diseases, contribute also to that of epidemics. The latter, also, are propagated by some causes of general operation, such as a scanty harvest, or produce of bad quality; a severe winter; a scarcity of fuel; an unusually crowded population; and, on some occasions, harassing and destructive wars. In some instances, the path has been prepared for one epidemic by the previous ravages of another: in other examples, the new epidemic has acquired an ascendency over existing ones, and has either modified or entirely extinguished them. In 1666 the plague imparted much of its own form to a low petechial fever prevailing in London, but minor diseases for a while disappeared. Even the smallpox was superseded by the more powerful malady.

7. In what the influence of atmospheric changes in causing or diffusing epidemics consists, it is impossible, in the present state of our knowledge, to explain. The most diligent observation has not connected the prevalence of those maladies with any ascertained condition, either physical or chemical, of the general atmosphere. With respect to oxygen and nitrogen gases, which constitute, at a mean of the barometer and thermometer, 98$\frac{1}{7}$ in 100 of its volume, an almost perfect uniformity is known to exist. In its carbonic acid no variation has been discovered by experiment, that can be supposed to affect the animal œconomy. The varieties of proportion in its aqueous vapour are, however, much greater; and when accompanied, as they often are, by sudden changes of temperature, and by disturbances of the electrical equilibrium, may interrupt the due performance of the bodily functions. But other causes are necessary to account for those epidemics (cholera, for instance,) which defy the influence of climate, seasons, and of all changes that are objects of meteorological research. It has been suggested that an 'epidemic constitution' of the atmosphere may depend on the presence of some substance alien to its ordinary elements. No fact, however, confirms this supposition, if we except an observation of Dr. Prout, that at a period coinciding with the appearance of cholera in London, the weight of a given volume of air, making due corrections for differences of pressure and temperature, seemed to rise to a small but sensible amount above the usual standard, and continued above it during six weeks*. This observation requires, however, to be frequently and carefully repeated, and extended to other epidemics, as opportunities occur, before any sound conclusion can be founded upon it.

8. Epidemics have been contrasted with contagious diseases,

* Bridgewater Treatise, p. 350.

and supposed to form a distinct and separate class. But it must not be forgotten that certain specific diseases, which by universal consent are allowed to be contagious, at times prevail so generally as to be with propriety said to be epidemic. Such are the smallpox, measles, scarlatina, and hooping-cough. But it is inconceivable that the specific poison, which in each of these instances is the efficient cause of the disease, and which is the undoubted product of vital operations, can be generated by any 'corruption of air,' or by any spontaneous changes in inanimate matter. The only way in which a general condition of the atmosphere can be supposed to influence the spread of specific diseases is, either by rendering it a better vehicle of their respective poisons, or by influencing the predisposition of the body to receive them. But if the view which has been taken (§. XVII.) of the state in which contagions exist in the atmosphere be correct, temperature alone, by modifying the elasticity of those vapours, can affect their diffusion. It is well known, however, that ascertainable conditions of the atmosphere, as to heat or cold, moisture or dryness, and sudden transitions from the one state to its opposite, produce in the animal body a predisposition to receive contagion. The same atmospheric variations may act also as exciting causes, calling into action contagious poisons already admitted into the system, but not yet manifested by the usual phænomena; and when they operate on numbers, may occasion those sudden and violent outbursts of epidemic diseases, of which several examples are on record. Other general influences, indeed, may prove exciting causes of such outbursts. They have followed closely, for example, upon seasons of riot and intemperance, and have spread rapidly in situations where those diseases were previously confined to few and scattered individuals.

It is equally unfavourable to the progress of knowledge to overestimate what we know, as to shrink from the just appreciation of difficulties opposed to its further advancement. On the subject of epidemics, they who have inquired the most will be most ready to admit, that our actual knowledge is bounded by very narrow limits. But we are not on that account to despair. The genius of philosophers of our own age has unfolded the most astonishing truths with respect to the subtile agents—light, heat, electricity, and magnetism. Every new conquest, that science achieves, enlarges our powers over nature; and we are fully entitled by the past to hope, that the physical condition of man will in future be progressively improved by his acquiring a command over external agents, which have never yet been subjected to his knowledge and control.

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Report on Animal Physiology; comprising a Review of the Progress and Present State of Theory, and of our Information respecting the Blood, and the Powers which circulate it. By William Clark, *M.D., F.R.C. F.G.S. F.C.P.S., late Fellow of Trinity College, and Professor of Anatomy in the University of Cambridge.*

That physiology should have been a science slow and uncertain in its progress is scarcely surprising, when we consider how extensive are its objects. It pretends to nothing less than to explain the phænomena of living nature,—the conditions upon which they depend,—the laws by which they are governed. Hence, it inquires not only into the relations of every component part of an individual to each other and to the whole, but also, as far as is possible, into the mutual relations of all living things to each other, and to the rest of the world. In its useful application, therefore, it is the foundation of agriculture, of husbandry, of medicine. Intentions thus ample can only be fulfilled when all particular sciences have gained their consummation. In earlier æras it was included in those ideal assumptions, from which, as from axioms, it was conceived that all the phænomena of nature might be deduced; whilst, in later times, the attempt to treat it merely as a branch of the prevailing chemical or mechanical philosophy of the day favoured its advance in particular directions only, and with very confined conceptions of its nature and extent; as if any two of these sciences had yet ascertained, by means of their own generalizations, a common proximate cause of their phænomena; or, as if particular sciences were something else than constructions of the intellect to explain phænomena between which similarity has been established.

Physiology, as a positive science, can only be founded in observation and experiment; and the value of these depends, as in other cases, not less upon the patience, the circumspection, the dispassionate, and unprejudiced character of the observer than upon his scientific and mental elevation. The multitude of physiological experiments daily accumulated, tells us how easily they may be made; the facility with which one set of experiments so frequently supersedes a former, how difficult it is to make experiments of real value. So numerous, indeed, are the conditions with which every vital phænomenon is complicated, that the effect may really be referrible to one or more of these entirely different from that to which the experimenter has referred it. And, since it is im-

possible to abstract many of the conditions without destroying life, innumerable modifications of the experiment can alone afford an approximation to certainty. It is to experiments, in the hands of able men, where the condition may be suppressed without destroying life, that we owe a knowledge of various portions of the nervous system which is no longer problematical*.

But we are not to expect too much from experiment. It may point out the variety and the extent of vital reactions, but can teach us (as Müller has pointed out,) nothing of the nature or fundamental cause of these. For here the experiment is not like one in chemistry, where, the known agent which excites reaction in another unknown, entering as an element into the effect produced and ascertained, we are able to infer from what is known of the nature of the one element that which was before unknown of the nature of the other. But although we are thus necessarily restricted to observation of the sequences of the phænomena, and of the conditions under which they occur and are modified, yet we cannot suppose that they are without some fundamental cause, however it may be hidden from us. "Falsò asseritur sensum humanum esse mensuram rerum; quin contrà omnes perceptiones, tam sensûs, quàm mentis, sunt ex analogiâ hominis, non ex analogiâ universi†."

When physiological facts have been accumulated by observation, extended through all living things, it is the object of the science to determine the general relations which subsist amongst them; to ascertain what is common to these relations; and thus, ascending constantly to more comprehensive generalizations, to arrive at that cause, least limited by conditions, which holds inferior causes in subordination. And this is all that any experimental science can pretend to.

On the contrary, however, the first philosophy of nature was almost entirely deductive. The authors of it persuaded, as rational creatures, that all parts of the creation are but portions of an harmonious whole—productions of the same intelligent first cause—were led to speculate on the nature of that cause, and thence deduced systems from assumed principles. The universal appeared to express itself in particulars. It became the object of philosophy to begin with the essence of things, and from it to derive and explain all their phænomena. Such a philosophy, dealing with abstractions, with primary essences of which the qualities and their relations were necessarily hypothetical, could scarcely have any application to a particular creation—to the world as it

* Müller, Introductory Essay to his *Vergleichende Physiologie des Gesichtsinnes.*

† *Novum Organum*, 41.

actually exists, however rigidly its conclusions might be deduced.

A different procedure was forced upon physicians: their very office constrained them to observe the same vital phænomenon under different circumstances,—to compare different phænomena,—to separate what was common and essential from that which was merely contingent and partial. Thus was established a new principle of explanation, a principle little agreeing, perhaps, with that deduced in the former way. As observation and experiment extended the boundaries of this inductive knowledge of causes, it continually encroached more and more upon the limits of hypothetical belief. And the principles which were thus established, being founded in realities, were really the expression of the phænomena from which they were derived. It has been, however, by slow and gradual steps that men have become willing to abstain from assuming, as a privilege of the understanding, the power of creating that spontaneously which can only be supplied by the long and patient contemplation of nature. The two systems have been, more or less, in conflict from the earliest to the present times.

Hippocrates was the first, whose writings have come down to us, who made experience the interpreter of nature. He collected a rich treasure of observations, the accumulated result of his own labours and of those of his family during 300 years. They relate to the investigation of the effect of changes in the external conditions of life,—viz. air, warmth, moisture, food,—upon its phænomena in man. On the other hand, his ideas of matter were founded on the speculations of the Pythagorean school. He taught that the four elements, variously combined, produced the four cardinal humours, and these again the different organs of the body. A vital principle, or principle of motion, φύσις, or ἐνορμῶν, was superadded, depending upon innate heat, its manifestations being excited by external things, &c. We see not how the theory has its application. Though Hippocrates did not, with many of the ancients, suppose that the vital phænomena may be explained by the properties of matter alone, but referred them to a principle of life acting under external conditions; yet his assumed properties of living matter are nowhere verified, nor the alterations asserted to be produced in such properties by alterations in the conditions of life in any way established.

Aristotle far excelled his predecessors in extending natural science by observation, and may be considered as the founder of comparative anatomy and zoology. His anatomical descriptions of the elephant and the whale have merited the eulogy of Camper. Those portions of his works in which he records his observations of the mental faculties of animals, and compares them with those

of man, are particularly valuable. These faculties he connected essentially with the organic body in which they are observed, and referred them to a principle entirely different from what was then considered elementary matter, which was the cause of all the phænomena observed in living bodies, and which controlled the qualities of matter to its own destined purposes. On observing the modes in which this principle manifests itself, he distinguished them logically as faculties: the *nutritive*, the *sensitive*, the *cogitative*, the *motive*. He then reasons on these logical distinctions as if they were real independent existences; and inquires whether they may not exist in different and in the same bodies as such. His conclusion is, that three of these faculties are faculties of one and the same real existence, wherever they are observed; but that the fourth, the cogitative faculty, or rational soul, has a real and independent existence. Thus he defined living bodies to be those which contain within themselves the cause of their own motion. But, far from supposing, as others have done, that this cause of motion can move itself, he expressly states that the fundamental causes of its motions are to be found elsewhere—in a supreme animating principle, φύσις; and asserted it to be the object of philosophy to ascertain them. These delegated powers, he contends, are four,—the material, the formal, the moving, the final causes. The unknown cause of volition and the mental faculties he distinguished as the *rational soul*; the unknown cause that produces and sustains the body, as the organic instrument of the former to effect its manifestations, he called the *sentient soul**. Thus, primary matter (ὑλῆ πρώτη, an abstraction,) is utterly devoid of properties; it receives from εἶδος all the shapes and powers which it possesses: and so are formed the various bodies observable in the universe with all their allotted qualities and energies.

If we reflect on this theory of Aristotle, and divest it of its scholastic form, we shall find that its generalizations do not very materially differ from those which have, after strict observation in modern times, been presumed to be the most just, and are now the axioms of physiological science: viz. peculiar vital properties inherent in peculiar material textures:—a cause of living motions operating, by means of these textures, according to fixed laws: and phænomena so remarkably distinguished as to lead to their division into those of animal and of organic life, and indicative of powers directed to a purpose which, in the first instance, is the preservation of the body in which they are manifested.

The Alexandrian school, founded by the Ptolemies, can scarcely be considered as having made an adequate scientific return. What

* Barclay *On Life and Organization.*

was valuable in the doctrines which they had adopted from the philosophers of Greece and Ionia, became obscure and vitiated by the additions of sophists; and experiment and anatomy, which had once been so highly cultivated by Erasistratus and Herophilus, fell nearly into disuse. I pass over, therefore, the vaunted restoration of the Hippocratic method by Serapion, the pupil of Herophilus, in the empiric school which rejected reasoning altogether, and affected to rely upon experience. I pass over, also, the methodic school of Asclepiades, which attributed, after Democritus, all natural phænomena to the fortuitous concourse of atoms, and the existence of bodies to the conjunction of these in a certain form, and their functions to the mechanical aggregation and separation of the same. Their doctrines have thrown no light on our science. Each of these schools, and others like them, had credit for a time; because, as they arose, men hoped to repose in them, wearied with balancing theories which, being founded on no extensive induction, and few just analogies, were not unfrequently at the same time false generalizations of the scanty instances upon which they were raised, and therefore necessarily contradictory.

The school founded by Galen has a just claim to the title of *eclectic*, which had been assumed by another; for its doctrines were a mixture of the philosophy of Plato, of the physics and logic of Aristotle, and of the practical knowledge of Hippocrates. He perceived the objection to Aristotle's theory, that it included under a generic term the organic functions of plants and animals, together with their manifestations of sense and intelligence*. He therefore proposed another arrangement of the phænomena of life, which deserves to be recorded, in as much as it contains the germ of all those different classifications of the functions which have prevailed in modern times. It is founded on the essential difference of the functions: first, that some are constantly necessary for the support of life, and can never be suspended; secondly, that some are accompanied by consciousness, and are subject to the will. The *vital* functions are those which cannot be interrupted without inducing death; the *animal*, those which are perceived, and for the most part voluntary; the *natural*, those which proceed irresistibly, and without the consciousness of the individual. These logical abstractions gave rise, unfortunately, to the invention of corresponding imaginary principles as their cause. Galen considered the heart, the liver, the brain to be respectively the centres of these principles,—the occult powers distributing their influences in proportion to the elementary qualities of those centres from which they emanated. He recognised, with

* Thompson's *Life of Cullen.*

Aristotle, four elements; and deduced, from the various proportions and mixtures of these, the elementary particles of the frame; and secondary qualities, or cardinal humours founded on the greater or less prevalence of one or other of the elementary principles, not greatly differing in this respect from Hippocrates. According to Galen, Nature presides over the vegetative, and the soul over the voluntive faculties*.

The theory of Galen prevailed through many successive centuries, its unestablished and mystical parts prevailing more or less over those which were founded on experience and reason, according to the degree of light and the character of the teachers during that long lapse of time so much disfigured by ignorance and barbarism.

At length, in the seventeenth century, Harvey's great discovery of the circulation of the blood gave an importance to anatomical inquiry which the successive and valuable contributions it had hitherto received had failed to bestow; whilst the discoveries of Hooke and of Boyle in pneumatic chemistry turned men's minds to study with increased ardour the minute details of every function, and to apply to the solution of the problem of life all those analogies which the advance of science in every direction so liberally afforded. Hence arose the chemical and the mathematical schools of physiology to eminence. The first includes the names of Van Helmont, Sylvius, Willis, John Mayo, Croone, Helvetius. Its insufficiency was exposed by Boerhaave, Hoffmann, and Pitcairne, and in this country practically by Sydenham.

The mathematical school of physiology gained a better reception. Its doctrines, recommended by the prevalence of the atomic theory of Descartes, gave the same direction to physiology and medicine with that in which science was principally advancing under the auspices of the Florentine Academy. The philosophy of Descartes appeared peculiarly applicable to such investigations, since no reason apparently could be assigned which should render that law inapplicable to organic bodies which referred all changes in matter generally to the figure and motion of the ultimate particles of which they were composed†. Hence we find the followers of Descartes representing in their works, the mathematical forms of the ultimate particles, of which they supposed the various organs to be composed, as figures for the application of mathematical reasoning. The most distinguished disciple of this school was Borelli. He united to all the anatomical information of the day a depth of mathematical knowledge which enabled him, in appearance, to apply its reasonings and its results

* Thompson's *Life of Cullen.* † *Ibid.*

to explain the action of the organic machine. Thus, he submitted muscular motion to calculation on the principle of the lever; explained the action of the heart and the motion of the blood upon hydraulic principles; and accounted for the secretions from the various diameters of the vessels. The proximate cause of muscular motion he asserted to be the rush of nervous fluid from the brain upon the muscular fibre. Bellini and Baglivi espoused the same theory, and extended its application by their writings; but, as if internally aware of its insufficiency, and proving that they merely reposed in it as that which was least objectionable, they laboured to separate the theory from the practice of medicine. Thus Baglivi was in practice a follower of Hippocrates and of Sydenham. John Bernouilli was a celebrated disciple of this school. He considered the elementary geometry of the Italians insufficient in its application to the animal body, in as much as this represents neither line nor plane either in itself or in the ultimate particles into which it can be resolved. Hence he had recourse to the calculus lately invented by Newton and Leibnitz and the theory of curves. His theory of muscular motion gained great celebrity, as well as his application of the analysis to determine the decrement of the body in consequence of the various transpirations and secretions. Another branch of the mathematical school was founded on the Newtonian theory of attraction, and had for its supporters in this country Keill, the Robinsons, Wintringham, and Meade.

These two schools, as may be well supposed, did not add very much directly to the science as a whole. But they prepared the way, each advancing it according to its own partial views. The intimate structure of parts and their connexions were sedulously ascertained by dissection, by the microscope, by chemical analysis, in order to ascertain the data upon which chemical or mathematical constructions were to be founded. It is not unreasonable to attribute to the hypothesis of Willis and of Vieussens, which ascribed the cause of all the sympathies so remarkable in the human body to the physical connexion of parts by means of nerves, that great perfection which the anatomy of the nervous system attained in their hands.

The followers of the chemical and mathematical schools either overlooked the necessity of having recourse to a vital cause for the operations they attempted to explain, or they had recourse to an animating principle as presiding over them. Hence arose what has been termed the *dynamic* school of physiology. In the system of Stahl the soul not only produces and forms the body, but maintains it in the performance of every voluntary and involuntary act. Those motions, even, which he allowed to exist exclu-

sive of muscular motion, which exemplify themselves by tension and relaxation of parts, and which he called *tonic* motions,—those, also, he considered as effects of the soul's power. He rejected the laws of physics or of chemistry, and the discoveries of anatomy, as throwing the least light upon the fundamental processes by which the corporeal manifestations are effected. He considered that the soul has no seat in any particular part, but that it is co-extensive with the body itself; that it perceives in the organs of sense, and operates in the muscles, independently of any connexion with the brain*. Had not Stahl failed to distinguish between the manifestations of his vital principle, according as it exemplifies itself by means of those organs which it has formed,—had he not described it as the 'rational soul',—his system, confirmed by subsequent observation as to the general principle upon which it would then have been founded,—that of vital properties inherent in the several tissues,—could scarcely have been justly censured. It was received in a modified form by many of those whom I have instanced (from the mode in which they applied it,) as disciples of other schools. In England it was defended by Bryan Robinson, and by Meade, and gained much celebrity from the writings of Hartley, who assumed its principle to explain the association of ideas. It was received also, in a modified shape, by Sauvages in France, by Bonnet in Switzerland, by Whytt in Edinburgh. The latter taught that the soul is the primary cause of all the motions observable in the body. These he divided into three kinds: *natural* motions, depending upon a gentle and equable supply of nervous influence (of which the tension of the sphincters and the general tone of parts are instances), and proceeding without the interference of the will or of stimuli; *involuntary*, excitable by stimuli affecting the nerves (and he attempts to show that in all motions produced by stimuli, whether in the muscles of the limbs or of the viscera, the soul acts of necessity); *voluntary* motions, under the immediate influence of the soul†. James Johnstone greatly modified this theory in England, but his opinions were not received by his own countrymen. He also assumed a vital principle to effect that which mechanical or chemical powers were obviously unable to perform. He placed its principal seat in the brain, thence to be propagated by the nerves, and pointed out an office of the ganglia, (which, indeed, had been hinted at by Winslow and Le Cat,) viz., that those organs which are supplied with nerves from the ganglia, performing their motions independently of the will, the ganglia are to be considered as so many subsidiary

* Thompson, *op. cit.*

† Whytt *On the Vital and Involuntary Motions, passim.*

brains, which continually supply the parts to which they distribute their nerves with new impulses and fresh activity, without immediate dependence upon the brain; and that hence it is that the vital functions are continued when the influence of the brain is suspended, as in sleep or in paralysis. These opinions of Johnstone respecting the ganglia were the foundation of that hypothesis respecting the nerves of organic life which represents them as a system distinct from the cerebral system, and which, more fully developed by Bichat and by Reil, was pretty generally received from them by physiologists, until it was shaken by the discoveries of Le Gallois and Wilson Philip.

In this way the physical and dynamic theories came to be variously combined. Their union gained its greatest perfection under Hoffmann and under Boerhaave, who insisted upon the primary influence of the nervous system in modifying and regulating all the organic functions, whether performed chemically or mechanically. Thus nervous power came to be considered as nearly equivalent to the anima of Stahl. But Stahl's system was not improved by the change; for nervous power, a manifestation of the vital energy by means of the peculiar matter of the nervous system which that energy has produced, and of which it is but a partial effect, cannot properly represent the entire cause; and it affords no explanation of the organic life of plants. For the vital principle appears to manifest its several activities by means of the organs which it has produced: and Stahl's error seems to have been that he connected its vegetative processes, which are defined and necessitated, with those of consciousness and intelligence, which are free, and are developed only with the development of the brain*.

The age of Haller, at which we have at length arrived, is the epoch from which modern physiology takes its date. The great object which that eminent person endeavoured to achieve, was to discover, experimentally, the conditions and the laws which govern those vital phænomena which the assumption neither of mechanical nor of chemical forces had been able to explain, and thus to render physiology as certain as other physical sciences. For this purpose he excluded those metaphysical subtleties by which his predecessors had so frequently veiled ignorance; excluded also mathematical and chemical science in all cases in which it was impossible to ascertain the elements upon which their application could be founded. He was willing, as he himself says, to confess himself ignorant of the manner in which the soul and body are united, and was content to proceed no further than those discoverable laws which the Creator has himself pre-

* Müller, *Physiologie.*

scribed, without inventing others unwarranted by experience. On this principle he instituted innumerable experiments to discover and illustrate the properties of the vital powers. He proved the existence of a property in muscle, to which he restricted the term *irritability*, which is only called into action by means of stimuli, which affects a much greater vivacity of motion than mere elasticity (a property of dead matter), the motions also consisting in alternate oscillations, with contraction, swelling, and wrinkling of the fibre, followed by extension, relaxation, and elongation of the same. He further attributed to the muscles a *nervous power*, distributed to them from the brain by means of the nerves, as a necessary condition of their irritability *, but which entirely differs from it. He concluded from his experiments, as detailed in his earlier works, that the following parts are destitute of irritability and nervous power: periosteum, peritoneum, pleura, ligament, tendon, articular capsules, the cornea, parenchyma of the viscera. In these tissues he admitted a force analogous to elasticity, inherent in their organic texture, which solicits them to contract slowly when divided, when exposed to cold, &c., and which only abandons them when entirely disorganized. He proved that sensibility is inherent in the nerves, but that they are destitute of irritability. He denied that irritability could be imparted to the muscles by the nerves, because, seeing that a nerve, on being stimulated, may excite motion in the muscle to which it passes, but offers not the slightest motion itself, it is impossible to suppose they should be the source of that to others which they never possessed themselves; and, more particularly, because he perceived that the excitement of muscles through nerves is a phænomenon not true of all, but only of certain, muscles.

He proved, universally, that irritability resides in all parts that have muscular fibre; that this power differs in intensity and permanence in various parts; that these qualities are most observable in the heart, more in the left ventricle than the right; that next in order come the intestines, the diaphragm, the voluntary muscles. From reiterated experiments he concluded that the heart and other involuntary muscles are not excited to contract by stimulating the nerves with which they are supplied, but that they require specific stimuli: thus, that the blood is to the heart what the will is to the voluntary muscles.

* Si insita eorum organorum (cordis, intestinorum, &c.) vis est, cur accipiunt nervos? Ii, nisi voluntatis imperia afferunt, quid agunt aliud? Primò sensum afferunt, qui absque nervis nullus est. Adferunt etiam ex cerebro efficacia imperia non voluntatis, sed legum, corpori animato scriptarum, quæ volunt, ad certos stimulos certos nasci motus.—*Elem. Phys.*, tom. iv. p. 516.

In this way Haller restricted the vital powers to two,—sensibility and irritability; the one exhibited in the brain and nerves, the other in muscular fibre. His doctrine was vehemently opposed by Whytt, De Haen, Verschuir; and strenuously defended by himself, by Bonnet, and by Fontana. It was seen that many parts in the animal body to which neither irritability nor sensibility, in Haller's sense, could be extended, were not the less alive. Thus during the numerous controversies which arose, errors on each side were detected; materials for more extended views were accumulated; experiments were infinitely multiplied and eagerly criticised; the excitability of various tissues, to which Haller had denied that quality, because he had not called it into action by an appropriate stimulus, was established on the one hand, and on the other the mistake of confounding nervous influence with sensibility was made apparent. Thus the more probable it became that irritability and innervation are separate powers, so did it follow the more necessarily that every different part should have its own excitability and its own degree of nervous power, and hence its own peculiar mode of life,—an opinion announced by Bordeu, Barthez, Blumenbach *. Independently of these expressions of vital energy in the various tissues, these physiologists admitted a fundamental power, which they termed *vitality*, or *vis vitæ*, of which the different degrees of excitability and sensibility were considered merely as modes, according to the organs in which the vital energy operated. But the analogies thus assumed between the phænomena were not established by any proof; the modifications of the original power were not accounted for; and this theory, apparently philosophic, has no firm foundation when its partisans would represent vitality, or oxygen, or galvanism, as a proximate cause of all the phænomena, residing in living matter as gravity does in dead †.

It might have been foreseen that this analytical mode of treating the living organism,—this isolation of powers which had

* They had all been anticipated by F. Glisson, who was President of the College of Physicians in 1677: but the opinions of a man who was a century in advance of the age in which he lived, and which were obscured by metaphysical subtleties and scholastic language, had no great influence upon those who were engaged with mathematical or chemical theories of life. He proved the existence of a peculiar quality of living bodies, which he first named Irritability; distinguished between perception and sensation, and adduced as instances of perception without sensation, the contraction under stimulation of the heart and muscles when separated from the body; insisted that it was only through this natural perception and sensation, and not immediately, that the animal appetite on the one hand, and the mind on the other, puts the innate irritability in action, and so produces all motions, which are either natural and vital, or sensitive.

† Thompson's *Life of Cullen.*

been intended by their concurrent acts to produce the phænomena of life, could scarcely lead to the detection of that controling cause which forced the whole to conspire to a common purpose. It became necessary, therefore, to consider the subject under a different aspect; to contemplate living bodies *in their approach* towards the possession of those powers which they exhibit when their organs are formed. The means for this have been supplied by the labours, extended through a long lapse of time, of Harvey, of Malpighi, of the Hunters, of G. F. Wolff, of Prevost and Dumas, of Meckel, of Tiedemann, of Serres, of G. de St. Hilaire, of Von Baer. The earliest examinations that can be made of plants or of animals present them as consisting of a minute globule of fluid, or a minute disc of slightly albuminous matter, *i. e.* under aspects not distinguishable in different future genera or species, as to properties or forms of their matter, by any tests which we possess. In the near neighbourhood of the disc is placed, in animals, a quantity of nutritive substance, by means of which it is destined to work. The effects, when produced, are definite for each species; but none occur except under certain conditions. These conditions are, a due degree of moisture, of air, and of warmth. When they are supplied, the disc is capable of being affected by the matter in its neighbourhood. It is *excited*, and it *reacts*. The consequence of the reaction is a gradual expansion of the disc to surround the nutrient matter; a separation of it into different superposed portions, which come into view; and a gradual appropriation of the nutrient matter. Upon the external portion of the disc, the first trace of the nervous system is observed; upon the internal portion, that of the intestinal canal; intermediate between them, that of the vascular system. Though at first simple, these objects have still a certain magnitude, and the later more complex formations are seen to arise from them as if by vegetation. "The first trace of the nervous system is not merely that of the spinal cord or of the ganglionic string, but is the potential whole of that system, of the brain and all its appendages. The first trace of the abdominal canal is not merely the rudiment of that canal, but of the whole glandular apparatus also, which may be seen gradually to spring from it*." And thus is the observed process of development altogether contradictory of the theory of Haller and of Bonnet, which represents each organ as absolutely existing in the germ, though in a miniature form. That the power which effects these changes, and thus controls the disposition of organic molecules, resides in the disc, is ascertained from the facts,

* Müller's *Physiology*, vol. i. p. 20 *et seq.*

that ova belonging to species the most different are all developed, according to their kinds, under similar external conditions, and that ova of the same species are true to their kinds under conditions which are not absolutely the same for any two individuals. If we call this power *vitality* with modern writers, or the *anima* with Stahl, these words can teach us nothing physiologically, unless we ascertain the law by which it operates: however we may see that the final cause of its operation is plainly in every case the production of those numerous bodies, definite with respect to families, genera, and species, which it develops for its own manifestations in each. Our eyes inform us that these bodies arise by means of the assimilative process, and that the original power exhibits its faculties by means of the organs which it has produced through this process. Our idea then of the vital power is this,—that it is connected with the matter of the germ in the act of its formation, and resides in it as the potential whole, or sufficient cause, of the entire future organism; that in consequence of the excitability of the organic matter of the germ, imparted to it in the same act of its formation, the expansion of the germ into portions or members occurs by the visible process of assimilation or nutrition, each portion thus acquiring its own excitability and its own reactive energy, which are but partial manifestations of the original power; and that in proportion as each part is developed, new internal conditions are introduced, in consequence of the new formation, which affect all that previously existed, by modifying the assimilative process in all. The phases of this process are strictly defined for each species, and the subsidiary means necessary for the purposed effect—as in the various forms of the respiratory organ in the fœtal state of the same individual to moderate the condition of external air—are amongst the most beautiful instances of provision for a definite end.

This formative act, this process of assimilation or nutrition, which is thus performed by animals and plants, and has a relation not only to the present, but the future also, appears to be the determination of a power acting according to Reason; and hence it must have been that Stahl referred it to the rational soul. But, seeing that reason cannot exist without consciousness,—a faculty which manifests itself only by means of the brain, a late product of this very power by the act of assimilation,—seeing also that the effect may be modified, within limits, (as in cases of monstrosity,) when the conditions are altered, we rather conclude with Harvey that it proceeds from a power acting according to fixed laws. "Vegetativæ operationes potiùs videntur

arte, electione et providentiâ institui, quam animæ rationalis mentisve actiones; idque etiam in homine perfectissimo."

A peculiar matter is necessary for the manifestation of vital phænomena: this matter is called *organic*. It is not the cause of life, but rather is its act; a production by means of the assimilative process, for the exemplification of the allotted faculties. The faculty is imperfectly manifested if the organ be imperfectly formed: the organ and its energy both vary with variations in the nutritive process*.

Hence those subordinate expressions of vital force, called *nervous power, force of secretion*, &c., cannot be considered as distinct and independent powers. They are produced, or evidenced, with their organs, by the force of assimilation, and are maintained by the same. They depend upon it for their manifestation and their due support †.

Vital power imparted to organic matter (which is itself the product of the living power of the parents), and exemplifying its faculties by means of the organs which it has developed through the force of nutrition, seems to be the last step to which observation and induction has hitherto led us. The induction is verified by observation. If the assimilative process be altered in any organ, the expression of excitability and of vital reaction peculiar to that organ is altered in the same degree.

There are then in living bodies as many species of excitability and as many modes of reaction as there are tissues. Every one of these has its own mode of both, which is called into action by its own appropriate stimuli. "Whatever these stimuli may be,—whether external, as air, light, warmth, food; or internal stimuli, the blood, nervous influence, the secreted humours,—each organ reacts in its own peculiar manner; a manner which supposes a peculiar organic power imparted to it in the act of its formation by the process of nutrition, and sustained by the same‡." "The stimulus may be that of a chemical, or mechanical, or organic substance; the reaction, however, is always vital, and indicates the existence of an organic force, of which it is the effect. The physical properties of the one are in some sort in a constant conflict with the vital properties of the other, and living bodies only preserve their character of life so long as they are able to resist the physical impression. When it is said that organic movements are occasioned by incitations, we do not admit

* Tiedemann, *Physiologie*. † Tiedemann.

‡ Tiedemann, *Physiologie*, vol. ii. In this excellent work, worthy of the great name of its author, the theory, of which I have given this hasty notice, is fully developed.

that they are the immediate effects of the mechanical or chemical impressions, but assert that they are the effects of powers which the external impression, be it mechanical or be it chemical, has thus solicited to act."

Of these excitants some are necessary conditions of life, and are therefore called vital stimuli. Plants cannot live any considerable time without air, water, warmth, and light; nor animals without the first three, and they become rickety when deprived of the last. These being indispensable for the due nutrition of parts, are necessary for the sustentation of those powers which are developed with the parts by the act of nutrition. But all animals are not dependent upon each of these excitants in an equal degree. Thus, the new-born of warm-blooded animals resist more easily the deprivation of air than of warmth. They are drowned more readily in cold water than in warm, within certain limits of warmth. They live longest under water between 20—30° R., and if the heat be above or below these limits die sooner. In general, the lower the place of the animal in the zoological scale, the longer can it bear to be deprived of these stimuli. Amphibia live from ten to thirty hours, in distilled water, under the air-pump; and frogs, whose lungs have been extirpated, may survive thirty hours.

With respect to the stimulus of food the same general rule prevails; the intervals of supply may be greater without destroying life in animals, according as their organization is less complicated, and their powers more limited. Thus, tortoises and serpents may be deprived of food for months, and many mollusca for yet longer periods.

Some also of the internal conditions of life may, in the lower animals and in the imperfect states of the higher, be suppressed or greatly altered, and yet life be supported for a longer or a shorter period. The experiments of Legallois and others lead to the conclusion, that this period varies inversely as the perfection of the organ whose action is suppressed. The Batrachia are found to live for many hours without the heart; a tortoise, whose brain was removed by Redi, lived after the operation for several months; in new-born rabbits, if the heart be extirpated, sensibility persists for about fourteen minutes; when they are fifteen days old, for only two and a half minutes; thirty days old, one minute*; and the young of man may, at the time of birth, be revived when the heart's action has ceased for a period after which, in the more adult state, it could not be restored.

In the more perfect forms of life there is a necessary dependence of the whole organism upon each of its parts, and of the

* *Essai*, Legallois, p. 142.

parts upon the whole. Thus, for instance, respiration is necessary to the heart's action, the heart's action to the respiratory process; neither can proceed after destruction of the nervous system, and this requires for the production of its energy a due supply of aerated blood. But this mutual relation of all and each, alternately as cause and effect, has been improperly assumed as a distinctive character of life. "The same is true of an automaton, in which the moving power is part of the thing moved: the same is also true of the planetary system as far as we are acquainted with it*."

Thus does the vital power, manifesting itself in the assimilative process, occasion all the forms of life upon the earth. Each living thing, according to the nature of that original power (of which we can know nothing but by its effects), requires its own modifications of the common conditions of life, and presents an organization (upon which classification is based,) adapted to the region and the element in which it is destined to exist. Of these creatures, all that are not microscopic are observed to proceed from parents similar in structure to themselves by modes of propagation peculiar to the kind; so that no one species, under any modification of external condition, has ever been known to assume the character or form which is distinctive of another. The consideration of the stratification of the earth assures us that all the families, genera, and species did not commence their existence at one and the same epoch. On the contrary, in the older strata are buried the remains of the simpler forms of life alone; in the more recent those of more complex organization; whilst the remains of the most perfect and of man have not been discovered in the most recent stratum. Of the remains which have thus been brought to light, some belong to species and genera which still exist, others to such as are lost. Some physiologists, taking their stand upon the general fact of this successive advance towards perfection of development in correspondence with the successive changes of the globe, have concluded that all the various modifications of life may be but successive metamorphoses of the first most simple form.

The undoubted fact that existing species have been perpetuated unchanged for several thousands of years, would have rendered such an opinion in the highest degree improbable, but for the observations relative to the apparently spontaneous production of animals and of plants from organic matter in solution —the apparent changes of species from simpler to more complex under favourable external circumstances—and the interchange of

* Treviranus, *Erscheinungen und Gesetze des organischen Lebens.*

animal and vegetable form. If the facts were really thus, then might the objection to the hypothesis of metamorphosis founded on the permanence of existing forms be encountered. It might be averred that, notwithstanding our ignorance of the means, the necessary conditions for such successive changes may have been supplied in the earlier periods of the world, at epochs so far removed, that the few thousands of years which have passed away since the appearance of man upon the globe bear no proportion to their immense distance, and only show that the rate according to which the conditions of change are produced is a very slow one.

Let us see to what conclusion the latest observations on Infusoria are tending.

It is well known that the experiments of Redi and of Vallisnieri were considered to have refuted the notions of the ancients concerning spontaneous generation, until those of Tuberville Needham, of O. F. Müller and of Wrisberg, performed with the most considerate exclusion (if that be possible) of circumstances likely to throw a doubt upon the result, revived them. Müller, repeating the experiments of Needham, concludes, that animal and vegetable matter, by solution in water, is reduced to minute membranous shreds, upon which, in a short time, are seen microscopic globular points. These enter into a tremulous motion, which gradually becomes more apparent; the globules are detached, and Infusoria are produced from them. These first Infusoria, he says, abound in all fluids, and are not to be confounded, as is usually done, with other Infusoria, being, on the contrary, elements which are the component molecules of all animals and plants.

The conclusions Spallanzani drew from his experiments were opposed to those of the above-named naturalists. He found the structure of the infusory animals to vary with the nature of the infusion, and explained their appearance upon the supposition that ova had been introduced with the animal matter, or had been suspended in the air, whose admission, at least in some degree, is necessary for the success of the experiment.

The experiments of Priestley, of Ingenhouz, of Treviranus, appeared to prove that the green matter of Priestley, produced in organic infusions on exposure to light, is first a mass of animalcules; then is resolved into green globules, which concrete into confervæ; then, after the solution of these, again becomes infusory animals and vegetables of a larger form. The organic particles appeared indestructible, and common to each form of life, passing from one to the other, and supplying the substance

from which each is formed, under the necessary external conditions*.

The recent experiments and observations of Ehrenberg have, however, tended to increase our doubts concerning the validity of these conclusions. He has not succeeded, as Spallanzani conceived he did, in producing definite forms of animalcules from definite infusions. On the contrary, he has found the forms to vary under circumstances the most similar. He has explained, however, how it is that Spallanzani might be mistaken in his conclusion. The species pass through many gradations of form in their progress to maturity, each of which forms may have been readily mistaken for a distinct species. These are not so very numerous; but the rate at which the individuals are multiplied is altogether extraordinary. For instance, the *Hydatina senta*, which was observed for eighteen days, is capable of a fourfold increase in twenty-four hours, which may give more than a million of individuals from a single ancestor in ten days, and on the most moderate computation may give nearly seventeen millions in twenty-four days. According to Ehrenberg, infusories exist in all waters, (except rain and dew, in which he could not discover them,) and in some parts of plants, though here, probably, only in a diseased state of the plant. Further, he has succeeded in detecting a complex organization in those animalcules lately considered of so simple a form. Even in the Monas, a creature not more than the twenty-thousandth part of an inch in diameter, the stomach is found to be of a compound structure, and its motions are effected by cilia. In others he observed ova, and propagation by means of them. If then, in the infusions of Treviranus and Needham, no animalcules were produced when the vessel was hermetically sealed, and the necessary quantity of air exposed to a high heat—if they *were* produced when fresh air was introduced after boiling—if the animalcules have been shown to be capable of producing ova, which, indeed, was never denied; it seems more reasonable to suppose, that those observers who did not succeed in discovering the complex structure of the creature so extremely minute, might fail also in discovering the first ova, though they really existed in the infusion, than that the animal should arise spontaneously. Ehrenberg has not succeeded in detecting these first ova. No very violent improbability is included in the supposition that bodies so infinitely small have been conveyed by the air, like the

* *Vide* a full critique of the experiments of previous authors, and an account of his own, in Treviranus, *Biologie*, ii. 267, 403.

motes visible in the sunbeam. We now know how numerous they must be*.

In the case of parasitical worms, (the *Distoma hepaticum*, for instance,) the ova are too large to be either conveyed by the air, or to be absorbed by vessels from the food and carried to their nidus in the viscera. Such worms have even been found in the viscera of embryos. If we must have recourse to hypothesis to account for the origin of these, let our hypothesis be supported by analogy. It is not impossible that a portion of an ovum may be able, as has been supposed by many, to germinate and produce a new individual, as a portion of a Polypus becomes a distinct and perfect animal of its kind†.

The opinion of the gradual production of all creatures from an original simple form has received confirmation, in the minds of many, from their having observed that the embryo of the highest forms of life passes by gradations through those which are permanent in inferior animals. They have, however, supposed this resemblance to be more complete than observation allows us to believe it to be. We have seen that the first observed embryo of all animals is extremely simple. With respect to this simplicity, which but implies the imperfection of our tests, a comparison may be allowed between embryos of a higher order and the simplest forms of life, when the animal presents no separation of distinct organs. As the development of the

* "Although Dr. Ehrenberg, in refuting the notion of the extreme simplicity of these animals, has overthrown one great argument in favour of their spontaneous origin, yet he has offered no explanation of their production in infusions which have been subjected to a heat sufficient to destroy any parent animals, or even ova, supposed to be present. In these cases, as is well known, the adversaries of the theory ascribe the origin of Infusoria to ova conveyed by the air; an assumption which the supporters of the doctrine regard as highly improbable, and which, if admitted as true, they consider inadequate to explain the production of Infusoria in all the conditions under which it is reported to have taken place by observers worthy of credit. It is true that Dr. Ehrenberg never witnessed the spontaneous origin of Infusoria; but before denying the possibility of its occurrence, and discarding the theory of spontaneous generation as unnecessary to account for the facts, it was incumbent on him to have subjected anew to a rigid examination the observations of those who have arrived at an opposite conclusion from himself, and either expose the fallacy of their experiments, or show how they were to be explained on a different view from that adopted by their authors. It is the more to be regretted that he has not favoured us with such a critical examination, as, from his extensive knowledge of the different species of the animals in question, his intimate acquaintance with their mode of life, and his superior methods of observation, he is singularly well fitted for the task."—*Dr. Sharpey, Account of Professor Ehrenberg's Researches on the Infusoria: Edinb. New Phil. Journal, Oct.* 1833.

† Entozoa have been found in embryos and in the eggs of birds: so also have pins and small pieces of flint.—Tiedemann's *Anat. und Nat. Gesch. der Vögel*, b. ii. s. 128. quoted by Treviranus.

embryo advances, we observe some organs superadded, though still in a very simple form; so that here also a certain resemblance subsists between the embryo in this second stage and animals a little more complex. As we continue to observe the embryo of the higher family, we see organs come into view, some of which are meant only for a transitory purpose and disappear; some which have no purpose during fœtal life, but are meant for an ulterior use. Here the resemblance between the embryo of the higher form, and the animal of the lower form with which we may most favourably compare it, is found to be less close. We find that the animal has organs suited to the activities with which it is endowed, which are not to be found in the embryo. Even if the two exist under similar external conditions of life, the organs adapted to these conditions are not the same in both. To instance these several statements: when no organs can be observed in the primitive streak of the embryo, it resembles the zoophyte, in which nutrition is performed by imbibition; but we observe in addition that the primitive streak extends into a membrane which becomes the vascular area. If we attempt the comparison when the body resembles a worm, in as much as it is cylindrical and has no limbs for motion, the resemblance scarcely extends further. The worm has rings and contractile bands for its motions, whilst the embryo has neither; and the simple tube, which represents the heart in both, gives indications of a higher organization in the embryo. If the worm resides in an aqueous medium like the embryo, it respires by means of gills, the embryo by a production of its abdominal tube—the umbilical vesicle (?), or the allantoïs, or the placenta. At another period remarkable apertures are observed, at regular distances, towards the head, between the imperfectly closed abdominal laminæ in the higher embryos, in which they resemble some of the cartilaginous fishes. But with the former the vessels that follow the arches do not divide for any respiratory purpose, whilst in the latter they are the respiratory vessels of the gills. If, in a still further stage of advancement, we compare the higher embryo with the turtle, we find that in both the double heart is rendered virtually single, but for very different purposes, and here the similarity is at an end*.

In these analogies, therefore, we look in vain for that precision which can alone support the inference that has been deduced. Far rather do we infer gradations of original power, which manifest their different energies at different epochs, under external conditions which may be similar according to a general plan, the expression of each that is superadded modifying that of all which preceded, and concurring with theirs to develop

* Weber in Hildebrandt's *Anatomy*, vol. i. p. 125.

others which may be still latent. In the lower creature, a particular organ or set of organs attain their purpose and are complete; in the imperfect state of the higher, the corresponding organs may in general resemble them, and may even perform a similar office, yet still they are seen to be more than sufficient for this lower purpose: in the midst even of this general similarity, the indication of a higher destiny, yet unattained, is apparent.

We are disposed to conclude, then, generally, that all the families, genera, and species of animated things were originally created in such forms as we observe them in at present; and that they continue to produce the organs which are the instruments and the expression of their several powers by the process of assimilation as a proximate cause. Amongst these different organs the brain is peculiarly distinguished. We are sensible in ourselves of ideas, of emotions, of desires—of powers which present themselves to us as pure energies, without any intermedium: we have self-consciousness. These activities are excited by our own will; we cannot contemplate them as observable processes in any other person. On the contrary, the energies of all the other organs are totally independent of our will; we are aware of them only as their effects are matters of observation by means of our outward senses, and we observe them better in other individuals than in ourselves. Life, thus presenting such remarkable differences in these two respects, has been distinguished as two forms. And this distinction is not merely logical, for in the vegetable kingdom we have an instance in which the one form of life exists totally separate from the other. But we find that even the higher form, the intellectual or purely animal life, requires for its manifestation a body*. In living creatures the two factors, though logically separable, exist as one reality. The two spheres approach and intermingle in various degrees in the different families of the earth, the animal powers depending upon the vegetative for the formation of their material organ. The life of the lowest animal scarcely appears to differ from that of the vegetable. "From these animals, which obtain food without any act of volition, we come to those which can only obtain it by such an act, but who still, without any act of this kind, obtain the influence of air, yet more immediately necessary to their existence. We arrive at length at the most perfect class, which can neither obtain food nor air except by an act of the sensorium. In them the sensorial power is as necessary for the inhalation of air, as the ingestion of food†."

* Burdach, vol. iv. p. 3. Burdach notices the impropriety of calling, with Bichât, the animal life 'vie externe,' and the organic 'interne.'

† Wilson Philip, *Phil. Trans.* 1834.

And there enters, on the other hand, even into those organic motions which we call *voluntary*, much that is neither willed nor is a matter of consciousness*.

The following, therefore, I would signalize as the great achievements of modern Physiology: viz.

The establishment of the general proposition, that peculiar vital powers are connected with, or inherent in, peculiar animal tissues;—dating from Haller:

The establishment of the theory of development;—dating from G. F. Wolff:

The further generalization which derives all the vital powers from modifications of the force of Assimilation;—more fully propounded by Tiedemann.

Having thus presented a rapid outline of theoretical physiology, in which I have purposely suppressed many details which may be introduced more conveniently in other parts of a review of the present state of physiology, I shall now proceed in that direction in which the science must for a long time attempt a progressive perfection, by endeavouring to ascertain, as far as is possible, the inferior rules by which the proximate cause operates. These include all the processes of vegetative life; and since they are all effected through a constant interchange between external matter and the matter of the various organs, I shall begin by pointing out the acquisitions added in late years to our knowledge concerning the vehicle of the former—the blood.

The Blood.—This fluid would be ill suited for its office, were not its constituent molecules held together, in the living state, by affinities so delicately balanced that they yield to every reactive energy that the different organs to which it is presented, can offer. Hence we account for the great discrepancies in the results of chemical inquiry concerning it, from the ease with which its components may be caused to combine in various proportions, and from the different effects which different quantities of the same reagent are capable of producing.

In the body it exists as a colourless transparent fluid, in which an infinite number of minute red bodies are equably diffused.

Out of the body it shortly coagulates, or separates into serum and coagulum.

It was the opinion of Home and Bauer, that the coagulum is formed by an aggregation of the corpuscles in the following way.

* Burdach, vol. iv. p. 3, &c.

The corpuscles consist of a nucleus inclosed in a membrane of coloured matter, which membrane bursts, and the nuclei thus allowed to escape attract each other and form the solid coagulum, which is coloured by the broken membranes, and from which the colouring matter may be washed out by water. This opinion, which appeared to be confirmed by the observations of Prevost and Dumas, of Dutrochet, and of H. M. Edwards, has been very generally received. But it is certainly unfounded. For the fibrin may be separated from the blood by stirring, whilst the corpuscles remain in the serum unbroken and unchanged: the serum, far from effecting their solution, supplies the best medium in which they may be preserved for observation: if in human blood the coagulation be retarded by adding a few drops of solution of subcarb. potass., the corpuscles descend, from their superior weight, before coagulation takes place: in the course of half an hour a tender coagulum is formed, of which the lower part (as far as the corpuscles reach) is red, the upper pale and thready: operating on the blood of the frog, Müller has succeeded in separating, by the filter*, the large corpuscles of that animal from the clear liquor, which last afterwards separates into fibrin and serum: fibrin is not soluble in water, the corpuscles are so in part; and, in general, the two present somewhat different chemical reagencies.

The Corpuscles.—These bodies, called collectively cruor, have been objects of much interest ever since they were first observed by Malpighi. All that relates to them is even yet matter of controversy,—their form, their size, their composition, the cause of their colour. They have been too frequently observed in water, rather than in serum, by which the two first qualities are speedily altered.

Form.—They exist in all vertebral animals as round or oval bodies, with well-defined edges. They are semi-transparent and pale when seen singly; but present the colour of the blood when seen by reflected light or in masses.

In all the Mammalia they are circular.

In the other Vertebrata they are oblong. De Blainville has observed both these forms in Fishes, and Müller in the Frog, who thinks that the round corpuscles, one sixth of the size of the others, belong to the lymph or the chyle.

In all the Vertebrata they are flat. Rudolphi states, that they are flattest in the Amphibia, less so in Birds, least of all in Man.

Hodgkin and Lister find the proportion of axes in Man to be 1: 4·5.

* Müller, p. 106. The filter was composed of delicate animal membrane, moistened bladder, and covered a glass tube from which the air was exhausted.

To the last-mentioned observers, as well as to Young, the corpuscles have not appeared uniformly flattened, but concave on their surfaces. Müller, however, who believes in the existence of a central nucleus, of which he finds the thickness to equal the lesser diameter of the corpuscle, has, if his observations admit not of another explanation, set that point at rest.

Size.—As a rule, the size is constant in the same individual, and the same species; and their measurement assumes an additional importance from the observation of Blundell, and of Prevost and Dumas, that death follows the transfusion of blood when its corpuscles differ in size from those of the animal which is operated upon.

In Man the size of the corpuscle, according to

Rudolphi, Sprengel, Hodgkin, and Lister	= 0·00033 in.	$\frac{1}{3000}$
Kater, Prevost, and Dumas	= 0·00025	$\frac{1}{4000}$
Wollaston, Weber	= 0·00028	$\frac{1}{3000}$
Young	= 0·00016	$\frac{1}{6000}$

If we take the mean of these, we find the size of the human corpuscle to be $\frac{1}{4000}$dth part of an inch in diameter.

In different Mammalia, (Prevost and Dumas,) the size of the corpuscle is the same as in Man, in the Dog, Hedgehog, Swine, Rabbit, Dolphin	= ·00025 inch.
Larger in *Simia Callithrix*	= ·00030
Smaller in Ass	= ·00022
Cat	= ·00020
Sheep	= ·00018
Chamois	= ·00017
Goat	= ·00014

Hodgkin and Lister have, however, found it smaller in the Swine and Rabbit than in Man.

Diameter in Man : major axis in Frog : : 1 : 4.

The corpuscles are of equal magnitude in arterial and venous blood and similar in form.

Structure.—Hewson, from observing the mode in which the fresh corpuscle appears to swell in water and to change, becoming gradually colourless, and appearing as a central nucleus surrounded by an integument, concluded that it really exists in the blood in this complex form. In this opinion he has been followed by Home, Edwards, Dutrochet, Prevost, and Dumas. The integument has been represented as the colouring matter, and the nucleus as fibrin. Raspail, however, gives it as the result of his observation, that when the

homogeneous corpuscle comes in contact with water or an acid, then first a change is effected; the density increasing towards the centre, and the colour, at first diffused through the whole mass, being then confined to the surface. But however this may be, whether the nucleus have preexisted or be now first formed, it is not soluble either in water or in dilute acetic acid, whilst the external portion with the colour is gradually removed by this. This has been pointed out by Müller with great precision in his late work: the subject of his examination, principally, was the blood of the Frog.

Berzelius refers the insolubility of the corpuscles in serum to the albumen which it contains: but this is not the only cause. J. Müller rather considers it to be an effect of the salts which the serum holds in solution; for he found, that on adding to a drop of Frog's blood under the microscope a drop of a solution of yolk of egg in water, the corpuscles lose their form and become round as quickly as in water, whilst a drop of a solution of such a salt as does not separate the blood (as subcarb. potass.) causes no such change to be effected.

It has been stated by some that iron does not exist in greater quantity in the cruor of the blood than in its other essential components. Engelhardt has discovered a remarkable property of chlorine, confirmed by H. Rose, by which the incorrectness of that opinion has been proved, and the conclusion of Berzelius established, viz., that all the iron of the blood belongs to the cruor. The chlorine precipitates the animal matter from its solution in water, and at the same time deprives it of the lime, soda, phosphorus, iron, which may have been connected with it. The liquor being strained, the oxide of iron may be precipitated by ammonia: but that precaution is necessary, for otherwise, the ammonia redissolves the organic matter, and the iron recombines with it. Engelhardt could obtain no iron from similar operations with serum and fibrin, though he did all the other salts; and there was no ash left on combustion.

Berzelius's estimate of the quantity of iron in the ash of the cruor was confirmed by these experiments. Of the entire blood metallic iron forms only one part in 1000.

It is yet undetermined whether the iron exists in the corpuscles of the blood in its reguline form or as an oxide, Engelhardt and Berzelius supporting the former opinion, and H. Rose and Gmelin the latter. For the former opinion it has been contended, that chlorine has a strong affinity for the metals, but none for their oxides; and that the oxide of iron, if present, would be dissolved by the mineral acids. But in Engelhardt's

experiment, the affinity excited is between chlorine and animal matter, not iron and chlorine. And if the mineral acids have not the same effect, this does not prove the iron to be in a metallic state; for if it were so, and unprotected by the animal matter, it would be oxidized, and then dissolved by the acid.

The labours of chemists to explain the mode in which the elementary substances are united to produce the colour of the cruor, however they may be unsatisfactory in this respect, have thrown much light upon the reactions of these substances. Such experiments, on the other hand, have introduced as constituents of the blood, products which are perhaps merely the effects of the chemical operations; or new combinations, not existing in nature, of its elements. The iron seems to enter in too small a quantity to form a metallic pigment for the cruor. Whatever changes the constitution of the blood, as a living product, also changes its colour. "Since its chemical composition is only a product of life, so are we unable by any aids derived from inorganic nature to produce it. The colour has its cause in the constitution of the blood as an organic whole; and each of its elements, iron amongst the rest, contributing to that constitution, enters into the production of its colour*."

The Lymph, or liquor sanguinis.—The clear fluid in which the cruor, or mass of corpuscles, is diffused. It separates spontaneously into two portions, fibrin and serum.

Fibrin.—I refer to Berzelius for all that is yet known concerning this substance. Since Müller's discovery, it is distinguished from the corpuscles; and De Blainville and Hodgkin have shown that its fibres do not consist of strings of minute globules.

Serum.—Lecanu has repeated the analysis of serum, and asserts, that certain oily substances exist as components, which were unnoticed by Berzelius and Marcet. His mode was, after desiccating a known quantity by moderate heat, and thus determining the quantity of water, to treat successively, with boiling water and boiling alcohol at 40°, the residue of desiccation. The water dissolved the soluble salts and extractive matters, the alcohol the fat matters. The watery solution was evaporated; the residue treated with alcohol to separate the extractive matters soluble in it. What was insoluble was calcined, to determine the proportion of organic matter it still contained; the residue again treated with boiling alcohol to separate the hydrochlorates.

The fatty matters taken up by the boiling alcohol were separated from each other by means of alcohol at 33°, which does not dissolve when cold the crystallizable fat, but does the oily.

* Burdach, iv. 85.

The albumen procured by means of boiling water and cold alcohol was dried, weighed, and calcined, and its salts determined. Traces of iron were found in such minute quantity in serum, that Lecanu presumes it would not furnish any if it were possible to procure it entirely separate from the colouring matter.

1000 parts of serum consist, according to this mode of analysis, of

	First Analysis.	Second Analysis.
Water	906·00	901·00
Albumen	78·00	81·20
Organic matters soluble in alcohol and water	1·69	2·05
Albumen combined with soda	2·10	2·55
Crystallizable fatty matter	1·20	2·10
Oily matter	1·00	1·30
Chlorure of sodium ... ——— potassium	6·00	5·32
Alkaline subcarbonate ——— phosphate.... ——— sulphate.....	2·10	2·00
Subcarbonate of lime ——— magnesia.. Phosphate of lime ——— magnesia ..... ——— iron	0·91	0·87
Loss	1·00	1·61
	1000·00	1000·00

These fatty matters will be better understood by considering Lecanu's analysis of the entire blood. He poured alcohol in excess on venous blood, separated the precipitate, and treated it frequently with boiling alcohol, obtaining thus a mass insoluble in alcohol, and a slightly rose-coloured liquor. This liquor, subjected to evaporation, became turbid towards the end of the operation, in consequence of the separation of a fat matter insoluble in the aqueous product. The residue of evaporation was treated with æther: a portion of it was dissolved. Hence an æthereal solution A, and a residue B.

A, on spontaneous evaporation, gave a brownish residue, bitter, of a consistence similar to that of turpentine, formed of two distinct matters, one solid, the other liquid and like oil. The residue was incompletely soluble in cold alcohol, the solid portion remaining attached to the sides of the vessel. When this solid portion had been separated, and dissolved in boiling alcohol, it formed, on cooling, white nacreous laminæ, similar to the fatty matter of brain.

On evaporating the alcohol with which this had been washed, and in which the oily portion of the residue had been dissolved, another residue, of a bitter taste and turpentine consistence, was obtained; insoluble in hot or cold water, soluble in alcohol or

æther, inalterable by hydrochloric and nitric acids, rendered brown by sulphuric acid, soluble in potass with slight heat, and precipitated from the solution by hydrochloric acid in white flakes. If the excess of acid be washed off with water until the latter no longer reddens the vegetable blues, it communicates to alcohol, when essayed therewith, the property of reddening those colours. Hence the oily part of the blood appears to be convertible by potass into an acid substance, and is considered by Lecanu as an immediate principle of the same.

From B treated with alcohol was obtained a brownish yellow liquid and a residue C.

The liquid, on being evaporated, furnished an orange yellow mass, insoluble in æther, but soluble in water and alcohol, which then manifested alkaline properties. The watery solution afforded a precipitate with hydrochloric and nitric acids, and with solution of galls;—the same as that considered by Berzelius to be a mixture of animal matter and lactic acid; by other chemists as resembling osmazome. It is considered by Lecanu to differ from osmazome in as much as the latter is not precipitable from its solution by acids.

C was found insoluble in æther and alcohol at 40°: treated frequently with boiling alcohol at 33°, it gave the hydrochlorates, and some extractive matter easily separable by alcohol at 40°. This new residue, treated with cold distilled water, was nearly entirely dissolved, except a small quantity of a brown matter insoluble in boiling water and alcohol, and considered as a mixture of colouring matter, albumen, and fibrin.

From a portion of the saline solution, white flakes were abundantly precipitated by acetic acid,—a gelatinous albumen, which Lecanu considers, from the mode in which he obtained it, to exist in this form in the blood. Another portion of the saline solution was evaporated, the residue calcined, and the salts determined*.

1000 parts of blood, according to the above, consist as follows:—

	First Analysis.	Second Analysis.
Water	780·145	785·590
Fibrin	2·100	3·565
Albumen	60·090	69·415
Colouring matter	133·000	119·626
Crystallizable fatty matter	2·430	4·300
Oily matter	1·310	2·270
Extractive matters soluble in alcohol and water	1·790	1·920
Albumen combined with soda	1·265	2·010

* *Annales de Chimie et de Physique*, tom. xlviii. p. 310.

	First Analysis.	Second Analysis.
Chlorure of sodium..... ——— potassium..... Subcarbonate, Phosphate, ... alkaline.. Sulphate, ...	8·370	7·304
Subcarbonate of lime..... ——— magnesia Phosphate of lime ..... ——— magnesia... ——— iron ..... Peroxide of iron.....	2·100	1·414
Loss	2·400	2·586
	1000·000	1000·000

"Doubtless under various modes of applying heat, alcohol, the subcarbonates, &c., to the blood, many substances may be made to appear, which are but variations of its essential components—(albumen, fibrin, and cruor). The knowledge of these may enrich animal chemistry, if the object were only to comprehend, comparatively and in the gross, the series of changes which any matter may undergo under different agencies, and not to discover new materials and declare them to be real components of the organic body. Tiedemann reckons the peculiar matter of saliva amongst the components of the blood; and lately urea has been counted amongst the number, because it has been found in the blood after extirpation of the kidneys. Incontestibly, in certain cases of suppressed secretion or of increased resorption, bile, the seminal fluid, &c., have been found there; but no special product of secretion can yet, on sufficient grounds, be proved to be a normal component of the blood, and from what we know of it, such a result is not to be expected*." Numerous colouring matters have been classed amongst the components of the blood, from the explanations of chemists respecting the causes of colour—as globulin, erythragin, &c. Burdach thinks that they are either the product of reagents acknowledged not to exist in the blood, or are modifications of albumen. Boudet, by treating very large quantities of blood, contends that all blood contains cholesterin†.

Gas.—Under the air-pump, air has been observed to escape from recent blood; its quantity has been variously computed. Most observers, as Sir Humphry Davy, Brande, Scudamore, Vogel, state that it is carbonic acid. The quantity is very variously estimated. Brande obtained two cubic inches from eight ounces of

* Burdach, iv. 68.

† *Annales de Chimie et de Physique*, tom. lii. p. 342.

blood; Scudamore half a cubic inch from six ounces; Sir Humphry Davy rather more than one cubic inch from twelve cubic inches of blood. Dr. Clanny finds that the gas developed is principally nitrogen. Dr. J. Davy asserts, from his experiments, that in no case is carbonic acid ever developed from fresh-drawn blood; that, on the contrary, it absorbs one fourth of its bulk of carbonic acid, which becomes combined. Müller has repeated the analysis, both with fresh sheep's blood and with that of man. Even under a heat which amounted to 200° F., one pound of the former gave off only 1·8 cubic inch of gas: of this quantity not one fifth cubic inch was absorbed by lime water; and this small quantity of carbonic acid he attributes to the action of the air contained in the tube of his apparatus upon the blood. He repeated the experiment in such a way as to exclude the air, and obtained no trace of carbonic acid, nor any, except the merest bubble, of any other gas. He further found, that blood artificially impregnated with carbonic acid did not yield it again under the air-pump*, and thus has confirmed the same observation of Dr. J. Davy. Mitscherlich, Gmelin, and Tiedemann have lately performed another experiment with the same result. They introduced small metallic tubes, provided with stop-cocks, into the artery and vein of a Dog. After all air was evacuated from the tubes, by allowing the blood to flow, they were brought under glass cylinders inverted over mercury. The cylinders, half filled with blood, were placed under the air-pump. On exhausting, bubbles arose, so that the quicksilver, which had stood half an inch above that in the cup, sunk about an inch. But when the air was gradually readmitted to the bell of the pump, they disappeared; showing that they did not consist of gas, but of watery vapour which had filled a vacuum. Both kinds of blood comported themselves similarly. The authors found that the blood contains carb. acid combined; for blood mixed with vinegar gave bubbles under the air-pump, which, when venous blood was employed, did not entirely disappear on readmission of the air: hence the alkaline nature of the blood depends not upon caustic alkalies, but upon their carbonates†.

The proportion of solid to fluid matter in the blood has been determined by Prevost and Dumas, in a great number of animals. They find that in Man the solid are to the fluid : : 1 : 9.

In carnivorous animals there are more cruor and fibrin together than in graminivorous: in young animals less than in old of the same species‡.

* Müller, *Physiologie*, i. 313.

† *Archiv. fur Anat. und Phys.*, Müller, 1834, 103. ‡ J. Davy.

In cold-blooded animals it is the quantity of cruor, not of fibrin, which is diminished*.

Michaelis gives the following analysis of the components of arterial and venous blood into the elementary gases†.

ALBUMEN.

	Arterial.	Venous.
Nitrogen	15·562	15·505
Carbonic acid	53·009	52·652
Hydrogen	6·993	7·359
Oxygen	24·436	24·484

FIBRIN.

	Arterial.	Venous.
Nitrogen	17·587	17·267
Carbonic acid	51·374	50·440
Hydrogen	7·254	8·288
Oxygen	23·785	24·065

COLOURING MATTER.

	Arterial.	Venous.
Nitrogen	17·253	17·392
Carbonic acid	51·382	53·231
Hydrogen	8·354	7·711
Oxygen	23·011	21·666

Arterial blood contains more nitrogen and oxygen, but less carbonic acid and hydrogen, than venous blood.

	Nit.	C. Ac.	Hyd.	Oxyg.
Arterial blood	16·800	51·920	7·534	23·746
Venous blood	16·720	52·107	7·765	23·408

Lecanu has made some interesting comparative analyses of the blood of individuals of different ages, sexes, and temperaments. To determine the proportion of the essential principles, he dried a portion of the serum after having weighed it, and thus determined its water, its extractive matter, and salts. He then divided the clot into two portions, dried one of them; to determine, by the loss, the quantity of water of the serum involved in it: and washed the other, to obtain the quantity of fibrin. By the first part of the process he ascertained that part of the weight due to the extractive matter and salts left by the water of the serum evaporated: subducting this he had the weight of fibrin and colouring matter. By the second part of it he ascertained the weight of the fibrin alone. The difference was the weight of the colouring matter. In this way, examining the blood of twenty healthy persons, ten males and ten females, he found the water to vary in 1000 parts of blood—

* Müller. † Poggendorf, *Annalen*, 1832.

In the females from 853·135 to 790·394:
Mean of the 10 cases 804·371.

In the males from 805·263 to 778·625:
Mean of the 10 cases 789·320.

In different temperaments:

Females, lymphatic,	mean of 5 cases	803·710.
———, sanguineous,	——— 4 ———	792·984.
Males, lymphatic,	——— 2 ———	800·566.
———, sanguineous,	——— 5 ———	786·583.

Female, mean quantity of clot from 10 cases	115·963.
Male, ———————————————	132·490.

Hence there is more water in the blood of females than of males. The proportion of water was not found to depend upon age, at least between the limits of twenty and sixty years. In individuals of the same age, it is less in the sanguineous than in the lymphatic. The proportion of albumen is found to be sensibly the same in the male and female, and not to be proportional to the age between the same limits; and nearly the same in quantity in sanguineous and lymphatic individuals of different sexes. The proportion of colouring matter is found to vary in the blood of individuals of different sex and age, in individuals of same sex and different age: and to be greater in the male than in the female; and greater in sanguineous than in lymphatic persons of same sex.

Denis found from comparison of his analyses, that in the blood of individuals ill nourished and accustomed to stimulant drinks, the proportion of colouring matter increases, and is even more abundant than in the blood of sanguineous subjects; but that the albumen, on the contrary, is in very small quantity*.

The principal change which J. Müller found in the blood of cholera patients, he states to be its propensity to coagulate even during life. He therefore recommends the use of the subcarbonates of soda and potass, particularly the last, in large doses, on the immediate accession of the disorder. Dr. O'Shaughnessy, Dr. Clanny, and Mr. Bell found the salts and serum also greatly defective in quantity in this disease.

There is no longer any question amongst physiologists as to the life of the blood. That which enters into the composition of all parts of the living body, from which they are produced, and sustained, and restored, and upon which the body itself reacts, must possess life. Many have even supposed that its red particles possess spontaneity of motion, as in the instances quoted by Professor Alison in the Appendix to his *Physiology*; but the phænomena alluded to appear to be explicable partly from acknowledged actions of the neighbouring living solids; and

* *Majendie's Journal*, ix. 221.

where they are not so, they are found to be not peculiar to minute organic products, but are observed also in unorganized molecules moving in fluids under the microscope*.

How quickly the body reacts upon the blood, is proved by the direct experiments of Thackrah, Scudamore and J. Davy, who found the quantity of fibrin to vary in different portions of the blood during the same bleeding: generally diminishing.

That a due supply of arterial blood is necessary for supporting the functions of the more important organs of the body, is seen from the singular anastomoses of the large arteries of the brain; from the care taken to guard some of them from pressure; from the imperfection observable in the muscular and nervous powers of persons in whom the septum of the heart is imperfect, or the ductus arteriosus open; and numerous other in-

* Thus, Dr. Czermack of Vienna observed a peculiar motion of the particles of the blood when one of the vessels of the gills in the larva of the *Salamandra atra* was cut through. The particles, under the microscope, of the effused blood were seen to have an irregular motion backwards and forwards at some points, but generally to move round in circles or ellipses. Dr. Sharpey has shown (*Edinburgh Medical Journal*, 1830,) that in the larva of the Frog and Salamander in the Mollusca and other inferior aquatic animals, the exterior covering of the body generally, but especially of the respiratory organs, possesses the power of impelling the water contiguous to it in a determinate direction along the surface, by which a constant current is kept up, and successive portions of water brought in contact with the gills, replacing the action of the respiratory muscles in higher creatures. Drs. Purkinje and Valentin have lately published an interesting paper in Müller's *Archiven*, Part V. 1834, in which are detailed additional observations of the same kind. Whilst seeking for ovules in the tubes of Rabbits which had been impregnated three days, they observed by the microscope that minute particles of the mucous membrane of the tubes presented a lively motion under water, rolling round their axes, and recognised it as a motion similar to that of the cilia of Infusories (*Flimmerbewegung*). The mucous membrane also of the entire uterus and generative passages exhibited similar motions, though with different degrees of vivacity. These distinguished observers were thus induced to inquire further. They found that in all Amphibia, as Serpents, Lizards, &c., in Birds, and in Mammalia, the entire surface of the mucous membrane of the oviduct presents this glittering motion (*Flimmert*); also the mucous membrane of the respiratory passages to their most minute subdivisions. It could not be observed in the mucous membrane of the glottis, of the vocal ligaments, of the mouth or gullet (*Nasen-schleimhaut* appears from the context to be a misprint for *Rachen-schleimhaut*), of any part of the digestive tube or its appendages. So much the more remarkable was the observation of it in the nose, whilst the phænomenon ceased exactly on the limits of these parts. Its presence serves as a sure criterion, where the membrane exactly begins to form a part of the respiratory organ. In the Amphibia, as the Salamander, where the mouth is not merely for swallowing, but is also a respiratory organ, the motion is very lively. The phænomenon could not be perceived in any fishes which were examined; but appeared to the authors, as far as their observations have hitherto extended, to be confined to the mucous surface of the respiratory organs, and of the female generative organs in Amphibia, Birds, and Mammalia. Such portions of the mucous membrane are provided with cilia, according to these observations.

stances. But Blundell's experiments prove, that diminution of quantity is not so immediately hurtful to life as alteration of the quality of the blood. Three fourths or more of all the blood in the body may be removed, and the animal still live. When a Dog had been rendered apparently dead by the loss of ten ounces of blood, it was recovered by transfusion of two ounces. This vital influence of the blood is shown by Prevost and Dumas to depend not so much upon the serum as upon the red particles. An animal bled to insensibility, is not revived by serum of the proper temperature, but is by blood from which the fibrin has been removed by stirring. But with respect to quality, changes of this, slight in appearance, produce important effects. Blundell found, that when he had drawn blood from a Dog's artery, and reinjected it into a vein before coagulation had commenced, (performing the operation until more blood had been thus transfused than equalled the whole weight of the animal,) though it recovered, it was ill for several days with oppressed respiration and impeded action of the heart. When an animal, therefore, is revived with blood of another of the same species, we may suppose that the operation is not entirely without danger of ill consequences. But when this is effected with blood from an animal of a different genus, the consequence is generally fatal. Of dogs revived with human blood, some died shortly after the operation, some on the following, others on the sixth day. When Prevost and Dumas transfused Calves' blood into Kittens or Rabbits, they seldom survived the sixth day: the pulse becoming quick, the warmth diminished, the evacuations bloody. The transfusion of the blood of an animal of another class, *i.e.* with differently shaped globules, almost always causes death. If blood with round particles be injected into the veins of a bird, it dies with symptoms of poisoning from substances which act on the nervous system*.

With respect to transfusion we cannot but assent to the following judicious observations of Burdach. The blood of every creature is in as special a relation to it as are any, the most important, organs of its body, and is its own production as much as they are. It is under the necessity of making its own blood for its own purposes. When an animal, therefore, is saved from death by means of transfusion, it is saved by a mode which introduces into its system causes of derangement in all its functions, and which it must throw off by its powers of secretion before healthy action can be restored.

Nysten's experiments, and those lately detailed by Majendie in his lectures†, seem to prove, that air injected into the veins

* Dieffenbach, quoted by Müller.

† *Lancet*, No. viii. 1834.

destroys life by mechanical interruption of the circulation. For small quantities even of irrespirable gases (except nitrous, sulphuretted-hydrogen, and ammoniacal gases,) are not fatal.

The interesting and difficult inquiry into the causes which produce coagulation of the blood has lately been ably resumed by Mr. Prater. His numerous comparative experiments show how important an element is the *quantity* of the agent which modifies that process: that the same agent which in a large quantity altogether prevents coagulation, in a small quantity favours it. He confirms also the fact, announced by Schrœder Van der Kolk, that as the proportion of serum is relatively increased, (as it is in blood last drawn,) so is the tendency to coagulation also increased. The impression derived from his works is one which strengthens our belief generally in the views of Hunter; viz. that no theory can satisfactorily explain the phænomena of coagulation which has not a regard to the vital properties of the blood; properties whose variations we are not yet able to connect with those corresponding alterations in the composition and aggregation of its molecules which we cannot but believe to accompany them. Mr. Prater attempts to infer from his experiments what the vital properties of the blood are which have hitherto been ascertained; viz. vital elasticity (irritability), and contractility. See *Essay on the Blood*, and *Experimental Inquiries in Chemical Philosophy*.

The Powers which circulate the Blood.—From the observations of Prevost and Dumas, of Baer, and of Baumgærtner, we find that many important organs of the body are traced out in the primitive matter of the germ, before there is any indication of blood or of vessel to contain it. According to Baumgærtner*, the motion of the blood is first perceptible in the Frog and Salamander seven or eight days after the rudiments of the brain and cord are visible, and even twenty-five days after that time in the Trout. In the same way rudimentary shapes, corresponding to skin and organs of sense, muscle and bone, digestive and respiratory organs, are traced out in the original organic matter before they receive any blood. The vessels and the blood which make up the first circulation between the vascular area and the heart, are formed simultaneously from the granular mass which has accumulated between the serous and mucous laminæ of the germinal membrane. The granules, gathering together into isolated masses, present fissures between them containing a yellowish fluid which gradually becomes red. The fissures increase in number; the masses diminish in individual magnitude, whilst they extend the vascular area over a larger space. In these

* *Beobachtungen über das Blut und die Nerven*, quoted by Burdach.

fissures, G. F. Wolff first traced the gradual formation of the walls of the vessels, and the conversion of the fluid into blood by the included masses. The formation of the heart is effected similarly from the granular mass.

When, therefore, the junction between the vascular area and heart has been effected, and the blood moves onwards by means of the heart's contractions, the direction which it takes is not indeterminate as it would be if left to the influence of that power alone. On the contrary, its course is determinate; it seeks the different organs whose lineaments are perceptible, being solicited thereto by the vital attraction which is now established between them. The streams which reach them vary in size, and subdivide through their masses in a way so peculiar for each organ, that even in the adult and perfect form, each of them preserves a mode of branching for its main vessels, and a figure for its capillaries, which are found in no other. What, therefore, is essential in the circulation of the blood, is an attraction subsisting between the particles of each organ and the particles of the blood, and a subsequent repulsion between the same, that which is repelled by one set of organs being attracted by another. What are the physical conditions of the organs and of the blood under which the phænomena are effected, has not been determined. Some have supposed that the particles of the organs and of the arterial blood are in different states of electric polarity. According to them, the organ, as the more fixed, attracts the blood; communicates to it its own electric state, and then repels it. It follows, therefore, that arterial and venous blood are in opposite electric states. J. Müller, however, could discover no electric current by the galvanometer, when one of its wires was placed in an artery, the other in a vein of a living animal.

Though a negative experiment of this kind is not conclusive,—for the electric organ of the Silurus does not affect the galvanometer,—yet Dutrochet's opinion that the red particles in the blood perform the office of galvanic plates, and his belief that he had effected the formation of muscular fibre by galvanizing a drop of serum, cannot be supported. His ingenious experiments are detailed in the *Annales des Sciences Naturelles*, 1831, p. 400. They have been repeated by Müller, who has shown that the supposed muscular fibre is merely a collection of granules of albumen, without consistence, coagulated by the acid of the decomposed salts of the serum; and that there is no sufficient reason for concluding that colouring matter and fibrin (the nuclei of the red particles according to Dutrochet,) are in opposite electric states*. It is on this principle, however, of vital attraction, as I conceive,

* Müller, *Physiologie*, p. 132.

that the phænomena of local inflammation, of determination of blood to particular regions, the emptiness of the arteries after death, &c., are to be explained*. It may be abnormal, as in many of these instances. It may also be suspended or destroyed. Nervous influence seems to be a condition of its continuance, where the animal has nerves. It is stated that when the main artery of a limb is tied, the circulation is not maintained by anastomosing branches if the nerve be also tied†. Many have recorded that in persons bled to fainting, the blood which last issues from the vein is arterial in colour.

The capillary vessels themselves seem to contribute nothing to the motion of the blood: the diameter of the small threads of blood in any of them is not seen to be in the least variable, whilst the heart's action continues the same and the muscles are at rest. They are intermediate between the minute arteries and the minute veins, and continually anastomose. Dr. Marshall Hall states that he has in vain sought for instances of the immediate termination of a minute artery in a minute vein: they all first pass into capillaries. In this way it is that the blood is brought into near contact with the parenchyme of the various tissues, and the processes of nutrition effected. It was the opinion of the older physiologists that all the minute arteries did not thus terminate, but that some ended by open mouths, thus allowing the blood itself to enter into the composition of the different organs of the body. If, however, the transparent parts of living animals be observed, all the particles of blood are observed to pass from the minute arteries through the network of the capillaries, and by these to enter into the veins. The minute anatomy of glands also shows that the last divisions of their ducts are closed tubules; that on these the capillary vessels are largely distributed, but never pass into them. The smallest visible capillaries are those which convey red particles of the blood in single files; their diameter therefore is about the $\frac{1}{4000}$dth part of an inch. That there are others so small as merely to convey the lymph of the blood is however the opinion of many physiologists. It was the opinion of Haller, and Bichât, and Bleuland that there are such vessels; the contrary that of Mascagni and Prochaska, and of Sœmmerring in his later writings. The primitive fibres both of muscle and of nerve are smaller, considerably, than the least visible capillaries.

In the lowest animals, this vital attraction between the molecules of their body and the nutrient fluid which penetrates, by imbibition, their uniform mass, appears to be sufficient for the purposes of their œconomy. But in higher creatures, a moving

* Kalken Brenner, *Majendie's Journal*, viii. 81. † Baumgærtner.

power, afforded by the contraction of living solids, is necessary to bring the nutrient matter within the influence of this attraction. For in them the organs are gradually collected into distinct masses and individualized, and their life depends upon the reciprocal action of these. That their activities may be maintained, a common nutrient fluid is conveyed to them on the one hand, and on the other, is brought into near contact with the air. Wherever the heart exists, it is the principal moving power; but it does not exist until there is a distinct apparatus for aerating the fluid. In the Medusæ and some Polypi, the nutrient fluid is the immediate product of digestion; the digestive cavity supplies the place of a heart, and tubules proceed from it through their substance. In insects a single tube represents the heart: it pulsates from behind forwards, and absorbs at every diastole a portion of a fluid mass which surrounds the intestine, and is the product of digestion. Here the vessel scarcely ramifies, that form being assumed by the respiratory apparatus which conducts the air to every part of the body. In the Crustacea the heart begins to be concentrated; there are arteries and veins: the single cavity of the heart receives the blood from the gills and distributes it to the body. In the Mollusca the heart consists of an auricle and ventricle. In the highest of this class, therefore, it resembles the systemic heart of Mammalia; but in some of the lowest it surrounds a part of the intestinal tube, and in this respect they form a link between the others and those animals in which the heart and digestive cavity are one and the same organ. In the Vertebrata, wherever the heart is single it belongs to the lungs; and here first the nutrient matter is absorbed from the digestive cavity by peculiar vessels, and then conveyed to the circulating system. As the pulsating vessel is gradually concentrated, so also is the nervous system. When the heart has the form of an oblong sac, the nervous system presents a series of swellings connected by a single or double cord. In the Mollusca the heart becomes more globose, whilst a central mass of nervous matter represents the brain, and in many instances surrounds the œsophagus as the heart does the intestine in some. In vertebral animals the ganglionic chain becomes a spinal cord, the ring a brain of a spheroidal shape, whilst the heart is a more perfect central organ; and here first a peculiar system of nerves is appropriated to the nutrient apparatus (the sympathetic), whilst there is a peculiar set of vessels for absorption. Thus do the nervous and circulating systems appear to advance simultaneously towards perfection, the one being a condition, but not a cause, of the other's existence. In all the vertebral animals the heart lies below the spinal cord and abdomi-

nal canal. In all the invertebral it is placed above the ganglionic string, and above the abdominal canal*.

Treviranus has given a table in which the weight of the heart is compared with that of the body, in the different classes of vertebral animals. In the Mammalia it varies from $\frac{1}{80}$ to $\frac{1}{100}$; in Birds, from $\frac{1}{50}$ to $\frac{1}{122}$; in Amphibia, from $\frac{1}{240}$ to $\frac{1}{270}$; in Fishes, from $\frac{1}{330}$ to $\frac{1}{760}$. So that, assuming its power to circulate the blood through the body to be in proportion to its weight relatively to that of the body, he concludes that its influence decreases with the descent of the animal in the series†.

Whether the arteries in any degree assist the heart in effecting the circulation of the blood, is a question upon which physiologists are by no means agreed. That the heart is able to perform this office alone, is proved by those cases where the circulation in the limb was continued, though the main arteries were completely ossified and incapable of contraction; and that it does do so in all cases has been inferred, from the fact that there is no systole and diastole observable in the smaller arteries and capillaries, whilst the blood is seen to flow more quickly in the veins on contraction of the heart. Poisseuille‡ has detailed experiments in which he has shown that the artery certainly dilates and contracts under the heart's action, which had been denied by Bichât and Parry, the cause of the pulse assigned by them being a motion of the entire artery in space, without alteration of diameter. By means of a metal cylinder capable of being opened like a box, he surrounded a portion of the carotid artery of a horse with water. A small graduated glass tube projected from the cylinder: the water rose and fell in the tube on expansion and contraction of the heart, thus evincing the varying volume of the artery. With another apparatus he measured the recoil of the same artery, having detached a portion of it from the body. A glass tube was fixed to each extremity of the artery laid horizontally. Both tubes then turned perpendicularly downwards for a space; then one perpendicularly upwards, the other upwards at an angle of 45°. The former had a stop-cock at the extremity near the artery, the other at the distant extremity. The artery, filled with water, was submitted to a given pressure by mercury and water, which filled the oblique tube, and mercury alone, which balanced it in the other. The artery being thus distended, the stop-cocks were turned. When that at the extremity of the oblique tube was opened, the recoil of the artery caused the mercury to rise in it, and to drive off a portion

* *Vide* Burdach, vol. iv. p. 451.

† *Erscheinungen und Gesetze des Organischen Lebens*, p. 225. 1831.

‡ *Majendie's Journal*, vol. ix. p. 48.

of the water. The perpendicular height of the remaining column of water appeared to show that the contractile power of the artery was greater than the power which dilated it by fifteen millimetres of mercury. We suppose, however, that little can be concluded from this experiment respecting the quantity of the contractile force of the artery in the living body. It is placed in the experiment in very different conditions. Being forcibly dilated between fixed points, its coats represent in the longitudinal direction a series of elastic cords, which are forced into curves, and the whole effect is due in part to the recoil of these, and in part to that of the circular fibres.

The elasticity of arteries, which lasts after death until decomposition takes place, differs from that vital exertion of it called tonicity, which is soon extinguished. By means of the two, particularly the latter, the artery always adapts itself to its contents. In consequence of the latter alone, the artery is smaller shortly after death than after the lapse of several hours. I know of no experiments which satisfactorily indicate any rapid contraction of arteries, which can be referred to muscularity. Their middle coat, in which that property has been supposed to reside, has been shown by Berzelius to differ from muscle, in being more elastic, in having less combined fluid, in being insoluble in acetic acid, soluble in mineral acids, but not precipitable from the solution by potass. Its fibre affords no trace of the transverse striæ which Hodgkin and Lister regard as a peculiar characteristic of muscle.

The pulse, depending upon dilatation of the arteries from the force of the left ventricle, has until lately been by many supposed to be synchronous throughout the body. E. H. Weber, after Sœmmerring, Majendie, Stocks and Carlisle, has shown that it is not exactly so; and for the reason that the arteries are not rigid tubes. The blood driven by the heart into elastic tubes, distends them by an undulation which is progressive. The pulse, which is the distension of the vessel, is synchronous only at equal distances from the heart, and in arteries at considerable distances from the heart follows its beat by one sixth or one seventh of a second.

Heart.—Poisseuille, in order to ascertain the force with which the heart drives the blood into the aorta, has repeated some of Hales's experiments in a more accurate manner. His apparatus consisted of a glass tube, bent into a semicircle, so that its branches were parallel. The shorter of these was again bent at right angles, and nearly to this level the parallel branches were filled with mercury. What remained of the shorter still empty, together with its horizontal portion, was filled with a solution of

subcarbonate of soda, in order to prevent coagulation of the blood when the extremity of the horizontal portion was introduced into an artery. The instrument being so introduced, the mercury was found to oscillate in the parallel branches; and the degree of oscillation and altitude of the mean point was measured by means of a graduated scale on the long limb, when the instrument was held so that this should be perpendicular. The oscillation was caused by the respiration of the animal, the mercury falling on inspiration and rising on expiration. The mid point was found (correction being made for the weight of the column of solution of subcarb. sod. in the short branch,) to stand always at the same place, whatever artery it might be into which the instrument was introduced, upon an average of many observations for each; and gave the height of a column of mercury equal to the mean pressure of the heart, thus shown to exert the same force throughout the whole arterial system. This mean pressure, multiplied by the area of the aorta, gives the statical force of the left ventricle.

The mean pressure was found to be in no degree proportional to the weight of the heart, and to differ so little in animals of very unequal size that Poisseuille is disposed to attribute the variations to individual circumstances of health, age, &c.; and thinks it not unreasonable to conclude that the blood is moved in great and small animals, and in different species, with the same force. From such principles it will follow, that the statical force of the heart in different animals will be proportional to the square of the diameter of their aortas. The mean pressure in the Dog, the Horse, the Mare, was between the limits 140 and 180 millimetres. Taking the mean of these limits, and measuring the aorta, the weight of a column of mercury equal to the statical force of the heart (that with which the blood moves in the aorta) is found in Man to equal 4lbs. 3oz.; in the Horse, 10lbs. 10oz. The statical force with which the blood moves in the radial artery in man under the same pressure equals half an ounce.

If the account which I have given of the beat of the pulse be correct, that of the heart against the sides of the chest will depend upon the contraction of the ventricles. I ought to mention, however, that Corrigan, and Carson, and Burdach are opposed to this opinion, and rather deduce the beat from the distension of the ventricles, the contraction of the auricles immediately preceding that of the ventricles. The last experiment which I have found on this subject is that of Müller*, performed in conjunction with Prof. Albers. The chest of a Goat

* *Physiologie*, p. 165.

was opened: whilst the animal lay on its back, the heart was visibly elevated during the contraction of the ventricles, and even the apex turned upwards. When the hand was laid upon the heart, the perceptible quivering was so powerful and so momentaneous, that it appeared impossible to assign the beat against the ribs to any other cause than the systole of the ventricles, for no agitation could be felt during the diastole.

This conclusion agrees with that which Mr. Carlisle derived from his experiments. The dissections also of this gentleman account for the tilting of the apex of the heart. The muscular fibres, which pass from the basis to the apex, are found by him to be considerably longer on the front than on the back part. They contract therefore more: the apex is drawn towards the basis, but at the same time forward*.

The causes of the two sounds which are perceptible by auscultation, and which occur between two consecutive beats of the heart, are scarcely yet determined to the satisfaction of physiologists. The dull and more enduring sound is quickly followed by one which is clearer and more brief, as was first well defined by Laennec. They follow each other with a slight interval, and then there is a pause. Laennec attributed the first to the contraction of the ventricles, the other to that of the auricles; but the interval between the sounds does not correspond to the interval between these contractions. All appear to be agreed that the first sound is synchronous with the pulse at the heart, and therefore they assign the cause of this as the cause also of the first sound. Thus, Corrigan and others deduce the first sound from the contraction of the auricles, the second from that of the ventricles; Williams, the first from the contraction of the ventricles, the second from the action of the valves; Hope, the first from the contraction of the ventricles, the second from the expansion of the ventricles by the blood†.

Majendie, in a memoir read before the Academy of Sciences of Paris, February 1834, has lately objected to all these explanations. He could perceive no sounds when the heart was laid bare; and therefore concludes that they cannot proceed from the respective play of its different cavities, nor from the action of the heart upon the blood, nor of the blood upon the heart. He then institutes a set of experiments, in order to discover the true cause of the phænomena in question. He found that though all sound ceased when the sternum was removed, yet when elastic bodies were brought in contact with the heart, sounds, variable according to the nature of those bodies, were produced: when

* Vide *Reports* of the *British Association*, vol. ii. p. 456.

† Compare Carlisle, *loc. cit.*, p. 458.

the sternum of a Goose was raised and replaced, the sound was annihilated and reproduced at pleasure; when air or water was injected into the left pleura, so as to keep the heart at a distance from the thorax, no sound could be perceived. Further, he found that if he introduced a slip of metal, thin and flat, into the thorax of a Dog, so as to prevent the shock of the apex of the heart against the parietes of the thorax, though the heart acted violently, the dull sound ceased; if introduced so as to prevent the right ventricle touching the thorax, the clear sound ceased. Majendie hence deduces the first sound from resonance of the thorax, caused by the stroke of the apex; and the second from resonance of the thorax, caused by the impulse afforded by the heart, under sudden dilatation from the influx of blood, to the anterior parietes of the right side of the chest.

Bouilland, in a letter to the Academy of Sciences, has protested against Majendie's explanation; asserting that it does not satisfy the conditions, and raises a doubt concerning the validity of that theory alone which assigns the double sound to the play of the valves; (Romanet, in a recent inaugural dissertation, having maintained that the one sound arises from the shock given by the blood to the tricuspid and mitral valves, the other to the shock on the sigmoïd valves of the aorta and pulmonary artery; and E. L. Bryan the same in the *Lancet*.) Bouilland further alleges his own experiments. No sounds were heard when the heart pulsated being emptied of its blood; they were heard when the heart *in situ* was laid bare, and had no connexion with the walls of the thorax. He further objects, that Majendie's theory does not account for the varieties of sound produced by organic lesions of the valves; nor for the fact, that sounds may be heard, as though distant, when fluid fills the pericardium and prevents the heart from reaching the thorax.

Here this subject rests for the present; but Majendie has undertaken to examine, in a second part of his memoir, whether his explanation will account for all the particular circumstances connected with each of the sounds of the heart.

Cause of the Heart's Action.—Haller, from observing that the heart continues to beat for a considerable time even when removed from the body; and that its contractions, in the body, may be affected by the direct application of mechanical and chemical stimuli to its fibres, whilst he could not influence them by irritation of the cardiac nerves, concluded that its power of contraction is inherent, and totally independent of the nervous system. His theory was afterwards fortified by the dissections of Sœmmerring and Behrends, which appeared to show that the cardiac nerves are distributed to the vessels of the heart alone.

And even after Fowler, Humboldt, and others had stated that the heart may be stimulated by galvanizing its nerves, and Scarpa had demonstrated that these are distributed to its substance as in other muscles, Haller's theory, though vehemently opposed at first, came to be very generally received. It, however, met a formidable opponent in Le Gallois, who published, in 1812, an essay, containing results of numerous experiments, from which it appeared that the heart's power is altogether derived from the spinal cord. He found, that if a Rabbit be decapitated, the heart's action is continued, artificial respiration being performed; that if a portion of the cord be destroyed, as in the lumbar region, the heart is unable to support the circulation, in a Rabbit twenty days old, longer than four minutes, whilst it is continued in one two days old; and that the destruction of the cervical and dorsal portions of the cord are still more suddenly fatal to the heart's action. He observed, on destroying successive portions of the cord, that even when the circulation is suddenly arrested life ceases, on the instant, only in those parts which derive their nerves from that portion of the cord which has been destroyed, continuing for a time in the rest of the body; that this time is greater the nearer the animal is to the epoch of its birth, and is determinate for each species. He concluded that those parts which die last, on partial mutilation of the cord, die because the power of the heart has been so much weakened that the circulation through the entire arteries cannot be maintained. He hence inferred, that if the work to be performed by the heart were diminished in proportion as its power was lost, the circulation might be supported. He found, accordingly, that if the aorta was tied opposite to the part of the cord to be destroyed, the circulation was continued through the remaining portion of the trunk in connexion with the heart. His general conclusions were, that the heart has no intrinsic power, but that it derives its power from every part of the spinal cord; that each part of the body is animated by that part of the cord from which its nerves arise; that the sympathetic system of nerves has its origin in the spinal cord, and not in the ganglia, its office being to bring the parts to which it is distributed within the influence of the whole nervous power of the cord; that the motions of the heart which are visible after excision from the body, are similar to those which may be excited in other muscles after they have been for some time dead, and are merely cadaveric phænomena.

In 1818, Dr. Wilson Philip published his essay on the laws of the vital functions, and reviewed Le Gallois' experiments and observations*. By unexceptionable experiments he showed,

* The first experiments of Dr. Philip are in the *Phil. Trans.* for 1815.

that if the sensibility of the Rabbit be destroyed by a blow on the head, the brain and spinal cord may be entirely removed, the heart still continuing to act. By other experiments he showed, that (the sensibility of the animal being destroyed,) the heart may be excited to act more powerfully by stimuli applied to any part of the brain and cord; that if the stimulus be very powerful, as crushing the central parts of the nervous system suddenly, the action of the heart is suppressed. He infers from hence that the mode in which Le Gallois destroyed the cord exhausted at once the excitability of the heart in those instances in which it entirely ceased to act, and impaired it in others. He remarked that the increased action of the heart could generally be observed as long as the stimulus, whether chemical or mechanical, was applied, unless it was of a nature to produce the sedative after the stimulant effect; and inferred that the former is a direct operation of the agent, when it is observed, and not a consequence of the latter. Dr. Philip therefore concludes, with Le Gallois, that the functions of the cord are independent of the brain; and that the heart is acted upon by every part of the cord. But he disproves altogether Le Gallois' opinion that the irritability of the heart is a quality derived from the cord. He proves that it receives no power from the central parts of the nervous system; but that nervous influence, like any other stimulant, is capable of exhausting its excitability; that it is acted upon not by the cord alone, but by every part of the central masses, the brain and cord: obeying a much less powerful stimulus than the muscles of voluntary motion, but that the stimulus must extend over a large surface of the brain or cord to affect the heart.

Flourens*, from his experiments on fishes, concludes that the power of the heart is inherent, and is influenced only by destruction of that part of the nervous system whose integrity is necessary for respiration, the medulla oblongata.

Dr. Marshall Hall†, making the Frog and Eel the subjects of his experiments, because the transparency of parts at different distances from the heart (in the former the web and lungs, in the latter the caudal, dorsal, and pectoral fins,) allowed him to test the varying power of the heart to circulate the blood, has deduced the following conclusions: that the heart's action is enfeebled from the moment it is deprived, at once, of the influence of the brain and cord; that it possesses an independent irritability, which however, like that of the voluntary muscles, is lost after the organ has been separated from the central masses of the nervous system; that the circulation is first enfeebled, then lost, in the most distant parts of the body from the heart,

* *Mém. de l'Institut*, tom. x. † *Essay*, 1831.

then in parts less and less remote; but that the power of circulation in each part does not depend upon that portion of the cord from which it derives its nerves. He has proved, by decided experiments, that Flourens' opinion of the dependence of the power of the heart upon that part of the central nervous masses which supplies the nerves to the respiratory muscles is unfounded, if that could be doubtful after Clift's experiments on the Carp, published in the *Phil. Trans.* 1815. Dr. Marshall Hall could not observe that opium or spirit of wine, applied to the brain or cord, accelerated the circulation, as recorded by Dr. Wilson Philip.

From these several experiments we conclude: that the heart's power is inherent, and not derived from the brain or cord; that it is under the influence of every part of the brain and cord; that it endures for a time, even when the heart is separated from the body.

The rhythmical contraction of the heart is an instance of that periodicity which occurs in all involuntary motions, even in the minute oscillations of the fibre on which the contractions of those muscles depend which we call voluntary. The whole contraction in the one case is periodical, for the stimulus is constantly recurring; in the other the stimulus is dependent on the will. Though the successive presence of the blood in the different cavities of the heart may, as Haller explained, be the ordinary stimulus to its activity, yet it cannot be the only one, for the rhythmical contraction occurs when the heart is empty, and even when placed *in vacuo*. Why then does the heart continue to act under such circumstances, and what is the stimulus? We have seen, from consideration of the growth of the embryo, that organic activity depends upon the mode in which matter is compounded under the assimilative process. Those organs which receive more blood, are more active than those which receive less; and such as are liable to be called into sudden and excessive action, as the voluntary muscles, receive most blood of all: the blood is there for assimilation as it is wanted; aerated blood, proper temperature, and most probably nervous influence being necessary conditions of the process by which each creature is enabled to maintain that form and mixture of its parts which is necessary to their life. Nutrition, then, or a constant interchange between the particles of the organ and of the blood, being necessary, it follows that something has occurred in the organ during its active state, some alteration, which requires repair. Activity has caused a change in that composition of its molecules which nutrition must restore. If the restoration do not occur, the subsequent reaction is different, or is impossible. The heart, when

removed from the body, responds to stimulation as long as the composition of its tissue is such as to render it capable of doing so. The stimulus may be the blood remaining in its vessels, if its cavities be empty; or may be the nervous influence which, there is reason to suppose, remains in its nerves for a time after they have been cut off from communication with the brain and cord. Under these two conditions it is even possible that a low degree of assimilation may yet go on, which however can never completely restore the state which the fibre possessed at the time when any individual contraction was performed. The excitability at length ceases. But even without this supposition, stimuli which do not at the same time afford food for the nutritive process, can, if food be not otherwise supplied, merely exhaust. The final cause of this perdurance of a certain degree of irritability in the heart, even when nutritive supply and nervous energy are suspended or imperfect, is obvious.

Dr. Carson has pointed out the effect which the empty state of the auricles produces upon the circulation in the veins, imparting to the heart the power of a sucking instrument; and also the effect of the resilient or elastic nature of the pulmonary tissue in subjecting the heart to a less atmospheric pressure than the rest of the body*.

Poisseuille, by means of his barometrical instrument, has confirmed Sir J. Barry's conclusions respecting the effect of inspiration on the venous circulation, as far as the large vessels near the heart are concerned. When the instrument was introduced into the vein of a Dog, towards the heart, the mercury rose 60 millimetres above the zero point during expiration, and fell 70 m. below it during inspiration; the degree of rise and fall varying with the struggles of the animal, but occurring synchronously with the respiratory movements. He did not find that inspiration at all influenced the veins of the extremities; but he confirmed Majendie's observation, that expiration assists not only the motion of the blood in the arteries, but that its effect extends through the capillaries to the veins; the blood rose in them all during expiration, (the instrument being attached to the peripheral portion in the case of the veins,) and during the systole of the heart.

From all these experiments we conclude, that the heart supplies the power which effectually moves the blood in the higher animals, not only through the arteries (to which Bichât confined its effect), but also through the capillaries and the whole venous system; that it is assisted by the elastic power of the arteries after they have been distended by the heart's action upon

* *Inquiry into the Causes of Respiration, &c.*

the blood; assisted also by pressure, whether atmospheric or otherwise, on account of the disposition of the valves in the arteries and in the veins; that the *vis à tergo* is more effectual during expiration; and that the return of the blood to the heart is facilitated by the empty state of the auricles and by inspiration; that the vital attraction and repulsion between the molecules of the organs and of the arterial and venous blood is a concurrent cause.

Hering has published some valuable experiments* made with a view to determine the time in which the circulation is effected. His method was to pour a solution of some harmless substance, easy of detection by tests, as prussiate of potass, into a vein; and to determine, by observation of the blood taken from another distant vessel at short intervals, how soon the presence of the injected solution could be discovered in the latter. In Horses it passed from one jugular vein through the lungs and great circulation, and was detected in the opposite jugular vein in a time varying from 20 to 25 and from 25 to 30 seconds; from the jugular to the saphæna, in 20^s; from the jugular to the external maxillary artery, in from 10^s to 15^s, and in another instance from 20^s to 25^s; from the jugular to the metatarsal artery from 20^s to 25^s, and from 25^s to 30^s; once it required 40^s.

From other experiments† Hering has concluded that the velocity of the blood is independent of the frequency of the heart's action. The prussiate of potass was not detected more quickly than usual when the heart's action had, in numerous instances, been greatly quickened by infusion of tinct. of white hellebore, camphorated spirit, &c. If, with Hales, we estimate the weight of a horse at 800lbs., and his blood at 40lbs., which is certainly not too high an estimate, and allow ten ounces to be thrown from the heart at each systole, (the greatest possible quantity,) then $1^m\ 37^s$ will be the least time in which the whole mass of the blood will go through the heart‡. And though the circulation consists not of one, but of many circles, the smallest being that of which the course through the coronary vessels of the heart forms part, and though each of these circles be performed in a different time, yet it appears difficult to make any probable supposition respecting the circuit taken by the substances injected in the above instances which will satisfy the rapidity with which they were detected. How then is their quick transference to be explained? Probably, as is suggested by Müller, the foreign fluid diffuses itself through the mass of the blood more rapidly than the latter circulates.

* *Zeitschrift für Physiologie*, vol. iii. † *Zeitschrift*, vol. v. part 1.
‡ Burdach.

Report on the Recent Progress and Present State of Zoology.—By the Rev. LEONARD JENYNS, *M.A., F.L.S. F.Z.S. F.C.P.S.*

THE following Report has been drawn up at the request of the Section for Natural History of the British Association. I cannot but express my regret that the task has not devolved upon abler hands. The science of Zoology comprises such a wide field, and so much has been effected in that field by the researches of modern times, that it is difficult for any individual to obtain a correct knowledge of all that is going on in different countries in its several particular departments. Still more difficult is it to form in all cases a true estimate of the relative importance of the many facts and discoveries which are every day coming to light,—to judge of their mutual bearing on each other, and their more or less immediate tendency to advance the progress of that science for the interests of which they are brought forward. I must therefore hope for much indulgence from those who may discover in this attempt, what it is almost impossible to avoid, many errors as well as omissions*. I have endeavoured to avail myself of whatever sources were open to me, in order to obtain the information requisite for the purpose; but so numerous are the channels through which such information is now published, that I can hardly hope to have gleaned on this subject all which may be expected of me.

It is right, however, that I should state in the outset, in what point of view, and within what limits, I propose to consider this subject. To follow it out in all its details would manifestly lead me far beyond the bounds to which a Report of this nature must necessarily be restricted. My intention, then, is principally to notice those researches which of late years have tended to elucidate the characters and affinities of the larger groups of animals, and thereby to advance our knowledge of their natural arrangement. This will include the consideration of such systems as have been brought forwards in illustration of this part of the subject. With reference to this point, however, I do not purpose commencing from an earlier period than 1817, the year of publication of the *Règne Animal* of Cuvier, whose general views respecting the classification of animals have been the basis of most of those which have appeared subsequently. I propose, nevertheless, in the first instance, to make a few general remarks

* I fear that these omissions will be found rather numerous, with respect to German works, some of which I have been unable to procure, whilst there are probably others altogether unknown to me.

on the state of zoology in the early part of the present century, and the circumstances which have led to the introduction of those principles upon which it is now studied.

I. *Introduction.*

It is now generally acknowledged, that the true and legitimate object of zoology is the attainment of the Natural System; and we may attribute it to this circumstance, and the consequent close investigation of structure and affinity to which it has led naturalists, that so striking a change has been effected of late years in this science, causing it to assume an aspect at once characteristic of a distinct epoch in its history. Little else appeared to be the aim of Linnæus and his followers beyond that of distinguishing species, and classing them simply in accordance with some law of arrangement arbitrarily assumed in the first instance, and too often pertinaciously adhered to in utter disregard of the general organization; and although it may have been their endeavour to group together those species amongst which there appeared a certain resemblance, yet they did not hesitate in numberless instances to associate in the same class and order, and often in the same genus, beings of the most discordant nature, rather than renounce the principle which they had adopted for their guide. It is undoubtedly to Cuvier that we are most indebted for the striking improvements which began to be made upon the Linnæan system towards the close of the last century*. This great master of modern zoology saw the importance of studying the entire organization of animals. He traced the connexion which subsists between their internal and external structure, observed how these accorded with their habits and œconomy, and perceived that in grounding a classification of animals upon characters taken from these sources collectively, we should make a near approach towards grouping them according to their true and natural affinities. Daubenton and Pallas had already furnished some materials for such an undertaking, and by their exact descriptions, paved the way for a more complete knowledge of animal structure; but it was reserved for Cuvier to erect the building of which they may be said to have only laid the foundation. Commencing with a re-investigation of the invertebrate animals, which, according to the statement of the French naturalist, Linnæus had left in a state

* It is not meant that there were none others besides Cuvier who had any share in effecting this change, but only that he appears the most prominent. Bruguières, Geoffroy St. Hilaire, Latreille, and Lamarck, especially the last, all contributed to this end in their several departments. The labours of some of these naturalists will be alluded to further on.

of worse arrangement than that of Aristotle, he afterwards passed on to that of the higher classes*, carrying with him that reform which the new principles he had adopted pointed out to be necessary. Cuvier's first memoir on the *Invertebrata* was offered to the notice of the Natural History Society of Paris in 1795, and the time should be remarked as commencing the æra of a most important revolution in zoological science. His *Tableau Elémentaire de l'Hist. Nat. des Animaux*, containing a still further development of his views, was only two years posterior to it. This was followed by the *Leçons d'Anatomie Comparée*, published in 1800 and 1805; a rich series of memoirs on the molluscous animals, which appeared in the earlier volumes of the *Annales du Muséum*; the *Recherches sur les Ossemens Fossiles*, of as great service to zoology as geology, and much of which was also first published in that collection; and lastly, in 1817, by the *Règne Animal*, in which it was attempted to arrange all known animals according to their natural affinities, as deduced from a comparative view of their whole organization.

The above works, of some of which I shall have occasion to speak further hereafter, were not only important in themselves, but in the consequences to which they led.

In the first place, it is worthy of remark, that since these admirable endeavours on the part of Cuvier to elucidate the true relations of animals by reference to their internal as well as external structure, and to the modifications, not of one or two arbitrarily selected organs, but of all the organs considered jointly, naturalists have everywhere felt the necessity of guiding their researches after the same manner, and building upon a similar foundation. If zoology has made much progress, as undoubtedly it has, since the publication of the first edition of the *Règne Animal*; if more enlarged views have been acquired of the science as a whole, and a more correct knowledge gained of some of its subordinate branches; if new forms of structure have been discovered, and affinities brought to light, which at that time were not even suspected to exist by its illustrious author; this is greatly due to the assistance which the science has derived from comparative anatomy: and it must never be forgotten, that it was Cuvier principally who first taught us, in the works above alluded to, how to bring this great and powerful instrument to bear upon the researches of the naturalist. Yet it must not be supposed from the intimate connexion which subsists between these two sciences, that there is no line of distinction to be drawn

* In the arrangement of the *Mammalia*, Cuvier was much assisted by Geoffroy. Their joint labours in this department form the subject of a memoir published in the *Magasin Encyclopédique*, tom. ii. p. 164.

between them. It is the object of the anatomist to investigate the details of structure, and to record all facts connected with the relative organization of animals. That of the zoologist is to arrange these facts, and to make them subservient towards determining the natural affinities of animals. Hence the latter is but little concerned with any details which do not exercise a marked influence upon the manner of life, or with those minute differences of structure which are not accompanied by corresponding differences in the rest of the organization*. What he seeks for is a subordination of characters, selected in the order of their importance, on which to build his system; and to judge of the value of any one in particular which anatomy presents to him, he must trace by observation how far it is connected with others, whether external or internal, or derived from the œconomy and mode of life, of which the value is known. On such a comparison, it may prove of too small importance to assist in determining the affinities of a single species. Yet we can hardly pronounce that it may not be found of some value hereafter; for although it may not in itself be sufficient to establish an affinity, it may tend to corroborate our ideas respecting those which would seem already indicated by other characters. And considered in this view, even the minutest anatomical details may prove of service to zoology. As an instance in point, we may refer to Mr. Owen's recent discovery of a peculiar modification of the stomach in the genus *Semnopithecus*†. This genus had been originally established by Geoffroy upon a slight difference

* It has been a complaint with some naturalists that zoology has of late years been too much invaded by comparative anatomy, and that it has been in danger of suffering from the encroachments of that allied science which was originally called in to its assistance. For remarks on this subject the reader is referred to the article Zoologie in the *Dict. Class. d'Hist. Nat.* (p. 727), and the Introduction to the *Hist. Nat. des Mammifères*, (p. 2, &c.) by M. Fred. Cuvier. Mr. Swainson would also seem to say as much in his *Preliminary Discourse on the Study of Nat. Hist.* (pp. 84, 169, &c.), published since this Report was read. To a certain extent there may perhaps be some ground for the complaint; but it appears to me that it is only called for in those cases in which it has been attempted to arrange animals *solely* from anatomical characters, no consideration being paid either to external form or to the habits and manner of life. We may fall into the error of attaching too much importance to differences of internal structure, as easily as we may in the case of those of external. The fact is, the whole must be considered collectively, and it is the *relative value* of the organs, when viewed in their mutual dependencies, which alone should decide on which of them we are to base our system. But after having determined our groups in this manner, we may generally succeed in finding, at least amongst the higher animals, some *external* character by which they may be distinguished. And wherever this is the case, I fully agree with Mr. Swainson (pp. 169 and 247), that for convenience sake such external characters should be exclusively employed.

† *Zool. Trans.*, vol. i. p. 65.

only in the dental system, and there were some doubts as to whether it should be retained. Now, however, that we find it also characterized by an accompanying difference in the internal organization, its claims to be admitted as a distinct group in the system are considerably strengthened. In another communication this able anatomist has expressed an opinion*, that even such details as tracing the convolutions of the brain "may advance zoology, by bringing to light additional instances of affinities between the different groups of *Mammalia*;" and he grounds this opinion upon the fact of his having observed a remarkable uniformity of structure in this organ, in groups which have been long since well established upon other characters. At the present day, however, it is amongst the lower animals that the researches of the anatomist will most assist zoology. The structure of the higher classes is in general well understood, and it is not likely that any future discoveries in anatomy will much affect our present arrangement of the leading groups in those classes, however they may contribute to the perfecting the details of the system. But amongst the *Invertebrata* it is far otherwise. There we not only find large groups of animals of whose internal structure we know but little, but they are often groups in which the external characters cannot be trusted, and in which it becomes necessary to resort to the same organs for distinguishing orders, families, and even genera, which in the *Vertebrata* would only be employed in characterizing classes, or groups of a still higher denomination. This arises from the much more variable structure of the lower animals, with which therefore it becomes the more necessary for the zoologist to be acquainted.

Another circumstance which has in some measure resulted from Cuvier's labours relates to the country in which these labours were exerted, and their fruits made public. His works have had a manifest influence over his countrymen. Those who surrounded him quickly adopted his new views and principles; and partly to this circumstance, partly to the magnificent establishment of the *Jardin des Plantes*, are we to attribute the gradual rise of a school of zoology in France, which has ever since maintained the highest reputation. It is only necessary to refer to some of the many valuable works which appeared during the early part of the present century, in order to appreciate the zeal and success with which zoology was cultivated in that country. Lamarck's *Système des Animaux sans Vertèbres* and *Philosophie Zoologique*, Dumeril's *Zoologie Analytique*, Latreille's *Hist. Nat. des Crustacés et des Insectes*, the *Genera*

* *Zool. Trans.*, vol. i. p. 136.

Crustaceorum et Insectorum by the same author, Brongniart's *Essai d'une Classification Naturelle des Reptiles*, Savigny's *Mémoires sur les Animaux sans Vertèbres*, Lamouroux's *Histoire des Polypiers Coralligènes Flexibles**, (not to dwell upon a rich series of memoirs in the *Annales du Muséum*, *Journal de Physique*, &c., by Geoffroy, Fred. Cuvier, Blainville, Péron, Lessueur, and others,) all appeared before the publication of the *Règne Animal*, and not only contributed greatly to the further illustration of the natural system, but furnished many valuable hints to Cuvier himself whilst engaged in that undertaking. England, we fear, has but little to produce as the result of her labours in zoology during the same period. Our countrymen were too much riveted to the principles of the Linnæan school to appreciate the value of the natural system. Although there were some good descriptive works in different departments, and a few excellent observers, amongst whom Montagu will ever hold a distinguished place, there was in general but little attention paid to structure with a view to elucidate the natural affinities of animals. The most remarkable, if not the only exception is undoubtedly to be found in Kirby's *Monographia Apum Angliæ*, a work which, though exclusively devoted to the illustration of a single Linnæan genus of insects, presents a model for naturalists in all departments, from the profound views of its very illustrious author. There were few, however, who followed up the path which was thus opened to them. There was a general repugnance to everything that appeared like an innovation on the system of Linnæus; and for many years subsequently to the publication of the above work, which appeared as far back as in 1802, zoology, which was making rapid strides in France and other parts of the Continent, remained in this country nearly stationary. It is mainly to Dr. Leach that we are indebted for having opened the eyes of English zoologists to the importance of those principles which had long guided the French naturalists. Whilst he greatly contributed to the advancement of the natural system by his own researches, he gave a turn to those of others, and made the first step towards weaning his countrymen from the school they had so long adhered to. The following are the principal works which resulted from Dr. Leach's labours in zoology about the period of time referred to. In 1813, he published the article CRUSTACEOLOGY in the *Edinburgh Encyclopædia*, in which he gave the system of Latreille, with some slight modifications. In 1814, he gave, in a paper to the Linnæan Society, "A tabular View of the ex-

* In the above list I have not included the splendid volumes by Desmarest, Vieillot, Audebert, &c., the object of which was more to illustrate species by coloured plates than to treat of their systematic arrangement.

ternal Characters of the four Classes of *Crustacea*, *Myriapoda*, *Arachnida*, and *Insecta*, with the Orders and other Subdivisions of the three first of these Classes." In the same year he commenced the *Zoological Miscellany*, which, though principally intended for the illustration of new or little known species, contains (the 3rd vol. especially, published in 1817,) an indication of many new groups in different classes of zoology, with their characters and natural affinities. In 1815, he published the article ENTOMOLOGY in the *Edinburgh Encyclopædia*; and in the same year he commenced the *Malacostraca Podophthalma Britanniæ*, which tended so much to our further knowledge of the *Crustacea*. Besides the above, Dr. Leach also wrote the articles ANNULOSA and CIRRIPEDES in the Supplement to the *Encyclopædia Britannica*, the latter containing an entirely new classification of these animals. It is much to be regretted, that soon afterwards the labours of this distinguished naturalist were interrupted by illness. He had prepared and nearly completed a valuable work on the British *Mollusca*, to the natural arrangement of which group he had devoted great attention. Part of it was printed, though never published. His other works, however, sufficiently testify the obligations conferred by him on zoology. At the same time they form a marked epoch in the history of this science, as connected with our own country. Since the time of their publication many other excellent naturalists have arisen amongst us to contribute to its advancement, to whom I need make no further allusion at present, as of some I shall find occasion to speak hereafter.

II. *Of the primary Types of Form, and other leading Divisions, in the Animal Kingdom.*

Cuvier considered the animal kingdom as exhibiting four primary types of form, to which he gave the names of *Vertebrata*, *Mollusca*, *Annulosa*, and *Radiata*. The leading characters are derived from the nervous system, which Virey was the first to point out* as the most important part of their organization, and therefore the most fit to be selected as the groundwork of the system. Cuvier's first enunciation of this arrangement was in a memoir published in the 19th vol. of the *Ann. du Mus.* in 1812, being five years before the appearance of the *Règne Animal*. In it he observes, that he regards these four types or general plans as those after which all animals appear to have been modelled, and of which the subordinate divisions are only comparatively slight modifications, founded on the development or addition of certain parts, which produce no essential change in the original plan.

* *Nouv. Dict. d'Hist. Nat.*, Art. ANIMAL.

Before the date of this memoir, naturalists had generally adopted Lamarck's primary division of *Vertebrate* and *Invertebrate* animals. Cuvier objected to this, on the ground that there were as great differences of structure amongst these last, as any of those by which they were separated from the *vertebrate* division. In his *Hist. Nat. des An. sans Vertèb.* (the first volume of which was published in 1815), Lamarck somewhat modified his former views, by distributing animals under the three divisions of *Intelligent*, *Sensible*, and *Apathetic*. As this arrangement, however, is obviously objectionable, and has not met with much reception, I do not consider it necessary to dwell further on it*. I shall proceed, therefore, to notice some modifications of Cuvier's system which have been proposed by different authors, as well as some new systems and principles of arrangement which have appeared since the publication of the *Règne Animal*, and which from their importance appear deserving of consideration.

The first in order of time, with which I am acquainted, is a modification of Cuvier's primary divisions proposed by Geoffroy in 1820, and which arose from his peculiar views respecting *the unity of composition* in animals. It is not necessary at the present day to enter into any detailed analysis of these views, which have been so long associated with the name of this distinguished naturalist, and which belong more to the department of comparative anatomy than zoology. It is sufficient to state that Geoffroy, who had previously endeavoured to show that all vertebrate animals were constructed so exactly upon the same plan as to preserve the strictest analogy of parts in respect to their osteology†, thought to extend this unity of plan by demonstrating, as it appeared to him, that the hard parts of *Crustacea* and Insects were still only modifications of the skeleton of higher animals, and that therefore the type of *Vertebrata* must be made to include them also. It is impossible in this Report to follow up the train of reasoning and anatomical research which guided Geoffroy in his attempt to establish this theory. The general results at which he arrives are, that the segments of the *Annulosa* are strictly analogous to the vertebræ of the higher animals,

* Although Lamarck's leading divisions are objectionable, there is much in his system which is extremely valuable, particularly as respects the arrangement of the *Invertebrata*. He was the first to point out that these last, if placed according to their true affinities, must be considered as forming two distinct subramose series, one consisting of the articulated, and the other of the inarticulated invertebrate animals.—See the *Supplement* to his first volume, p. 457.

† Geoffroy's principal memoirs relating to this subject were collected into one volume, and published in 1818 under the title of *Philosophie Anatomique*. Several others however, more or less connected with it, are to be found in the *Ann. du Mus.*

and that the former live *within* their vertebral column, in the same manner as the latter do *without*. It is clear that, assuming the correctness of these views, it becomes necessary to make some alteration in the leading divisions of Cuvier's system. The following is the arrangement proposed by Geoffroy :

Animaux	Vertébrés.	Hauts-Vertébrés.	(*Vertébrés*, Cuv.)
		Dermo-Vertébrés.	(*Articulés*, Cuv.)
	Invertébrés.	Mollusques.	(*Mollusques*, Cuv.)
		Rayonnés.	(*Rayonnés*, Cuv.)

Thus we have a primary division into *vertebrate* and *invertebrate* animals, before arriving at Cuvier's four types, taking however the term *vertebrate* in a much more extended sense than did Lamarck, or any other previous author, and likewise that of *invertebrate* in a more restricted one. Geoffroy's memoirs on this subject were published, as already stated, in 1820, in the *Journal Complémentaire**, &c. He subsequently followed up the same views in some other publications, more especially in a paper in the *Mém. du Mus.* for 1822†, in which he entered into a strict analysis of the structure of the vertebra, first as it occurs in the higher animals, and afterwards as it appears, though modified, in the segments of the *Annulosa*‡. His theory, I believe, has been adopted by many of the French and German naturalists, as well as by some in other countries. Amongst the former, I may particularly mention Robineau-Desvoidy, who in 1828 published a work§ in order to substantiate, by still further illustration, the vertebral structure of the *Crustacea*, *Arachnida*, and *Insecta*. Towards the conclusion, he has pointed out the necessity (as it appears to him) of instituting several new classes amongst the annulose animals. It may be much doubted, however, whether these new classes will ever be adopted generally, whatever may be the fate of those theoretical views which have alone suggested them ||.

* "Mémoires sur l'Organisation des Insectes," *Journ. Complém. du Dict. des Sci. Med.*, tom. v. p. 340; and tom. vi. pp. 31 and 138.

† tom. ix.

‡ See also his *Cours de l'Hist. Nat. des Mammifères*, Leç. 3e, published in 1829.

§ *Recherches sur l'Organisation vertébrale des Crustacés, des Arachnides, et des Insectes.* Paris, 1828, 8vo.

|| As connected with the subject of the differences and resemblances between vertebrate and invertebrate animals, I may refer to two recent memoirs, of which abstracts will be found in *L'Institut.* The first, entitled "Recherches sur les Parties dures des Animaux Invertébrés, par M. Dupuy," was read to the Academy at Toulouse, Jan. 1833. (*L'Institut*, 1833, p. 3.) The other is a memoir by Dutrochet, "Sur l'Opposition qui existe entre les Animaux Vertébrés et les Animaux Invertébrés," read to the Academy of Sciences at Paris in March last. (See *L'Instit.* 1834, p. 90.)

A slight modification of Geoffroy's views has been adopted by M. Dumortier, and recently published in a memoir on the comparative structure of plants and animals, in the 16th vol. of the *Nov. Act. &c. Nat. Cur.**. Like Geoffroy, he considers the hard parts of *Crustacea* and Insects as strictly analogous to the osseous system of the *Vertebrata*; but instead of the two primary groups into which he distributes animals, M. Dumortier would adopt the three divisions of *Endosceleta*, *Exosceleta*, and *Asceleta*, the second answering to Geoffroy's *Dermo-Vertébrés*, and the third to his *Invertébrés*. These are given in a tabular form, with the secondary groups into which he thinks the animal kingdom should be divided, amounting to twelve in number, also annexed. In a former part of his paper, M. Dumortier has entered into considerable details on the subject of the analogies which may be observed between the above three primary groups of animals, and the corresponding primary groups in the vegetable kingdom. It would, however, occupy too much room to enter into any more extended analysis of his views.

In 1821, Mr. MacLeay published the second part of his *Horæ Entomologicæ*, in which he proposed a new arrangement of the leading groups of the animal kingdom, and considered them as referrible to *five* primary types, instead of *four*, the number adopted by Cuvier. The new type, which he has called *Acrita*, he intended should include the least organized of the *Entozoa* of Rudolphi, as well as Cuvier's classes of *Polypi* and *Infusoria*, all which he considered as not sufficiently showing the true radiated structure characteristic of the type to which Cuvier referred them. Mr. MacLeay observed, that the necessity for this step had been previously pointed out, though indirectly, by Lamarck and Blainville. The establishing of this new group was not, however, the most important feature in the *Horæ Entomologicæ*. Mr. MacLeay announced some new principles connected with the classification of animals, which, from the circumstance of their having led to a peculiar school of zoologists in England, it will be necessary to consider a little more in detail. The most important of these principles† are: 1st, *That all natural groups, of whatever denomination, return into themselves, forming circles;* 2ndly, *That each of these circular groups is resolvable into exactly five others;* 3rdly, *That these five groups always admit of a binary arrangement, two of them being what he calls* typical, *the other three* aberrant; 4thly, *That while proximate groups*

* p. 306.

† It may be observed, that Mr. MacLeay has nowhere formally stated these principles as above. They are only gathered from what he has written on the subject.

in any circle are connected by relations of affinity, *corresponding groups in two contiguous circles are connected by relations of* analogy. Mr. MacLeay has also observed*, that, *in almost every group, one of the five minor groups, into which it is resolvable, bears a resemblance to all the rest; or, more strictly speaking, consists of types which represent those of each of the four other groups, together with a type peculiar to itself.* These principles had been partly brought forward by Mr. MacLeay, two years before, in the first part of the work above mentioned. It was then, however, with exclusive reference to the natural arrangement of the Lamellicorn Insects, in which group we are told it was that he was first led to detect them. It was not till 1821 that he applied them more generally, in showing that a tendency to circles prevailed throughout nature, and that the same principles which he had observed to regulate the natural arrangement of the above group, appeared to regulate that of the entire animal kingdom. It is somewhat remarkable, and certainly tending to confirm Mr. MacLeay's views, that in the same year, and apparently without any knowledge of the first part of the *Horæ Entomologicæ*, M. Fries, in Germany, published his *Systema Mycologicum*, in which he announced principles somewhat similar to those above stated, as regulating the natural distribution of *Fungi*. This gave rise to a paper from Mr. MacLeay, read the following year to the Linnæan Society†, in which he commented on this identity (so far as the identity prevailed,) of the principles which they had respectively adopted. He also pointed out wherein they differed; one difference consisting in the determinate number, which M. Fries considered as four, being the same as that formerly advanced by Oken. Mr. MacLeay's arrangement of the Lamellicorn Insects in the first part of the *Horæ Entomologicæ* was the result of rigid analysis, and is therefore deserving of the greatest attention; that however of the entire animal kingdom in the second, was chiefly deduced from synthetical investigation, and was moreover confined to the larger and more important groups. It is not, therefore, surprising that many endeavours should be made subsequently by himself, as well as by those who had adopted more or less of his theory, to illustrate his new principles by a more close application of them to different departments of zoology. The first result was a paper by Mr. Kirby, in 1822‡, in which he described some insects that appeared to exemplify Mr. MacLeay's doctrine of affinity and

* *Hor. Ent.*, p. 518.

† *Linn. Trans.*, vol. xiv. p. 46.

‡ *Linn. Trans.*, vol. xiv. p. 93.

analogy. In 1823*, Mr. Vigors made an application of Mr. MacLeay's principles to the class of Birds, pointed out the orders and families, and endeavoured to show that the natural affinities which connect the several groups in that class obeyed the same laws as those laid down in the *Horæ Entomologicæ*. The same author subsequently followed up this inquiry in some particular families of the same class†. In 1824‡, Mr. MacLeay applied his own principles to the arrangement of the *Mollusca Tunicata*. In the same year Mr. Swainson endeavoured§, with reference to the circular and quinary system, to work out the natural affinities of the family of *Laniidæ* in ornithology. In 1825, appeared the first number of the *Annulosa Javanica*, in which Mr. MacLeay again brought his views to the test by applying them to the natural arrangement of the insects collected in Java by Dr. Horsfield. Circumstances prevented Mr. MacLeay from proceeding with this arrangement beyond that of a small portion of the *Coleoptera*; but Dr. Horsfield himself subsequently proceeded to publish the *Lepidoptera* ‖ classed according to the same principles. In the same year, (1825,) Mr. Gray published an attempt at the natural distribution of the *Mammalia* into tribes and families¶, and likewise of the genera of the classes *Reptilia* and *Amphibia***. Both these, but the former more especially, were intended to illustrate Mr. MacLeay's principles. In 1826††, Mr. MacLeay gave the result of some anatomical investigations, which tended to confirm the accuracy of Mr. Vigors's arrangement of Birds. In the same paper he considered the affinities which connect the various orders of *Mammalia*, the point of transition from this class to *Aves*, and the true analogies existing between the respective orders of the two classes. In 1827‡‡, Mr. Swainson gave a sketch of the natural affinities of the *Lepidoptera diurna* of Latreille, being also an application of Mr. MacLeay's principles. Lastly, in 1831, appeared the second part of the *Fauna Boreali-Americana*, in which Mr. Swainson, still adopting Mr. MacLeay's views in part, but modifying them according to what (since his former

* *Linn. Trans.*, vol. xiv. p. 395.

† *Zool. Journ.*, vol. i. p. 312. and vol. ii. p. 368.

‡ *Linn. Trans.*, vol. xiv. p. 527. § *Zool. Journ.*, vol. i. p. 289.

‖ *Descriptive Catalogue of the Lepidopterous Insects contained in the Museum of the Hon. E. India Company, &c., with introductory Observations on a general Arrangement of this Order of Insects.* 4to, 1828, &c.

¶ *Ann. Phil.*, vol. xxvi. p. 337. ** *Id.*, vol. xxvi. p. 193.

†† *Linn. Trans.*, vol. xvi. p. 1.

‡‡ *Ann. Phil.*, vol. i. p. 180.

papers) he has conceived to make a nearer approach to the true natural system, endeavoured to work out an amended arrangement of some of the principal groups of birds. The modifications which Mr. Swainson has been led to make in this work of Mr. MacLeay's principles are these. He conceives, that although every natural group is resolvable into five others, the *primary* division is into *three*, each of which forms its own circle: he thus rejects Mr. MacLeay's binary distribution of his five groups into *typical* and *aberrant*, which last not forming circles, would seem to be rather at variance with his own principles. He has also stated more precisely the law by which it appears to him the relations of analogy are governed. It is thus given: *The contents of every circle or group are symbolically represented by the contents of* all *other circles in the same class of animals; this resemblance being strong or remote, in proportion to the proximity or the distance of the groups compared**. This principle, which Mr. Swainson terms the *theory of representation*, he considers as affording the *only certain test* of a natural group. Mr. MacLeay had considered such a test to be afforded by a group returning into itself, which Mr. Swainson thinks not sufficient, on the ground that there is not one group in three which *can* be so tested; this arising partly from our superficial acquaintance with forms, and partly, as he believes, from there being many real gaps in the chain of continuity. It will be observed that Mr. Swainson has been the first to bring forward any new laws of arrangement at all analogous to those originally developed in the *Horæ Entomologicæ*; and it is right to state, that the above are not mere hypothetical deductions, but have resulted from eight years' close analysis of the order *Insessores* in the class of birds, with reference to which order principally it is that he has illustrated them in the *Fauna Boreali-Americana*.

It is evident that the necessary limits of this Report forbid any further analysis of Mr. MacLeay's theory, or of the several works and memoirs above referred to. To some of these last I shall have further occasion to allude afterwards. What has been advanced may tend, however, to point out the influence which this theory has had over our own naturalists; and if they have not been all equally successful in their endeavours to apply it to

* M. Isidore Geoffroy St. Hilaire, in France, has also attended to the subject of analogies in zoology, and endeavoured to refer them to some general law. The reader is referred to a note attached to a memoir published by him in the *Nouv. Ann. du Mus.*, tom. i. p. 380, in which he has given a slight sketch of his views on this point. He proposes to make it the subject of a distinct paper at some future opportunity.

different branches of zoology, these attempts have on the whole certainly advanced our knowledge of natural groups, and developed many affinities before unsuspected. At the same time it is difficult to believe that there is not some truth at the bottom of this theory, however erroneous it may be in its details; and that some of its details are erroneous, as well as many of the subordinate arrangements in the system which has been built upon it, is almost certain, from many facts which have been brought forwards of late years, as well as from that difference of opinion* which exists with respect to these details amongst those who admit the fundamental principles. Neither are these fundamental principles entirely new. Mr. MacLeay has himself shown† that his doctrines have all been in some measure advanced by authors prior to the publication of the *Horæ Entomologicæ*; which circumstance, while it tends to strengthen our conviction that they have more or less of truth in them, does not detract from Mr. MacLeay's merits in having developed them far beyond what any of his predecessors had done. To him we are certainly indebted for having pointed out the exact *nature* of the difference between affinity and analogy in natural history, however these two kinds of relation may have been observed by former authors‡. He was also the first to establish by proof circular affinities. He has sufficiently demonstrated their existence in certain groups, to lead us to suspect that it is only our as yet imperfect knowledge of forms, and the gaps necessarily arising from the circumstance of many forms having become extinct, which prevents us from tracing their existence generally. And these are by far the most important of Mr. MacLeay's principles. Whatever of error there may be in the rest of his views, whatever modifications already have been, or may yet further be made in them, by the help of the above principles he appears to have approached nearer than any before him to the

* This difference of opinion more especially respects the determinate number. While Mr. MacLeay considers it as five, and Mr. Swainson as three, Mr. Kirby is of opinion that it will turn out to be seven. (*Introd. to Ent.*, vol. iii. p. 15.) It must be stated that this gentleman has hitherto brought nothing forward in support of this last number. It has, however, found an advocate in Mr. Newman, who has also endeavoured to establish some other modifications of Mr. MacLeay's theory. See a small tract, called *Sphinx Vespiformis*, published in 1832.

† *Linn. Trans.*, vol. xvi. p. 8.

‡ I add this because, some time back, there was a controversy between M. Virey and Mr. MacLeay on the question of priority with respect to the above distinction. See a review, by the former, of some of Mr. MacLeay's opinions in *Bull. des Sci.* 1825. tom. iv. p. 275, in which M. Virey states having made this distinction long before in the *Nouv. Dict. d'Hist. Nat.*, Art. ANIMAL. Mr. MacLeay has replied to M. Virey in *Zool. Journ.*, vol. iv. p. 47.

true natural system, and (as has already been twice observed*) been enabled to "reconcile facts which upon no other plan can be reconciled."

It is necessary now to revert in point of time, for the purpose of noticing some works which appeared on the Continent during the above period. In 1821, Oken published his *Esquisse de Système d'Anatomie, de Physiologie, et d'Histoire Naturelle.* This celebrated German naturalist is well known to have imbibed some very original views connected with the classification of animals, which have led to a peculiar school of zoology in Germany, in like manner as those of MacLeay have in England. I regret that I am unable to say much of his system, which however I believe to be only a modification of one which he had before published in some of his earlier works†. It is based upon a theory which supposes the animal kingdom to be developed after the same order as that in which the organs are in the body. He considers that these organs form, characterize, and represent the classes; and that there are the same number of classes as there are organs. He also attaches to them names derived from the organs. Fanciful as this theory appears, it has not only had many followers in Germany, but has given rise to several attempts at a natural classification of animals founded upon analogous principles. Such is the "Synoptic Table of the Animal Kingdom," published at Dresden, in 1826, by Ficinus and Carus‡, in which the leading divisions are based upon views somewhat similar to those of Oken. In 1827, Leuckhart also published, at Heidelburg, "An Attempt towards a Natural Classification of Intestinal Worms, followed by a Table of the Affinities of Animals in general," constructed upon the same principles§. I am unable to notice these works more particularly, but I conceive that it would be unnecessary, were it in my power to do so.

In 1822, Blainville published his *Principes d'Anatomie Comparée*, annexed to which are some Synoptic Tables of the Animal Kingdom, containing a slight modification of a new system first brought forward in 1816 in the *Journal de Physique*‖. In this system, the primary divisions, which are called sub-kingdoms, and are three in number, are established on characters

* Kirby, *Introd. to Entom.*, vol. iv. p. 359; and Swains. *Fn. Bor. Am.*, part 2. p. xlvi.

† *Philosophy of Nature*, (in German,) Jena, 1809, 3 vols. 8vo. Also, *Treatise on Natural History*, (in German,) Jena, 1816. Oken is also the editor of a valuable German periodical, called *Isis*, containing many important papers in zoology. He was the first in Germany to abandon the Linnæan system.

‡ *Bull. des Sci.* 1829, tom. xvii. p. 258.

§ *Id.*, 1829, tom. xvii.

‖ tom. lxxxiii. p. 244.

taken from the general form, which Blainville finds in accordance with those derived from the nervous system when this is present. The first of these subkingdoms he terms *Artiomorphes* or *Artiozoaires*, being that in which the form is symmetrical, or the parts disposed symmetrically on each side of the body; the second, *Actinomorphes* or *Actinozoaires*, in which the parts radiate from a common centre; the third, *Hétéromorphes* or *Hétérozoaires*, in which the form is indeterminate. The *Artiomorphes* are referred to three secondary types, characterized from the arrangement of the locomotive organs: (1.) *Osteozoaires*, in which the body and limbs are composed of several pieces articulated together, the articulations not being visible from without; (2.) *Entomozoaires*, in which the body and limbs are likewise articulated, the articulations being externally visible; (3.) *Malacozoaires*, in which the body is of one single piece, and not divided into several parts. The *Osteozoaires* are the same as the *Vertebrata* of Cuvier. The *Entomozoaires* answer nearly to his *Annulosa*, including, besides the classes referred to that type in the *Règne Animal*, the *Entozoa*, and likewise the *Cirripeda* and the genus *Chiton*. These two last groups, however, form a subtype, which Blainville calls *Malentozoaires* or *Molluscarticulés*. The *Malacozoaires* correspond to the *Mollusca* of Cuvier, excluding the *Cirripeda* and the genus *Chiton* just mentioned. The second subkingdom, *Actinomorphes*, comprises the *Radiata* of Cuvier, with the exception of the *Sponges*, *Infusoria*, and *Stony Corallines*, which compose the third subkingdom, or *Hétéromorphes*. Blainville's system, though different from Cuvier's, deserves to be studied, from its indicating many new affinities which had not been before noticed. Its author however has adopted, and in many instances very unnecessarily, an entirely new nomenclature, which alone has been sufficient to prevent it from having been generally received by naturalists.

In 1825, Latreille published his *Familles Naturelles du Règne Animal*, in which he considers the animal kingdom as primarily divided into three great series: *Vertebrata*, the essential character of which group he does not derive however from the vertebral column, so much as from the presence of a brain, consisting always of a cerebrum and cerebellum, and the great sympathetic nerve, whereby it is particularly distinguished from his second group, *Cephalidia*, in which the brain is only rudimentary, and the third, *Acephala*, in which it no longer exists. His *Cephalidia* embrace the *Annulosa* and *Mollusca* of Cuvier, with the exception of the *Acéphales sans coquilles*. These, with the *Zoophytes* of the same author, constitute his *Acephala*.

Before arriving at the classes of the *Vertebrata*, Latreille adopts a previous division of this series into *Hæmatherma* (with warm blood,) and *Hæmacryma* (with cold blood). This last is again divided into *Pulmonea* and *Solibranchia*, according as the respiration is carried on by lungs or gills. In like manner we find his second series, *Cephalidia*, divided into the three races of *Mollusca*, *Elminthoida* (comprising the classes *Cirripedes* and *Annelides*), and *Condylopa* (with articulated feet). The *Acephala* also into *Gastrica* and *Agastrica*. Few will probably prefer Latreille's three primary divisions to Cuvier's four types, or judge his arrangement on the whole to be more natural than that of the *Règne Animal*. His *Cephalidia*, in particular, bring together under one head two very distinct groups which are well separated by Cuvier.

The above are some of the principal systems, or modifications of that of Cuvier, which have been brought forwards since the first edition of the *Règne Animal* *. In 1829 appeared the second edition of the work just mentioned, in which however there is no material alteration, at least as far as regards the distribution of the leading groups.

It may be thought by some that the subject is hardly deserving so much notice; that the consideration of different systems, some of which perhaps we feel sure are grounded upon erroneous principles, may be passed over as of not much importance to zoology. Cuvier will teach us to judge otherwise. He observes† that the affinities of animals are so complicated, that we ought thankfully to receive every endeavour to set them before us in a new point of view. There are few systems which do not contribute something to our knowledge on this subject, and which do not thereby enable us to make some further advance towards that which is the end and object of the science, the *natural* system itself‡.

III. *Of the several Classes in the Animal Kingdom.*

In entering on the consideration of the several classes of ani-

* I have been obliged to omit the notice of certain works which may perhaps contain some new views respecting the arrangement of animals, but which I have been unable to get sight of. Such are the *Elements of Zoology*, (in Italian,) by Ranzani, published at Bologna in 1819, &c.; and the *Manual of Zoology*, (in German,) by Goldfuss, published at Nuremberg in 1820. In 1828, Van der Hoeven also published a *Tabular View of the Animal Kingdom*, equally unknown to me except by title.

† *Hist. du Progrès des Sci. Nat.*, tom. iv. p. 182.

‡ Mr. MacLeay has well observed that "every discovery of an affinity is, in part, a discovery of natural arrangement." (*Hor. Ent.*, p. 324.)

mals, I may observe, that it is not my intention to do more than to convey a general notion respecting the state of our knowledge of the principal groups contained in them. At the same time I shall notice any recent researches which appear to throw light on their affinities, or to illustrate more clearly either their external or internal characters. Of these last I confine myself to such as are of immediate importance to zoology.

I. Vertebrata, *Cuv.*

1. *Mammalia.*—Cuvier and Geoffroy greatly contributed to our knowledge of this class during the early part of the present century. The former by his investigation of fossil species supplied us with many new forms, serving in several cases as links to connect groups which before were widely separated. He also found it necessary, in order to determine the above with accuracy, first to examine more closely the structure of such species as are living at the present day. Owing to this circumstance his *Ossemens Fossiles* has conferred a lasting benefit on this department of zoology. His researches served to elucidate the history of numberless genera, and even led to the establishment of one entire family*, of which the true affinities had previously been quite misunderstood. Geoffroy also laboured much, and indeed has continued to do so to the present time, at the natural arrangement of these animals. His various memoirs in the *Annales du Muséum* and other French periodicals, more particularly those on the *Marsupialia*, *Cheiroptera*, and *Quadrumana*; his splendid work also, the *Histoire des Mammifères*, undertaken conjointly with M. Fred. Cuvier, are well known, and deservedly celebrated. Yet notwithstanding the laborious researches of these, and many other eminent zoologists, perhaps it is not advancing too much to affirm, that we are still in many cases far from understanding the real affinities of the *Mammalia*, and less agreed about the primary groups into which they ought to be distributed, than in the instance of some other classes lower down in the system†. This will appear by referring to the principal classifications which have been published since that of the *Règne Animal*. Cuvier, in the work just mentioned, admits the following eight orders: *Bimana*, *Quadrumana*, *Feræ* (Carnassiers), *Rodentia*, *Edentata*, *Pachydermata*, *Ruminantia*,

* The *Herbivorous Cetacea.*

† This probably arises in a great measure from the paucity of forms which this class presents compared with others. Mr. MacLeay has observed, (*Annul. Javan.*, p. xi.) that we are more likely to detect the natural arrangement amongst Insects, from the circumstance of their presenting such a multiplicity of species, than in any other part of the system.

Cetacea. Desmarets, in his *Mammalogie*, published in 1820, follows Cuvier. Blainville* distributes the *Mammalia* primarily into the two subclasses of *Monodelphes* and *Didelphes*, this last being instituted for the reception of the *Marsupialia*, and *Monotremata* of Geoffroy. His subclass of *Monodelphes* includes seven orders; of these the first five are the same as Cuvier's, only the *Rodentia* and *Edentata* are transposed, and the latter includes the Cetaceous animals, with the exception of the *Lamentines*: these last, with the *Proboscidiens* of Cuvier, form his sixth order, called *Gravigrades*: his seventh order, *Ongulogrades*, comprises the rest of Cuvier's *Pachydermata* and his *Ruminantia*. Latreille† considers the *Monotremata* as a distinct class altogether. His class *Mammalia* comprises Cuvier's eight orders, besides the three additional ones of *Cheiroptera*, *Amphibia*, and *Marsupialia* (in the *Règne Anim.* only subordinate groups in the order *Carnassiers*‡.) Mr. MacLeay, in a paper in the *Linnæan Transactions*§ already alluded to in a former part of this Report, dated 1826–7, adopts as primary divisions the old groups *Primates*, *Feræ*, *Glires*, *Ungulata*, and *Cetacea*, the first three, and last, being identical with the four Linnæan orders bearing the same names, the fourth (adopted from Aristotle and Ray) including the Linnæan orders *Brutæ*, *Pecora*, and *Belluæ*. Mr. MacLeay has made some important and interesting remarks on the series of affinities connecting the above orders which deserve to be consulted, but which would occupy too much room here. He attempts to show that the chain returns into itself, forming a circle. He considers the whole class as passing off to the Birds by the *Glires*||, and as also indicating an affinity to the *Reptilia* in the *Monotremata*. In 1827, Temminck published the first part of his valuable *Monographies de Mammalogie*, at the end of which he gives a systematic arrangement of the whole class. He adopts, in addition to Cuvier's orders, those of *Cheiroptera* and *Monotremata*: the former is inserted between the *Quadrumana* and *Carnivora*; the latter is placed at the end of the whole series, as serving to point out the transition to Reptiles and Birds.

* *Principes, &c.*, tab. 3. † *Fam. Nat.*

‡ Latreille thinks that at the end of the *Quadrumana*, the *Mammalia* divide themselves into two series: one composed of the *Cheiroptera*, *Marsupialia*, *Rodentia*, and *Edentata*; the other of the *Feræ*, *Amphibia*, *Pachydermata*, *Ruminantia*, and *Cetacea*. See *Fam. Nat.*, p. 59, note (1).

§ vol. xvi. p. 1.

|| The analogy which exists between the organization of the *Mammalia Rodentia* and that of *Birds*, was pointed out by Professor Otto of Breslau the same year. See *Bull. des Sci.* 1827, tom. xii.

The same year Lesson published his *Manuel de Mammalogie*: his arrangement, however, is the same as that of Cuvier. In 1829, appeared the 2nd edition of the *Règne Animal*, in which the *Marsupialia* are considered as a distinct order; in all other respects the arrangement is the same as that of the first. The same year Fischer published his *Synopsis Mammalium*. His orders approach more in character to those of Linnæus: he adopts, however, two more than that author: one, *Cheiroptera*, placed between the *Primates* and *Feræ*; the other, which he terms *Bestiæ*, and which includes the *Insectivora* and *Marsupialia* of Cuvier, following the order last mentioned. Also in 1829, appeared a valuable treatise on the *Mammalia* by Fred. Cuvier, in the 59th volume of the *Dict. des Sci. Nat.* His arrangement differs in one respect from that of all his predecessors, in as much as he has thrown together in one order the first two families of the *Carnassiers* of the *Règne Anim.* and the *Insectivorous Marsupialia*, while of the *Frugivorous Marsupialia* he has made a separate order. He has also made distinct orders of the *Amphibia* and *Monotremata*.

In 1830, Wagler published his *Natürliches System der Amphibien*, to which he has prefixed a classification of *Mammalia* as well as of *Aves*. His orders in the former of these classes, amounting to eighteen, are much more numerous than those of any other author. It is hardly necessary to specify them, as few, I conceive, will be disposed to adopt them all as primary divisions. They more properly deserve the name of families. Wagler considers the *Monotremata* as a distinct class, to which he gives the name of *Gryphi*. It may be observed, that he also includes in it the fossil *Ichthyosauri* and *Plesiosauri*, as well as the *Ornithocephalus* of Sömmerring*.

In 1831, Charles Lucien Bonaparte published an arrangement of the *Vertebrata*† differing in some respects from that of his predecessors. The *Mammalia* are primarily divided into the two subclasses of *Quadrupeda* and *Bipeda*, the latter being intended to receive the Cetaceous animals. The *Quadrupeda* are again divided into the two sections of *Unguiculata* and *Ungulata*. His orders resemble those of Fischer, excepting that he isolates the *Marsupialia*, referring the *Insectivora* (with which they are associated by this last author) to the order *Feræ*. He also

* Mr. MacLeay has suggested in his *Horæ Entomologicæ*, p. 267, that possibly the *Ornithocephalus* may have been the connecting link between *Mammalia* and Birds.

† *Saggio di una Distribuzione metodica degli Animali Vertebrati.* 8vo, Rom. 1831.

makes a distinct order of the *Amphibia*. The *Monotremata* he considers as a separate class*.

On a review of the above systems it will appear how much difference of opinion exists respecting the value of certain groups, more particularly the *Cheiroptera*, *Marsupialia*, and *Monotremata*. To the number of those systematists who regard the *Cheiroptera* as a distinct order, we may add Geoffroy, whose opinion will have weight, when we remember the particular study which for many years he is known to have made of these animals. We may refer to the twelfth and thirteenth Lectures in his *Cours de l'Histoire Naturelle des Mammifères*, as presenting considerable details respecting the general organization of these animals and their several peculiarities. He regards them as holding an intermediate place between the *Quadrumana* and *Feræ*, but requiring to be separated from both.

The *Marsupialia* will continue to perplex us until we can determine the true value of that peculiar character by which they are so remarkably distinguished from all other *Mammalia*. Is it to controul the characters derived from the organs of mastication, digestion, and motion, which may be referred almost to as many types as there exist genera amongst these animals? Even adopting that it ought, as most naturalists seem disposed to do, we have still to decide, whether the *Marsupialia* constitute merely a peculiar order, or a group of any higher denomination as supposed by Blainville. Although Cuvier has only admitted them to the former, he observes that they might almost be supposed to form a distinct class parallel to that of the ordinary *Mammalia*, and divisible into similar orders. The solution of these difficulties must probably be sought in a more profound study of the *relative* internal organization of these and other *Mammalia*. This subject has, indeed, for some time already engaged the attention of Geoffroy†, and more recently it has been taken up by Messrs. Morgan‡ and Owen§. We may reasonably hope that by the combined researches of these eminent anatomists, some new light will before long be thrown upon the affinities of these singular animals.

* Since this Report was read, I have seen a sketch of a new arrangement of the *Mammalia* recently proposed by M. Duvernoy. Like Blainville, he considers the *Marsupialia*, (under which series he includes the *Monotremata*,) as a group equivalent to the rest of the *Mammalia* taken together, for which last he retains Blainville's name of *Monodelphes*. His orders are very numerous. See *L'Institut*, No. lxv. p. 261.

† See the article Marsupiaux in the *Dict. des Sci. Nat.*, tom. xxix.

‡ *Linn. Trans.*, vol. xvi. pp. 61 and 455.

§ *Proceedings of the Zool. Soc.* 1831, p. 159; 1833, p. 128.

The *Monotremata*, which are involved in quite as much obscurity as the *Marsupialia*, have been for some time, but particularly within the last two years, a subject of great controversy amongst the first naturalists. Although belonging more to the department of anatomy, it will be necessary to say something of this discussion, from its great importance to the science we are considering. The controversy has chiefly turned upon the existence or not of true mammary glands in these animals, and their consequent claims to be admitted among the *Mammalia*.

Lamarck was the first to maintain, in 1809*, arguing from the supposed absence of these glands, and the consequent probability that the *Monotremata*† were oviparous, that they ought to form a separate class. This opinion was subsequently adopted by Geoffroy in the *Bulletin de la Soc. Phil.* 1822‡; and also by Van der Hoeven in a memoir on the *Ornithorhynchus*, published in 1823 in the *Nov. Act. &c. Nat. Cur.* §. In 1824, Meckel announced, in *Froriep's Notizen*, that he had discovered these glands, and in 1826 he published his Anatomy of the *Ornithorhynchus*||, in which their nature and situation were more fully illustrated. In the course of the same year (1826) Geoffroy endeavoured to show¶, that the supposed mammary glands seen by Meckel were not truly *lactiferous*, but analogous to certain glands which he had observed in the genus *Sorex***. In 1827†† Meckel replied to Geoffroy, adducing further arguments in support of his former opinion. The same year Geoffroy published a memoir on the structure of the genital and urinary organs in the *Ornithorhynchus*‡‡, from an examination of which he was still led to infer that it was certainly oviparous. This belief was soon after much strengthened by the receipt of information from Dr. Grant of the supposed discovery of the eggs of the *Ornithorhynchus*§§, which circumstance gave rise to

* *Phil. Zool.*, tom. i. pp. 145 and 342.

† The name of *Monotremata* dates from 1803, when Geoffroy, who first applied it in consideration of the peculiar structure of the genital organs, made simply a new order of these animals. See *Bull. de la Soc. Phil.*, tom. iii. p. 225.

‡ p. 95. § tom. xi. part ii. p. 351.

|| *Ornithorhynchi Paradoxi Descriptio Anatomica.* fol. Lipsiæ, 1826.

¶ *Ann. des Sci. Nat.*, tom. ix. p. 457.

** These latter glands form the subject of a memoir published by Geoffroy, in 1815, in the first volume of the *Mémoires du Muséum*; to which I refer the reader.

†† *Archiv. für Anat.*, band x. p. 23. ‡‡ *Mém. du Mus.*, tom. xv. p. 1.

§§ See an account of the discovery, accompanied by a description of these eggs, in the *Edinb. New Phil. Journ.* for Jan. 1830, p. 149.

another memoir on the part of Geoffroy, published in the *Ann. des Sci.* for 1829*. In 1832, the controversy respecting the existence of the mammary glands again arose. In June of that year, Mr. Owen read a paper to the Royal Society†, in which he entered into a close investigation of the structure of these glands, and decided altogether in favour of Meckel's opinion that they were strictly lactiferous. This opinion was further confirmed by a statement made the following September by Dr. Weatherhead to the Zoological Society‡, respecting the positive discovery of milk in the instance of a female Ornithorhynchus lately taken with its young in the interior of New South Wales. In October of the same year, Mr. Owen laid before the Zoological Society § the results of an anatomical investigation of the mammary glands of the *Echidna Hystrix*, in which animal he was also led to believe that they were really lactiferous. In February 1833, Geoffroy published a memoir in the *Gazette Médicale*||, in which he stated that the secretion of these supposed mammary glands was not really milk, but mucus, destined for the nutriment of the newly hatched young. In the same month, Blainville read a memoir¶ to the Academy of Sciences at Paris in support of Mr. Owen's opinion. In March, Geoffroy made a communication to the Zoological Society** on the subject of his last memoir, to which Mr. Owen replied, alleging arguments against the probability of the secretion being mucus as Geoffroy supposed. In July the controversy between these two individuals was resumed††. Several other memoirs‡‡ have been

* tom. xviii. p. 157. † *Phil. Trans.* 1832, p. 517.
‡ *Proceedings of Zool. Soc.*, p. 145. § *Proceedings of Zool. Soc.*, p. 179.
|| See *Proceedings of Zool. Soc.* 1833, p. 28.
¶ *Nouv. Ann. du Mus.*, tom. ii. p. 369. In this memoir, although Blainville considers the *Monotremata* as mammiferous, he retains his former opinion with respect to the propriety of instituting a subclass for them, as forming the transition from viviparous to oviparous animals. In the same subclass he suspects the fossil *Ichthyosaurus* would claim a place. This, it will be observed, accords with the views of Wagler already alluded to.
** *Proceed.*, p. 28. †† *Proceed. of Zool. Soc.*, p. 91.
‡‡ For abstracts of these memoirs see *L'Institut*, Nos. 4, 7, 9, 32, 33, 40, 45, and 46. From some of the later ones it will be seen that this controversy has not been confined to the subject of the *Monotremata*. Geoffroy endeavoured to make it appear probable that the mammary glands of the *Cetacea* were of a similar nature with the *Monotrematic* glands (as he terms them) in the *Ornithorhynchus*; and that if this were proved to be the case, the *Cetacea* also should be made to constitute a distinct class. Several facts and statements[1], however, have been brought forward to demonstrate that these glands are certainly lactiferous in the *Cetacea*, and I believe Geoffroy himself has since changed his opinion on this head.

[1] See an article by Dr. Traill in the *Edinb. New Phil. Journ.* for July of the present year (1834), p. 177.

also read by Geoffroy to the Academy of Sciences at Paris, both during the last and the present year, connected with this question. Nothing, however, has as yet been brought forward serving to prove the incorrectness of Mr. Owen's views, which certainly on the whole appear far more probable than those of Geoffroy. We may add in conclusion, that Mr. Owen has recently dissected a young *Ornithorhynchus*, the stomach of which was found filled with coagulated milk*, which milk examined under a high magnifying power, and compared with that of the cow, was found strictly analogous to this last in respect to its ultimate globules. This seems almost decisive of the matter. At the same time the mode of generation in these animals, whether oviparous, or ovoviviparous as appears more likely, remains yet to be ascertained.

No one has paid so much attention to those organs in the *Mammalia* employed by zoologists in characterizing genera and species as M. Fred. Cuvier. The teeth have been particularly studied by him with reference to this point. His memoirs on this subject in the *Ann.* and *Mém. du Mus.*† formed the basis of a complete work‡, published in 1825, in which he has given an accurate description of the dental system in each of the principal genera throughout the *Mammalia*, illustrated by figures. He has observed a remarkable uniformity of character in the *molares*, in all those genera which are manifestly natural, and generally admitted to be such by naturalists§. With reference also to their zoological characters, he has more recently made a study of the various productions of the cuticle. As yet he has only treated of the structure of the spines of the Porcupine||, which he selected in the first instance as most readily examined, and likely to throw much light on the structure and development of hair in general. His researches, as far as they have been hitherto conducted, lead him to regard the hair as furnishing the zoologist with characters of more importance than has been usually supposed. He proposes, however, to follow up this subject on another occasion. In a memoir published in the same volume with the one just alluded to, F. Cuvier has pointed out

* *Proceed.* &c. 1834, p. 43.

† toms. x. xii. and xix. of the former, and tom. ix. of the latter.

‡ *Dents des Mammifères considérées comme Caractères Zoologiques.* 8vo, Par. 1825.

§ On the subject of the teeth of the *Mammalia*, their structure and zoological characters, see a memoir recently published by Geoffroy. (*Mém. de l'Institut*, tom. xii. p. 181.) His chief object is to prove that the long anterior teeth of the *Rodentia*, usually considered as incisors, strictly represent the canine teeth.

|| *Nouv. Ann. du Mus.*, tom. i. p. 409.

some valuable characters for distinguishing the species of *Vespertilionidæ*. These are derived from the form of the head, which he refers to three distinct types; the form and direction of the auricle, which he refers to seven types; and the form of the tragus. He observes that in the restricted genus *Vespertilio*, the organs of mastication and motion present but little variation.

2. *Aves.*—The structure of birds in general is perhaps quite as well understood as that of *Mammalia*, and the leading groups are on the whole better determined. It is also curious to observe that the orders most generally adopted at the present day nearly coincide with those of Linnæus, thus evincing the tact with which that great naturalist in some instances seized affinities. The only alterations which we find in the *Règne Anim.* consist in the union of the two Linnæan orders *Picæ* and *Passeres*, (between which it is certainly not easy to define,) and the separation of the *Scansorial* birds from the former to constitute a distinct order by themselves. It will be well, however, to notice the principal arrangements of this class which have appeared since Cuvier's, in some of which we shall find a desire to deviate more widely from the system of Linnæus. This will also afford an opportunity of pointing out those individuals who have most contributed to the recent progress of this department of zoology. The first is that of Vieillot, which appeared in 1818 in the 2nd edition of the *Nouv. Dict. d'Hist. Nat.* (Art. ORNITHOLOGIE). Its author was previously well known for his many valuable works on ornithology, in one of which* he had already given a slight sketch of his arrangement. Vieillot's orders are five in number, and similar to those of Linnæus, excepting that with Cuvier he throws together the *Picæ* and *Passeres* to form one, which he calls *Sylvicolæ*. For the terms *Grallæ* and *Anseres*, he also substitutes Illiger's names of *Grallatores* and *Natatores*. In 1820, Temminck published the 2nd edition of his *Manuel d'Ornithologie*, to which is prefixed a sketch of a general arrangement of birds, professedly grounded on the habits and organization. Perhaps, however, this is the least valuable part of a work, exceedingly rich in practical information relating to this class, and indispensable to ornithologists on all other considerations. Temminck's system, which is a slight modification of that given in the first edition of his Manual, cannot be considered as natural. His orders, amounting to sixteen, are greatly overmultiplied, and are far from being groups of equal value. In fact, he has not distinguished between orders and

* *Analyse d'une nouvelle Ornithologie élémentaire.* 8vo, Par. 1816.

families. Blainville's arrangement of this class* is grounded upon the form of the *sternum* and its appendages (*clavicle* and *os furcatorius*), according to a plan first developed in a memoir read to the Institute in 1812. At the same time, for the sake of convenience, its author has had recourse to the usual external characters for distinguishing the groups. As the sternum gives attachment to the principal muscles of flight, and thereby necessarily exercises a certain influence over the œconomy, it may assist in determining many natural affinities which would otherwise escape us. Hence Blainville's system deserves to be regarded, although we may not be disposed to adopt it entirely. One of its chief peculiarities consists in the forming a distinct order of the Parrots, which stand first in the arrangement. Blainville thinks that not only the form of the sternum, but the whole organization and habits of these birds justify this step. With the rest of the *Scansores*, which are separated from the above by the intervention of the Birds of Prey, he associates the *Syndactyli* and *Caprimulgidæ*, groups not referred by Cuvier to this order. He has also made distinct orders of the *Pigeons* and *Ostriches*. His other orders nearly coincide with those of the *Règne Anim*. Although not immediately following in point of time, I may here notice an elaborate memoir on the sternum of birds by M. L'Herminier, published, in 1827, in the *Ann. de la Soc. Linn. de Paris*†, in which he has endeavoured to draw the attention of naturalists afresh to the great importance of this part. He has studied its structure in a large number of species, and founded upon it a new classification entirely different from all former ones excepting that of Blainville. He divides birds into two subclasses: the first comprises all those in which the sternum is constantly furnished with a keel, and is distributed into thirty-three families; the second forms but a single family, containing the Ostrich, Cassuary, and a few others, in which the keel is always wanting. M. L'Herminier thinks that the birds just mentioned conduct to the Reptiles, and not to the *Mammalia* as is generally supposed. In 1823 appeared Mr. Vigors's "Observations on the natural Affinities connecting the Orders and Families of Birds‡," to which allusion has been already made, as containing an application of Mr. MacLeay's principles. His primary divisions are the same as Cuvier's, excepting that he sinks the order *Scansores*, which he considers as only a subordinate group of his order *Insessores*, which name he has substituted for that of *Passeres*. The names adopted for his other

* *Principes*, &c., tab. 4.

† tom. v. p. 3—93.

‡ *Linn. Trans.*, vol. xiv. p. 395.

four orders are taken from Illiger*, viz. *Raptores* (*Raptatores*, Ill.), *Rasores*, *Grallatores*, and *Natatores*†. Mr. Vigors has traced out the chain of affinities which connects the above groups, and endeavoured to show that it returns into itself, forming a circle. Latreille in his arrangement‡ follows Cuvier, with some slight modifications. Thus, he has a primary division of the whole class into the two sections of *Terrestres* and *Aquatici*: he has also made a distinct order of the *Columbæ* and *Alectrides*, Vieill., to which he gives the name of *Passerigalli*. Wagler's orders § are more numerous than even those of Temminck, and deserve to be considered in many cases rather as natural families. He has annexed a synopsis of the genera of birds, arranged in the order of their affinities||. In 1831, M. Lesson published his *Traité d'Ornithologie*, containing the result of a careful examination of the collections at Paris, to which in some measure it serves as an accompanying catalogue. In this arrangement, which professes to be according to the natural system, we have a primary division of birds into *Anomalous* and *Normal*, these groups being analogous to M. L'Herminier's subclasses, and characterized in like manner from the sternum and its appendages. The former comprises the five genera of *Struthio*, *Rhea*, *Casuarius*, *Dromaius* and *Apteryx*¶. The latter is divided into orders, on the whole similar to Cuvier's, the *Scansores*, however, forming only a suborder among the *Passeres*. The *Columbæ* and genus *Penelope*, Merr., which Cuvier associates with his *Gallinacés*, are also referred to the *Passeres*, where they form a portion of another suborder, called from Latreille *Passerigalli*. In the same year (1831), Mr. Swainson published the second volume of the *Faun. Bor. Amer.*, in which he has stated his views with respect to the natural arrangement of birds, although he has only illustrated them at length with reference to one order. Mr. Swainson's principles, which have been before alluded to, lead him to re-

* *C. Illigeri Prodromus Systematis Mammalium et Avium.* Berol. 1811. A work extremely useful even at the present day, on account of its containing a very complete terminology with reference to the above two classes.

† Mr. Vigors places the *Struthionidæ* among the *Rasores*. By Cuvier they are associated with the *wading birds*.

‡ *Fam. Nat.*

§ *Natürliches System, &c.*

|| Wagler had previously published, in 1827, a portion of a work entitled *Systema Avium.* It was not so much, however, a systematic arrangement of birds, as a collection of treatises on different genera, those being selected in the first instance which he had studied most thoroughly. It was his intention to have arranged them afterwards in a systematic table. The work, however, was never completed, and its talented author has recently met with a premature death.

¶ See a paper by Mr. Yarrell on this anomalous genus in *Zool. Trans.*, vol. i. p. 71.

cognise three primary groups into which the class *Aves* is divisible. To these he does not affix names, but merely designates them as the *typical*, the *subtypical*, and the *aberrant*. His secondary divisions, at least those adopted in the above work, which are equivalent to the orders of other authors, are the same as those of Mr. Vigors. In the details of the arrangement the systems of these two authors are in many respects different. The latest arrangement of this class with which I am acquainted is that of C. L. Bonaparte, in his *Saggio di una Distribuzione*, &c. He divides it into the two subclasses of *Insessores* and *Grallatores*: the former containing the orders *Accipitres* and *Passeres*, Cuv.; the latter those of *Gallinæ*, *Grallæ*, and *Anseres*.

The above are the principal authors who have treated of the systematic arrangement of this class of late years. The general leaning seems to be towards the adoption of the same orders as those just mentioned*. The group which presents most difficulties in the way of a natural classification is undoubtedly that of *Scansores*, on the value of which naturalists are not agreed. Latreille considers it as forming a parallel order to that of the *Passeres*. It will probably, however, be allowed ultimately to be only a subordinate group in this last order, as is already the opinion of Vigors, Lesson, and others. In the details of the system there is still much uncertainty, though more in some groups than others. And this uncertainty can only be cleared up by a more rigorous analysis of external characters, combined with anatomical investigation. This last has already been successfully resorted to in some families, for the determination of true affinities. Thus, Mr. Yarrell, by studying the internal structure of the *Anatidæ*, has sketched out an arrangement of this group†, which Mr. Swainson finds in accordance with his own views on the subject‡ derived from the external characters and habits. The same gentleman has recorded some important notes§ respecting the internal organization of *Cereopsis* and some allied species, serving in like manner to confirm the notions previously entertained respecting the affinities of these birds. There can be no doubt also that we may derive much assistance from studying the systems of those authors who, like Blainville and L'Herminier, have taken some one of the internal organs as the basis of their arrangement. For however it may be true that no such arrangement can be natural in itself, founded upon characters derived from one organ exclusively, yet it affords an in-

* I speak of the groups themselves without reference to any particular names for them.

† *Linn. Trans.*, vol. xv. p. 378.

‡ *Fn. Bor. Am.*, vol. ii. p. 436.

§ *Proceed. of Zool. Soc.* 1831, p. 25.

sight into the method of variation of that organ, teaches us in consequence its exact value, and when viewed in connexion with other systems previously established upon other characters, may serve to correct and perfect many details in these last beyond what we might be able to do by any other method. With reference to this end, besides the above, I may refer to a system of Dr. Ritgen, in the Transactions of the Cæsarean Academy at Bonn*, established upon the characters of the *pelvis*, as one, not to be adopted entire, but capable perhaps of furnishing some valuable hints which might otherwise be lost†.

The external characters of birds have recently received much attention from M. Isidore Geoffroy St. Hilaire, who has published a memoir on this subject in the *Nouv. Ann. du Mus.*‡ which deserves to be consulted by all ornithologists. He has reviewed those in most general use, and pointed out several of which he thinks the proper value has not been correctly appreciated. He particularly mentions the emargination of the bill, so much trusted to in characterizing the *Dentirostres*, as one to which too much importance has been attached. On the other hand, he regards the disposition of the toes, in the *Passeres* more particularly, as not having been sufficiently studied in a general point of view. His researches indeed on this point have led him to propose a new arrangement of the order just mentioned, which he divides into the three groups of *Zygodactyles*, *Syndactyles*, and *Deodactyles*, this last comprising the great bulk of the genera, which have the toes divided in the regular way. Hence it will be seen that he does not side with those who regard the *Scansores* as forming a distinct order. The feet of the *Passeres*, and the characters which they furnish, have likewise been much attended to by M. De la Fresnaye, who has also proposed a new arrangement of this order§, though not exactly upon the same plan as Geoffroy's. The year previously to that in which Isidore Geoffroy published the above memoir, he gave some new observations in the *Annales des Sciences*‖ relating to the characters of the *Strigidæ* in particular, to which however it would occupy too much room to allude more particularly.

The structure and mode of development of feathers, which has been so ably illustrated by Fred. Cuvier¶, and subsequently by

* tom. xiv. p. 217.

† The *pelvis* of birds has been recently studied by M. Bourjot St. Hilaire, and made the subject of a memoir, read to the Royal Academy of Sciences at Paris in August last. See *L'Institut*, No. 66, p. 266. ‡ tom. i. p. 357.

§ See an abstract of M. De la Fresnaye's memoir in the report of the *French Congress* held at Caen in 1833, p. 69. See also other memoirs by him on the same subject in Guérin's *Magasin de Zoologie* for 1832 and 1833.

‖ 1830, tom. xxi. p. 194. ¶ *Mém. du Mus.* 1825, tom. xiii. p. 327.

other observers*, is perhaps too much within the province of pure animal physiology to require notice here. The laws, however, which regulate the assumption and changes of plumage are of the utmost consequence for the exact discrimination of species. These laws have received great attention from Mr. Yarrell, who has lately added one to those previously established†, viz. that "*Whenever adult birds assume a plumage during the breeding-season decidedly different in colour from that which they bear in the winter, the young have a plumage intermediate in the general tone of its colour compared with the two periodical states of the parent birds, and bearing also indications of the colours to be afterwards attained at either period.*" In the same paper Mr. Yarrell has stated some experiments, the results of which fully establish the point that in many cases a change of plumage is certainly occasioned by a change of colour in the feather itself‡, quite independently of moulting.

The difficulty of finding specific characters for birds which shall be applicable to both sexes and all ages, particularly in those groups in which the changes of plumage above alluded to are most prevalent, has been severely felt by ornithologists. Mr. Macgillivray has considered this subject in a paper published in the 4th volume of the *Wernerian Memoirs*§. He has pointed out the insufficiency of some of those in common use, particularly such as are derived from colour. He thinks it would be possible to obtain others, from the situation, form, and position of the feathers, which would be more preferable, as being of general application and founded upon permanent and essential organs. Mr. Macgillivray has annexed, as examples, the characters of several species drawn up in this manner. His suggestions deserve to be considered, although it may be questioned whether such characters will be found "sufficiently diversified" to admit of being adopted in all cases.

3. *Reptilia*, Cuv.—The study of the animals belonging to this division of the *Vertebrata* is difficult, and has received far less attention from naturalists than that of either of the preceding classes. Hence we are at present but little advanced in the details of their natural arrangement. The propriety of separating off the *Amphibia*, and considering these last as a distinct class, is becoming every day more generally acknowledged. This separation was first proposed by Latreille || so long ago as in

* See more particularly Macgillivray in *Edinb. New Phil. Journ.* 1827.

† *Zool. Trans.*, vol. i. p. 13.

‡ This had been often suspected to be the case, (see Whitear in *Linn. Trans.*, vol. xii. p. 524, and Fleming in *Edinb. Phil. Journ.*, vol. ii. p. 271,) but never before ascertained by direct experiment.

§ p. 517. || *Nouv. Dict. d'Hist. Nat.*, 1st edition.

1804. It has not been adopted however by Cuvier, who divides the whole group into four orders, *Chéloniens*, *Sauriens*, *Ophidiens*, and *Batraciens*, being the same arrangement as that of Brongniart*. Blainville† follows Latreille in considering the *Reptilia* and *Amphibia* as distinct classes, but differs from all his predecessors in his subordinate groups. Attaching more importance to the organs of generation than those of locomotion, he has thought fit to unite the Saurians and Ophidians under the name of *Bispéniens*; at the same time detaching the Crocodiles to form a distinct order, which he calls *Emydosauriens*. The class *Amphibia* he divides into four orders, *Batraciens*, *Pseudosauriens* or Salamanders, *Subichthyens* (*Proteus*, *Siren*, &c.), and *Pseudophydiens* (*Cæcilia*). In 1820, Merrem published his arrangement of the *Amphibia*‡, under which name, although he includes both the above classes, he considers these as forming two divisions, which he calls *Pholidota* and *Batrachia* respectively. His *Pholidota* are distributed into three orders, which correspond with those of Blainville, but are called *Testudinata*, *Loricata*, and *Squamata*. The *Batrachia* include the three subordinate groups of *Apoda* (*Cæcilia*), *Salientia* (*Rana*, &c.), and *Gradientia* (*Triton*, *Proteus*, &c.). Mr. MacLeay§, adopting the *Amphibia* as a distinct class, would divide the true *Reptilia* into the five groups of *Chelonians*, *Emydosaurians*, *Saurians*, *Dipod Ophidians*, and *Apod Ophidians*. He considers the first and last of these groups as meeting in the *Emys longicollis*, thus causing the five to unite and form a circle. He looks upon the whole class as connected with that of *Aves* by means of the *Chelonians*. Latreille||, preserving the *Reptilia* and *Amphibia* as distinct classes, divides the former into the two sections of *Cataphracta* and *Squamosa*. His *Cataphracta* include Blainville's two orders of *Chelonians* and *Emydosaurians*. The *Squamosa*, answering to the *Bispéniens* of Blainville, comprise, as two other orders, his *Sauriens* and *Ophidians*. The *Amphibia* are divided into the two orders of *Caducibranchia* and *Perennibranchia*. In 1825, Mr. Gray published in the *Ann. of Phil.* an arrangement of the classes *Reptilia* and *Amphibia*, in conformity with MacLeay's principles. As his primary groups are slightly modified in a later treatise on these animals, to be alluded to presently, perhaps it is unnecessary to specify them particularly. In 1826, Fitz-

* *Essai d'une Classification naturelle des Reptiles.* Paris, 1805.

† *Principes*, &c., tab. 5.

‡ *Tentamen Systematis Amphibiorum.* Marpurg. 1820, 8vo. This work is, strictly speaking, a second edition of one published by the same author in 1800.

§ *Hor. Ent.*, p. 263.

|| *Fam. Nat.*

inger published a new classification of these animals* founded upon their natural affinities. He considers the *Reptilia* and *Amphibia* of Latreille in the light of orders only, to which he affixes the names of *Monopnoa* (Reptiles breathing all their life by lungs only), and *Dipnoa* (breathing by lungs and gills at the same time). It will be seen that these two groups correspond to the *Pholidota* and *Batrachia* of Merrem. The *Monopnoa* he divides into four tribes, the first three being the same as Merrem's orders, with the same names; the fourth, called *Nuda*, embracing the single family of *Cæciliæ*. The *Dipnoa* he separates into the two tribes of *Mutabilia* and *Immutabilia*, the former comprising those *Amphibia* which do, and the latter those which do not, undergo metamorphosis. In the *Nov. Act. &c. Nat. Cur.* for 1828, Dr. Ritgen has published an arrangement of the *Amphibia* in which he admits but three orders, answering to the *Ophidia*, *Chelonia*, and *Sauria* of other authors. This last, however, is made to include the *Batrachia* as well as the true *Saurians*. He has selected for most of his groups new terms, which from their great length will never be adopted generally. Wagler† has very much augmented the orders of this class, in like manner as he has done those of the *Mammalia* and Birds. He adopts eight: *Testudines*, *Crocodili*, *Lacertæ*, *Serpentes*, *Angues* (comprising the genera *Acontias*, *Chirotes*, *Chalcides*, and *Amphisbæna* of the *Règne Anim.*), *Cæciliæ*, *Ranæ*, and *Ichthyodi* (*Subichthyens* of Blainville). In 1831, Mr. Gray published his *Synopsis Reptilium*, of which only the first part has as yet appeared, comprising the *Cataphracta* of Latreille, whose arrangement is for the most part adopted, with the exception of a new order instituted for the reception of the *Ophiosauri*, the second division of Latreille's order *Saurii*. C. L. Bonaparte, in his *Saggio di una Distribuzione*, &c., published the same year, adopts the term *Amphibia* as a general name for the whole group of which we are treating. These he divides into the two subclasses of *Reptilia* and *Batrachia*, which are again divided into sections, the former into four, and the latter into two, before arriving at the orders. Thus we have Sect. 1. *Testudinata*, comprising the single order of *Chelonii*; Sect. 2. *Loricata*, comprising the two orders of *Enaliosaurii* (*Ichthyosaurus and Plesiosaurus*,) and *Emydosaurii*, Blainv.; Sect. 3. *Squamata*, comprising the three orders of *Saurii*, *Saurophidii* (*Amphisbæna*), and *Ophidii*; Sect. 4. *Nuda*, comprising the single order of *Batrachophidii* (*Cæcilia*). In the subclass *Batrachia*, we have Sect. 1. *Mutabilia*, comprising the order *Ca-*

* *Neue Classification der Reptilien*, &c. 4to, Vienn. 1826.
† *Natürliches System*, &c.

ducibranchia; and Sect. 2. *Amphipneusta* (*Immutabilia*), comprising the two orders of *Cryptobranchia* and *Perennibranchia*. The most recent work in this department is that by Duméril and Bibron*, of which only the first volume has appeared hitherto, containing remarks on the organization of Reptiles in general, and the Chelonians in particular. There is also a very complete Bibliography with reference to this branch of zoology. The systematic portion of the work is not yet entered upon. The authors, however, have it in view to adopt the same orders as those of Cuvier.

The above are the principal authors who have treated of this class as a whole, but some of its orders have received the particular attention of different naturalists, and derived much illustration from their researches. No one has contributed so much to our knowledge of the Chelonian Reptiles as Mr. Thomas Bell. Several memoirs from him on these animals are to be found in the *Linnæan Transactions* and *Zool. Journal*, amongst which I may more particularly mention his "Monograph of the Tortoises having a moveable Sternum" published in 1825†, and his "Characters of the Order, Families, and Genera of the *Testudinata*" published in 1828‡. More recently (1833) Mr. Bell has commenced a splendid work§ on this order, in which it is intended to describe and figure all the known species, arranged according to their affinities. Seven parts have already appeared, which for beauty and accuracy of illustration it is impossible to surpass. Before quitting this group I may just allude to a paper in the *Ann. des Sci.* for 1828‖, by MM. Isid. Geoffroy St. Hilaire and J. G. Martin, on some parts of the internal organization of these Reptiles. Being purely anatomical, I should not have noticed it, did it not contain the statement of a curious fact respecting the affinity well known to exist between the *Chelonia* and the *Monotremata*. It is observed, that with regard to the urinary apparatus, the analogy between the *Ornithorhynchus* and the *Testudo Indica* is even much greater than that which is found between this last species and many other reptiles belonging to the same order.

The *Emydosauria* were closely investigated by Cuvier and Geoffroy, by the former more especially, in the early part of the present century, and since their researches¶, I am not

* *Erpétologie générale, ou Histoire Naturelle Complète des Reptiles*, tom. i. Paris, 1834.

† *Zool. Journ.*, vol. ii. p. 299. ‡ *Zool. Journ.*, vol. iii. p. 513.

§ *Monograph of the Testudinata*. fol. 1833, &c. ‖ tom. xiii. p. 153.

¶ See the earlier volumes of the *Ann. du Mus.*, more particularly vol. x., containing a valuable memoir by Cuvier on the different species of living *Crocodiles*, and their distinctive characters. For the structure of these animals, see his *Ossemens Fossiles*.

aware that much addition has been made to our knowledge of this group. Nevertheless there is great need of further examination in order to determine the value of those characters which have been hitherto employed in distinguishing the species. It may reasonably be questioned whether these have not been overmultiplied, from placing too great reliance upon slight differences in the form and number of the nuchal, cervical, and dorsal plates. I may mention a memoir by Geoffroy on the *Gavials*, as more recent than his others, published in 1825*, in which he has treated largely of their organization and affinities. He considers the former as offering sufficient peculiarities to warrant the establishing of a distinct genus of this group, which Cuvier regarded as merely a subgenus of *Crocodilus*.

The *Saurian* Reptiles have been much attended to by Mr. Gray. In the *Ann. of Phil.* for 1827†, he has given a synopsis of the genera belonging to this group. In a subsequent paper published in the same volume‡ he has made some additions and corrections to his first communication. He has made it a particular object to revise the species of *Chamæleon*. To M. Milne Edwards we are indebted for a paper in the *Ann. des Sci.* for 1829§, which though relating only to the restricted genus *Lacerta*, may be found valuable in a general point of view from the remarks which it contains on the zoological characters of this group. Those who have studied these reptiles know what difficulty attends the discrimination of species. Milne Edwards has sought to remove this difficulty. He has ascertained that in this genus the best distinguishing characters are derived from the different kinds of scales, more especially the large squamous plates which cover the upper part of the head. He particularly dwells on the *relative size of the occipital and parietal plates*, and the forms of the scales between the eye and the ear‖. He does not place much reliance on the character derived from the number of femoral pores, which he finds often varying in the same species, although considered as constant by Merrem and Blainville. In the same volume with the above memoir is one by M. Duges, treating partly of the same subject; and it is satisfactory to find that he confirms what Edwards says respecting the characters of the scales. It may be observed that Wagler appears to have derived much assistance from the teeth in characterizing both the *Emydosaurian* and *Saurian* Reptiles. In one portion of his work he has treated of this subject in great

* *Mém. du Mus.*, tom. xii.

† vol. ii. N.S. p. 54.

‡ p. 207.

§ tom. xvi. p. 50.

‖ Merrem and others had previously availed themselves of these characters, but according to Edwards, they have not made a judicious use of them, or selected those scales on which any reliance can be placed.

detail, and given minute descriptions of the teeth as they occur in all the different genera in the above two orders.

Among the *Ophidia* more perhaps remains to be done than in any other order of Reptiles. Many new genera and species have been discovered of late years, and described by different authors; but of several the characters and synonyms are ill determined, and their affinities still more so. Mayer has proposed a new arrangement of this group*, founded on the presence or absence of rudimentary posterior extremities, which he has succeeded in detecting in many genera in which they were not before known to exist. He would adopt as three subordinate divisions: 1. *Phænopoda*, in which these extremities are externally visible; 2. *Cryptopoda*, in which they are entirely concealed beneath the skin; 3. *Chondropoda*, in which the rudimental feet are reduced to mere cartilaginous slips, and *Apoda*, in which they are entirely wanting. M. Duvernoy, in the *Ann. des Scien.* for 1832†, has entered upon the consideration of the anatomical characters which serve to distinguish the venomous from the innocuous serpents. As these groups are kept distinct by Cuvier, as well as by some others, in their systematic arrangement of the *Ophidia*, such researches may prove serviceable to the zoologist in helping him to the true situation of some genera. In a later volume of the same work‡, M. Duvernoy has followed up this inquiry, as well as treated of some other parts of the internal organization of serpents in general. On the subject of the alimentary canal, he particularly observes that it offers sensible differences in different genera, and such as may serve to confirm or lessen the propriety of adopting some of those which have been established by naturalists. In this last communication, he has also made some remarks§ on the forms and arrangement of the scales on the head and body considered as zoological characters. He thinks that such characters require to be compared afresh with those derived from the internal structure, in order that their true value may be more correctly ascertained. The genus *Cæcilia*, which by some has been associated with the true Reptiles, by others with the *Amphibia*, has been recently discovered by M. Müller‖ to possess gills in the very young state, which fact seems to corroborate its claims to a place in the class last mentioned.

The structure of the *Amphibia* has been much studied of late years, and has given rise to many excellent memoirs on the part of different observers. As these, however, are for the most part

* *Nova Acta Acad. Nat. Cur.*, tom. xii. p. 819. † tom. xxvi. p. 113.
‡ tom. xxx. pp. 5 and 113. § p. 25.
‖ *Ann. des Scien.* 1832, tom. xxv. p. 89.

purely anatomical, it would be out of place to dwell on them in this Report. Yet we may allude to one as of more importance to zoology than some others. I refer to Dr. Davy's discovery of a second auricle in the heart of these animals, which will lead us to correct what was always considered as one of their distinguishing characters, viz., their having a single heart like Fishes *. The *Perennibranchiate Amphibia* received some time back much illustration from Cuvier, whose researches on this subject will be found in the first volume of Humboldt's Comparative Anatomy† He was led to regard the *Siren* and *Proteus* as adult animals, but suspected the *Axolotl* to be a larva. In 1819, MM. Configliachi and Rusconi published a valuable monograph ‡ on the *Proteus anguinus*, containing a full account of the structure and natural history of this singular animal. Dr. Rusconi is the author of another work, published in 1821 §, in which he has treated of the aquatic Salamanders, detailing some interesting observations respecting the mode of development of these Reptiles. This subject had not been previously followed up with so much closeness of research. It may be stated that in this last work Dr. Rusconi has doubted the accuracy of Cuvier's views respecting the *Siren* being an adult animal. Cuvier has reconsidered the subject in his *Ossemens Fossiles* ‖ ; but still adheres to his former opinion on this point. From examining the osteology of this reptile, he feels satisfied that it never acquires hind feet, as Rusconi supposes, and deems it very improbable that it ever changes its form or loses its branchiæ. That the Siren is not the larva state of the *Amphiuma* of Garden, as some imagine, Cuvier has endeavoured to prove in a memoir upon this last genus published in the *Mém. du Mus.* ¶ for 1827.

* *Edinb. New Phil. Journ.* 1828, p. 160. Dr. Davy's researches went no further than to show the existence of a second auricle in several species of the genus *Rana*; but reasoning from analogy, he thought it probable that the same would be the case in all the other genera of this group. These suspicions have been since partly confirmed by Mr. Owen, (*Proceed. of Zool. Soc.* 1834, p. 31,) who has lately given the results of an examination of the hearts of several genera of the *Perennibranchiate Amphibia*, in all of which he finds it consisting of three distinct cavities, as in the higher *Reptilia.*

† "Recherches anatomiques sur les Reptiles regardés encore comme douteux par les Naturalistes; faites à l'occasion de l'Axolotl, par M. Cuvier."—*Humb. Anat. Comp.*, tom. i. p. 93—126.

‡ *Del Proteo Anguino di Laurenti Monografia.* Pavia, 1819. An excellent analysis of this work will be found in the *Edinb. Phil. Journ.* for 1821, vols. iv. and v.

§ *Amours des Salamandres aquatiques, et Développement du Tetard de ces Salamandres, depuis l'Œuf jusqu'à l'Animal parfait.* Milan, 1821. An analysis of this work also will be found in the *Edinb. Phil. Journ.* for 1823, vol. ix.

‖ tom. v. Pt. II. p. 418, &c. ¶ tom. xiv. p. 1.

4. *Pisces.*—It is generally allowed that this class is connected by close affinity with those *Batrachian Reptiles* which have permanent gills. That it also leads back to the *Mammalia* by means of the viviparous sharks, which approach the cetaceous animals, will scarcely be doubted by any who have considered the relative organizations of these last groups. Yet no one appears to have thought of placing the Fish between the *Mammalia* and *Amphibia* before Mr. MacLeay, whose circular arrangement of the classes of *Vertebrata* is certainly the only one yet given that conforms itself to nature. As a class, the Fish have received but comparatively little attention from naturalists; and from the time of the appearance of the first edition of the *Règne Animal* of Cuvier, to that of the *Hist. Nat. des Poissons* by the same illustrious author, but few attempts have been made by other individuals to elucidate their true affinities. Several works of great merit, descriptive of the fish of different countries have appeared, and many detached memoirs on particular genera and species, but no work of a regular systematic character since that of Lacépede.

Cuvier's system, as developed in the first edition of the *Règne Animal*, is very different from that of Lacépede, which he objects to as having all the secondary groups established upon characters drawn from the presence or absence of the opercle and branchiostegous rays, which Cuvier observes will often lead to glaring violations of natural affinity, not to mention the circumstance that in many instances Lacépede has assumed these parts to be wanting where they are really present. Cuvier adopts as primary divisions the two groups of *Cartilaginous* and *Osseous* Fishes, commencing with the former, which he divides into the two orders of *Chondroptérygiens à branchies fixes* and *Chondroptérygiens à branchies libres*. The osseous fishes he divides into six orders. The first of these, *Plectognathes*, is characterized by a peculiar mode of articulation of the jaws, and comprises some of the genera before included in the old order of *Branchiostegi*, which is here abolished. The second, *Lophobranches*, is founded upon a peculiar form of the gills, and includes but the two genera *Syngnathus* and *Pegasus* of Linnæus. The remaining orders comprise the *Malacopterygii* and *Acanthopterygii* of Artedi, the former group being divided into three orders according to the position of the ventrals, the latter kept entire as one order.

Blainville's arrangement of this class* does not differ materially from that of Gmelin, excepting that the leading groups have new names affixed to them, and new distinguishing characters.

* *Principes, &c.*, tab. 6.

Thus, the osseous fishes he terms *Poissons Gnathodontes*, from having their teeth implanted in the jaws, in contradistinction to the cartilaginous fishes, which he calls *Dermodontes*, from the teeth in this group adhering simply to the skin. In like manner he calls the *Branchiostegi* of Gmelin by the name of *Hétérodermes*, or fish in which the structure of the skin is variable in its nature, as opposed to the ordinary fish, which he terms *Squamodermes*. The subordinate groups are established upon the presence or absence, and on the position (either jugular, thoracic, or abdominal,) of the ventrals, leading in too many instances to unnatural combinations as well as separations.

Latreille in his *Familles Naturelles* considers the cartilaginous and osseous fishes as forming two distinct classes in his great division of *Solibranchia*, which he terms *Ichthyodera* and *Pisces* respectively. He removes, however, the chondropterygious fishes with free gills into the latter class, which is primarily divided into the two groups of *Anomalia* and *Normalia*. The *Anomalia* comprise, besides the *Sturionii* of Cuvier, his two orders *Plectognathes* and *Lophobranches*. The *Normalia* include the remaining orders of that naturalist, arranged however somewhat differently from what they are in the *Règne Animal*. In the order *Acanthoptérygiens*, before arriving at the families, he adopts a primary division into the two sections *Kystophora* and *Akystica*, characterized respectively by the presence or absence of a swimming bladder.

Risso, in the 3rd volume of his *Hist. Nat. de l'Eur. Mérid.*, published in 1826, has given an arrangement of this class according to his own views. His orders, however, are nearly the same as those of Gmelin. He only substitutes the orders *Plectognathes* and *Lophobranches* of Cuvier for the *Branchiostegi* of the former author.

Besides the above, I am not acquainted with any systematic arrangements of this class, deserving notice, prior to that of the *Hist. Nat. des Poissons* by MM. Cuvier and Valenciennes. In this work, of which the first volume appeared in 1828, the leading groups remain the same as in the *Règne Animal*. The details of the arrangement are however slightly modified *. And un-

* One alteration consists in the commencing with the *osseous*, instead of the *cartilaginous* fishes. Cuvier observes, however, with reference to this point, that, strictly speaking, these groups form two parallel series, neither being superior or inferior to the other. See *Hist. Nat. des Poiss.*, tom. i. p. 419, and *Règne Animal* (second edit.), tom. ii. p. 376.—Latreille seems to consider the Fish as forming two series, which, after a time, unite and become one. His arrangement of the groups in these parallel lines is, however, different from Cuvier's. See *Fam. Nat.* p. 115, note (1).

doubtedly much, very much remains still to be done before we can consider these details as not susceptible of any further improvement. Cuvier's groups are on the whole natural and well characterized; but the true position of many of them is extremely doubtful *, and their relative value as yet undetermined. He has done much, however, towards determining the value of certain characters which had been considered in very different points of view by former ichthyologists, especially that derived from the structure of the dorsal rays. He states it to be his firm opinion, deduced from a careful study of the entire organization in several hundred species, that the acanthopterygious fishes ought to be kept quite distinct from the others, and that whatever characters may be resorted to for the further subdivision of the normal fishes, they must be held subordinate to the one above mentioned. It is mainly in consequence of having attached too little importance to this character, and set too high a value upon that derived from the position of the ventrals, that Linnæus and several of the more recent authors have entirely failed in their attempts at a natural arrangement of this class. No one has made better use than Cuvier of the characters derived from the structure of the jaws †, and the nature and position of the teeth; and perhaps in certain groups we can hardly select any of more importance. For the teeth he has adopted a peculiar set of terms, expressive of the different forms which they assume. These terms are, however, better adapted to the French than to the English language. On the whole, it may be observed, that although there may be some parts of his arrangement found defective, Cuvier has done more for this department of zoology than any one else. His *Histoire des Poissons* must ever be considered as forming a real epoch in ichthyology. If we look to the profound erudition it displays, the thorough knowledge of its author with respect to all that had been done by previous writers on this class, the close and accurate researches which he has made into every part of the internal as well as external organization of the subjects of which he treats, the minuteness of detail which characterizes the description of species, at the

* It is more than probable that Cuvier has in some instances mistaken relations of *analogy* for those of *affinity*. One such instance has been pointed out by Mr. Bennett (see *Zool. Journ.*, vol. iii. p. 372,) in the case of the genus *Lophius*.

† Cuvier first called the attention of naturalists to this part in a memoir published in 1815, in the first volume of the *Mém. du Muséum* (p. 102.). One of the conclusions at which he arrives is, that the characters derived from the pieces of the upper jaw and palatine arch, their various positions, proportions, &c., serve to indicate *genera*, but cannot be employed in distinguishing *orders*, if we wish to preserve natural affinities.

same time that every attempt is made towards generalization, it will be thought impossible to speak too highly of its merits. It is almost a perfect model for works of this nature, and deserves to be consulted by all naturalists engaged in similar undertakings. It cannot but be a subject of deep regret that its talented author has not lived to complete a work, for which he tells us he had been forty years collecting materials. Let us hope, however, that this may be yet effected by M. Valenciennes, whom, fortunately for the scientific world, M. Cuvier had from the beginning engaged as his coadjutor.

A slight modification of Cuvier's arrangement appears in the *Saggio di una Distribuzione, &c.*, of C. L. Bonaparte, published in 1831, principally as regards the value of some of the groups. In the first place, the osseous and cartilaginous fishes are considered as two subclasses. The former are then primarily divided into three sections: 1. PECTINIBRANCHII, comprising the two orders *Acanthopterygii* and *Malacopterygii*; 2. LOPHOBRANCHII, including the single order of *Osteodermi* (*Syngnathus*); and 3. PLECTOGNATHI, comprising the two orders *Gymnodontes* and *Sclerodermi*. The *Malacopterygii* are subdivided into the three tribes of *Abdominales*, *Subbrachiani*, and *Apodes*. Thus we have two of Cuvier's orders raised to a higher rank than that which he assigned to them; while on the other hand there are three lowered to a subordinate denomination. In like manner we have the cartilaginous fishes divided into the two sections of CHISMOPNEI and TREMATOPNEI: the former comprising the two orders of *Eleutheropomi* (*Sturiones*) and *Acanthorrhini* (*Chimæræ*), the latter those of *Plagiostomi* and *Cyclostomi*. A similar alteration in the value of some of Cuvier's groups will be found here.

The most recent work on ichthyology, and one of the most important which has yet appeared, is that by M. Agassiz, now in course of publication*. Although the object of its author is more particularly to illustrate the fossil fishes, it is his intention to bring forward an entirely new classification of fish in general. The details of his arrangement are not yet published. He has, however, put forth a slight sketch of his system, such as will serve to show the striking changes which he contemplates in

* *Recherches sur les Poissons Fossiles, par Louis Agassiz*, 1833, &c. Only two numbers have hitherto appeared.—M. Agassiz was before known to ichthyologists from having assisted Spix in the description of his Fishes of Brazil. This work was published in 1829 under the following title: *Selecta Genera et Species Piscium quos in Itinere collegit Spix; descripsit L. Agassiz.* fol. In 1830, M. Agassiz had also announced the prospectus of a work on the *Freshwater Fishes of Europe*. This last has, however, not yet appeared.

this department of zoology. Thus, he adopts but four orders, in each of which are to be found both osseous and cartilaginous fishes,—both Acanthopterygians and Malacopterygians,—both apod and abdominal genera,—and, in two out of the four, thoracic and jugular genera besides. Hence it will be seen that his principal divisions are founded neither on the degree of ossification of the skeleton, nor on the structure of the vertical fins, nor on the position of the ventrals, as is the case in those systems which have been hitherto most generally adopted. M. Agassiz thinks he finds in the *differences of the scales* the most exact indication of the natural affinities of all fish. Accordingly it is from the scales that he has drawn the diagnostic characters of his four orders, (which bear respectively the names of *Placoides*, *Ganoides*, *Cténoides*, and *Cycloides*,) although in forming them he has kept in view all the rest of the organization. Ichthyologists will doubtless be impatient to see the full development of a system founded upon views so entirely opposed to all those which they had previously entertained on the subject.

The science of ichthyology has been so little cultivated, that there are but few individuals to whom it is necessary to refer in this Report, besides those who have been already mentioned. Many have made great contributions to the anatomy of fish, amongst whom Geoffroy St. Hilaire stands preeminent; but I allude to such only as have thrown light upon the affinities of the larger groups, or helped us to a better knowledge of their zoological characters. I must not, however, omit to mention an important paper on the fishes of the Lake of Geneva by M. Jurine, published in 1825, in the third volume of the *Mém. de la Soc. de Phys. et d'Hist. Nat. de Genève.* It is not merely valuable as a local catalogue, but as containing several new characters for distinguishing the species of *Cyprinidæ*, which is perhaps one of the worst understood families in the whole class *. This memoir is accompanied by remarkably accurate figures of all the species found in the above locality. The scales of fish were, some years back, particularly studied by M. Kuntzmann, whose memoir † on this subject will have acquired fresh interest since naturalists have had their attention again directed to it by M. Agassiz. M. Kuntzmann has not only entered into considerable details with respect to the structure of these organs in

* Cuvier has somewhere observed that in general the freshwater fishes, at least those of foreign countries, are much less known and understood than those found on the coasts.

† *Verhandlung der Gesell. Nat. Freunde in Berlin*, vol. i. No. 5, 1824, p. 269. I am, however, only acquainted with the analysis of it in *Bull. des Sci. Nat.* 1826, tom. vii. p. 118.

different groups, but considered their value as furnishing characters for distinguishing species. He seems to think, that in general, if not in all cases, they are available for this purpose, and advises that selection be made of those which are placed on the middle of the sides of the body, and near the lateral line, not only as being the largest, but as those in which the form is most constant in a given species. M. Kuntzmann has instituted several divisions or classes amongst scales, in which they are arranged according to their form and structure. It would, however, occupy too much room to follow him in this part of his subject. Before quitting this class, I may just allude to two papers by Dr. Hancock, in the *London Quarterly Journal of Science* for 1830 *, in which he has made some remarks on the composition of the fin rays in fishes. Dr. Hancock has dwelt much upon the importance of the character derived from the *number* of these rays, which he considers as offering the best diagnostic marks for the discrimination of species. This character, however, must be employed with some limitation, since it will be found much more variable in some groups than others.

II. Annulosa, *Cuv.*

Cuvier, in his *Règne Animal*, places this division below that of the *Mollusca*, which last he appears to have regarded as standing higher in the scale of organization on account of its circulatory system. Geoffroy, guided by his peculiar views respecting the vertebral structure of the *Annulosa*, to which allusion has been already made, has disputed the propriety of this arrangement †, and considers that the *Mollusca* should decidedly give precedence. It is obvious, however, that these two groups are formed upon such entirely different plans, that they scarcely admit of direct comparison in this respect. Each has its own peculiar marks of affinity with the higher animals; and it is only by supposing two points of departure from the *Vertebrata*, and arranging the *Invertebrata* in a double series, that we shall present a system at all conformable with nature. This double route, indeed, was long since pointed out by Lamarck ‡, and subsequently by Latreille §, MacLeay ||, and Blainville ¶. Latreille has reconsidered the subject in his latest work, the *Cours d'Entomologie*, published in 1831. He there supposes ** the *In-*

* pp. 136, 287.

† See his *Cours de l'Hist. Nat. des Mammif.*, Leçons 2 & 3.

‡ *Hist. Nat. des An. sans Vert.*, tom. i. p. 457.

§ In a memoir published in 1820 under the title of *Passage des Animaux Invertébrés aux Vertébrés.* 8vo.

|| *Horæ Entom.* p. 206, and elsewhere.

¶ *Principes d'Anat. Comp.*, tab. 2.

** p. 15.

vertebrata to be arranged on two lines, one occupied by the *Crustacea*, *Arachnida*, and *Insecta*, the other by the *Mollusca* and *Zoophyta:* he then supposes a lateral branch from the *Mollusca* to the *Crustacea*, passing successively through the *Cirripeda*, *Annelida*, and *Entozoa*, the connecting link at this end of the ramification being found in the *Lernææ* of Linnæus. That this arrangement is, however, not quite correct, is rendered probable by discoveries connected with the *Cirripeda* to be hereafter spoken of, and by the indisputable affinity between the *Annelida* and *Cyclostomous Fishes*, which affinity points to the former group as being necessarily at the head of one series, and therefore not forming part of any lateral ramification *.

The following are the classes considered by Cuvier as belonging to the Annulose type: *Annelida*, *Crustacea*, *Arachnida*, and *Insecta*.

Mr. MacLeay adopts five classes † independently of the *Annelida*, which he regards as an osculant group connecting the vertebrate and annulose animals. Two of these are the *Crustacea* and *Arachnida* of Cuvier. Two others are formed out of the old class *Insecta*, and are the same as Clairville's groups of *Mandibulata* and *Haustellata*. The fifth, to which the name of *Ametabola* is given, includes the *Myriapoda* and *Thysanura*

* Latreille seems to consider, as he had done in his original memoir on this subject, that the *Crustacea* are the most perfect of the articulated animals, and that therefore they necessarily approach nearest to the *Vertebrata*. Mr. MacLeay has controverted both these points. He maintains that *Insects* are more highly organized than *Crustacea*. Furthermore he observes, that so far from its being by the *most* perfect, it is by the *least* perfect group in the series that we might naturally expect to find a passage to the *Vertebrata*. Every vertebrate animal would seem to "have been constructed with reference to one type, and every annulose with reference to another; and as the former is more imperfect in its organization according as it approaches the annulose structure, so the latter is more imperfect in proportion as it possesses a distinct system of circulation and other characteristics of the *Vertebrata*. It thus follows that the animals which connect them ought to be extremely imperfect in their organization." Such animals are the *Cyclostomous Fishes* on the one hand, and the *Annelida* on the other, the striking affinity between which groups has been noticed both by Lamarck and Cuvier. See *Hor. Ent.*, p. 292, &c., for a further development of Mr. MacLeay's reasoning. It cannot be doubted that his views on this last point are correct. They likewise fall in with those of Straus-Durckheim, who has touched on the same subject in the Introduction to his valuable work on the structure of the Articulated Animals (*Consid. Gén. sur l'Anat. Comp. des An. Artic.*, pp. 13, 15, 20.) published in 1828. Geoffroy, however, agrees with Latreille in thinking that the *Crustacea* should follow immediately after the Fish. See *Mém. du Mus.*, tom. xvi. p. 2; also *Cours de l'Hist. Nat. des Mammif.*, Leç. 3, p. 18. Robineau Desvoidy entertains the same opinion. *Recherches, &c.*, p. 78.

† *Hor. Ent.*, pp. 288 and 390.

of Latreille, the *Anoplura* of Dr. Leach, and a portion of the *Entozoa* of Rudolphi. Mr. MacLeay has endeavoured to show that these five groups unite to form a circle.

In Blainville's *Principes*, &c., we find the *Annulosa* forming his third type, *Entomozoaires* *, which he divides into eight classes, characterized according to the presence or absence, and when present the number or nature, of the appendages for locomotion. The *Annulosa* with articulated feet he distributes under the six classes *Hexapodes*, *Octopodes*, *Décapodes*, *Hétéropodes*, *Tétradécapodes*, and *Myriapodes*; the first including the true Insects, the second the *Arachnida* of Cuvier, the third, fourth, and fifth the *Crustacea* of that author. The inarticulated *Annulosa*, comprising the *Annelida* of Cuvier, form his seventh and eighth classes, called *Chétopodes* (with setiform appendages,) and *Apodes* (deprived of appendages altogether). The last of these two includes also some of the *Entozoa*. Few will probably be disposed to adopt this arrangement, which leads to divisions of very unequal value.

In the *Familles Naturelles* of Latreille, the *Annulosa* (or, as they are there termed, *Condylopa*,) are primarily divided into the two sections of *Hyperhexapi* and *Hexapoda*, according as the feet are more than six, or six only, in the adult state, the former term being adopted from Savigny. The *Hyperhexapi* include the three classes *Crustacea*, *Arachnida*, and *Myriapoda*, this last being adopted from Dr. Leach, who first instituted it in a paper read to the Linnæan Society in 1814 †. The *Hexapoda* comprise the single class of *Insecta*. The *Annelida* are referred by Latreille to a different branch of his arrangement of the Animal Kingdom.

Straus-Durckheim, in his *Consid. Génér. sur l'Anat. Comp. des Anim. Artic.* published in 1828, considers the articulated animals as including the five classes *Annelida*, *Myriapoda*, *Insecta*, *Crustacea*, and *Arachnida*. To the end of the Introduction of his work he has annexed two synoptic tables, in which he has represented what he conceives to be the true chain of affinities connecting these classes, and the principal groups contained in them. It would, however, occupy too much room to follow him in these details.

In the *Cours d'Entomologie*, published in 1831, Latreille has adopted the same divisions as in the *Fam. Nat.* He only substitutes the name of *Apiropoda* for that of *Hyperhexapi*.

I shall now proceed to consider the progress and state of each

* Tab. 7. † *Linn. Trans.*, vol. xi. p. 306.

of the classes referred by Cuvier to this type of structure separately. To these I shall add Dr. Leach's class of *Myriapoda*.

1. *Annelida*.—This class was established by Cuvier in 1802. Lamarck, who adopted it from him, was, however, the first to assign to it its present name. The animals which it includes, although possessing great interest from the circumstance of their forming the passage from the Annulose to the Vertebrate type, have been comparatively but little studied, and have received the attention of only a few naturalists. It is principally to Cuvier, Savigny, Blainville, and to the more recent researches of Audouin and Edwards, that we are indebted for what knowledge we have respecting them as a class. Cuvier more especially examined their internal organization. His arrangement, in both editions of the *Règne Animal*, is grounded upon the respiratory organs, which furnish him with the characters of three groups, which he terms orders: (1.) *Tubicoles*, in which the branchiæ are in the form of tufts attached to the head or anterior part of the body, generally inhabiting shelly tubes; (2.) *Dorsibranches*, in which they are arranged down the back or along the sides of the body; and (3.) *Abranches*, in which there are no distinct branchiæ visible. Savigny, whose valuable memoirs on these animals * are inserted in the great French work on Egypt, paid more attention to their external structure. He particularly studied the conformation of those elastic and often brilliant metallic-like *setæ*, which in a great number of genera serve as organs of motion. He also entered into a detailed examination of the jaws, antennæ, branchiæ, and the membranaceous appendages attached to the several articulations. His arrangement of this class is very different from Cuvier's. He divides it into five orders: (1.) *Néréidées*, comprising such genera as have retractile feet furnished with setæ, a distinct head, and a mouth in the form of a proboscis, generally armed with jaws; (2.) *Serpulées*, in which there are also feet furnished with setæ, some of these being hooked, but no distinct head; (3.) *Lombricines*, without feet or distinct head, but nevertheless furnished with small setæ; (4.) *Hirudinées*, without distinct head, feet, or setæ, but with a mouth in the form of a sucker; (5.) The last order, of which he has not treated, he has left without a name. The result of Blainville's researches into the structure of these animals, which form his class *Chétopodes*, will be found in the

* *Recherches pour servir à la Classification des Annelides;* and *Tableau systématique de la Classe des Annelides.* The first of these memoirs was presented to the Royal Academy of Sciences in 1817. An analysis of them both will be found in Latreille's Report, published in the *Mém. du Muséum*, tom. vi. p. 93.

Bull. de la Soc. Phil. for 1818. He divides them into three orders, the characters of which are drawn from the similarity or dissimilarity of the segments of the body with relation to the appendages, and the more or less marked separation of these segments into head, thorax, and abdomen. It is not necessary to give the names of his orders, as he has changed them in a more recent dissertation on these animals published in the *Dict. des Scien. Nat.* *, and to which I refer the reader for a full development of his views respecting their organization and arrangement.

The memoirs of MM. Audouin and Edwards on the *Annelida*, which are the most recent, and at the same time the most valuable that have yet appeared, are contained in the *Annales des Sciences* for 1832-33. These acute observers have not only discovered a vast many new species, but found some exhibiting such peculiar characters, as render it necessary to institute several new groups, and to remodel entirely the classification of former authors. They remark that the system of Cuvier, although adapted to the small number of species then known, cannot be employed for the arrangement of many which have been since discovered, without entailing violations of natural affinity. In fact, they find that the presence or absence of the appendages termed *branchiæ* does not by any means constantly coincide with the true characteristic marks of the different types of organization presented by these animals, and that more than one instance might be adduced of species presenting these two modifications of structure, yet identical in all other respects, and indisputably belonging to the same family, if not to the same genus. The systems of Savigny and Blainville they state to be attended by similar difficulties. What they propose is, instead of confining their attention to the *branchiæ* only as the basis of their classification, to take into account the different membranaceous appendages in general, the consideration of which will lead to more natural divisions. It would seem indeed from their researches, that although the branchiæ are occasionally much developed, so that it is impossible to mistake their function, or to confound them with the cirri and tentacula, yet in other cases respiration is carried on by some of the other membranaceous appendages, all of which take up this function by turns in different cases. Hence by considering these organs collectively, and attaching the same value to all of them, we shall obtain characters of the first importance for the classification of the *Annelida*. It is accordingly from these organs, which the

* tom. lvii., Art. VERS. Also published separately under the title of *Manuel d'Helminthologie.*

authors term *the soft appendages of the body*, that they derive the characters of their four primary divisions or orders, to which they attach the names of *Annelides errantes*, *Annelides tubicoles ou sedentaires*, *Annelides terricoles*, and *Annelides suceuses*. Audouin and Edwards have paid particular attention to the external organization of the *Annelida*, and have made some interesting discoveries with respect to the structure and use of the *setæ* with which the feet are provided in the animals of their first division, being those in which the organization is most complex. They have observed that these setæ are not mere ornaments or organs of motion, but offensive weapons of a very particular structure, and such as can only be compared to the stings of bees or the spines of certain fish. Savigny had noticed that they were in general capable of being exserted from the body and retracted at pleasure, but does not appear to have entered so deeply into the details of their structure as these authors. MM. Audouin and Edwards have submitted them to a close and microscopic examination, and have ascertained, that while some are simple, assuming a great variety of different forms, others are compound, always consisting of two parts, united by an articulation, which gives way when the seta is employed for offensive purposes, leaving the apical portion in the body of the animal attacked.

From giving a detailed account of the external organization of the *Annelida* in general, MM. Audouin and Edwards proceed to the subordinate groups. So far as they have hitherto advanced in the subject, they have described at length the characters of all the families and genera; but in regard to species, of those only found on the coasts of France. To give any further analysis of their labours would, however, be inconsistent with the limits to which this Report must be restricted. It is, moreover, necessary that we should proceed to notice several individuals who, though they have not written on this class as a whole, have thrown great light upon some particular parts of it.

The *Hirudinidæ* especially have received more general attention than any other group, which is doubtless owing to the valuable services of these animals in medicine. Dr. Rawlins Johnson is the author of two memoirs in the *Philosophical Transactions* for 1817, in one of which he has detailed some interesting observations with respect to the habits and mode of propagation of the *Hirudo vulgaris*; in the other he has instituted the genus *Glossopora* * for those species in which the mouth is furnished with a projectile tubular tongue, including the *H. com-*

* The same as the genus *Clepsine* of Savigny.

planata and *H. stagnalis* of authors, and some others. Dr. Johnson has also written two treatises on the *Medicinal Leech*, to the last of which is appended a reprint of the memoirs above alluded to. In the *Turin Memoirs* for 1820*, Professor Carena has published a complete monograph of the genus *Hirudo*, in which, notwithstanding the labours of Savigny, who paid great attention to this family, he has described several new species, besides elucidating the history and synonyms of others known before. A supplement to Carena's monograph will be found in the twenty-eighth volume of the same Memoirs. In the *Ann. des Scien.* for 1825 †, M. Rayer has published some interesting observations with respect to the capsules and ova of several species of *Hirudo*, and the gradual development of the young. In 1827 appeared nearly at the same time two other valuable monographs on this family, one by Moquin-Tandon ‡, the other by Blainville §, this last being in part an extract from the *Dict. des Scien. Nat.* In these works, which may serve as points of departure to future observers, the history of these animals is nearly complete up to the above time. In both will be found considerable details with respect to their anatomy, physiology, and habits, and likewise with respect to species. Of these last Blainville enumerates thirty-six. Moquin-Tandon describes thirty-seven, besides four which he considers as doubtful. It may be stated that Derheims has also written upon this family; but Moquin-Tandon does not speak favourably of his work ||, which I have not seen myself.

The *Lumbrici*, which received a large share of Savigny's attention, and of which he has described upwards of twenty species ¶ (as he considers them), before confounded under the general name of *L. terrestris*, have been since much attended to by Léon-Dufour, Dugès, and Morren. Léon-Dufour's observations, contained in two memoirs in the *Ann. des Scien.* for 1825 and 1828, chiefly respect the mode of reproduction, which he asserts to be *oviparous*, and not viviparous as supposed by Montègre** and Sir Everard Home ††. He has discovered the capsules at the depth of five or six feet in the earth, and found them analogous to those of the genus *Hirudo*. M. Dugès is the author of an elaborate memoir in the *Ann. des*

* vol. xxv. p. 273. † tom. iv. p. 184.

‡ *Monographie de la Famille des Hirudinées, par Alfred Moquin-Tandon.* Paris, 1827. 4to.

§ *Essai d'une Monographie de la Famille des Hirudinées.* Paris, 1827. 8vo.

|| *Histoire Naturelle et Médicale des Sangsues.* Paris, 1825. 8vo.

¶ The characters of these species will be found in Cuvier's *Analyse des Travaux* for 1821.

** *Mém. du Mus.*, tom. i. p. 242. †† *Phil. Trans.* 1823, p. 143.

Scien. for 1828 *, which has principally for its object the anatomy of Cuvier's entire group of *Annelides Abranches*. So far as respects the *Hirudinidæ*, he has added little to what may be found in Moquin-Tandon's work on this subject; but he has thrown much valuable light on the structure and physiology of Cuvier's first family. His researches, which relate to the organs of circulation, respiration, and reproduction, have been made on two species of *Nais* and six of *Lumbricus*, which he commences by characterizing. The latter he does not appear to be able to identify in all cases with those of Savigny. Like Léon-Dufour, he considers these animals as oviparous, and thinks that what Montègre took for living young were only intestinal worms. Morren's work †, which was crowned by the University of Ghent, was published in 1829, and is of the most elaborate nature. Taken in connexion with the researches of the French naturalists, it leaves scarcely anything to be desired as far as regards the anatomy and physiology of the *Lumbrici*. Its author seems in doubt, however, about the numerous species described by Savigny and others. He is more inclined to regard them as simple varieties. He in some measure reconciles the conflicting testimonies of Montègre and Léon-Dufour with respect to the mode of reproduction, by asserting it to be both oviparous and ovoviviparous.

The structure of the genus *Nais* has been also investigated by Dr. Gruithuisen. He has published two memoirs on the anatomy of certain species belonging to this group in the *Nova Acta &c. Nat. Cur.* ‡.

Before quitting this class, it may be remarked that the true situation of the genus *Dentalium*, placed by Cuvier amongst his *Annelides tubicoles*, is undetermined. M. Deshayes, who has made it the subject of a monograph published in the *Mém. de la Soc. d'Hist. Nat. de Paris* §, and who has entered into much detail with respect to its anatomy, seems to regard it as belonging to the *Molluscous* type. Further researches are, however, necessary in order to establish this opinion as correct.

2. *Crustacea.*—Until within these few years Latreille and Dr. Leach were almost the only naturalists who had studied the animals of this class collectively with any degree of care or minuteness of detail. The latter gentleman is well known to have devoted a great deal of his attention to their arrangement and natural affinities. His treatises in this department, consisting

* tom. xv. p. 284.

† *De Lumbrici Terrestris Historia Naturali nec non Anatomia Tractatus.* Bruxell. 1829. 4to.

‡ tom. xi. p. 235, and tom. xiv. p. 397.

§ tom. ii. p. 321.

of the article CRUSTACEOLOGY in the *Edinb. Encyclop.*, a paper in the *Linn. Trans.* *, and the *Malacostraca Podophthalma Britanniæ*, this last giving descriptions and coloured representations of a large portion of the British species, have been already alluded to in a former part of this Report. These works were all published before the first edition of the *Règne Animal* of Cuvier. Nevertheless it may be well to give a slight sketch of Dr. Leach's arrangement, which, though founded upon Latreille's †, is somewhat different from that proposed subsequently by this last author.

In the *Linnæan Transactions*, above referred to, Dr. Leach distributes the *Crustacea* primarily into the two large groups or subclasses of *Malacostraca* and *Entomostraca*. The *Malacostraca* are then divided into two other groups, or legions as they are called, bearing the names of *Podophthalma* and *Edriophthalma*, according as the eyes are either pedunculated or sessile. The *Podophthalma* include the two orders *Brachyura* and *Macroura*, comprising, the former thirty-three, and the latter twenty-two genera. The *Edriophthalma* are not divided into orders, but merely distributed into thirty-eight genera, which are grouped according to the form of the body, and other characters derived from the antennæ and feet. In this division are several new and curious genera, entirely unknown till Dr. Leach first made them public. The *Entomostraca* had received so little attention when Dr. Leach published his system, that he did not attempt to arrange them according to their true affinities, but merely gave an artificial distribution of the genera, to serve till such time as we were made better acquainted with their structure.

The arrangement of Latreille in the third volume of the first edition of the *Règne Animal* ‡ is different, as already alluded to, from that adopted formerly by this author. In this work the *Crustacea* are divided into five orders: *Decapodes*, *Stomapodes*, *Amphipodes*, *Isopodes*, and *Branchipodes*; the characters of which are taken from the situation and form of the branchiæ, the mode of articulation of the head with the trunk, and the organs of manducation. The *Decapoda* are divided into the two families of *Brachyures* and *Macroures*, answering to Dr. Leach's two orders bearing the same names. The *Stomapoda* include one family, formed out of the Fabrician genus

* vol. xi. p. 306.

† I allude to the system given by Latreille in his *Genera Crustaceorum et Insectorum*. 4 vols. 8vo. Paris, 1806.

‡ Latreille undertook all that portion of the above work which treats of the *Annulose Animals with Articulated Feet*, comprising the classes *Crustacea*, *Arachnida*, and *Insecta*.

Squilla. The *Amphipoda* consist principally of such *Crustacea* as were referred by Fabricius to his genus *Gammarus*. The *Isopoda* answer to the *Onisci* of Linnæus. The *Amphipoda* and *Isopoda* together constitute Dr. Leach's second legion, *Edriophthalma*. Latreille's fifth order, *Branchiopoda*, includes the *Entomostraca* of Müller and Leach, referred by Linnæus to his genus *Monoculus* *.

Since the appearance of the *Règne Animal*, other naturalists have occupied themselves with this class. Latreille has also modified his own arrangement in some subsequent publications, availing himself of many valuable researches on the part of different individuals, relating more particularly to the *Entomostraca*.

In the *Familles Naturelles*, published in 1825, we find the *Crustacea* divided primarily into the two sections of *Maxillosa* and *Edentata*. The former comprises, in addition to the old orders *Decapoda*, *Stomapoda*, *Amphipoda*, and *Isopoda*, three new orders,—one, *Læmodipoda*, for the reception of the *Isopodes Cystibranches* of the *Règne Animal*, placed between the *Stomapoda* and *Amphipoda*; the other two, *Lophyropoda* and *Phyllopoda*, taken out of the old order *Branchiopoda*, and terminating the first division. The second section contains the remainder of the *Branchiopoda* arranged under the two new orders *Xyphosura* and *Siphonostoma*. Thus we have the *Entomostraca*, which before constituted but one order, here forming four. Latreille in his last work, *Cours d'Entomologie*, has increased the orders still further. He has adopted three other new ones, called *Dicladopa*, *Ostrapoda*, and *Trilobita*. The first of these, inserted between the *Isopoda* and *Lophyropoda*, includes the genera *Nebalia*, *Pontia*, *Condylura*, and *Cuma*. The second, instituted by Straus, comprises the genera *Cypris* and *Cytherea*, and is placed between the *Lophyropoda* and *Phyllopoda*. The third, adopted for the fossil *Trilobites*, forms the last order in his first division of *Maxillosa*. In other respects his system is the same as that in the *Familles Naturelles*.

The same year as that in which the *Fam. Nat.* of Latreille appeared, Desmarest published his *Considérations Générales sur la Classe des Crustacés*. In this work, which is one of considerable merit as well as utility †, we have the systems of Latreille and Leach in some measure combined. Thus, the *Malacostraca*

* The above arrangement by Latreille was adopted, with some slight modifications, by Lamarck in the 5th vol. of his *Hist. Nat. des An. sans Vert.*

† M. Desmarest was the first to draw the attention of naturalists to the different regions marked out on the upper surface of the *carapace* in the *Decapoda Brachyura*, and to show their exact accordance with the internal organs which they respectively cover.

and *Entomostraca* of this last author are retained as primary divisions, and the former is still divided into the two secondary groups of *Podophthalma* and *Edriophthalma*; but the groups next in succession are the same as Latreille's orders. At the same time there is a slight modification of these orders among the *Entomostraca*.

Risso, who has paid considerable attention to the *Crustacea*, adopts, in his *Hist. Nat. de l'Eur. Mérid.*, published in 1826*, nearly the same arrangement as that of Desmarest.

The most important, as well as most recent, additions which have been made to our knowledge of the *Crustacea* are due to the researches of MM. Audouin and Edwards, who have for some years back, the latter gentleman more especially, given particular attention to this class of animals. Indeed it is impossible to speak too highly of their labours in this department. Bearing in mind the close connexion which subsists between zoology properly so called, and comparative anatomy and physiology, they have commenced by studying closely the internal as well as external organization of the *Crustacea*, before proceeding to investigate their natural affinities. The results of their researches on this branch of the subject are contained in a series of memoirs published in the *Annales des Sciences*, of which any lengthened analysis here would lead too much into anatomical details. It may be just stated, that in their first two memoirs, published in 1827†, they have treated of the circulation of the blood, concerning the true course of which there prevailed before much difference of opinion. They have determined with accuracy the exact method in which the circulation is effected, and found it to be in some respects analogous to that which is known to prevail in the molluscous animals‡. In a third memoir, published in 1828§, they have entered into considerable details with respect

* The *Crustacea* are contained in the fifth volume. Risso had published some years previously a work entitled, *Histoire Naturelle des Crustacés des Environs de Nice*, 8vo, Paris, 1816.

† *Ann. des Scien.*, tom. xi.

‡ Two memoirs on the circulation of the *Crustacea* have been also published in Germany by M. Lund, the one prior, the other subsequent, to those of Audouin and Edwards. In the first (*Isis*, 1825,) the author observes that he has never been able to discover the slightest trace of veins in the *Crustacea*, which he thinks are without them, and in consequence deprived of a complete circulation. In the second (*Isis*, 1829,) he confines himself to some remarks on the researches of Audouin and Edwards, who have arrived at such different results from himself. He allows that they have discovered a system analogous to the venous system of the *Vertebrata* and *Mollusca*, but does not agree with them as to a near affinity between the *Crustacea* and *Mollusca* in regard to their circulatory organs.

§ *Ann.*, tom. xiv. p. 77.

to the nervous system. Their particular object is to show that in the *Crustacea* this system exhibits a unity of composition, and that all the different modifications which it presents in different families may be easily referred to one type, these modifications depending simply on a greater or less approximation, and tendency towards centralization of the medullary ganglions. In a fourth memoir, read the same year to the Royal Academy of Sciences*, they have considered the respiratory organs of these animals, their researches on which head have led them to discover the true method of respiration in those *Crustacea* which are capable of living for a considerable time out of water. They have ascertained that it is not by any organ analogous to lungs, as was formerly supposed, but by the help of a peculiar structure, enabling them to retain the water within the respiratory cavity as in a reservoir, from whence is supplied the necessary moisture for a free exercise of the branchial laminæ. In a subsequent memoir on this subject†, published in 1830, M. Edwards has expressed an opinion that the respiratory apparatus will be found to afford some valuable characters for the determination of natural groups.

The above memoirs on the anatomy of the *Crustacea*, with the exception of the last, were undertaken by MM. Audouin and Edwards jointly. During the present year (1834), M. Edwards has published singly the first volume of a general work ‡ on the natural history of this class, in which he has embodied the researches just alluded to, as well as treated of the classification and systematic description of these animals. The following is a sketch of his arrangement. He divides the *Crustacea* primarily into the three subclasses of *Crustacés Maxillés*, *Crust. Suceurs*, and *Crust. Xyphosuriens*. The first of these groups commences with the legion *Podophthalmiens*, including the two orders *Décapodes* and *Stomapodes*; then follows the legion *Edriophthalmes*, comprising the three orders *Amphipodes*, *Isopodes*, and *Læmipodes*; next in succession are the legions *Branchiopodes* and *Entomostracés*, which he thinks form two parallel series, the former containing the two orders of *Phyllopodes* and *Cladocères*, the latter those of *Ostrapodes* and *Copépodes*, this last being nearly the same as the order *Dicladopes* of Latreille. The legion *Trilobites* is placed provisionally at the end of the first subclass. The second subclass is divided

* A report by Cuvier and Dumeril on this memoir will be found in the *Ann. des Sci. Nat.*, tom. xv. p. 85.

† *Ann. des Sci.*, tom. xix. p. 451.

‡ *Histoire Naturelle des Crustacés, comprenant l'Anatomie, la Physiologie, et la Classification de ces Animaux*, par Milne Edwards, tom. i., Paris, 1834.

into the two legions of *Parasites Marcheurs* and *Parasites Nageurs*, the former comprising the single order *Aranéiformes*, the latter the two orders *Siphonostomes* and *Lernéens*. The third subclass consists of the single order *Xyphosures*. It will be seen that Edwards has adopted a large number of Latreille's principal groups. At the same time he has introduced some changes in the arrangement of this author. He has augmented the number of orders, and likewise altered the limits of some of these divisions. Two of the additional orders are for the reception of the *Pycnogonida* and *Lernææ*, which Latreille does not include in the present class. In the descriptive portion of his work, M. Edwards has as yet proceeded but a little way. In fact he has only got through the first two families of the *Decapoda Brachyura*. A few years back, however, he published a monograph on the *Crustacea Amphipoda*, to which those may be referred who want information on that particular order. An extract from it will be found in the *Ann. des Scien.* for 1830*.

Some researches on the *Crustacea* by a naturalist of this country, of great importance, though leading to results which it would be well to have confirmed by other observers, may be noticed in this place. I allude to Mr. Thompson's supposed discovery of a metamorphosis in the animals of this class, announced in 1828, in the first number of his *Zoological Researches*†. It is stated by this gentleman, that having examined the newly hatched young of the common Crab (*Cancer Pagurus*), he found them presenting exactly the appearance of the *Zoea Taurus*, the *Monoculus Taurus* of Slabber, which animal he conceives to be the first state of the species above mentioned. From this circumstance he was led to conclude, that metamorphosis was general throughout the *Decapod Crustacea*; that in the first stage of their existence they are essentially natatory, but that after a time the greater number of them lose the power of swimming, acquire chelæ, and have their feet adapted for crawling only. In a communication made by letter to the Zoological Society in 1830‡, Mr. Thompson stated, in support of the universality of this metamorphosis, that he had ascertained the newly hatched animal to be a *Zoea* in eight genera of the *Decapoda Brachyura*, viz. *Cancer*, *Carcinus*, *Portunus*, *Eryphia*,

* tom. xx.

† *Zoological Researches, and Illustrations; or Natural History of Nondescript or imperfectly known Animals.* By J. V. Thompson. Cork, 1828, &c.—Of this work only five numbers have as yet appeared. In it will be found some other valuable memoirs relating to the *Crustacea* besides that above alluded to, more particularly one on the genus *Mysis*, and another on the *Shizopoda*.

‡ *Proceed. of Zool. Soc.*, p. 17.

Gecarcinus, *Thelphusa?*, *Pinnotheres*, and *Inachus*; and in seven genera of the *Macroura*, viz. *Pagurus*, *Porcellana*, *Galathea*, *Crangon*, *Palæmon*, *Homarus*, and *Astacus*.

No direct observations have been as yet made by other naturalists sufficient to establish the existence of any error in these results at which Mr. Thompson has arrived. There is, however, enough on record to prove that this metamorphosis is not universal; and some excellent observers have been led by their own inquiries to regard it as rather improbable altogether. The researches of Rathke are decidedly opposed to it. This profound anatomist is the author of an elaborate treatise on the development of the young *Cray-fish**, which he has traced through all its stages from its earliest existence; and so far from observing any metamorphosis in this species, he particularly states that the young at birth scarcely differ *externally* from the adult except in size. M. Edwards has made some remarks upon Mr. Thompson's theory, which he does not consider as tenable, without the support of further and more accurate observation. At the same time he thinks it very possible that none of the individuals of the genus *Zoea* hitherto observed by naturalists had reached their adult state†. We are informed by Latreille‡, that this gentleman had it in view to institute some particular researches under the hope of throwing light on this matter. I am not aware that any decisive results have been hitherto made public. The subject, however, is undergoing investigation in our own country, and will probably before long be satisfactorily cleared up.

The above doubts respecting the metamorphosis of the *Crustacea* relate only to its existence amongst the *Decapoda*. That it takes place in some of the other orders in this class is quite certain. Jurine long since detected it in the case of some of the *Entomostraca*. More recently M. Edwards has observed striking changes of form, almost, if not quite amounting to metamorphosis, taking place in several genera of the *Crustacea Isopoda*, in one genus (*Cyamus*, Latr.) of the *Læmodipoda*, and in one genus (*Phronima*, Latr.) of the *Amphipoda*§. At the same time he has fully ascertained, that in other genera, more particularly *Gammarus* and *Idotea*, this kind of metamorphosis does

* See an analysis of this memoir in the *Ann. des Sci. Nat.* for 1830, tom. xx. p. 442.

† *Ann. des Sci.*, tom. xix. p. 459. See also *Hist. Nat. des Crust.*, tom. i. p. 199; and *Dict. Class. d'Hist. Nat.*, Art. Zoe.

‡ *Cours d'Entomol.*, p. 385.

§ These researches are contained in a memoir, of which an analysis will be found in the *Ann. des Scien.* for Dec. 1833, p. 360.

not occur. The genus in which the change of form is most conspicuous appears to be that of *Cymothoa*. In this instance he has observed the young to be not only deficient in some parts which are developed in the adult,—thus, having six instead of seven thoracic segments, and consequently only twelve instead of fourteen feet,—but possessed of others well developed, which in the adult state are merely rudimentary. Thus, the adult has the head extremely small, and the eyes scarcely perceptible externally. The young, on the contrary, have the head large, and the eyes remarkably conspicuous. A similar difference occurs in the segments of the abdomen, which in the adult are very short and almost linear, whereas in the young they spread out almost as much as those of the thorax*.

Naturalists who have studied this class have too frequently confined their researches to the *Malacostraca*. The *Entomostraca*, although everywhere to be met with, like some other equally neglected groups, have received, at least of late years, but comparatively little attention. In this country they have been scarcely noticed at all. The works of Müller† and Jurine‡ still retain their value as the great storehouses of original observations relating to these animals, and are indispensable to those who may feel induced to study them. The latter, which is of recent date compared with Müller's, deserves especially to be pointed out, as, though well known and duly appreciated on the Continent, it does not appear to be familiar to our own naturalists. It embraces the history of Müller's genera *Cyclops*, *Daphnia*, *Polyphemus*, *Lynceus*, and *Cypris*, including descriptions of such species as are found in the neighbourhood of Geneva. Jurine has paid the most scrupulous attention to the habits and œconomy of these minute animals. Many of them he has traced through every stage of their existence; and, amongst other valuable researches, has ascertained that the genera *Amymone* and *Nauplius* of the Danish naturalist are only young states of the genus *Cyclops*. This work is illustrated with beautifully coloured figures of all the species. There is also appended to it a detailed and valuable memoir by Bénedict Prévost

* M. Edwards has sought to refer to some general principles these and other similar facts which he has observed relating to change of form in the *Crustacea*. He has arrived at the following generalization: That "the different changes of form which the *Malacostraca* (or higher *Crustacea*) may experience after quitting the egg, tend *always*, whatever be their nature or importance, to alienate the animal from the type common to the greater number of these beings, and in some measure to individuate it more and more." See *Ann. des Scien.*, l. c.

† *Entomostraca, seu Insecta Testacea, &c.* 4to, Lips. et Haun. 1785.

‡ *Histoire des Monocles qui se trouvent aux Environs de Genève.* 4to, Genève, 1820.

on the *Branchipus* of Latreille, or, as the author here calls it, *Chirocephalus**.

If to the above works we add a few separate memoirs devoted to particular genera by different individuals,—that of the younger Jurine on *Argulus foliaceus* published in 1806†, Straus's two memoirs on the genus *Daphnia* published in 1819 and 1820‡, a third the year following by the same author on the genus *Cypris*§, and Brongniart's memoir on the *Limnadia Hermanni* published in 1820||,—we shall have enumerated by far the most valuable contributions which have been yet made to our knowledge of this portion of the *Crustacea*¶. Straus's memoirs in particular, which for patient research and close anatomical investigation, considering the minuteness of these animals, can scarcely be equalled, deserve the highest commendation. It was principally in consequence of the labours of this observer and those of Jurine, which were subsequent to the appearance of the first edition of the *Règne Animal*, that Latreille was led to make such striking alterations in the arrangement of the *Entomostraca* in his *Familles Naturelles*. These alterations have been already pointed out; and they clearly show what we may yet expect from further researches into the structure of other groups which have not hitherto received so close an examination. The only recent contributions of any moment, at present known to me, are, a memoir by Dr. Gruithuisen on the Anatomy of *Daphnia Sima* published in 1828**, a second by Milne Edwards on the structure of the mouth in the Siphonostomous *Entomostraca* published in 1833††, and a third published within these few months by Mr. Thompson on the *Artemis salinus*‡‡. The principal object of M. Edwards's essay is to show that notwithstanding the apparent differences between the mouth of the *Siphonostoma* and that of the rest of the *Crustacea*, the parts are strictly analogous in the two cases, and there is still kept up a unity of composition. Thompson's memoir contains observations on the gradual development of the young of the *Artemis salinus*, and the metamorphoses which it undergoes before arriving at an adult state. These metamorphoses are found to cor-

* This memoir had been previously published in the *Journal de Physique* for 1803, tom. lvii.

† *Ann. du Mus.*, tom. vii. p. 431.

‡ *Mém. du Mus.*, tom. v. p. 380, and tom. vi. p. 149.

§ *Id.*, tom. vii. p. 33.

|| *Id.*, tom. vi. p. 83.

¶ A treatise on the *Monoculi* was published at Halle, in 1805, by Ramd'hor, who, according to Latreille, has anticipated Straus and Jurine in some of their anatomical researches. I have not seen the work myself.

** *Nov. Act. &c. Nat. Cur.*, tom. xiv. p. 368.

†† *Ann. des Scien. Nat.*, tom. xxviii. p. 87.

‡‡ *Zool. Researches*, No. 5, Mem. 6.

respond with those noticed in *Branchipus*, *Apus*, and other genera of the *Phyllopoda*, to which the *Artemis* is allied. Mr. Thompson has endeavoured to prove that there is a close affinity between the *Artemis salinus* and the fossil Eyeless *Trilobites*.

I may also refer to a paper by Audouin and Edwards in the *Annales des Scien.* for 1826*, containing an account of a very singularly organized animal, forming a new genus (*Nicothoe*) among the *Siphonostoma* of Latreille. It is of parasitic habits, and was discovered firmly attached to the gills of the Lobster. Perhaps there is no group in the *Entomostraca* in which we may expect so many new forms yet to occur, and of whose œconomy in general we know so little, as that just mentioned. With reference to this last point we may, however, except the genus *Argulus*, Jurine's memoir before spoken of leaving us scarcely anything further to be desired in the history of that animal.

3. *Arachnida*.—This class, which Lamarck was the first to separate from that of Insects, has until very recently been much neglected by naturalists. The consequence is that our knowledge of many of the groups contained in it is extremely imperfect. Even its limits are far from being determined; and some are of opinion that it ought to be resolved into two classes, on account of the great differences which occur in the respiratory organs. Dr. Leach was the first to entertain this last idea, in the third volume of his *Zoological Miscellany*, published in 1817. In an article in this work† "On the Characters of the *Arachnides*," he has restricted this class to the five families of *Scorpionidæ*, *Tarantulidæ*, *Phalangidæ*, *Solpugidæ*, and *Araneidæ*, in all of which respiration is effected by means of pulmonary sacs. The Trachean *Arachnida* of Latreille, excepting the genera *Pycnogonum*, *Phoxichilus*, *Ammothea*, and *Nymphum*, (whose situation he considers doubtful,) and the genera *Phalangium*, *Solpuga*, and *Trogulus*, (and perhaps *Siro*,) he thinks constitute a peculiar class, which he proposes to name *Acari*.

Although Latreille himself subsequently adopted this same opinion respecting the propriety of forming two classes of the Pulmonary and Trachean *Arachnida*‡, he has not acted upon it in any of his published works. In the *Règne Animal* these groups simply stand as two orders, the first including the two families of *Fileuses* (*Aranea*, Linn.) and *Pédipalpes* (*Tarantula*, Fab., and *Scorpio*, Linn.), the second those of *Faux Scorpions*, *Pyc-*

* tom. ix. p. 345. † p. 46.
‡ *Fam. Nat.*, p. 317, note (1). *Cours d'Entom.*, p. 161.

nogonides, and *Holetræ* (*Phalangium* and *Acarus*, Linn.). In the *Familles Naturelles* his arrangement is nearly the same. There is simply a change with respect to the order in which the families stand, with the addition of some new ones amongst the Trachean *Arachnida*. But in the *Cours d'Entomologie* we find a third order, termed *Aporobranches*, occupying a middle station between the other two. This new group, which is characterized by having gills without any external opening, Latreille intends should include the *Pycnogonida*. It has been already mentioned that these anomalous animals, which seem to form the passage from the *Arachnida* to the *Crustacea*, are considered by Edwards as belonging to the class last mentioned.

It may be stated that Mr. Kirby appears likewise to be of opinion that the *Pulmonary* and Trachean *Arachnida* should not be included in the same class*.

Mr. MacLeay has, however, expressed himself differently. He maintains "that the division of the organs of respiration and circulation is not to be depended on in the classical arrangement of the *Annulosa*†."

This last opinion, which will probably in the end be generally assented to, has been adopted by Dugès in a valuable memoir on the *Acari*, published during the present year‡. In the introduction to this memoir Dugès has made some observations on the relation which subsists between the *Acari* and the rest of the *Arachnida*. He remarks that there is nothing in the external structure of these animals at all corresponding to those differences in the respiratory and circulatory organs which some authors have made the basis of their arrangement. He thinks that the value of the characters derived from these organs has been overrated; and in proof of this, that it is only necessary to observe the striking changes which such organs undergo (in the case of the Batrachian Reptiles and aquatic insects) in the same individual at different stages of its life.

Instead, then, of making the external form subordinate to the organs of respiration and circulation, M. Dugès adopts the former as the groundwork upon which he establishes his principal divisions. The following are what he considers as the true distinguishing characters of the class *Arachnida*: 1stly, the presence of eight feet adapted for walking; 2ndly, the absence of antennæ§ and reticulated eyes; 3rdly, the constant union of one

* *Introd. to Entom.*, vol. iii. p. 21. † *Hor. Ent.*, p. 382.

‡ "Recherches sur l'Ordre des *Acariens*," *Ann. des Scien. Nat.* for Jan. 1834, p. 5.

§ Lamarck had observed, and formerly Latreille also, how strikingly the true *Arachnida* were distinguished from the two classes of *Crustacea* and Insects by

or more segments of the thorax with the head. The class thus characterized, which, according to a new nomenclature of his own*, he calls *Aranistes*, he divides into the two subclasses of *Acarulistes* and *Aranulistes*; the former containing the single order *Acariens*; the latter the three orders of *Phalangiens*, *Aranéens*, and *Scorpioniens*. Each of these orders is again divided into several families.

The rest of Dugès's memoir is restricted to the investigation of the *Acari*, and contains some novel and highly important researches on this group of animals. These relate more especially to the gradual development of the young, and the metamorphoses which many of them undergo before arriving at the adult state. M. Dugès has satisfactorily ascertained that many of the hexapod genera constituting Latreille's family of *Microphthira* are only the larvæ of others, and he has sufficiently multiplied his observations to lead him to suspect that this will be found ultimately to be the case with all of them; that is to say, that there will be found no instance of any of the *Arachnida* having only six feet in the adult state. He has proved *Leptus* to be only the young of *Trombidium*, and he has strong reasons for supposing *Ocypete* and *Astoma* to be so likewise. The genus *Achlysia* of Audouin† he has shown to be the larva of *Hydrachna*: the genus *Caris* he suspects to be the larva of *Argas*. Although these striking researches necessarily lead to the suppression of many genera instituted by former naturalists, Dugès has discovered or established others more than sufficient to make compensation. In his arrangement of these animals we still find twenty-four genera, distributed under seven families, the former exceeding by five the number adopted by Latreille. It is his intention to treat of each of these genera separately. As yet, however, his valuable memoir remains unfinished.

But few individuals besides Dugès have hitherto devoted much of their attention to the *Acari*. In 1826, Heyden published a systematic arrangement of this group‡, in which he increased

the want of antennæ. Latreille, however, was led subsequently to take a different view of the subject, and to regard what are usually called the mandibles or cheliform palpi in the *Arachnida* as representing the intermediate pair of antennæ in the *Crustacea Decapoda*, only in the former class exercising a different function and being always adapted for manducation. Thus the deficient parts he considered to be the true mandibles, and not the antennæ. See *Fam. Nat.*, p. 307. See also some remarks on this hypothesis of Latreille, by MacLeay (*Hor. Ent.*, p. 383,) and likewise by Dugès (*l. c.*, p. 9.).

* In this and some other instances Dugès has very unnecessarily changed names which had long been consecrated by time, and adopted generally.

† *Mém. de la Soc. d'Hist. Nat. de Paris*, tom. i. p. 98.

‡ *Isis*, 1826, p. 608.

the number of genera to sixty-nine, but in the opinion of Dugès many of these rest on doubtful if not erroneous characters. Léon-Dufour, Audouin, and De Théis have all contributed memoirs to the *Ann. des Sci. Nat.* on particular genera*. According to Latreille†, this last gentleman is engaged in a new work on these animals, to be illustrated by plates.

The most important group among the Pulmonary *Arachnida* is that of the *Araneidæ*. Nevertheless, like all the others in this class, it has been greatly neglected. Walckenaer, Latreille, and Léon-Dufour in France, De Hahn in Germany, and Mr. Blackwall in our own country, are almost the only individuals who have given it any attention of late years. Walckenaer, who has studied it most deeply, and whose *Tableau des Aranéides*, published in 1805, has been hitherto the only guide for naturalists in this department, has recently proposed a new arrangement of these animals in a memoir read to the Entomological Society of France‡. The principal groundwork of his system is the same as in his *Tableau*, and he still adopts the two large divisions of *Théraphoses* and *Araignées*, founded upon the position of the jaws with respect to the rest of the body, and the articulation of the mandibles. The number and position of the eyes serve afterwards for characterizing some well-marked groups subordinate to these two large tribes. Walckenaer observes that the species of Spiders have been greatly overmultiplied, from sufficient regard not having been paid to the changes incident to different ages with respect to size and colour. Léon-Dufour has more particularly occupied himself with the structure and internal anatomy of the *Araneidæ*. He is the author of some important memoirs§ on this part of the subject, in one of which he has instituted a new division of this group into the two sections of *Tetrapneumones* and *Dipneumones*, founded on the number of pulmonary sacs, which he was the first to discover are double on each side of the abdomen in certain species, amounting to four in all. The *Tetrapneumones*, which comprise the *Théraphoses* of Walckenaer, as well as a small portion of his *Araignées*, form the subject of a memoir in the *Nouv. Ann. du Mus.*|| by Latreille, who speaks highly of this new principle of arrangement. He thinks that it will serve as an immutable

* *Ann. des Sci.*, tom. xxv., xxvi., and xxvii. † *Cours d'Entom.*, p. 546.

‡ An extract from this memoir will be found in *L'Institute*, 1833, No. 18. M. Walckenaer has also lately commenced the publication of a work entitled, *Les Aranéides de France classées par leur Organisation, &c.* (*L'Instit.* 1834, No. 53.)

§ *Ann. des Scien. Physiques de Bruxelles*, tom. v. and vi. || tom. i. p. 61.

foundation for a natural distribution of the genera in this extensive family. Latreille has adopted it in the second edition of the *Règne Animal*, as he had previously done in his *Familles Naturelles*. It must be observed, however, that Walckenaer does not attach so much importance to this modification of the respiratory organs. He states that it is not accompanied by any corresponding differences in other parts of the structure, and that, taken as the basis of a division, it leads to the separation of certain genera which, according to his views, are connected by the closest affinity. Besides the above memoirs on the structure of the *Araneæ*, Léon-Dufour has published several others descriptive of new or ill-understood species*. He has particularly attended to the species found in Spain, as well as to the species of *Phalangium* met with in the same country†. He has discovered a new method of preserving the *Araneæ*‡, which it is to be hoped may induce fresh labourers to enter upon this field. It is greatly owing to the difficulty which has been hitherto experienced in preventing the changes which occur after death in these animals, that they have been so much neglected by naturalists.

De Hahn is the author of a work now in course of publication, the object of which is to illustrate by coloured plates the genera and principal species of this family§. Mr. Blackwall has published some important memoirs on subjects connected with the structure and œconomy of the *Araneidæ*||, as well as others descriptive of some undescribed genera and species¶.

Before leaving this class it may be mentioned that in the third volume of the *Zoological Miscellany***, Dr. Leach has published an article on the characters of the genera of the family *Scorpionidæ*, accompanied by descriptions and coloured representations of all the British species of *Chelifer* and *Obisium*. Some additions to these genera by Théis will be found in the *Ann. des Sci.* for 1832††.

4. *Myriapoda*.—There can be no doubt that a certain affinity exists between this class and the *Annelida*, as Latreille was the first to point out in a memoir on the articulated animals published in 1820‡‡. The *Myriapoda* have not been much attended to. In the third volume of the *Zoolog. Miscell.* is a valuable paper by Dr. Leach on these animals, in which he has given the

* *Ann. des Sci. Phys.*, tom. iv. *Ann. des Sci. Nat.*, tomes ii. and tom. xxii.
† *Ann. des Sci. Nat.*, tom. xxii. ‡ *Id.*
§ *Die Arachniden getreu nach der Natur abgebildet und beschrieben*, von C. W. Hahn, 1831, &c. || *Linn. Trans.*, vols. xv. and xvi.
¶ *Lond. and Edinb. Phil. Mag. and Journ.*, 1833, vol. iii.
** p. 48. †† tom. xxvii. p. 57. ‡‡ *Mém. du Mus.*, tom. vi. p. 116.

characters of the genera which it comprises, as well as descriptions of all the British species*. He divides the class into the two orders of *Chilognatha* and *Syngnatha*, the former answering to the Linnæan genus *Iulus*, the latter to that of *Scolopendra*. This arrangement is adopted by Latreille. Savi has made a particular study of the *Iuli*. In two memoirs, one published in 1817, the other in 1819†, he has recorded some valuable observations relating to the œconomy of certain species of this family. I am not aware of any recent contributions to our knowledge of this class excepting a paper by Léon-Dufour on the internal structure of the *Lithobius forficatus* and the *Scutigera lineata*. This memoir is published in the *Ann. des Sci. Nat.* for 1824.

5. *Insecta.*—It is impossible to do more than to treat of this class in the most general manner. Indeed from its great extent, the immense additions which have been made to it of late years, and the large number of individuals who have contributed to its progress, it may well deserve to be made the subject of a separate report. I shall simply state, 1stly, the leading groups which have been adopted or proposed in this class; 2ndly, the most important works and memoirs which have appeared in illustration of its structure; 3rdly, the principal authors who have contributed to the advancement of particular parts of it. As the chain of affinities connecting the several orders is far from being determined with certainty, and much difference of opinion exists on this subject, to discuss which would lead to considerable details, I shall be silent on this point altogether.

(1.) In the first edition of the *Règne Animal* the following orders are adopted by Latreille, exclusively of the *Myriapoda*, which he afterwards acknowledged as a distinct class. 1. *Thysanura*, Latr.‡; 2. *Parasita*, Latr.‡; 3. *Suctoria*, De Geer; 4. *Coleoptera*, Linn.; 5. *Orthoptera*, Oliv.; 6. *Hemiptera*, Linn.; 7. *Neuroptera*, Linn.; 8. *Hymenoptera*, Linn.; 9. *Lepidoptera*, Linn.; 10. *Rhipiptera*, Latr. (*Strepsiptera*, Kirb.); 11. *Diptera*, Linn.

In the same year (1817), Dr. Leach published his amended arrangement of the orders of the class *Insecta* in the third volume of his *Zool. Miscellany*. In this work we have a primary division into the two subclasses of *Ametabolia* and *Metabolia*. The former includes the *Thysanura* and *Parasita* of Latreille, the name of this last order being changed to *Anoplura*: the latter, Latreille's remaining orders, with the five additional orders of *Dermaptera*, De Geer, (gen. *Forficula*, Linn.); *Dictyo-*

* Dr. Leach has described several new species of *Iulus* from the South of Europe in the *Transactions of the Plymouth Institution*, 1830, p. 158.

† See *Bull. des Sci. Nat.*, 1823, tom. iv. p. 330.

‡ These two orders were established by Latreille in some of his earlier works.

ptera, (gen. *Blatta*, Linn.); *Omoptera*, (gen. *Cicada*, *Thrips*, *Aphis*, &c., Linn.); *Trichoptera*, Kirb. (gen. *Phryganea*, Linn.); and *Omaloptera*, (gen. *Hippobosca*, Linn.); amounting in all to sixteen. Latreille's name of *Suctoria* is changed to that of *Aptera*.

Mr. MacLeay in his *Horæ Entom.* (1821) has proposed *Bomboptera*, *Megaloptera*, and *Rhaphioptera* as three new osculant orders in his class *Mandibulata*, including the genera *Sirex*, Linn., *Sialis*, Latr., and *Boreus*, Latr., respectively. The first he considers as connecting the *Hymenoptera* and *Trichoptera*; the second, this last and *Neuroptera*; the third, this last and *Orthoptera*. He regards *Dermaptera* and *Strepsiptera* likewise as osculant orders, the former connecting *Orthoptera* and *Coleoptera*, the latter this last and *Hymenoptera*.

Blainville* divides the Insects (forming with him his class *Hexapoda*) into three subclasses, *Tetraptera*, *Diptera*, and *Aptera*. The first of these contains as subordinate groups the orders *Coleoptera*, *Orthoptera*, *Hemiptera*, *Lepidoptera*, *Neuroptera*, and *Hymenoptera*.

In 1823, Duméril published his *Considérations Générales sur la Classe des Insectes*. His work, however, which is of an elementary nature, offers nothing new on the subject of classification. His orders, eight in number, are the same as those of Linnæus, with the addition of *Orthoptera*. M. Duméril advocates very strongly the dichotomous, or, as he terms it, the analytical method of arrangement, which he had adopted in his former works.

In the *Fam. Nat.* (1825) Latreille adopts as a primary division of this class the two sections of *Aptera* and *Alata*. The former comprises the orders *Thysanura*, *Parasita*, and *Siphonaptera* (name substituted for that of *Suctoria*); the latter, the remaining orders of the *Règne Animal*.

In 1826 appeared the fourth volume of the *Introduction to Entomology*, in which Mr. Kirby proposes to adopt twelve orders. Seven of these are the same as those of Linnæus; the remaining five are *Strepsiptera*, *Dermaptera*, *Orthoptera*, *Trichoptera*, and *Aphaniptera* (*Siphonaptera*, Latr.).

In the second edition of the *Règne Animal*, Latreille's arrangement is the same as in the first. But in his last work, the *Cours d'Entomologie* (1831), he has again taken that of the *Familles Naturelles*, excepting that he has adopted one additional order, the *Dermaptera* of Leach.

(2.) Our knowledge of the structure of Insects, both external and internal, has been greatly advanced of late years by the re-

* *Principes, &c.*, tab. 7.

searches of many excellent observers. Some of the most important contributions on the subject of their external anatomy have arisen out of an endeavour to trace analogies of structure in the relative conformation of different groups in this class, as well as in that of insects in general compared with the rest of the *Annulosa*. Savigny was the first to draw the attention of naturalists to inquiries of this nature in two memoirs on the structure of the mouth of the articulated animals, published in 1816*. In one he demonstrated that the same parts were to be found, though modified, in this organ as it occurs both in the *Mandibulata* and *Haustellata*, notwithstanding the apparent dissimilarity of its structure in these two groups. In the other he extended his researches, with the view of establishing similar analogies, to the mouth of the *Arachnida*, *Crustacea*, and *Entomostraca*. The year 1820 was rich in memoirs of a similar nature to those just alluded to. Latreille first published one on the structure of the wings of Insects†, in which he sought to refer to some general law of conformation the organs of locomotion in this class, as well as in those of *Arachnida* and *Crustacea*. Latreille's memoir was followed by three from Geoffroy on the *Organization of Insects*, already referred to in a former part of this Report as containing the first enunciation of his views respecting the vertebrate structure of Insects and *Crustacea*‡. The same year two memoirs were brought forwards by Audouin on the same subject. The object of one was to point out analogies of structure between the true Insects and the *Crustacea* and *Arachnida*, more particularly as regards the head and its appendages, and the relative development of the segments of the trunk§. That of the other was to generalize an extensive series of observations with respect to the various parts which enter into the composition of the thorax in the different orders of Insects‖. Latreille also published two other memoirs besides

* "Théorie des Organes de la Bouche des Crustacés et des Insectes," *Mém. sur les An. sans Vert.*, Part I.

† *De la Formation des Ailes des Insectes.* 8vo.

‡ *Journ. Comp. du Dict. des Sci. Méd.*, tomes v. and vi. It was in consequence of Geoffroy's first memoir on this subject, read to the Academy of Sciences Jan. 3, 1820, that Latreille was induced to write his memoir (before alluded to) entitled *Passage des Animaux Invertébrés aux Vertébrés*, which memoir of Latreille was published, together with his former one, *De la Formation des Ailes des Insectes*, as an 8vo pamphlet.

§ I am ignorant as to where this memoir was published, or whether it was ever published at all. I know it only from Cuvier's Report in the *Analyse des Travaux*.

‖ This memoir was subsequently published in the *Ann. des Sci. Nat.* for 1824, tom. i. pp. 97 and 416. A short analysis of it had appeared previously in the *Bull. de la Soc. Phil.* for 1820.

that already alluded to; one on the supposed elytra of the *Strepsiptera*, and on the appendices of the trunk of Insects in general*; the other on the general relations of the external structure of the articulated *Invertebrata* †. In 1821 Latreille published a memoir containing further observations on the external structure of the *Annulosa*, principally with a view to fix the nomenclature of the principal parts‡. The same year appeared the first of a series of elaborate memoirs by Chabrier on the organs of flight in insects, with a detailed account of all the parts contributing to the motion and articulation of the wings §. In 1825 an important memoir was brought forward by Mr. MacLeay on the structure of the tarsus in the Tetramerous and Trimerous *Coleoptera* of the French entomologists‖. Its object was to show the defects of an arrangement founded on this part, and to prove that such arrangement must necessarily lead to the violation of natural affinities. In 1826 appeared an elaborate dissertation on the external anatomy of Insects in the third volume of the *Introduction to Entomology* by Kirby and Spence. In this work there is given a collected view of the researches of previous naturalists on this subject; at the same time there are some material additions made to what had been already done by others. In the *Bull. des Sci. Nat.* for 1828, is an abstract of a memoir by Haan on the organs of manducation and motion in the articulated animals¶. It was during that year that Straus-Durckheim published his great work on the Comparative Anatomy of the Articulated Animals **. This last is perhaps the most important and elaborate treatise of its kind that has hitherto appeared. It is the first of a series of monographs which the author intends publishing on the structure of the different orders of insects. It contains some general remarks on the organization of the *Annulosa*, after which the author proceeds to the investigation of that of the *Coleoptera* in particular, the *Melolontha vulgaris* being taken as the type. In the first part of his subject, Straus-Durckheim has endeavoured to refer the different modifications of structure which the organs undergo in passing through different groups of articulated animals, to general laws. In 1830 Straus-Durckheim read to the Royal Academy of Sciences at Paris a portion of another work, treating in like manner of the structure of the Hymenopterous Insects, the common Hornet

* *Mém. du Mus.*, tom. vii. p. 1.
† *Id.*, tom. vi. p. 116.
‡ *Id.*, tom. viii. p. 169.
§ *Id.*, tomes vi., vii., and viii.
‖ *Linn. Trans.*, vol. xv. p. 63.
¶ tom. xiii. p. 443.
** *Considérations générales sur l'Anatomie Comparée des Animaux Articulés, auxquelles on a joint l'Anatomie descriptive du Melolontha vulgaris* (Hanneton), *donnée comme exemple de l'Organisation des Coleoptères.* Paris, 1828, 4to.

(*Vespa Crabro*, Linn.) being selected as the type. I am not aware that this second monograph has been yet published*. During the same year an elaborate memoir appeared in this country by Mr. MacLeay on the structure of the thorax in winged insects, in which he has not only given the result of his own inquiries, but reviewed the previous labours of Audouin and Kirby on this subject, especially the nomenclature of the different parts of the thorax as assigned by these authors respectively†. In the *Annales des Scien.* for 1832‡, is a memoir by Dugès on the structure of the genus *Pulex*, with the particular view of discovering its true affinities. This genus constituting in itself an entire order of insects, the memoir is of considerable importance. In the *Entomological Magazine*§, Mr. Westwood has also made some remarks on these insects more particularly relating to the structure of their antennæ. In the *Nouv. Ann. du Mus.* for the same year||, Latreille has published a valuable memoir on the external structure and affinities of the *Thysanura*, which his researches lead him to think form the transition from the *Myriapoda* to the true Insects. They are the only insects in which Latreille has not been able to discover *stigmata*; the absence of which he regards as one of the distinguishing characters of this order¶. Lastly, I may refer to some papers by Mr. Newman on the external anatomy of Insects in general, recently published in the *Entomol. Mag.*** Latreille has also treated of the whole subject in his *Cours d'Entomologie*††.

It would be out of place to dwell much on the internal anatomy of insects in this Report. I shall do little more than observe that it is principally to the researches of Marcel de Serres, Léon-Dufour, Dugès, and Straus-Durckheim in France, and to those of Herold, Gaede, Carus‡‡, Suckow, Meckel, and Müller

* An analysis of it will be found in the *Bull. des Sci. Nat.* for 1830, (tom. xxii. p. 347,) also in Cuvier's *Analyse des Travaux* for the same year.

† *Zool. Journ.*, vol. v. p. 145.

‡ tom. xxvii. p. 145.

§ vol. i. p. 359.

|| tom. i. p. 161.

¶ The *Thysanura* have been sadly neglected by entomologists. Latreille observes that with respect to the *Poduræ* there has appeared nothing new since the time of De Geer.

** vols. i. and ii.

†† I may state in this place that two general introductory works on entomology have appeared recently which I have not seen, both entering into details on the subject of the organization of insects. One of these is the *Handbuch der Entomologie*, published by Burmeister at Berlin in 1832. The other is the *Introduction à l'Entomologie* by Lacordaire, of which the first volume has only just appeared. See *L'Instit.*, No. 73, p. 324.

‡‡ Carus has the particular merit of having discovered the circulation of the blood in Insects. This remarkable fact, which was observed in the larvæ of certain *Neuroptera*, was first announced at the meeting of German naturalists held at Dresden in 1826.

in Germany, that we are indebted for the recent progress which has been made in this department. Mr. Newport in our own country has also lately entered upon this subject*. There can be no doubt that our knowledge of the natural affinities of Insects will be ultimately much benefited by the laborious investigations of such observers, although there may not have been acquired hitherto a sufficient number of facts to warrant any extensive generalizations. Those of Léon-Dufour may be more particularly alluded to as throwing some light on this subject. This patient anatomist, in one of a series of the most elaborate memoirs on the internal structure of the *Coleoptera*†, observes that by dissecting insects he has been enabled to determine the value of many purely entomological characters, to clear up doubts with respect to the distinction of the sexes in certain cases, and to add to the number of those characteristic marks which had already been acquired from a study of the mouth, antennæ, and feet, and employed as the foundation of families and genera. His researches have satisfied him that the system of Latreille is for the most part in perfect harmony with anatomical facts.

(3.) Since the science of entomology has become so extensively cultivated, and the field which it embraces been found to be so extremely large‡, naturalists have given up all attempt at a complete *Species Insectorum*. They have even in many cases found it impracticable to obtain a correct knowledge of any particular order, regard being paid to *all* the included species. Hence they have generally confined their researches to the more subordinate groups, or to the insects of particular countries: and it is to such works that we must have recourse, in order to learn the present state of our knowledge of the different orders which are comprised in this class. It is not my intention, indeed it is not practicable on the present occasion, to do more than indicate in a general manner a few of the most valuable of such works which have appeared of late years.

It is to the Count De Jean that we are indebted for the most extensive work which has been published on the order of *Coleoptera*§, although it has not extended as yet beyond the *Cicindelidæ* and *Carabidæ*. In a separate publication he has undertaken, conjointly with M. Boisduval, the illustration of such species as are found in Europe‖. Several important monographs have,

* *Phil. Trans.* 1832. † *Ann. des Sci. Nat.* 1824, &c.

‡ Messrs. Kirby and Spence have estimated the probable number of existing species of Insects at not less than 400,000. See *Introd. to Entom.*, vol. iv. p. 477. See also some remarks on this subject by Mr. Westwood in Loudon's *Magazine of Natural History*, vol. vi. p. 116.

§ *Species général des Coléoptères.* 8vo, Par., 1825, &c.

‖ *Iconog. et Hist. Nat. des Coléop.*, 1827, &c.

however, appeared upon particular families, some of which are now in course of publication. Thus, Zimmermann has made a study of the *Carabidæ* *; Erichson of the *Dyticidæ* †; Gory and Percheron of the *Cetoniæ* and some allied genera ‡. Schönherr has published two valuable works on the *Curculionidæ*; one § giving a general view of the subordinate groups in this extensive family; the other ||, which has been only recently commenced, entering into the details of species. Lastly, I may refer to an important monograph on the *Staphylinidæ* by Count Mannerheim ¶.

The *Orthoptera* have been made the subject of particular works by Zetterstedt ** and Audinet Serville ††. Toussaint de Charpentier has also published a monograph on he European species of this order in his *Horæ Entomologicæ*.

Hahn has undertaken an illustrated work on the *Hemiptera* ‡‡. Schummel has written a monograph on the particular genera *Hydrometra, Velia,* and *Gerris,* constituting Latreille's family *Ploteres* §§.

The *Lepidoptera*, at least the European species, have been particularly treated of by Treitschke, Godart, and Duponchel. The first has continued the valuable and well-known work of Ochsenheimer ||||. Godart is the author of a work on the *Lepidoptera* of France, which was commenced in 1822, but interrupted in 1825 by his death. Duponchel has carried it on from that time ¶¶. Boisduval has published a valuable monograph on the *Zygænidæ* ***. He has also commenced two other works on this order, one serving to illustrate the *Lepidoptera* of North America †††, the other the caterpillars and metamorphosis of the species found in Europe ‡‡‡. In the former he is assisted by

* *Monographie der Carabiden.* Berlin and Halle, 1831. See *Entomolog. Mag.*, vol. i. p. 306; a work to which I am indebted for the only knowledge I have of some of these monographs.

† *Genera Dyticeorum.* Berlin, 1832. (See *Ent. Mag.*, vol. i. p. 501.)

‡ *Monog. des Cétoines, et Genres voisins, &c.*, 1833. (See *Ent. Mag.*, vol. i. p. 418.

§ *Curculionidum Dispositio methodica, &c.* Lips., 1826.

|| *Genera et Species Curculionidum, &c.*, 1833, &c.

¶ *Précis d'un nouvel Arrangement de la Famille des Brachélytres, de l'Ordre des Insectes Coléoptères.* St. Petersb. 1830.

** *Orthoptera Sueciæ.* Lund., 1821.

†† *Revue Méthodique des Insectes de l'Ordre des Orthoptères.* (See *Ent. Mag.*, vol. i. p. 304.)

‡‡ *Die Wanzenartigen Insecten, &c.*, Nürnberg, 1831, &c. (See *Ent. Mag.*, vol. i. p. 308.)

§§ See *Ent. Mag.*, vol. i. p. 307.

|||| *Die Schmetterlinge Europ., mit Fortsetzung von F. Treitschke.* 1825, &c.

¶¶ Godart and Duponchel, *Hist. Nat. des Lepidoptères, ou Papillons de France.* 8vo, Par., 1822, &c.

*** *Essai sur une Monographie des Zygénides.* Par., 1829, 8vo.

††† *Hist. Génér. et Iconograph. de tous les Lepidop. et Chenilles de l'Amér. Septentrionale.* 8vo.

‡‡‡ See *Ent. Mag.*, vol. ii. p. 110.

Léconte, in the latter by Rambur and Graslin. Dr. Horsfield has thrown much light upon the arrangement and affinities of these insects in his *Lepidoptera Javanica*, already alluded to in a former part of this Report.

The *Neuroptera* have been particularly attended to by Toussaint Charpentier and Vander Linden, who have each published a monograph on the European *Libellulæ*: that of the former is contained in his *Horæ Entomologicæ*. The *Phryganeæ* (*Trichoptera*, Kirb.) form the subject of an elaborate and valuable work recently published by M. Pictet of Geneva*.

The only recent works devoted to the *Hymenoptera*, with which I am acquainted, are those of Lepelletier de St. Fargeau, Gravenhorst, and Nees ab Esenbeck. The first has published a monograph on the *Tenthredinidæ*†. The second has treated at great length of the European species of *Ichneumonidæ*‡. The third has written upon the more aberrant groups of the family just mentioned§.

The *Diptera* have received great attention of late years from several excellent entomologists. Fallen's *Diptera Sueciæ* is rather anterior to the period of time we are considering. Wiedemann's *Diptera Exotica* ||, Meigen's *Diptera* of Europe¶, and Macquart's *Diptera* of the North of France**, are of more recent date, and have greatly contributed, the last two especially, to advance our knowledge of this order of insects. I may also allude to a most elaborate work by Robineau-Desvoidy, which though treating only of the Fabrician genus *Musca*, contains descriptions of nearly 1800 species, referred to nearly 600 genera. This astonishing production, which is entitled *Essai sur les Myodaires*, occupies the entire second volume of the *Mém. des Savans E'trang.*, published in 1830.

Besides the above works, I may mention Stephens's *Illustrations of British Entomology*, now in course of publication in our own country, as one which promises great additions to all the orders. The *Coleoptera* and *Lepidoptera* have already appeared. Curtis's *British Entomology* is confined to the illustration of the genera of British Insects, but as a work in the

* *Recherches pour servir à l'Histoire et à l'Anatomie des Phryganides.* 4to, Genève, 1834. (For an analysis of this work, see *L'Instit.*, No. 73.)

† *Monographia Tenthredinetarum Synonymia extricata.* Par., 1823, 8vo.

‡ *Ichneumonologia Europæa.* Vratislav., 1829, 3 vols. 8vo.

§ *Hymenopterorum Ichneumonibus affinium, Monographiæ, Genera Europæa et Species illustrantes.* vol. i. Stuttgart. et Tubing, 1834.

|| *Aussereuropaische Zweiflugelige Insecten.* Hamm, 1828—1830, 2 vols. 8vo.

¶ *Systematische Beschreibung der bekannten Europaischen Zweiflugeligen Insecten.* Aachen, 1818—1830, 6 vols. 8vo.

** Published in the *Recueil des Travaux de la Société d'Amat. des Sciences, &c., de Lille.* 1826—1829.

illustrative department, is unrivalled in the beauty and accuracy of its delineations. It is also extremely valuable from the number of dissections which it contains.

There are also many other valuable monographs, not published separately like those already alluded to, to be found in Germar's *Magazin der Entomologie*, Guerin's *Magasin de Zoologie*, Silbermann's *Révue Entomologique*, in the *Entomological Magazine*, and in the *Annales de la Soc. Entomologique de France*.

In concluding my remarks on this department of zoology, I may observe that it has received a powerful impulse from the recent establishment of two Entomological Societies, one in France, and the other in our own country. This last was only instituted in 1833*.

III. Mollusca, *Cuv.*

It is undoubtedly to the researches of Poli, Cuvier, Lamarck, Férussac, and Blainville that we are to attribute the great advance which has been made of late years in our knowledge of the animals belonging to this type. Poli's work, consisting of two volumes, on the anatomy of the Bivalve and Multivalve *Testacea*, is well known. In 1826, a third volume was published by Chiage, in which the anatomy of the Univalves was commenced upon the same plan as that adopted in the two former volumes. Cuvier's *Memoirs on the Mollusca*, most of which had been previously inserted in the *Annales du Muséum*, were in 1816 collected by himself into one volume and published separately. They contributed greatly to our better knowledge of the natural affinities of these animals, and furnished the basis of the system developed the year following in the *Règne Animal*. In this last work the *Mollusca* are divided into six classes†, *Cephalopoda*, *Pteropoda*, *Gasteropoda*, *Acephala*, *Brachiopoda*, and *Cirrhopoda*, the characters being derived from the general form, between which and the internal structure Cuvier observes there is a pretty constant relation. The *Cephalopoda* are simply divided into genera according to the nature of the shell. The *Pteropoda*, a class instituted by himself in 1804 for the reception of the genera *Clio*, *Pneumoderma*, and *Hyale*, are divided into two sections, founded on the presence or absence of a distinct head. The *Gasteropoda* are distributed under seven orders, characterized according to the position and form of the respiratory organs. The *Acephala* comprise the two orders of *Testaceous* and *Naked Acephala*. The *Brachiopoda* include the genera

* Since this Report was read, the Entomological Society of London has published the first part of a volume of Transactions, containing several interesting and important communications on this branch of Zoology.

† Three of these classes, *Cephalopoda*, *Gasteropoda*, and *Acephala*, had been established by Cuvier in his *Tabl. Élém. de l'Hist. Nat.* in 1798.

Lingula, *Terebratula*, and *Orbicula*, which had previously formed a part of the class last mentioned. The *Cirrhopoda* comprise the two genera *Anatifa* and *Balanus*, which Cuvier considers as in some respects intermediate to the Molluscous and Articulated Animals.

The benefits conferred upon this department of zoology by Lamarck belong to a period of time somewhat anterior to the publication of the *Règne Animal*. We may, however, make a few remarks on the system adopted in the fifth and two succeeding volumes of the second edition of the *Animaux sans Vertèbres*, which appeared in the years 1818—1822. Perhaps it is in the details of the science, the grouping of genera, and the characterizing an immense number of new species, that Lamarck's tact and penetration appear most conspicuous. His leading divisions present several peculiarities which are scarcely warranted by the organization of these animals. Thus, he has separated altogether from the *Mollusca* the *Naked Acephala*, and made of them a distinct class under the name of *Tuniciers*, which he refers to quite another place in his system, below the Articulated Animals which intervene. Again, the rest of Cuvier's *Mollusca* he divides into only three classes, which we are naturally led to infer he considers therefore as groups of equal value. The first is that of *Cirripèdes*. The second, or *Conchifères*, answers to the *Testaceous Acephala* of Cuvier, including also the *Brachiopoda*. The third, to which Lamarck restricts the name of *Mollusques*, comprises all the remaining classes of the *Règne Animal*. The ground of primary subdivision in Lamarck's second class is more entitled to our regard than that on which his higher groups are established, although not particularly noticed by Cuvier. It is the number of the muscles of attachment and the impressions caused by them on the shell, points to which Lamarck was the first to call the attention of naturalists in a memoir in the *Ann. du Mus.* for 1807. These give rise to the two orders of *Dimyaires* and *Monomyaires*. The secondary groups in this class are founded on the form and structure of the shell, the situation of the ligament, and the form of the foot of the animal; the families resulting from these principles of arrangement being on the whole natural, though not in all cases distinguished by characters of the same importance. The third class, *Mollusques*, is divided into five orders, one of which answers to the class *Pteropoda* of Cuvier, and another to the *Cephalopoda* of the same author: the remaining three are formed out of Cuvier's class *Gasteropoda*, and bear the names of *Gastéropodes*, *Trachélipodes*, and *Hétéropodes* respectively. In this part of his system Lamarck has not only altered the value of some of Cuvier's groups, but adopted peculiar views with regard to their

relative degrees of organization. Thus, he considers the *Heteropoda*, comprising the genera *Carinaria*, *Firola*, &c., as deserving to be placed at the head of all the *Mollusca*, and as forming the transition to the Fish, an opinion which few will be inclined to adopt besides himself.

In 1819 appeared the first numbers of that splendid work which M. de Férussac has devoted to the Land and Freshwater *Mollusca*, a work which for beauty as well as accuracy of illustration has perhaps never been surpassed. It is principally, indeed, to this department of the subject that De Férussac's labours have been directed, and no one has done more towards elucidating the history of that immense assemblage of species which belong to the Linnæan genus *Helix*. In order, however, to point out the relation between the land and freshwater genera and the rest of the *Mollusca*, he has added a general arrangement of all the Molluscous animals, which though nearly the same as that of Cuvier, presents nevertheless two or three slight modifications. Thus, before arriving at the classes, we have a primary division into two sections, grounded on the presence or absence of the head. The first section, or that of *Cephalous Mollusca*, includes the first three classes of Cuvier. The second, or *Acephalous* section, comprises the classes *Cirripeda*, *Brachiopoda*, *Lamellibranchia* (name taken from Blainville), and *Tunicata*, this last being admitted as a group of a higher denomination than that assigned to it by Cuvier. There is also a slight difference in the subordinate divisions. Thus, the *Cephalopoda* are divided into the two orders of *Decapoda* and *Octopoda**. Amongst the *Gasteropoda*, we find a new order established for the reception of the *Operculated Pulmonifera*. It may be stated that Férussac's work, which for some time was interrupted, has been recently recommenced, and it is much to be desired that it may yet be completed according to the original plan.

In 1820, Schweigger published in Germany a Manual of the Inarticulate Invertebrate Animals†. In this work, which I have not seen, the arrangement of the *Mollusca* is said to be on the whole similar to that of the *Règne Animal*.

In 1821, Mr. Gray published in the *London Medical Repository*‡ a new systematic arrangement of the *Mollusca*, founded upon the internal organization. In this system, one of the principal features is an entirely new nomenclature for the primary divisions, which constitute seven classes, in other respects

* These groups are adopted from Dr. Leach. See his "Synopsis of the Orders, Families, and Genera of the Class *Cephalopoda*," in his *Zool Miscell.*, vol. iii. p. 137. † *Handbuch der Naturgeschichte, &c.* 8vo, Leips. 1820.

‡ vol. xv. p. 229.

nearly the same as those of former authors. The *Cirripeda*, however, are not included. The groups subordinate to the classes are established principally upon the organs of respiration. The arrangement of the families and genera of the *Gasteropoda* is grounded upon the form of the opercle, which leads in many cases to very natural relations. Mr. Gray has the merit of having studied this part more profoundly than any of his predecessors.

In 1824, M. Latreille published in the *Ann. des Sci. Nat.** a sketch of a new arrangement of the *Mollusca*, which was more developed the following year in the *Familles Naturelles*. In this last work, the primary division of these animals (from which the *Naked Acephala* and *Cirripeda* are entirely excluded,) is into *Phanerogama* and *Agama*, the former including all those in which copulation is necessary in order to reproduction, the latter such as impregnate themselves. The *Phanerogama* are further divided into two large sections, the characters of which are derived from the organs of motion. The first of these, which is termed *Pterygia*, includes two classes, the *Cephalopoda* and *Pteropoda* of Cuvier. The second, *Apterygia*, includes the class *Gasteropoda* of the same author. In this last class, before arriving at the orders, which are characterized from the organs of respiration, there is a subdivision according as the sexes are separate, or united in the same individual. In the second great division, or that of *Agamous Mollusca*, we likewise find two sections, grounded upon the presence or absence of an apparent head. The first, *Exocephala*, comprises a new class, called *Peltocochlides*, established for the reception of the *Gasterop. Scutibranchia* and *Cyclobranchia* of Cuvier. The second, *Endocephala*, includes the *Brachiopoda* and *Testaceous Acephala* of Cuvier, Lamarck's name of *Conchifera* being adopted for the class last mentioned.

In 1825 appeared the *Malacologie* † of Blainville, who had already contributed many valuable memoirs to the *Journ. de Physique* and *Bull. de la Soc. Phil.* on this department of zoology. No one, after Poli and Cuvier, has done so much as Blainville in illustration of the anatomy of the *Mollusca*. At the same time his arrangement, which differs in several respects from all preceding ones, can hardly be considered as preferable to that of the *Règne Animal*. It has also the disadvantage, like all the rest of his system, of being attended by a peculiar nomenclature, embracing many names for the primary groups entirely

* tom. iii. p. 317.

† *Manuel de Malacologie et de Conchyliologie*. 8vo, Paris, 1825. The greater part of this work had previously appeared in the *Dict. des Sci. Nat.* under the Art. Mollusques.

different from those generally adopted. Blainville's primary subdivision of his type *Malacozoaires* is into three classes, established upon the characters of the head. In the first class, *Céphalophores*, which answers to the *Cephalopoda* of Cuvier, the head is well distinguished from the body. In the second, *Paracéphalophores*, it is less strongly marked. In the third, *Acéphalophores*, it can be no longer observed. The *Paracéphalophores* include the *Gasteropoda* and *Pteropoda* of Cuvier, though arranged upon a very different plan, the characters of the subordinate groups being derived in the first instance from the reproductive organs, and afterwards from the respiratory organs. Thus we have the three subclasses of *Paracéph. Dioiques*, *P. Monoiques*, and *P. Hermaphrodites*, each of which is divided into two or more orders, according to the structure of the branchiæ. The third class, *Acéphalophores*, is divided immediately into four orders, which are likewise characterized from the respiratory organs. The first of these orders, *Palliobranches*, answers to the *Brachiopoda* of Cuvier; the second, *Rudistes*, comprises the Lamarckian family of bivalve *Mollusca* bearing the same name; the third, *Lamellibranches*, includes the great bulk of Cuvier's *Testaceous Acephala*; and the fourth, *Hétérobranches*, his *Naked Acephala*. Blainville does not include either the *Cirripeda* or the *Chitones* amongst his true *Malacozoaires*, but regards them as forming a subtype, *Malentozoaires*, leading directly off to the Articulate Animals. In this group they constitute the two orders of *Nématopodes* and *Polyplaxiphores* respectively.

The latest systematic work in this department with which I am acquainted, is the excellent little *Manuel des Mollusques* * by M. Rang, published in 1829. This gentleman is also the author of a valuable monograph on the genus *Aplysia*†, as well as of some other important memoirs relating to the *Mollusca*. His arrangement of these animals is nearly the same as that of the *Règne Animal*. At the same time there are some alterations with respect to the primary divisions. Thus, he sinks the class *Brachiopoda*, regarding that group as only an order among the *Acephala*, in which last class he admits as another additional order the *Rudistes* of Blainville. He has also adopted some new orders in the class *Gasteropoda*. Some of his families and other subordinate divisions he has borrowed from Lamarck and Férussac. This work contains many new and original observations.

The arrangement of the *Mollusca* in the second edition of the

* *Manuel de l'Histoire Naturelle des Mollusques et de leurs Coquilles, &c.* Paris, 1829. † *Histoire Naturelle des Aplysies.* Paris, 1829, fol.

Règne Animal, also published in the year 1829, does not differ materially from that in the first. There are simply two additional orders in the class *Gasteropoda*; one, named *Tubulibranches*, including the genera *Vermetus*, *Magilus*, and *Siliquaria*; the other, that of *Hétéropodes*, adopted from Lamarck.

From a review of the above systems, which have been briefly sketched out in the preceding pages, it would seem that even the primary groups in this branch of the animal kingdom are not all determined with certainty. At the same time it is probable that whatever alterations may be suggested by further researches, they will not greatly interfere with those established by Cuvier, and adopted with more or less modification by the generality of naturalists. What we most want is a more exact determination of their relative values. The *Cirripeda*, however, probably do not belong to the Molluscous type at all, as appears from researches to be further alluded to hereafter. There is also great uncertainty with respect to the exact situation, as well as limits, of some of Cuvier's smaller groups, such, for instance, as his *Gasteropoda Cyclobranchia* and *Scutibranchia*, of which Latreille makes a distinct class. The genera *Capulus*, *Crepidula*, *Navicella*, and *Calyptræa*, which are by most authors referred to the *Scutibranchia*, and which Cuvier himself placed in that order in the first edition of the *Règne Animal*, in the second he has referred to the *Pectinibranchia*, stating it as his opinion that they come near the *Trochidæ*. Indeed, in none of the classes has the chain of affinities been hitherto worked out with any degree of certainty. We still require further anatomical investigations, both in order to determine with more exactness the actual structure of many entire families, and to learn the relative importance of those organs from which naturalists have drawn their principal characters. Where we find the organs of motion, circulation, and respiration, as well as the mode of reproduction, all varying to the degree they do in these animals, it is clear that we must proceed with great caution in endeavouring to ascertain the respective degrees in which they are entitled to our confidence.

Before, however, quitting this division of the subject, it will be right to notice several important memoirs which have appeared of late years, connected with the structure and affinities of some of the above classes in particular.

1. *Cephalopoda*.—All, except Lamarck, allow that this class stands at the head of the Inarticulate *Invertebrata*, although it is not decided to which of the Vertebrate classes it shows most affinity. Cuvier, who was the first to make us acquainted with the anatomical details of these animals, and who has particularly noticed the striking development of some parts of their organization, nevertheless does not allow that they conduct to

any other groups placed higher in the system *. Mr. MacLeay has endeavoured to show that in their general structure they make the nearest approach to the Chelonian Reptiles †. He allows, however, that the hiatus occurring between is very considerable. M. Latreille, in a memoir published in 1823 ‡, has pointed out several resemblances between them and Fish, and thinks that they show considerable affinity to the Rays and other Cartilaginous Fishes. These resemblances refer exclusively to the external structure of the two classes. More recently the *Cephalopoda* have been much investigated by MM. Laurencet and Meyraux. In a memoir read to the Royal Academy of Sciences at Paris in 1830 §, these naturalists attempted to lessen the gap that was supposed to exist between them and the *Vertebrata*, in like manner as Geoffroy had previously done with respect to the gap between these last and the *Annulosa*. They would demonstrate that the plan upon which the *Cephalopoda* are constructed does not depart so widely as was imagined from that of the structure of the *Vertebrata*; that the same organs appear in both groups, though somewhat modified and transposed; and that in order to make the structures conformable, we are only to suppose a vertebrate animal doubled back upon itself, when the relative position of the several organs in this last will be essentially the same as in a Cephalopod. Geoffroy, in his report on this memoir to the French Academy, took occasion to observe how favourable the results at which these anatomists had arrived were to his peculiar views respecting the *unity of composition* in the animal kingdom. Cuvier, who was opposed to these views, replied to Geoffroy; and for some time after a sharp controversy was kept up between these two distinguished naturalists on this subject. To state the several memoirs, and verbal communications to the Royal Academy of Sciences, which arose on both sides of this question, would lead us too far from the present subject ||. We may mention, however, one memoir by Cuvier, in which he states, with reference to the singular opinion advanced by Laurencet and Meyraux, the results of a rigid comparison which he actually made between a Cephalopod and a Vertebrate Animal doubled back in the manner they di-

* *Mém. sur les Céphalop., &c.*, p. 43. † *Hor. Ent.*, p. 254 to 258.

‡ *Mém. de la Soc. d'Hist. Nat. de Paris*, tom. i. p. 269.

§ *Quelques Considérations sur l'Organisation des Mollusques.* I am ignorant as to whether this memoir has been hitherto published.

|| Geoffroy's memoirs were afterwards collected by himself into one volume, and published under the following title: *Principes de Philosophie Zoologique, discutés en Mars* 1830, *au sein de l'Acad. Roy. des Sciences.* Par. 1830, 8vo. Cuvier also expressed a determination to publish his under the title of *De la Variété de Composition des Animaux.* I am not aware, however, that these last ever appeared.

rect. This memoir, which was published in the *Ann. des Sci. Nat.* *, is illustrated by coloured sections of the two animals, and its author shows that there are still many organs present in each not found in the other, and that many of those common to both are not, as was supposed would be the case, in the same relative situation. In short, he attempts to demonstrate that, pushed beyond a certain point, the analogy utterly fails. During last year (1833) a second memoir appears to have been read by M. Meyraux on these animals †, in which he still retains his former theory, and, moreover, expresses an opinion that the *Cephalopoda* ought to constitute an intermediate class between the *Mollusca* and the *Vertebrata*, their general organization departing much from the type of the former division, at the same time that it approaches that of the latter. This is in accordance with the opinion formerly advanced by Mr. MacLeay, who in his *Hor. Entom.* considered the *Cephalopoda* as constituting an osculant group between the two large divisions just mentioned ‡. Like Mr. MacLeay, M. Meyraux would seem also to consider them as showing considerable affinity to the Chelonian Reptiles. Perhaps, however, the final elucidation of this point must wait for the discovery of some intermediate form, which it is not too much to hope may yet occur at some future period.

A few other memoirs require to be pointed out as valuable contributions to our knowledge of this class, although not connected with the subject particularly discussed in those just alluded to. Foremost amongst these is a memoir by Mr. Owen on the *Pearly Nautilus*, published in 1832 §. This very valuable treatise contains a detailed account of the anatomy of the animal inhabitant of the above shell, so often sought for since the time of Rumphius, its original but imperfect describer. The specimen dissected, which is the only one that has been discovered in modern times ||, notwithstanding the frequent occurrence of the shell itself, was taken by Mr. George Bennett off the New Hebrides in 1829. Mr. Owen has shown that its organization, although exhibiting some differences, more par-

* tom. xix. p. 241.

† See *L'Institut*, No. 21, p. 180. I only know the memoir from the analysis which is there given of it.

‡ Meckel is also stated to have proposed the making a distinct division of the *Cephalopoda*, intermediate to the *Vertebrata* and *Invertebrata*. I am unable, however, to refer to the work in which he has advanced this proposal.

§ *Memoir on the Pearly Nautilus* (Nautilus Pompilius, *Linn.*), *with illustrations of its external form and internal structure.* Lond. 1832, 4to.

|| A fragment of a Cephalopod animal, supposed to belong to the *Nautilus Pompilius*, was brought from the Moluccas by MM. Quoy and Gaimard, and described in the *Ann. des Sci. Nat.* (tom. xx. p. 470.), but there are great doubts as to its identity with that species.

ticularly in the respiratory and circulatory systems, is on the whole strictly conformable to that of the higher *Cephalopoda*, between which and the *Gasteropoda* it constitutes an osculant form*. At the conclusion of his memoir Mr. Owen has given the characters of two orders, *Dibranchiata* and *Tetrabranchiata*, into which he proposes to divide the *Cephalopoda*, these characters being founded on the details of the organization of the *Nautilus Pompilius*.

Dr. Grant has also added considerably to our knowledge of the structure of this class. In the *New Edinb. Phil. Journ.* † he has given the anatomy and external characters of an apparently new species of *Octopus* ‡ from the Frith of Forth. In the *Zool. Trans.* § he has also published an account of the genus *Loligopsis* of Lamarck, the very existence of which was before disputed by some naturalists: he has examined its structure, and found it to constitute a new form in this class, possessing characters hitherto known only in the Testaceous Cephalopods, with others common in the naked species. In the same volume ‖ is a second paper by this distinguished naturalist on the anatomy of the *Sepiola vulgaris*.

The controversy respecting the animal inhabitant of the *Argonaut* is not yet decided, at least not to the entire satisfaction of all parties. Future observation will, however, probably confirm the opinion of Poli ¶ and Férussac **, that the animal hitherto alone found in that shell (*Ocythoë*) strictly belongs to it. The former author expresses himself decidedly with respect to this point, asserting that he has traced the gradual development of the shell from the egg. Mr. Broderip appears still to entertain doubts on the subject, but the evidence which he has advanced on the other side of the question is simply negative ††.

* This circumstance seems to point out the impropriety of considering the *Cephalopoda* as a distinct division of the animal kingdom, according to the views of Meckel, Laurencet, and Meyraux.

† 1827.

‡ According to De Férussac, under the names of *Octopus vulgaris*, *Loligo vulgaris*, and *Sepia officinalis*, several very distinct species of *Cephalopoda* have been hitherto confounded.

§ 1833, vol. i. p. 21.

‖ p. 77.

¶ See *Ann. des Sci. Nat.* (1825), tom. iv. p. 495.

** *Mém. de la Soc. d'Hist. Nat. de Paris*, tom. ii. p. 160.

†† See *Zool. Journ.* vol. iv. p. 57. Mr. Gray is also of opinion that the *Ocythoë* is only parasitic in the shell of the *Argonauta*; and I may state, that since this Report was read he has brought forward what he considers as a new argument in support of this side of the question. This argument is founded on the size of what Mr. Gray terms the *nucleus* of the shell, or that original portion of it which covered the animal within the egg, and which in some specimens of young shells of *Argonauta Argo* and *A. hians*, lately exhibited to the Zoological Society, he has shown to be many times larger than the largest eggs of the *Ocythoë* found within the Argonaut shells. From this Mr. Gray has inferred

Great additions have been made to our knowledge of the minute Polythalamous *Cephalopoda* by M. D'Orbigny, whose memoir on these animals, read to the French Academy of Sciences in 1825, will be found in the seventh volume of the *Ann. des Sci. Nat.* He confirms the propriety of assigning them a place in this class, to which they had been referred previously, more from analogy than from any positive knowledge of their real characters. He has studied far more closely than any former observer the structure and development of the shell in this group, as well as in many cases the structure of the animal. He has ascertained that the former is internal, or at least entirely covered by a membrane, and destitute of a siphon; and that the latter is possessed of true arms, or tentacula, analogous to those of the larger *Cephalopoda*. He considers these animals as forming a large and well-marked group in the present class, to which he assigns the name of *Foraminifera*. He is acquainted with upwards of six hundred species, nearly half of which have been discovered by himself.

M. D'Orbigny has undertaken an arrangement of these shells, which has led to a revision of that of the entire class of *Cephalopoda* by himself and De Férussac jointly. It is the intention of these authors to publish an extensive work * on this class, which D'Orbigny divides into the three orders of *Cryptodibranchia, Siphonifera*, and *Foraminifera*. In the *first*, the shell is either monothalamous, or internal and rudimentary, never polythalamous: in the *second*, polythalamous, external, or partially covered by the animal, which is capable of retiring either wholly or in part within the chamber above the last septum; a siphon always continuous from one chamber to another: in the *third*, the shell is polythalamous, and always internal; the last septum terminal; no siphon, but only one or more apertures causing a communication between the different chambers †. It may be observed that this arrangement by D'Or-

that it must have been produced by an animal whose eggs are of much greater magnitude, and that therefore the *Ocythoë* cannot be the true artificer of the shell in question. Mr. Gray's communication on this subject, which is not yet published, will shortly appear in the *Proceedings of the Zoological Society.*

* Since this Report was read, I have seen the first three numbers of this splendid work which have recently appeared under the following title: *Monographie des Céphalopodes Cryptodibranches, par MM. De Férussac et D'Orbigny.* Paris, 1834, fol. The plates are extremely beautiful. The *Ceph. Siphonifera* and the *C. Foraminifera* are to form the subjects of two other distinct monographs.

† The same year in which D'Orbigny brought forward his memoir, De Haan published at Leyden an important treatise, entitled, *Monographiæ Ammoniteorum et Goniatiteorum Specimen.* In this work, which I have not seen, there is said to be also a new arrangement of the *Cephalopoda*, and a similar division

bigny has been adopted by Rang in his *Manuel des Mollusques* already alluded to.

2. *Pteropoda.*—De Férussac has given a systematic arrangement of this class in the *Bull. des Sci. Nat.* for 1827 *. Rang has made several important additions to it, as well as recorded many valuable observations respecting genera and species which were already known. Nevertheless we have still but an imperfect knowledge of this group.

3. *Gasteropoda.*—This being the typical and the most extensive class among the *Mollusca*, it has received more general attention than any of the others. Many of the families and genera contained in it have been made the subject of valuable monographs by different individuals, which, however, it would lead too much into detail to allude to more particularly. Naturalists do not appear to be agreed as to the exact value of characters derived from the shell in distinguishing the genera of this class. M. Deshayes, in a paper in the *Ann. des Sci. Nat.* for 1831 †, has recorded some anatomical details, which would seem to have been undertaken with the view of throwing some light on this matter in the case of the *Helices*. His object is to discover whether there may not be found some peculiarity in the internal structure of the animal sufficient to warrant the adoption of many genera in this family, which having been established solely upon the characters of the shell, have not hitherto been received by all naturalists. I am not aware, however, that he has carried on this investigation beyond the case of Draparnaud's genus *Succinea*, which is the only one treated of in the above paper.

The opercle of shells, which, as already stated, has been much employed by Mr. Gray in his arrangement of the *Gasteropoda*, has been since studied with great care by Blainville, who in a memoir in the *Bull. de la Soc. Philom.* for 1825 ‡, proposes to adopt characters derived not merely from the presence or absence of this part, but from its form and structure, its position, and mode of attachment to the animal. In the *Ann. des Sci.* for 1829 §, Dugès has also a paper on this subject. His principal object is to trace the analogies between this part and the upper valve of the Inæquivalve *Acephala*, more particularly as respects its mode of growth, and the production of the striæ on

of the Testaceous genera into two groups, characterized by the presence or absence of a siphon. I believe De Haan was the first to make use of this character, although D'Orbigny is said to have had recourse to it without any knowledge of De Haan's work. See *Dict. Class. d'Hist. Nat.*, tom. xi. p. 56.

* tom. xii. p. 345. † tom. xxii. p. 345. ‡ pp. 91 and 108.
§ tom. xviii. p. 113.

its surface. As these striæ, however, have been used in some cases for characterizing the genera of the Pectinibranchiate *Gasteropoda*, this memoir is not without its importance to the systematist. During the last year Mr. Gray has again turned his attention to this subject. In a paper in the *Phil. Trans.* for 1833, he has detailed some observations on the structure of the part in question, as well as on the structure and œconomy of shells in general. He considers that the mere fact of the presence or absence of the opercle is of small importance, but that in its form and structure it offers some of the most constant characters for the distinction and arrangement of families and genera.

4. *Brachiopoda.*—Mr. Owen has recently published * an important memoir on the anatomy of this group, in which he has offered some remarks with respect to its value and affinities. He observes that in all essential points of structure these animals closely correspond with the *Acephalous Mollusca*, although inferior to the *Lamellibranchia* as far as regards their respiratory and vascular systems. He considers them as holding a middle place between these last and the *Tunicata*; not, however, possessing characters of sufficient importance to justify their being regarded as a distinct class, but forming a separate group of equal value with those above mentioned.

5. *Tunicata.*—Whether we admit this group as a class or only as an order, it is one which will always possess interest as affording a natural passage to the *Radiata* of Cuvier. It is especially to the researches of the naturalist just mentioned, and to those of Savigny, that we are indebted for the first accurate knowledge obtained respecting these animals. While the structure of the simple *Tunicata* was beautifully illustrated by the dissections of the former †, the latter had the merit of discovering the true organization of those singular compound *Ascidiæ* which until his time had always been confounded with the zoophytes ‡. Péron, Desmarest, and Lesueur have all likewise contributed to render this group better understood. What recent additions have been made to our knowledge of it are due principally to Mr. MacLeay, MM. Quoy and Gaimard, MM. Audouin and Edwards, and Dr. Meyen. Mr. MacLeay is the author of a paper, read to the Linnæan Society in 1824 §, in which he has given the anatomical details of some new forms from the Northern seas, at the same time that he has thrown

* *Zool. Trans.* 1834, vol. i. p. 145.

† See *Ann. du Mus.*, tom. iv., and *Mém. du Mus.*, tom. ii. p. 10.

‡ *Mém. sur les An. sans Vertèb.*, Part 2.

§ See *Linn. Trans.*, tom. xiv. p. 527.

out several remarks respecting the arrangement and affinities of these animals in general. Quoy and Gaimard have communicated some new observations relating to the habits and anatomy of the *Salpæ* which they made during their voyage with Freycinet *. Audouin and Edwards, who paid great attention to the *Compound Ascidiæ* during their residence on the Chausey Islands, have made some interesting discoveries respecting the mode of development of these animals †. They have ascertained that, although in their adult state they are united to form one common mass, and are immoveably fixed to some rock or other marine substance, they enjoy at birth a separate individuality, and are, moreover, endued with the power of swimming freely in the water from place to place. It is not till after two days that this locomotion ceases. They then seek a place favourable to their further development; and while some return to the parent mass from which they first emanated, others attach themselves afar off and found new colonies. These observations are of great value. They not only throw light upon the history of these animals, but serve to establish very important relations between them and other groups in which similar facts have been noticed, connected with the early development of the young. Dr. Meyen's researches are confined to the genus *Salpa*, which forms the subject of a memoir by him in the *Nov. Act. &c. Nat. Cur.* for 1832 ‡. He has revised the characters of more than thirty species.

6. *Cirripeda.*—The doubtful situation of this class has been already alluded to. Indeed there are few groups whose true affinities have been involved in so much uncertainty. The most recent observations, however, seem decidedly in favour of the opinion of those naturalists who regard it as partaking more of the *Annulose* than the *Molluscous* structure, and approaching, on the whole, nearest to the *Branchiopod Crustacea.* Straus was the first to announce this affinity in his memoir on the genus *Daphnia*, published in 1819. He was led to observe it from a comparison of the relative structures of the genera *Pentelasmis* (Leach) and *Limnadia* (Brong.). Two years afterwards, Mr. MacLeay, apparently without knowledge of Straus's memoir, pointed out the same relationship §, dwelling, however, more particularly on the affinity between *Pentelasmis* and *Daphnia.* I am not aware that anything further was written on this subject till 1830, in which year Mr. Thompson published the third

* *Ann. des Sci. Nat.* (1825), tom. vi. p. 28.; and *Bull. de la Soc. Philom.* (1826), p. 123.

† See *Ann. des Sci. Nat.*, tom. xv. p. 6.

‡ tom. xvi. p. 363.

§ *Hor. Ent.*, p. 308.

number of his *Zool. Researches*, containing some observations on the *Cirripeda* which appear to be quite decisive of their close affinity to the *Annulosa* in general, and the *Branchiopod Crustacea* in particular. This gentleman asserts that he has observed that these animals undergo a metamorphosis. He states having discovered swimming freely in the sea a small crustaceous animal furnished with a shell composed of two valves like those of *Daphnia*; that being desirous of watching it further, he kept it in water, and was much surprised, after a few days, at seeing it throw off its bivalve shell, attach itself to the bottom of the vessel, and become transformed into the *Balanus pusillus* of Pennant *. For some time afterwards these alleged facts were thought to require confirmation from other observers; more especially as in a communication made to the Zoological Society last year †, Mr. Gray advanced some statements respecting the condition of the young of *Balanus Cranchii* (Leach) observed *in ovo*, as well as of the young of the genera *Pentelasmis* and *Otion*, which appeared to militate against the accuracy of Mr. Thompson's views. They have, however, been fully established by Dr. Burmeister, who has recently published a treatise on these animals announcing this circumstance; and judging from his own observations, combined with those which had been previously made by others, Dr. Burmeister infers that the *Cirripeda* ought to be arranged with the *Crustacea*, forming a particular tribe in that class ‡.

It may be stated that M. Martin-St.-Ange is said to be engaged in a work on the organization and affinities of the *Cirripeda*. The results of his researches have been already given to the public in a memoir read to the Royal Academy of Sciences at Paris towards the end of last year §. They likewise favour the opinion that these animals, at least the pedunculated genera, are truly articulated, and allied to the lower forms of *Crustacea*. M. Martin-St.-Ange thinks that they also show some points of affinity to the *Annelida*.

* It is a curious fact that, according to Mr. Thompson, the young animal should not only possess the power of locomotion, which is denied to the adult, but *distinct organs of sight*, which, after the transformation into *Balani*, gradually become obliterated. This is analogous to Edwards's observation (already alluded to) in the case of the development of the *Cymothoæ*. It is, however, yet more striking.

† See *Proceed. of Zool. Soc.* (1833), p. 115.

‡ The above statements are on the authority of De Férussac's Introduction to his recently published Monograph on the *Cephalopoda*. I have not seen Burmeister's work myself, which is said to be entitled *Beitrage zur Naturgeschichte der Rankenfüsser*. 4to, Berlin, 1834.

§ See *L'Institut*, No. 27. p. 226, and No. 62. p. 231.

The classification of the *Cirripeda* was greatly advanced by the labours of Dr. Leach, who made a particular study of this class, and instituted several new genera in it. His arrangement is founded upon characters derived from the shelly covering of these animals, which he submitted to a more minute and rigorous analysis than any previous observer had done before him *.

Mr. Gray has also attended to this subject. In the *Ann. of Phil.* for 1825 †, he has published a synopsis of the genera arranged in natural families.

IV. Radiata, *Cuv.*

As we descend the scale of organization we find the groups defined with less and less certainty. In the present division, our knowledge of their exact limits, we may even say of the number of primary types of form which this division comprises, is so imperfect, that it would be to little purpose to detail all the different arrangements which have been proposed for these animals, the classification of which is probably still destined to undergo great and important revolutions. After all, it is doubtful whether we must not admit with MacLeay that they form two groups, each of equal value with that of the *Vertebrate*, *Annulose*, and *Molluscous* divisions, instead of one only as Cuvier supposes. In this state of uncertainty, I shall merely take Cuvier's classes in the order in which they stand in the *Règne Animal*, and under each state some of the principal additions which have been made of late years to our general knowledge of these animals. This will naturally lead to the mention of several important steps which have been gained towards an improved classification of them.

The following are the classes into which Cuvier divides the Radiata: *Echinodermes*, *Intestinaux* (*Entozoa*, Rudolp.), *Acalèphes*, *Polypes*, and *Infusoires*.

1. *Echinodermata.*—To our knowledge of this class I am not aware of many important additions that have been made recently. Since the publication of Tiedemann's work on the anatomy of these animals, which gained the prize from the French Institute in 1812, and which served to clear up many points in the details of their organization, no one appears to have studied their structure more deeply than Delle Chiaje. Several memoirs have appeared by this last author ‡ treating of the genera *Echinus*,

* See the article Cirripedes in the *Suppl. to the Encycl. Brit.* Also *Zool. Journ.* (1825), vol. ii. p. 208.

† vol. xxvi. p. 97.

‡ *Memorie sulla Storia e Notomia degli Animali senza Vertebre.* 4to, Nap. 1823, &c.

Asterias, *Holothuria*, and *Siphunculus*, all which he has submitted to a close investigation. His researches on the genus *Siphunculus* lead him to think that this group has been wrongly placed by Cuvier in the present class, and that it belongs more properly to the *Annelida*.

In 1827, Mr. Thompson published an account of a newly discovered recent species of *Pentacrinus**, a genus well known in a fossil state, but one of which the true situation in the system was before rather doubtful. From an examination of this species, the structure of which in its several stages of development he has given in full detail, Mr. Thompson fully proved that the *Crinoidea* (so ably illustrated by the late Mr. Miller†) are closely allied to the *Asteriæ*, and especially to the genus *Comatula* of Lamarck. The only previously known recent species of this tribe, the *P. Caput Medusæ*, found in the West Indies, had not been brought to Europe in a fit state to allow of any investigation of its structure.

Mr. Gray has lately submitted to the Zoological Society‡ specimens of the shelly covering of a new genus, which is interesting as forming a distinct family, if not order, intermediate to the *Echinidæ* and *Asteriidæ*. It is allied to the latter in having only a single opening to the digestive canal; while it agrees with the former in form and consistence, differing however from it in not being composed of many plates. For this genus, which Mr. Gray thinks bears a near affinity to the fossil *Glenotremites paradoxus* of Goldfuss, he proposes the name of *Ganymeda*.

In the *Ann. of Phil.* for 1825§, Mr. Gray has published a natural arrangement of the families of the *Echinidæ*‖.

2. *Entozoa*.—In this group, as it stands in the *Règne Animal*, we find an assemblage of animals which, though not much studied in this country, have received great attention from several German and French naturalists, from whose combined researches it seems now quite certain that they can no longer be arranged all in the same class. Cuvier divides the *Entozoa* into two

* *Memoir on the Pentacrinus Europæus, &c.* 4to, Cork, 1827.

† *Nat. Hist. of the Crinoidea, or Lily-shaped Animals, &c.* 4to, Bristol, 1821.

‡ *Proceedings of the Zool. Soc.* (1834), p. 15. § vol. xxvi. p. 423.

‖ Since this Report was read, a short but important communication on the external structure of the *Echinodermata* and their mode of growth has been published by M. Agassiz. His chief object is to show that the *Echinodermata*, although usually considered as partaking of a radiated structure in which all the parts of the body are similar, nevertheless exhibit a *bilateral symmetry*; furthermore, that the addition of new plates, as the animal increases in size, takes place in a *spiral* and not in a *vertical* succession, as would appear at first sight to be the case. M. Agassiz announces it to be his intention to publish a monograph on these animals. See *Lond. and Edinb. Phil. Mag. and Journ. of Sci.* for Nov. 1834, p. 369.

orders, which he calls *Intestinaux Cavitaires* and *Intest. Parenchymateux*, the former answering to the *Nematoidea* of Rudolphi, the latter comprising the last four orders of this author. Cuvier admits, however, that there is a great difference in the respective organizations of these two groups. In fact, the *Nematoidea*, raised so much above the other *Entozoa* by their distinct nervous system, are now generally allowed to approach closely the *Annulose* structure, if not to belong to that division of the animal kingdom. Mr. MacLeay long since referred them to that type, observing, that in a natural arrangement it seems hardly possible to separate them far from *Lumbricus* and *Gordius**. With Blainville they also form a portion of his *Entomozoaires Apodes*†. In a more recent publication‡ this last author has gone further into detail with respect to the arrangement of the *Entozoa* in general. He thinks they constitute two classes at least; the greater portion forming the last class in his type *Entomozoaires* (in which class he includes the *Hirudinidæ*); the remainder (comprising the third and fourth families of Cuvier's *Intest. Parenchymateux*) forming a subtype intermediate to the *Entomozoaires* and *Actinozoaires* (or *Zoophytes*), though on the whole approaching nearest to the former. Blainville does not admit that in the classification of the *Entozoa* we should be at all more influenced by their peculiar habitat than in that of other animals. He looks only to the organization, which leads him to place in the same order (*Oxycéphalés*, Blain.) *Filaria*, *Gordius*, and *Vibrio*, genera certainly not very dissimilar in structure, though residing in very different situations. His other orders in the class *Entomozoaires Apodes* include in like manner both external and internal worms. There can be no doubt that this principle is just to a certain extent. Indeed it is supported by the opinions and researches of others. Lamarck and Bory-St.-Vincent both suspected an affinity between the *Vibriones* and the true *Vermes*. Dugès, in the *Ann. des Sci.* for 1826§, has instituted a close comparison between the *Vibriones* and the *Oxyures* of Rudolphi, and from an examination of their digestive and reproductive systems, seems decidedly to think that they belong to the same group. Professor Baer of Königsberg, whose researches have tended greatly to elucidate the structure and affinities of the *Entozoa*, has in a memoir (or rather one of a series of memoirs) on the lower animals, published in the 13th volume of the *Nov. Act. &c. Nat. Cur.*, endeavoured to show that neither the *Entozoa* nor

* *Hor. Ent.*, p. 224. † *Principes d'Anat. Comp.*, tab. 7.

‡ Art. Vers in the 57th volume of the *Dict. des Sci. Nat.*, published in 1828. This treatise also appeared separately under the title of *Manuel d'Helminthologie.* § tom. ix. p. 225.

Infusoria can be preserved as distinct classes. It should be stated, however, that he has embraced some peculiar views respecting the systematic distribution of animals, of which it is impossible to give any detailed account here. I may also allude to a curious memoir by Dugès in the *Ann. des Sci.* for 1832*, as affording fresh suspicion that the *Entozoa* do not form a natural class of themselves to the exclusion of other animals. He describes a new and very singular genus found free in water amongst duckweed, which appears to be closely allied to the *Tæniæ* and *Bothriocephali*. It is small, but has its body divided into segments like those animals, these segments being of a similar form, and varying in number from four to eight. Dugès thinks it not improbable that this may have been the supposed *Tænia* which Linnæus is said to have met with free in water. He gives it the name of *Catenula Lemnæ*.

The *Planariæ*, again, present us with a group of animals *not* parasitic, which are now universally admitted amongst the Parenchymatous Worms, and considered as belonging to the *Tremadota* of Rudolphi. Cuvier indeed (as Lamarck and others had already done) assigned them this place in the first edition of the *Règne Animal*, but it was not without doubts as to their true situation. These doubts are now quite removed by the researches of Dr. Baer and M. Dugès, both of whom have investigated the structure of these animals, the former in the memoirs before alluded to, the latter in the *Ann. des Sci.* for 1828 and 1830†. The result is, that neither of these observers has been able to detect any muscular, or ganglionic nervous system; and the latter thinks that it is the absence of these systems principally which serves to separate them from the *Hirudinidæ*, with which they have been so often classed. At the same time, Dugès points out several respects in which they clearly approach the group just mentioned. It may be added, that Dugès has proposed in his memoir to raise the *Planariæ* to the rank of a family, in which he particularizes three distinct genera. These he has characterized from the structure of the digestive organs, and the situation as well as number of the orifices.

As there are some groups which, though *not* parasitic, require to be associated with the *Entozoa*, there are others which *are* parasitic, and which many have arranged with these animals, but of which the true situation is extremely doubtful. Such are the *Lernææ*, presenting such evident affinities to the *Siphonostomous En-*

* tom. xxvi. p. 198.

† I may also allude to two papers by Dr. Rawlins Johnson in the *Phil. Trans.* for 1822 and 1825, containing the result of some inquiries into the power of reproduction possessed by these animals. This subject, however, had been previously investigated by Mr. Dalyell in his interesting memoir on the *Planariæ*, published at Edinburgh in 1814.

tomostraca, to which they are referred by Blainville, Straus-Durckheim, Edwards, and others, although placed by Cuvier at the end of his *Intestinaux Cavitaires*. Blainville has made a particular study of this family, in which he has characterized eight distinct genera*. Nevertheless, we stand much in need of further information respecting their structure and œconomy†. On the other hand, the *Acephalocysti*, and the *Hydatids* in general, appear so low in the scale of organization, that it may be questioned whether they can be placed in the same class with *all* the other groups included in Cuvier's second order. Nitzsch and Leuckart, as well as Dugez, think that the *Acephalocysti* are allied to the *Volvoces* and other vesicular *Infusoria*‡. M. Kuhn, in a memoir lately published §, does not consider them as true animals, but thinks that they should have a place assigned them amongst those ambiguous beings which hold a middle rank between the animal and vegetable kingdoms, and to which Bory St. Vincent has given the name of *Psychodiaires*.

From the above observations it will be seen how much remains yet to be done towards a natural arrangement of these animals. Those who would enter into the details of their history, will do well to consult,—besides the memoirs already alluded to, and the works of Rudolphi, which are well known,—the works of Bremser||, Cloquet¶, Creplin**, and Leuckart††. Bremser, in

* See *Journ. de Phys.* (1822), tom. xcv. pp. 372 and 437; also the 26th vol. of the *Dict. des Sci. Nat.*, art. LERNE'E.

† According to the observations of Dr. Surrirey of Havre, the *Lernææ* undergo a metamorphosis, and are very different in their young state from what they are in their adult. (See Blainville in *Dict. des Sci. Nat.*, tom. xxvi. p. 115.) Since this Report was read, I have learned that the above fact has been recently confirmed by M. Nordmann, who is said to have published several very interesting researches connected with the gradual development of these animals, and such as leave no doubt of their forming part of the same group with the *Siphonostomous Crustacea*. These observations are contained in a work entitled, "*Mikrographische Beitrage zur Naturgeschichte der Wirbellosen Thiere*," Berlin, 1832. Not having seen it, I can make no further allusion to it in reference to this subject.

‡ *Ann. des Sci. Nat.* 1832.

§ *Mém. de la Soc. d'Hist. Nat. de Strasbourgh*, tom. i. part 2.

|| Bremser published at Vienna, in 1819, a work on the human *Entozoa*, which in 1824 was translated into French by Grundler and Blainville, and enriched with many valuable observations from this last author.

¶ Author of *Anatomie des Vers Intestinaux*. Par. 1824, 4to.

** Creplin has published two treatises on the Intestinal Worms, one in 1825 under the name of *Observationes de Entozois*; another, entitled *Novæ Observationes, &c.* at Berlin in 1829. These works, which I have not seen, are said to contain descriptions of a great many new species, along with many detached observations on these animals.

†† Leuckart is the author of a natural classification of Intestinal Worms, in German, published at Heidelberg in 1827. This work has been before alluded to as containing an arrangement in conformity with the principles of Oken.

addition to his treatise on the *Entozoa* of the human species, has published a series of plates intended to illustrate Rudolphi's genera, in which, by engraving on a dark ground, the white and transparent parts of these animals are brought out in an admirable manner. Van Lidth de Jeude has also published more recently (1829) a collection of lithographed plates of these animals*.

3. *Acalepha.*—Our knowledge of this class must be considered as very imperfect, notwithstanding it has engaged the attention of many excellent observers. This is in a great measure to be attributed to several difficulties connected with the study of these animals, particularly those arising from their very delicate structure, which renders the preservation of specimens in many cases almost impossible. Péron and Lesueur published some valuable memoirs on the *Medusæ* (taking this term in its full extent) in the 14th and 15th volumes of the *Ann. du Mus.*, which contained a far more detailed history of this tribe than any that had appeared before, and contributed greatly towards an improved classification of it. These authors are, however, generally allowed to have overmultiplied the species, and to have established several genera upon insufficient observation. Many additions to this class, and to our knowledge of its structure, were made subsequently by Chamisso and Eisenhardt in the 10th volume of the *Nova Acta &c. Nat. Cur.*, and a few in the 11th volume of the same Transactions by Otto. Quoy and Gaimard also collected much information with respect to the habits and organization of these animals during their voyage with Freycinet. Some of their observations were published in the *Ann. des Sci.* for 1824† and 1825‡. In this last volume, their remarks, so far as the *Acalepha* are concerned, relate only to the genus *Beroe*. In the *Bull. de la Soc. Phil.* for 1824§, M. De Fréminville has published some observations on the *Physalia pelagica*, to which are annexed descriptions of three new species belonging to that genus. Some researches on the structure of the *Physaliæ* were published about the same time in the 9th volume of the *Petersburgh Memoirs* by Eichwald. In 1825, Rosenthal published some collections towards the anatomy of the *Medusæ*||, the species principally examined being the *M. aurita*, Linn. In 1827, another memoir was published by Quoy and Gaimard in the *Ann. des Sci.*¶, containing an account of a vast number of new

* Besides the above works, I may mention that of Nordmann, already alluded to, from which some valuable extracts will be found in the *Ann. des Sci. Nat.* for 1833, tom. xxx.

† tom. i. p. 245 ‡ tom. vi. p. 28. § p. 42.

|| *Bull. des Sci. Nat.* (1826), tom. ix. p. 253. ¶ tom. x.

marine animals discovered by them the year before in the Straits of Gibraltar, where they were detained some days by a calm soon after the commencement of a second voyage with Captain D'Urville. Amongst these are several new genera belonging to the group of *Diphyes*, Cuv., which the authors consider as entitled to rank as a family. This memoir contains by far the most valuable details respecting the organization of these remarkable animals which had appeared up to that time. In 1828, Rang published in the *Mém. de la Soc. d'Hist. Nat. de Paris** a memoir on the genus *Beroe*, which he considers as forming another distinct family amongst the free *Acalepha*, in which he describes two new genera. Rang thinks that the free *Acalepha* may be divided into three families, having for their respective types *Beroe*, *Medusa*, and *Diphya*. The characters of these he proposes to take from the organs of locomotion. In the first (*Beroides*, Rang,) they consist of a number (always an even number) of longitudinal ribs formed by very numerous series of small ciliæ; in the second (*Medusaires*), these organs are membranes, sometimes entire, sometimes fringed or cut into leaflets, and ranged in a circle round an umbrella; in the third (*Diphides*), these organs are found only in the margin of the principal opening, and sometimes also in a membrane bordering the circumference of it.

By far the most valuable work which has yet appeared in this department of zoology is said to be the *System der Acalephen, &c.* of Dr. Eschsholtz, published at Berlin in 1829†. Its author is well known as the naturalist who accompanied Captain Kotzebue in his voyage of discovery, and as having some time back published valuable observations on the *Physaliæ*, *Porpitæ*, and *Velellæ*, made by himself during that voyage‡. In the present work he has given a detailed account of the structure of the *Acalepha* in general, as well as presented a new arrangement of these animals. Their organization, according to his researches, would seem to be of a more complex nature than was formerly supposed. He has discovered a very perfect vascular system in the *Beroe* tribe, which has led him to place this group at the head of the series. In his classification he adopts three orders, *Ctenophora*, *Discophora*, and *Siphonophora*, the characters of which are taken from the presence or absence of a central digestive cavity, and from the form and structure of the organs of locomotion.

* tom. iv. p. 166.

† I have not seen this work myself. The above notice of it is from the *Bull. des Sci. Nat.* (1831), tom. xxiv.

‡ See *Kotzebue's Voyage*, vol. iii. Append.

Since the appearance of Eschsholtz's work, three or four valuable memoirs have been published by different observers in further illustration of the *Acalepha*. One of these is a monograph on the genus *Diphya* by Lesson*, containing several new remarks on these animals. He thinks that many of the genera instituted by Quoy and Gaimard are only separate pieces, or articulations, detached from the aggregate mass of the animal which forms his genus *Plethosoma*. A second is a memoir by Tilesius, published in 1831†, in which are descriptions and figures of many species of *Medusæ*, more particularly belonging to the genus *Cassiopea*, accompanied by general remarks on this group. A third is a paper by Milne Edwards on the structure of *Carybda marsupialis*, in the *Ann. des Sci.* for 1833‡; and a fourth, one by Dr. Grant on that of the *Beroe Pileus*, published the same year§. These last two memoirs, although treating only of single species, are of importance as tending to raise our notions still further with respect to the organization of these animals. The *Carybda marsupialis* is a species belonging to that portion of the *Medusæ* which have been hitherto considered as having no stomach, and in this and other respects, as possessing a structure even far more simple than the rest of this family. Edwards has found this to be erroneous, by tracing the existence not only of a stomach and mouth, but of biliary ducts, as well as ovaries. He shows that its structure is quite as complicated as that of any other of the *Medusæ*. Dr. Grant, in dissecting *Beroe Pileus*, has discovered an arrangement of filaments and ganglia which, from their general appearance and mode of distribution, he considers as constituting a nervous system. This is a great step gained in our knowledge of the structure of the *Acalepha*. Rosenthal sought in vain for traces of a nervous system. Quoy and Gaimard, as well as many others, seem satisfied with respect to its entire absence. Dr. Grant however observes, that although nerves have not hitherto been shown in the *Acalepha*, he thinks they will be found even in the simpler forms of *Medusæ*, which he has shown elsewhere to be affected by light, as well as *Actiniæ*, *Hydræ*, and *Furcocercæ*.

An important work was published by Blainville in 1830, in which he has embodied a vast deal of information relating to the structure, history, and classification, not only of the present tribes, but of all the other animals belonging to Cuvier's divi-

* Published in his *Centurie Zoolog.* Nov. 1830.
† *Nov. Act. &c. Nat. Cur.*, tom. xv. p. 247. ‡ tom. xxviii. p. 249.
§ *Zool. Trans.*, vol. i. p. 9.

sion of *Radiata*, with the exception of the *Entozoa*. I speak of the 60th volume of the *Dict. des Scien. Nat.*, the greater part of which is taken up with the article ZOOPHYTES by the above author*. Blainville, however, has exposed some peculiar views respecting the affinities of certain families hitherto considered as belonging to the *Acalepha*, to which it is necessary to make some allusion†. These relate particularly to the *Physaliæ*, which, he observes, are of a very anomalous character, and in some measure seem to depart from every known type. He has, however, ventured an opinion, grounded on an examination of specimens of *Physsophora* and *Stephanomia* communicated to him by Quoy and Gaimard, that the *Physaliæ* ought to be removed from the place usually assigned them, and made to constitute a distinct order among the *Mollusca*, near the orders called in his system *Polybranches* and *Nucléobranches*. Blainville appears to have been led to this idea more from observing the arrangement of the external parts of these animals, than from any close investigation of their internal organization. On this ground, Cuvier expresses himself as decidedly opposed to it‡. He observes, that before we can admit them to a place in that division, it ought to be shown that they possess a nervous, as well as vascular system, a heart, and liver, as well as male and female organs of generation, all which he (Cuvier) has in vain sought for. Blainville in like manner differs from other naturalists with respect to the affinities of the *Diphyæ*, which he thinks constitute a group intermediate to the *Salpæ* and *Physsophoræ*. Also the genus *Beroe* he thinks should be removed from the great family of *Medusæ* (*Arachnodermaires*, Blainv.), with which it is so constantly associated. It must be obvious that many speculations will arise with respect to the situation and affinities, not of these groups only, but of several others amongst the lower animals, until we are made better acquainted with their organization and habits. These offer to us the only sure grounds upon which we can proceed in the endeavour to determine their place in the natural system; and very many researches relating to these points remain yet to be made amongst the *Acalepha*. The *Diphyæ* in particular astonish us by the singularity of their form and structure. Composed of two polygonal, subcartilagi-

* The *Entozoa* are treated of in a former volume under the art. VERS, which includes also the *Annelida*. To this article allusion has been already made in a former part of this Report.

† A second edition of the above work has been published during the present year (1834) under the title of *Manuel d'Actinologie*. The views of its author remain, however, unchanged with respect to the above affinities.

‡ *Analyse des Trav.*, 1828.

nous, transparent parts, found constantly in a state of union, naturalists seem hardly to be agreed, whether these parts belong to the same animal, or whether they constitute two distinct individuals, although in form always more or less dissimilar. Blainville embraces the former opinion; Quoy and Gaimard, as well as Cuvier, seem inclined to the latter. It would not be difficult to point out other instances in which we want further information with respect to the *Acalepha*. The limits of this Report forbid, however, our dwelling any longer upon this class. It is one especially in which every new observation will have its value; and it is only to be regretted that so few persons have it in their power to study these animals in a recent state, in which alone they admit of such an examination as is likely to conduct to any important discoveries.

4. *Polypi.*—It is not advancing too much to affirm that naturalists are only just beginning to get an insight into the natural arrangement of that immense assemblage of beings which constitutes Cuvier's fourth class of Zoophytes, and that even this insight extends but as yet to comparatively few families. Their researches, however, are sufficiently advanced to prove clearly, that the true situation and affinities of these animals are in many cases very different from those which have been assigned to them in the *Règne Animal*. Some have been shown to possess a structure entitling them to a higher place in the scale of organization; while in others the animal powers seem so reduced, the structure at the same time offering such peculiarities, that they appear to constitute a distinct class, far below the generality of other Zoophytes. One great drawback to our better knowledge of these groups has arisen from the circumstance, that until lately, naturalists, with some few exceptions, scarcely paid any attention to the animals of the *Incrusted Polypi**, which constitute so large a portion of them. They looked only to the characters of the calcareous covering; and it is not surprising that with this half-knowledge they should fall into many erroneous notions with respect to affinities, in their attempts to arrange the species systematically. It is this which at the present day detracts somewhat from the value of the works of Lamouroux†, notwithstanding their great merit in other respects, and the powerful influence which they undoubtedly had over the progress of Zoophytology at the time when they appeared. He has made us ac-

* The *Polypes à Polypier* of the French, for which we have no adequate expression in our language.

† *Histoire des Polypiers Corallines Flexibles, &c.* Caen, 1816, 8vo. And *Exposition Méthodique des Genres de l'Ordre des Polypiers*, Paris, 1821, 4to.

quainted with a vast number of new species, as well as established several distinct genera which had not been before indicated, but his classification is decidedly artificial. Adopting from the first the artificial distinctions of *Polypier flexible*, *Polypier pierreux*, and *Polypier sarcoide*, he has been necessarily led, as Blainville observes, to a similarly artificial arrangement of all his subordinate groups. A better prospect has, however, opened upon us in this respect. Naturalists are now guided in this department of zoology by the same principles which have for some time back directed their researches in the other branches of the science. They see the importance of studying the entire organization. And while this has led them to a close investigation of the *Polypi* themselves in those zoophytes in which they are really present, it has also led them to distinguish, and to separate from these last, others, in which it is now clearly ascertained that no *Polypi* ever exist.

I can only make a brief allusion to a few important steps which have been gained of late years in our knowledge of these animals. One of these relates to the *Madrepores*, the animals of which have been proved, by the researches of Lesueur*, Eysenhardt, and Chamisso, and more recently, as well as more decidedly, by those of Quoy and Gaimard, to hold a much nearer affinity to the *Actiniæ* than to the *Hydræ*. Blainville, who has attempted to characterize the genera† from a consideration of the hard and soft parts conjointly, considers them as true *Actiniæ*, in the parenchyma of which is deposited a considerable quantity of calcareous matter, producing what the French call the *Polypier*. He observes, that we may even find a gradual transition in this respect from the softest of the *Actiniæ* to the most solid and most calcareous of the *Madreporæ*. He accordingly throws them both together in one class (*Zoantharia*, Blainv.), in which however they form two distinct orders. Quoy and Gaimard paid particular attention to the Polypiferous Zoophytes during their voyage with Freycinet‡, and ascertained the nature of the animals in several genera in which they had not been described before, or only in an imperfect manner. Amongst others may be mentioned the *Tubipora* of Linnæus, which had been supposed by some to

* *Mém. du Mus.*, tom. vi. p. 271.

† *Dict. des Scien. Nat.*, art. ZOOPHYTES.

‡ See the volume of Zoology annexed to that voyage; also *Ann. des Scien. Nat.*, tom. vi. p. 273, and tom. xiv. p. 236. The former of the two memoirs just cited contains some remarks on the supposed rapid growth of Coral Islands, and the power possessed by the *Polypi* of raising perpendicular walls from the bottom of the ocean. According to their observations, the labours which have been ascribed to these animals have been very much exaggerated, and the accounts which have been sometimes given of them altogether erroneous.

be inhabited by an annelidous animal. MM. Quoy and Gaimard have shown it to be a true Polype. Delle Chiaje is said also to have described the animals of certain species which had previously been unnoticed. Dr. Fleming in our own country has made many interesting researches connected with the genera and species found on the British shores*. More important contributions, however, to our knowledge of this class of animals were made in 1825, and the two succeeding years by another of our countrymen, whose labours in this department have acquired for him the highest reputation. I allude to Dr. Grant, whose series of papers on the Sponges and other zoophytes are replete with new and valuable observations. Those on the Sponges especially, published in the *Edinb. Phil. Journ.* for the above years†, contain the results of a far closer investigation than had been before made into the nature of these anomalous productions. Indeed he was the first to discover their true organization and functions. He clearly ascertained that they do not possess any *Polypi*, nor even the power of contracting and dilating their orifices, as had been formerly supposed. He was the first to draw the exact distinction between the fæcal orifices and the pores; as well as to point out the nature and directions of the currents which are constantly passing out from the former. He also succeeded in determining the origin and mode of development of the ova. The memoirs just alluded to relate to the Marine Sponges. In a separate communication‡, he gave the results of a similar investigation into the nature of the *Spongilla friabilis* of Lamarck, found in fresh water, which he showed to bear a close resemblance to the above in all essential respects, although more simple in its structure, and occupying a still lower place in the scale of organization. In 1827 Dr. Grant extended his researches to the *Flustræ*, and published a detailed account of the structure and œconomy of this tribe of *Polypi*, which were before but imperfectly understood. Several other equally valuable papers relating to the zoophytes appeared from him about the same time, to which however I can only just allude. In one of these§, he has described a new and highly interesting genus, forming a connecting link between *Alcyonium* and *Spongia*; "allied to the former by its contractile fleshy texture, and by its distinct though microscopic *Polypi*; to the latter, by its

* See *Edin. Phil. Journ.*, vol. ii. p. 82; and *Wern. Mem.*, vol. iv. p. 485; also his *British Animals*, published in 1828.

† vols. xiii. and xiv.; also vols. i. and ii. of the *New Series*.

‡ *Edinb. Phil. Journ.*, vol. xiv. p. 270.

§ *Edinb. Phil. Journ.*, N.S., vol. i. p. 78.

siliceous tubular spicula, ramified internal canals, tubular papillæ, regular currents, and the distribution of its ova." In another, published the same year in the same journal, he has detailed some observations on the spontaneous motions of the ova of several species of zoophytes, a motion which, though long since observed by Ellis in the case of the *Campanularia dichotoma*, Lam., scarcely seems to have attracted notice afterwards, notwithstanding its importance as connected with the mode of generation in these animals. In 1827, Dr. Grant also published two papers in the *Edinb. Journ. of Science*, one* on the structure and mode of generation of the *Virgularia mirabilis* and *Pennatula phosphorea*, the other† on the generation of the *Lobularia digitata*. A supplement to his first memoir appeared in the same journal‡ in 1829.

About the same time as that when Dr. Grant was engaged with these researches, two or three observers in France were busied in a similar investigation, as well of the Sponges as of some of the freshwater gelatinous *Polypi* of Cuvier's second order. Raspail and Robineau-Desvoidy first read a memoir to the Royal Academy of Sciences in 1827§ on the *Alcyonella* of Lamarck. Their object was to elucidate the structure of this ill-understood zoophyte, and more especially to show that the supposed *Polypi* seen in it by Lamarck were only parasites, probably belonging to the genus *Nais*, the tubes of the *Polypier* being naturally imperforate. This opinion was, however, retracted by Raspail in a second and very elaborate memoir on this zoophyte read the same year||, in which he acknowledged the existence of the *Polypi*, but sought to prove by a course of detailed observations that this genus was not distinct from *Cristatella* or *Plumatella*; that in fact these three genera, as well as *Difflugia* of Lamarck, were one and the same animal in different stages of development¶. Raspail also made several observations on the structure of Sponges, in some respects analogous to those by Dr. Grant. In a memoir, likewise read in 1827 and published the year following**, he gave the results of a microscopic examination into the structure of the *Spongilla friabilis*, many of which results, however, differed very materially from those arrived at by our own countryman. Part of his object was to point out an analogy be-

* vol. vii. p. 332. † vol. viii. p. 104. ‡ vol. x. p. 350.

§ Cuv., *Anal. des Trav.*, 1827.

|| This second memoir was subsequently published in the *Mém. de la Soc. d'Hist. Nat. de Paris* (tom. iv. p. 75).

¶ Further researches seem necessary in order to establish beyond doubt the identity of the above genera. The opinion of Raspail on this point has not been universally adopted.

** *Mém. de la Soc. d'Hist. Nat. de Par.*, tom. iv. p. 204.

tween the siliceous spiculæ found in this genus and in the other Sponges, and the spiculæ of oxalate of lime met with in certain plants. In 1828, the *Spongilla* was again made the subject of a memoir, by Dutrochet*. He confirmed Dr. Grant's observations, particularly those relating to the existence of currents (which Dutrochet attributed to endosmose) and the entire absence of *Polypi*. Dutrochet, however, considered the *Spongilla* as a vegetable.

A series of valuable observations relating to the zoophytes were also published in 1828 by Audouin and Edwards †, being a portion of the researches of these indefatigable naturalists at the Chausey Islands. They afford fresh confirmation of the accuracy of Dr. Grant's views respecting the *Sponges* and *Flustræ*. They also seem to lead to the important discovery that many of the species in this last group possess an organization more complex than has been hitherto supposed, and such as brings them into near affinity with some of the compound *Ascidiæ* ‡. The same complexity of structure is stated to have been seen by them to a certain extent in many *Vorticellæ*. These observers indeed have found such great differences in the organization of the class of *Polypi* in general, so far as they have had an opportunity of examining them, that they propose a fresh division of this class into four sections, each of which will constitute a natural family characterized by a peculiar type of structure. The *first* of these groups will embrace the *Sponges*; the *second*, the fixed *Polypi*, whether naked or incrusted, in which the digestive cavity is in the form of a *cul de sac* hollowed out in the very substance of the body (*Hydræ*, *Sertulariæ*, many *Vorticellæ*); the *third* will include those *Polypi* having a cavity in the body, in the middle of which is suspended a membranaceous digestive canal, communicating outwards by a single opening, and bearing at its lower extremity appendices in the form of small intestines, which appear to perform the office of ovaries (*Lobulariæ*, *Gorgoniæ*, *Pennatulæ*, *Veretillæ*, *Cornulariæ*, &c.); the *fourth* will include the *Flustræ* and other *Polypi*, in which the digestive canal communicates outwards by two distinct openings, and the organization of which approaches that of the compound *Ascidiæ*.

* *Ann. des Sci. Nat.*, tom. xv. p. 205. † *Id.*, tom. xv. p. 5.

‡ Cuvier states that similar observations had been made by Spallanzani, and also more recently by Blainville. He adds, however, that according to Quoy and Gaimard, there are certainly other species in which the animals are true *Polypi*; and that hence it would be very desirable to ascertain which belong to one type of structure, and which to the other. See *Règne Animal*, tom. iii. p. 303. note (5).

It is probably to this last group of zoophytes, containing the more perfectly organized genera, that the animal belongs which Mr. Thompson has described under the name of *Polyzoa* in the fourth number of his *Zoological Researches*. This name he has applied as a general title for the animal inhabitants of several zoophytes, which in their organization he considers as belonging to the *Acephalous Mollusca*, being possessed of a distinct gullet, stomach, intestine, and ovarium. Such a structure he has noticed in *Sertularia imbricata*, *S. Cuscuta*, *S. spinosa*, and *S. pustulosa*, and he thinks that it will probably be found in all the other species of *Sertularia* "not furnished with oviferous receptacles, distinct in size, shape, and situation from the cells occupied by the animals, and consequently in all the *Serialaria* of Lamarck." Mr. Thompson has also observed the same organization in the *Flustræ*, thus confirming the observations of Audouin and Edwards, with which, however, he does not appear to be acquainted*.

The memoirs which have been noticed above relate for the most part to particular groups in the class under consideration. The only work that has appeared of late years treating of this entire department of zoology (I except Blainville's, which is of a more general nature,) is one published by Rapp in 1829†. This work is divided into two parts. The first treats of the classification of the *Polypi* in general, presenting an arrangement in which due consideration is had to the form of the animal. The

* I may take this opportunity of stating, that at the same meeting of the British Association at which this Report was read, Mr. Graham Dalyell brought forward a memoir containing some highly interesting observations connected with the mode of propagation and development of the *Sertulariæ*, as well as of some other zoophytes found on the coast of Scotland. An abstract of this memoir will be found in the *Edinb. New Philos. Journ.* for October last, p. 411.

I may also observe, that since then an important memoir "On the Structure and Functions of tubular and cellular *Polypi*" has been published by Mr. Jackson Lister in the second part of the *Philosophical Transactions* for 1834. The principal feature in this memoir is the discovery of the existence of currents within the stems of the *Tubularia indivisa* and of all the species of *Sertularia* which were examined by the author. The circulating fluid, which appears to be in some respects analogous to that observed in *Charæ*, Mr. Lister is disposed to regard as an important agent in the absorption and growth of the parts.

† *Ueber die Polypen im allgemeinen, und die Actinien ins besondere.* Weimar, 1829, 4to. I should state that a work appeared in 1819 by Schweigger, entitled *Anatomisch-physiologische Beobachtungen über Corallen*, which is said to contain a great many valuable observations on the structure and œconomy of zoophytes: I have not, however, seen it myself. Some of his researches went far to prove that the *Corallinæ* are only calcified plants. See an analysis of his experiments on this subject by Dr. Grant in the *Edinb. New Phil. Journ.*, vol. i. p. 220.

primary division is grounded on the position of the ovaries or germs, which are either external or internal, and give rise to two groups accordingly. The former includes the genera *Hydra*, *Coryne*, *Sertularia*, and *Tubularia*, united to form a small family; and the genus *Millepora*. The latter comprises the *Alcyonia*, or *Polypi tubiferi* of Lamarck; the *Tubiporæ*; the Corals (*Corallium*, *Gorgonia*, *Isis*, and *Antipathes*); the *Pennatulæ*; the genera *Zoanthus* and *Cornularia*; and the *Madrepores*. The second part of Rapp's work is confined to the *Actiniæ*, and may be regarded as a kind of monograph on that difficult tribe, the species of which have been in general so ill-determined.

The same year as that in which Rapp published the above work, he also published a paper, in the fourteenth volume of the *Nov. Act. &c., Nat. Cur.*, on the structure of some species of *Polypi* from the Mediterranean.

It is not pretended, in what has gone before, to point out all the discoveries which have been made in this class of late years; and possibly there may be some of more importance than any mentioned which I have omitted, through ignorance, to notice. But whatever our knowledge may amount to, we may safely say that it bears but a small proportion to what remains to be acquired. This is indeed true with respect to every department of zoology, but it is most especially so with regard to the present. As a proof, it is only necessary to mention that in Blainville's work (I speak of the second edition, which appeared during the present year,) there are upwards of fifty genera (without including those which have been hitherto only found fossil) the characters of which commence with *animaux inconnus**. I need scarcely add what a field is here open to the naturalist, or how far we must be removed from understanding the structure and the true natural affinities of all the above groups.

5. *Infusoria.*—So complete a revolution has been effected in this group by the recent brilliant discoveries of Professor Ehrenberg, as entirely to sink the value of every arrangement that had been previously brought forward of the animals which it includes. It is not, however, a department of zoology which had before been much cultivated. Since the time of Müller, to whom we are indebted for the first accurate researches into the history of these minute beings, but little progress has been made in our knowledge respecting them, till the period we are about to speak of. The most important contributions were those of

* One of these genera is *Antipathes*, the animals of which, however, have been discovered by Mr. Gray, who read a short notice respecting them to the Zoological Society in 1832. See *Proceed. of Zool. Soc.* for that year, p. 41.

Nitzsch in 1816, who illustrated the structure of the *Cercariæ* and *Bacillariæ*, and with whom rests the merit of having first ascertained the existence of eyes in several species belonging to the former of these groups. Many other observers have published descriptions of new species, as well as instituted new genera; but not having had a sufficiently correct idea of the real organization of these animals, they have in too many instances established their characters upon considerations which are found at the present day to be of no importance whatever. This is particularly the case with many new groups instituted by Bory St. Vincent in the *Dict. Class. d'Hist. Nat.*, in 1826. In this work, under the Art. MICROSCOPIQUES (which name he substitutes for that of *Infusoria*), he has given a new systematic arrangement of all the animals belonging to this class; but being unfortunately based on the external forms, not only are his genera and species greatly overmultiplied*, but his classification is entirely artificial, and since the researches of Ehrenberg, become perfectly useless. More important views on this subject were entertained by Professor Baer in a paper published in the 13th volume of the *Nov. Act. &c., Nat. Cur.*, to which allusion has been already made in a former part of this Report. He particularly noticed the great differences which appear in the organization of these animals. Carried away, however, by peculiar notions, which led him to consider them as only the imperfect prototypes of other classes, he was for placing them respectively in these classes, and suppressing that of *Infusoria* altogether.

Ehrenberg's researches, which form quite an epoch in this department of zoology, were first made known in a memoir read to the Berlin Academy in 1830, and published in the Transactions of that body for that year. So many excellent analyses of them have already appeared†, that it is not necessary, neither would it be consistent with the length of this Report, to enter here into any detailed account of them. I shall simply mention some of the chief results at which he has arrived with respect to the structure of these animals, and which he has made the basis of an improved classification of them. The principal feature is the discovery that the *Infusoria* possess a much more complex organization than naturalists before had any idea of. By sup-

* The extent to which this has been carried, not only by Bory St. Vincent but by other writers on these animals, may be judged of from a statement by Ehrenberg, who observes that Müller has made of the *Vorticella Convallaria* twelve species, which form with Lamarck, Schrank, and Bory St. Vincent six genera.

† See the *Edinb. New Phil. Journ.* for 1831 and 1833. Also the *Ann. des Sci. Nat.* for March, &c., of the present year.

plying them with organic colouring matter as nutriment, he has clearly ascertained that they are not mere homogeneous gelatinous masses supported by cutaneous absorption, as was formerly supposed, but organized bodies, provided in all cases with at least a mouth and digestive system. This last indeed he has found subject to great variation of structure, being sometimes simply a round sac in the centre of the body, at other times a long canal, often very much convoluted, and furnished with a great number of cæcal appendages, which he considers as so many distinct stomachs. The mouth also varies in its structure, and presents good characters for distinguishing the subordinate groups. In the simpler *Infusoria*, it is a mere unarmed opening, surrounded with a greater or less number of ciliæ. In those of a higher order, however, it is much more complicated, and in some cases even provided with a distinct pair of serrated mandibles. Besides a digestive apparatus, Ehrenberg has discovered a generative, and often a muscular system, and has even in one or two instances observed traces of what he considers as vascular and nervous systems. The existence of these last, however, is at present somewhat problematical.

These striking discoveries have naturally led Ehrenberg to reject entirely the principles upon which all former classifications of these animals had been grounded, and to construct a new one after the internal organization. His arrangement is based upon the structure of the digestive system, which gives rise to the two natural classes of *Polygastrica* and *Rotatoria*; the former consisting of such as are provided with several stomachs or internal cavities; the latter of such as have only one, the mouth at the same time being surrounded by a peculiar rotatory apparatus. Of these two classes the last is much more complex in its structure than the former. It would even seem to be more highly organized than some other classes in the system, to which the animals included in it have been hitherto always thought subordinate. With respect to the inferior groups, those of the *Polygastrica* are characterized from the presence or absence of an excretory orifice, the relative positions of the mouth and anus when this last is present, and from the presence and situation of the ciliæ and other processes: in the *Rotatoria*, the families are characterized from the mode of arrangement of the ciliæ which form the rotatory organ. In each class the genera form two parallel series, one consisting of the naked *Infusoria* (*Nuda*, Ehrenb.), the other of such as are protected by a crustaceous or horny covering (*Loricata*, Ehrenb.), these two series appearing to be intimately allied, and often presenting no other difference beyond that which has been just alluded to.

Ehrenberg has rejected from the *Infusoria* several genera which were formerly classed with these animals; amongst others the genus *Vibrio*, before spoken of as having been thought by Dugès and Blainville to show an affinity to some of the *Entozoa*. It also appears probable from some of his observations, that the genus *Monas* and several allied genera are not distinct animal forms, but only the young state of some *Kolpodæ*, *Paramæcia*, &c. This idea has been subsequently adopted by others*.

Ehrenberg has since published a second memoir†, in which he has extended his researches to several points of great interest in the history of these animals. He has endeavoured to ascertain the duration of their existence, as well as the mode of their development. He has also made some further discoveries with respect to their structure. He has detected eyes (which before he had only observed in some of the *Rotatoria*) in many of the *Polygastrica*, and he has found them to furnish distinctive characters of great value in classification. He has fixed a nomenclature for all the principal external organs and appendages, which he describes at much length. He has also made some further remarks on the modifications of the alimentary canal, as well as on those of the dental system. Since the date of his first memoir he has found the teeth existing under several distinct forms, and even ascertained their presence in some of the *Polygastrica*. These discoveries have suggested some new principles of arrangement.

The above is a condensed abstract of the striking researches made by this acute observer. We may judge what important views they open to us, not only in respect to the structure of these animals, but in respect to what *may* be the structure of some other groups, in which also the organization has been considered hitherto as of the most simple kind. It is moreover not improbable that they may ultimately help us in determining the true limits of the animal and vegetable kingdoms, if between them any fixed limits really exist. A complexity of structure, of such a nature as is found in no vegetables, has been shown by Ehrenberg to exist in those forms which were formerly regarded as very near the boundary; and although we now know of some groups of a much more anomalous character than the *Infusoria*, it is only extending our researches a little further, and we may possibly be able to detect their real nature. There is one in-

* See a paper entitled "Observations upon the Structure and Development of the *Infusoria*," by Dr. Rudolph Wagner of Erlangen, in the *Edinb. New Phil. Journ.* for October 1832.

† *Berlin Memoirs* for 1831.

quiry in particular which forcibly suggests itself. Is there any similar complexity of structure, anything approaching to an alimentary sac or stomach, in those monads, which, it is asserted by so many observers, become fixed after a time, and transformed into *Confervæ*? The determination of this point will go far towards determining the true situation of a host of ambiguous genera at present hovering between the two kingdoms, and having almost equal claims upon the notice of the zoologist and botanist*.

IV. *Conclusion.*

In the preceding pages I have endeavoured, though I fear very imperfectly, to give a condensed view of the principal researches which have been made of late years in Zoology, at least such as have tended to throw light upon the affinities of animals, and thereby to advance our knowledge of the natural system. It was my original intention to have proceeded here to the consideration of some other parts of the subject, such as the state of our knowledge with respect to the actual number of species in the several classes, and also with respect to the zoology of particular countries. The former, however, is rendered unnecessary from the appearance of an article in the *Edinb. New Philos. Journ.* of last year †, expressly devoted to this branch of inquiry. The latter would afford an opportunity of alluding to several valuable works which have been recently published in this and other countries, some containing many new and interesting forms of importance to the science in general. But the length to which this Report has already been extended precludes my entering upon this subject. Considered also in connexion with that of the geographical distribution of animals, it would furnish ample materials for a separate communication. I shall therefore, in conclusion, merely offer a few remarks connected with the further progress of zoology, and its advancement in this country in particular.

(1.) Its general progress, viewing the natural system as the true object of the science, and considering the very imperfect knowledge we have of this system at present, must clearly depend upon the discovery of new forms, and a more thorough investigation of those already known to us. If the former be necessary in order to supply some of the numerous links that are yet wanting to complete the chain of affinities, the latter is not

* For further information respecting these anomalous productions, the reader is referred to the article ARTHROIDEES in the *Dict. Class. d'Hist. Nat.*, and to the articles NEMAZOAIRES and ZOOPHYTES in the *Dict. des Sci. Nat.*

† No. 30, for July 1833, p. 221.

less so to determine the parts of the system to which these links belong. But of these two, there can be no doubt the latter is what we stand most in need of. I question whether we shall not be rendering more service to zoology by paying closer attention to the species we are already acquainted with, than by further augmenting the immense collection of uninvestigated forms which exists now in our cabinets. We have, perhaps, sufficient materials on our hands, though not for discovering the whole natural system, at least for solving many important problems in zoology, were we only better instructed in the nature of these materials. It has been shown in the course of this Report, that there are large groups, even whole classes, of which the true situation and affinities are either not determined at all, or involved in much uncertainty, from the imperfect knowledge we have of their structure and œconomy; and in the details of the system, there is not one class which does not present many genera, and a vast many more species in this predicament. Here then is where the researches of naturalists should be directed. Until we shall have more closely analyzed the characters of these groups, and learnt both the method of variation and relative importance of all the organs, until we shall come to understand their whole structure as compared with those structures we are already acquainted with, we can neither determine the affinities of these groups, nor of any others allied to them which we may hereafter discover.

Researches of the above nature are, perhaps, best embodied in monographs. The value of such works has been every day more and more appreciated since the science has become so extensive, and since its legitimate object has been better understood, especially when they refer to every point in the history of the group treated of, and when due care is taken first to ascertain what others have written on the same subject *. Many excellent monographs fulfilling these conditions already exist, some of which have been alluded to, and others might have been had it been allowable to enter so much into the details of the subject. Nevertheless it would be extremely desirable to have them multiplied. By the help of such works we may arrive step by step towards a more complete generalization of the large number of facts embraced by zoology, at the same time that we greatly facilitate the researches of other naturalists. But all inquiries into the structure and œconomy of animals presuppose an exact discrimination of species. Without this the most detailed observations are rendered of little use, and it is the

† See the article Monographie, by Decandolle, in the *Dict. des Sci. Nat.*

want of it which detracts from the value of much that has been recorded by those who have not sufficiently attended to this matter. Hence it should be one object of a monograph to investigate species with a view to their exact differences, and to elaborate the synonyms of those which have been noticed by other authors. This is especially necessary in some groups, in which great confusion exists on this head. Cuvier was particularly sensible of the importance of this step. In his *Histoire Naturelle des Poissons* it is impossible not to be struck with the care which he has shown in endeavouring to trace every species to its first describer, and to disentangle its synonymy, before proceeding to other points in its history. No researches have been spared which could throw any light on this part of his subject. Every author has been consulted; even the most ancient writers on this branch of zoology he has had recourse to, under the hope of being able to identify the species they have noticed. And he has more than once observed in some other of his works, that there is greater service done to natural history in thus extricating from error and confusion the history of old species, than in publishing and describing new ones.

But not all have it in their power, from the want of requisite materials, to furnish a complete monograph of any entire group. Such persons may, notwithstanding, still contribute greatly to the advance of zoology by restricting their monograph to the species in their own neighbourhood: only let such works be conducted with the same care, the same original observation and research, which are thought necessary in the productions just alluded to. Faulty catalogues, or even works of a more elaborate kind, if merely compiled from other authors, are utterly worthless. Whereas good local Faunas, or portions of a Fauna, however limited the district, *may* be rendered of the greatest possible value. By studying with scrupulous exactness the structure and habits, although only of a few species, we may be able to throw much light upon their natural affinities *, we may accumulate enough facts to make some approaches to generalization ourselves; at any rate we are amassing the best materials for enabling others to do so.

(2.) With reference to the further advancement of zoology in this country in particular, I cannot forbear observing, that while there are some branches of the science which are most sedulously cultivated by us, there are others, and those too such as, from our insular position, it might be thought would be among the first to attract our notice, which have for a long

* Witness the researches of Thompson with respect to the *Cirripeda*.

time lain comparatively neglected. I allude to Ichthyology and the study of the marine *Invertebrata*. I need scarcely say how small is the number of individuals who have added anything recently to our knowledge of the fish even of our own seas, notwithstanding the opportunities for so doing which daily present themselves to naturalists resident on the coast. The fact has been repeatedly noticed. With regard to marine *Invertebrata*, I refer more particularly to the *Radiata* of Cuvier, although there is reason to believe that our knowledge of the *Mollusca* is far below what it might become by a more diligent inquiry into these tribes. Excepting the important researches of Dr. Grant and Mr. Thompson, excepting also a few detached papers by Drs. Fleming and Coldstream, and more recently by Dr. Johnston *, we have hardly any original observations with respect to the radiated animals since the time of Montagu †. In the several classes of *Echinodermata*, *Acalepha*, and *Polypi*, it is impossible to say what and how many species are to be found on our own shores, or what important additions might not be made to our general knowledge of these groups, as parts of the natural system, by those whose situation and opportunities afford the means of studying them. As a striking illustration of what might be done, it is only necessary to look to the results obtained by two French naturalists ‡ (whose example deserves to be imitated) during a series of annual excursions to different parts of their own coast. There is no occasion to specify these results in detail. Many, of the greatest possible interest and importance to zoology, have been already alluded to in former parts of this Report. I may, however, just state the fact, that on their return from the Chausey Islands, which were selected one season as the scene of their researches, they enriched the Paris museum with upwards of 600 species of marine *Invertebrata*, of which at least 400 were considered by them as either entirely new, or before imperfectly understood §.

While it is thus in our power to do much for this science as individuals, I conceive it is also in our power to do something as a nation; and in no respect more than by encouraging and promoting expeditions to foreign countries, deputing naturalists

* This gentleman has lately published a useful paper on the recent Zoophytes found on the coast of North Durham, in the *Trans. of the Newcastle Natural History Society.*

† I should also make an exception of Mr. Graham Dalyell, whose researches on Scottish Zoophytes were brought forwards at the same meeting as when this Report was read. These, however, have not yet been published in detail.

‡ MM. Audouin and Edwards.

§ Cuvier's *Anal. des Travaux*, 1828.

to those parts of the globe which have been least explored, and affording the means of making known to the public the fruits of their researches. France has long since set us an example in undertakings of this nature. In the splendid volumes of zoology annexed to the voyages of Captains Freycinet and Duperrey; in the appropriation of a yearly sum for the support of travelling naturalists for the benefit of the Royal Museum, which mainly to this circumstance owes its unrivalled celebrity *; we see marks of an anxious endeavour on the part of that nation to uphold the interests of this science. I will not say that in no instance has anything of the kind been done here. Within these few years we have seen a work emanate from the British press, the *Fauna Boreali-Americana*, under the immediate sanction and patronage of our own government. I believe, however, it is the first, wholly devoted to zoology, which ever appeared under such auspices. And with respect to researches in foreign lands, whatever may have been done for other sciences, or for this science by private individuals, I apprehend we have effected very little as a nation which will bear to stand in competition with what has been done in this way by France, and some other nations on the Continent which might be mentioned †.

Such are the hints which, with much diffidence, I would ven-

* M. D'Orbigny, who has been for these few years past exploring South America in the above capacity, has recently returned to Paris with rich and valuable collections in all departments. He is said to have acquired no less than forty-six new species of *Mammalia* alone, a surprising addition when we reflect that the whole number before known scarcely exceeded 1200. (See *L'Instit.*, No. 50.) It was observed eight years ago, that "in the present advanced stage of information, it cannot be expected that many new recent species of *Mammalia* should be discovered.[1]" M. D'Orbigny, however, has shown that even in this class novelties are far from at an end to reward those who will go in quest of them.

† Perhaps it may not be without its use, to call the attention of the members of the British Association to the following proposition which was made and adopted at the congress of French *savans* held at Caen, July 1833. It was resolved "to encourage travels of discovery; to recommend naturalists and all persons interesting themselves in the progress of natural history, to organize these kinds of travels by means of subscriptions, and to direct them towards those parts of the globe which have been least explored." See *Congrès Scientif. de France*, 1833, p. 261. I will not presume to say how far it would be practicable for the British Association to set on foot any such project in this country.

Since this Report was read, Mr. Swainson has published his *Preliminary Discourse on the Study of Natural History*, in which he has treated, at some length, of the *Present State of Zoological Science in Britain* as compared with other countries. Without wishing it to be thought that I subscribe to *everything* stated in that volume, I may refer to Part 4. as containing more ample details in reference to this inquiry than it was possible for me to enter into.

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[1] Bicheno's *Address to the Zoolog. Club*, 1826, p. 5.

ture to throw out for the further promotion of zoology. I have only to add, that with reference to the progress it is actually making in our own country, and the promise which is held out of uninterrupted advancement, comparing this country, not with others, but with itself at former periods, there is ground for much exultation. Looking to what has been effected of late years, however more striking in some departments than others, to the important works and memoirs which have appeared amongst us, and to the channels which have been opened for the more successful cultivation of this science, it is impossible not to anticipate the most valuable results. There is one institution in particular, of which I have hitherto not spoken, but which more than anything has contributed to this impulse. I allude to the Zoological Society, founded in 1826. The scale and plan upon which this Society is conducted are calculated to obtain for it the highest place amongst institutions of this nature. Its Museum and Gardens, the latter for the reception of living animals; its extensive correspondence with naturalists in foreign countries, by which it has been enabled to acquire some of the richest and most valuable collections; are too well known to the members of this Association to require being dwelt upon more particularly. I may state, however, that it has recently commenced the publication of Transactions, of which two parts are before the public, containing memoirs of the first importance to zoology, and such as will bear competition with any of those which have emanated from other quarters.

But it is not merely in the institution of the Zoological Society that we trace a rising spirit of inquiry in this branch of science. We see it in the establishment of Natural History Societies in almost all the principal towns of England. It is unnecessary to specify these individually. It is enough to be able to record the fact of their existence. This circumstance alone speaks to a more generally diffused taste for zoology, which is the first step towards the advancement of zoology itself. It is only necessary to give a proper direction to the researches of these societies, to point out those departments which need most cultivation, and we may reasonably hope that the time is not far distant when England will no longer be considered behind her continental neighbours in this, any more than in other sciences.

Report on the Theory of Capillary Attraction. By the Rev. James Challis, *late Fellow of Trinity College, Cambridge.*

In the Report which I had the honour of drawing up last year on the analytical theory of hydrostatics and hydrodynamics, a distinction was made between problems on the common theory of fluids, and those in which molecular attraction and the repulsion of heat are explicitly taken account of, and the former kind alone came under review. That distinction, it was said, depended on the different bases of calculation, which in the former class of problems are observed facts; in the other, certain hypotheses, which can be verified only by a comparison of the results of calculation with experience. The latter kind of questions are of the more comprehensive nature, because frequently it is proposed in them to account for the facts which serve for bases of calculation in the other class, and an explanation of every such fact must include the explanation of all those that can be mathematically shown to be dependent on it. The above distinction ought to be kept in mind when we regard the part which calculation has to perform in our inquiries into the nature and properties of matter. It is not sufficient to say that analysis serves to classify facts of observation, and to prove that several which are allied to each other are consequences of some one *observed* fact; for we have been taught by the labours of Newton, that there are facts which are not *phænomena*, the existence of which can only be proved by calculation. It may now be considered an established fact that all bodies attract each other proportionally to their masses, with forces varying inversely as the square of the distances; but the evidence for this truth is essentially mathematical. So, if the existing theories respecting the internal constitution of bodies, and the nature of the forces which emanate from their molecules, should be established by the progressive advance of science, the evidence on which they will rest must be mathematical. For these reasons, this, the highest department of physical science, may be properly denominated *Mathematical* Physics*. The great problem of universal gravitation, which is the only one of this class that can be looked upon as satisfactorily solved, relates to the large masses of the

* In the former Report I have inadvertently written *Physical Mathematics*. It might perhaps be questioned whether these terms be not equally proper; but when, in addition to what is said above, the title of Newton's *Principia* is recollected, the other would seem to be preferable.

universe, to the dependence of their forms on their proper gravitation, and the motions resulting from their actions on one another. The progress of science seems to tend towards the solution of another of a more comprehensive nature, regarding the elementary constitution of bodies, and the forces by which their constituent elements are arranged and held together. Various departments of science appear to be connected with each other by the relation they have to this problem. The theories of light, heat, electricity, chemistry, mineralogy, crystallography, all bear upon it. A review, therefore, of the solutions that have been proposed of all such questions as cannot be handled without some hypotheses respecting the physical condition of the constituent elements of bodies, would probably conduce, by a comparison of the hypotheses, towards reaching that generalization to which the known connexion of the sciences seems to point. This end is kept in view in the following Report. It happens that with respect to fluids two problems have especially engaged the attention of mathematicians, which in a very marked manner lead to the consideration of molecular forces and the repulsion of heat, viz. capillary attraction, and the propagation of motion as affected by the development of heat. The one refers to fluid in equilibrium, the latter to fluid in motion. It was my intention originally to embrace both these in one Report, but the time required for becoming acquainted with works on these subjects which have not been very long before the public, and contain new trains of thought and mathematical investigation, did not allow of preparing the Report, such as it was intended to be, in time for the present meeting; and the matter connected with capillary attraction alone will perhaps be thought sufficient, and of sufficient interest, to form the subject of a separate report.

The distinction above stated as applicable to two sorts of hydromechanical questions, applies equally to statical and dynamical questions respecting solids. Some may be treated on the supposition of perfect rigidity, as is the case in most of the problems that occur in the common elementary treatises on mechanics: in others the solids must be supposed to be elastic; and if the elasticity be regarded not as a datum of observation, but as a result of molecular attraction and repulsion, then, to take account of it, certain hypotheses must be made respecting the nature of these forces and the molecular arrangement, plainly analogous and intimately related to the like hypotheses with respect to fluids. Questions of this kind have of late largely engaged the attention of some French mathematicians; and the nature of their theories, and the results of the calculations

founded on them, deserve to be brought as much as possible into notice.

Capillary Attraction.—The theory of capillary phænomena will be best exhibited by tracing historically the principal steps by which it has arrived at its present state.

Dr. Hooke is among the earliest speculators on the cause of capillary attraction. He attributed the rise of the fluid to a diminution of the pressure of the atmosphere within the tube, by reason of friction against its interior surface. This opinion was shown to be erroneous when the fluid was found to rise as high under the receiver of an air-pump as in the open air.

Hauksbee*, whose experiments on the capillary action of tubes and glass plates have not even yet lost their value, made the beginning of a true theory of the phænomena, by ascribing them to the *attraction* of the tube or plate. Having ascertained by experiment that the thickness of the matter of the tube made no difference as to the height to which the fluid ascended, he saw that the attractive force must emanate entirely from the particles of the tube situated at its interior surface. He does not, however, pronounce a decided opinion whether the sphere of their attraction extends *mediately* or *immediately* to the particles of the fluid situated about the axis of the tube; and he is in error in supposing that this attractive force, by pressing the fluid particles perpendicularly against the capillary surface at all the points with which the fluid is in contact, diminishes the weight of the suspended column.

In this last particular the explanation of Hauksbee was shown to be untrue by Dr. Jurin†, who found by experiment, that the height to which water would rise in a tube, of which the portion occupied by the fluid consisted of two cylinders of different diameters, depended only on the diameter of the upper cylinder. If the lower cylinder was the larger of the two, as soon as it was completely filled, the water rose in the upper cylinder to the same height above the external level as it would have done in a tube uniformly of the same bore as this latter. Hence he was led to ascribe the phænomena to "the attraction of the periphery or section of the surface of the tube to which the upper surface of the water is contiguous and coheres." On this hypothesis he easily showed that the heights of ascent in tubes of the same matter are inversely as their radii. For the

* *Physico-Mechanical Experiments.* London, 1709. pp. 139—169. Also various papers in the *Philosophical Transactions* for the years 1711 and 1712.

† *Philosophical Transactions* 1718, No. 355, p. 739. An additional paper on the same subject, *Phil. Trans.* 1719, No. 363, p. 1083.

quantity of fluid raised being proportional to the raising periphery, and consequently to the radius, and being proportional also to the product of the height and the square of the radius, it follows that the height is inversely proportional to the radius. This reasoning is not incorrect, but defective, as we shall presently see. In a postscript to his paper, Dr. Jurin intimates that the principle of his explanation was not unknown to Newton and Machin, who, however, do not appear to have supported their views by like experiments. He says also, that the same two mathematicians suggested to him that what he calls the periphery of the concave surface of the tube, is in reality "a small surface whose base is that periphery, and whose height is the distance to which the attractive power of the glass is extended." It is sufficiently evident that the theory of capillary attraction had engaged the attention of Newton, from the 31st query in the last edition of his *Optics*, which was published a short while previously to the reading of Jurin's paper.

In this 31st query, Newton is speculating respecting the nature of molecular forces, to which he is of opinion that chemical combinations are owing. In proof of the existence of such forces, he appeals to several instances of attraction of the kind which it has been agreed to call capillary attraction or cohesion. One very singular instance is the suspension of a column of mercury in a barometer tube, to more than double the height at which it usually stands, by its adhesion to the top of the tube, which it leaves only by being considerably shaken. Of the same kind with this phænomenon, Newton considers the rise of water between two parallel plates of glass held at a very small distance from each other and dipped in the fluid. The height to which the water rises, he says, "will be reciprocally proportional to the distance [between the plates] very nearly; for the attractive force of the glasses is the same, whether the distance between them is greater or less, and the weight of the water drawn up is the same, if the height of it be reciprocally proportional to the distance of the glasses." This explanation, though true, does not prove that Newton had formed any very distinct idea of the extent of action of the attractive force of the glass, and the mode in which the water is influenced by it. He asserts, moreover, that the height to which water rises in a slender glass pipe, will be reciprocally proportional to the diameter of the cavity of the pipe, and will equal the height to which it rises between two planes of glass, if the semidiameter of the cavity of the pipe be equal to the distance between the planes, or thereabouts." These are not, however, theoretical deductions, but the results of experiments made before the Royal Society.

Hauksbee records an experiment by which it appears that if a large tube of glass be closely filled with ashes, and one end be dipped in water, in the space of a week or fortnight the water will rise within the tube to 30 or 40 inches above the level of the water without. Newton, in noticing this experiment, says correctly, that the rise is owing "to the action only of those particles of the ashes which are upon the surface of the elevated water, the particles which are within the water attracting or repelling it as much downwards as upwards."

Another experiment by Hauksbee shows that a drop of water inserted between two plates inclined to each other at a very small angle, and touching at their edges, is attracted to the junction of the edges by a force varying inversely as the square of the distance from it. Newton attempts to account for this phænomenon, but unsuccessfully. The true explanation was reserved for Dr. Young and Laplace.

When two planes inclined at a small angle are immersed in water, with the line of their junction vertical, the outline of the water that rises between them is on each plane nearly an hyperbola, of which the asymptotes are the line of intersection of the plane with the horizontal surface of the fluid, and the line of junction of the two planes. Taylor first ascertained this by measurement*. It is a simple consequence of the law accounted for by Newton, of the rise of water between parallel planes to a height inversely proportional to the interval between them; for the planes being inclined at a very small angle, opposite elements of them may be considered parallel.

The early theories of capillary attraction were defective in two respects: they contained no calculation founded on the hypothesis of an attraction sensible only at insensible distances from the attracting centres, although the existence of such forces was already recognised, and Newton had given an example of calculation made with reference to force of this nature in the instance of the passage of light through a dense medium; and they took no account of the cohesive attraction of the parts of the fluid for each other. The necessity of considering the mutual attraction of the particles of the fluid would seem to be very evident when once the law of attraction, sensible only at insensible distances, was admitted; for supposing the capillary tube to attract only the fluid particles at insensible distances from its surface, a column of water of sensible breadth could not be suspended except by the intervention of a cohesive power resi-

* *Philosophical Transactions*, 1712, No. 336, p. 538.

dent in the fluid. Yet Clairaut was the first to see the necessity of taking account of the action of the fluid on itself; and this addition to the theory of capillary attraction is the principal feature of the propositions on this subject introduced, in rather a cursory manner and beside his main purpose, into his celebrated treatise on the Figure of the Earth*. After stating the insufficiency of the method of Jurin, he proceeds to a careful consideration of all the forces concerned in raising the fluid, both those due to the tube and those due to the fluid, as well at the upper part of the column raised, as at the lower extremity of the tube. His method of considering the forces, which is stated clearly and illustrated by good diagrams, has been for the most part followed in succeeding treatises on the same subject. But although Clairaut asserts that the forces concerned in this problem are sensible only at very small distances, he does not seem to be aware that the distances must be considered altogether insensible. This is not a necessary condition in his view of the mode in which the forces act; respecting the law of the variation of which, as the distances increase, he makes no other hypothesis than that the function which expresses it is the same both for the tube and the fluid. He is consequently unable to prove that the height at which the fluid stands in a capillary tube is inversely proportional to the diameter.

By reasoning on the hypothesis just named, Clairaut arrives at the following conclusion: "If the attraction of the capillary tube should be of less intensity than that of the water, provided it be not so small as half the other, the water will still rise." This he confirms in another method, the principle of which will be exhibited by showing as follows, that if the attraction of the fluid for itself were exactly double that of the tube for the fluid, the surface of the fluid within the tube would be horizontal, and consequently on a level with the surface without. Let us suppose the fluid surface to be everywhere horizontal, and consider the equilibrium of a particle in contact with the vertical surface of the solid, which we will suppose to be plane. Now as the resultant of the forces acting on the particle must be perpendicular to the fluid surface, and therefore vertical, the horizontal attractions destroy each other. Therefore the horizontal attraction of the solid, which is its total action, is equal to the horizontal attraction of the fluid, which is only half what it would be if the fluid were continued above the particle as it is below, and consequently placed under the same circumstances of at-

* *Théorie de la Figure de la Terre.* Paris, 1808, pp. 105—128.

traction as the solid. Hence the truth of the proposition is manifest. We shall have occasion in a subsequent part of the Report to allude to this demonstration.

In 1751, Segner*, aware that Clairaut had written some articles ("*articulos quasi episodicos*") on capillary attraction, but not having seen his work, attempted to determine theoretically the form of the surface of a drop of water resting on a horizontal plane, on the hypothesis of the attraction of the parts of a fluid for each other. This is a problem of the same nature as that of determining the form of the upper surface of the column of fluid sustained in a capillary tube, neither of which had yet engaged the attention of mathematicians. Segner begins with admitting the *tenacity* of fluids, and ascribes it to the action of an attractive force resident in their constituent molecules, the law of which he does not pretend to assign, but assumes only that the sphere of the activity of each particle is of insensible magnitude†. Setting out with these correct principles, he is led to refer the shape which the drop assumes to the action of its *superficial* particles, which form, as it were, a sheet encompassing it, and by their tenacity counteract the tendency of the drop to spread in obedience to the force of gravity. The sequel of this essay is not equally successful. In estimating the superficial tension considered as depending on the curvature at each point of the surface of the drop, the author commits an error in taking account only of the curvature of the sections made by vertical planes through its axis, and neglecting the effect of the curvature in planes perpendicular to these. He intimates in a note at the end of the essay that he became aware of some defect in his theory.

A considerable time after the theory of Segner was published, Monge asserted, at the end of a memoir‡ on certain effects of the apparent attraction and repulsion of small bodies floating on fluids, that "by supposing the adherence of the particles of a fluid to have a sensible effect only at the surface itself, and in the direction of the surface, it would be easy to determine the curvature of the surfaces of fluids in the neighbourhood of the solid boundaries which contain them; that these surfaces would be *lintearia*, of which the tension, constant in all directions,

* *Comment. Soc. Reg. Gotting.* tom. i. Ann. 1751, p. 301.

† "Generatim autem spatium illud sphæricum, intra quod particulæ activitas consistit, adeo exiguum est, ut nullo adhuc sensu percipi potuerit." (p. 303.) Segner appears to have been the first to apply to capillary phænomena mathematical calculation founded on this hypothesis.

‡ *Mémoires de l'Acad. des Sciences*, An 1787, p. 506.

would be everywhere equal to the adherence of two particles; and the phænomena of capillary tubes would then present nothing which could not be determined by analysis." The process here indicated Monge did not follow up by mathematical calculations. The main purpose of the memoir, from which the above sentence is extracted, is to give an explanation of the apparent attraction and repulsion observed to take place between small substances when they float near each other on the surfaces of fluids. These phænomena are of three kinds. (1.) If two floating bodies are each surrounded by a depression of the fluid surface, and are separated at first by a small interval, they will move towards each other as if mutually attracted. (2.) When the fluid rises up around them, they will in this case also appear to be attracted when brought near each other. (3.) When one is surrounded by an elevation of the fluid, and the other by a depression, they will appear to be mutually repelled. The motion of the bodies towards each other in the first case, is owing to the circumstance that the depression of the fluid about one is increased on their mutual approach by the depression about the other, at those parts of each that are neighbouring; which occasions an unequal hydrostatic pressure against each of the bodies in the horizontal direction, the pressure being greatest where the depression is least. This explanation was first given by Mariotte. To explain the second phænomenon, Monge reasons as follows. If a plate of any substance be dipped with its plane vertical in fluid, the fluid will rise by capillary attraction on each side of it. The surface of the raised water, being stretched like a chain, will draw the plate in a horizontal as well as vertical direction, but equally on both sides, so that it will remain in its vertical position. If now another plate of the same matter, and exactly alike circumstanced, be brought near the other, the fluid, it is well known, will rise between them. The total quantity of fluid raised above the ordinary level will remain the same as if the actions of the two plates did not interfere with each other, because the raising forces will be the same. But the weight of water raised between the plates, being suspended from a diminished quantity of fluid surface, the superficial tension within will become greater than that without, and will more than counterbalance the latter. The plates will consequently be drawn together. The third phænomenon is explained by saying, that when a body, surrounded by an elevation of the fluid, is brought near one surrounded by a depression, there is occasioned a diminution of depression on the side of the latter nearest the other, and a consequent inequality of hydrostatic pressure, the excess being on the side where the depression is least. This

body will consequently be repelled from the other. More exact explanations of these phænomena have since been given by Young, Laplace, and Poisson, but not materially differing in principle from the above.

We ought now to notice the labours of Dr. Young in the theory of capillary attraction, as being next in order of time; but as his paper on this subject was published only a short interval (something more than a year) before the "Treatise on Capillary Action" by Laplace, and as it contains an idea which is not in Laplace's theory, and which may be considered an additional step towards the complete explanation of the phænomena, it will be convenient to deviate from the historical order for the purpose of exhibiting more clearly the progressive steps by which the theory of capillary attraction has arrived at its existing state. I will, therefore, now endeavour to give some notion of the principles of Laplace's theory, and of the extent to which they will explain phænomena.

This essay was published in 1806, as a supplement to the tenth book of the *Mécanique Céleste*. It contains explanations more exact than had hitherto been given of the several facts we have had occasion to mention in the foregoing part of the Report, and of others in addition to these: and the explanations are sustained throughout by mathematical calculations. The hypotheses of the theory are, that the fluid is perfectly incompressible; that there is as well an attraction of the particles of the fluid for each other, as a mutual attraction between the particles of the fluid and the particles of the tube, and that these forces are sensible only at insensible distances from the attractive centres. From these principles a fundamental equation relative to the upper surface of fluid raised by capillary action, is derived by a process of the following nature.

Conceive an infinitely slender canal, of uniform transverse section, to be drawn from any point of the fluid surface, supposed to be *concave* by reason of the capillary action, to a point of the horizontal surface which is unaffected by the same cause. Let the canal be everywhere beyond the sphere of the attraction of the solid from which the capillary action proceeds. Suppose its two ends to terminate perpendicularly to the surfaces, and to be rectilinear for a distance from each of them not less than a certain small quantity λ, the extent of the sphere of activity of the fluid's attraction: it is proposed to determine the condition of equilibrium of this canal. It is plain that any point of it distant by more than λ from its extremities will be attracted equally in all directions. The case is different with all points situated within the distance λ from either of the extremities.

The attraction of the surrounding fluid on the points of the canal so situated at the extremity which terminates in the horizontal surface, will produce a pressure, which Laplace calls K, on the canal *downwards*. If a tangent plane be drawn at the other extremity, the fluid below this plane will produce an equal pressure on the canal at this end, and similarly directed. These two pressures acting in opposite directions along the canal, will destroy each other by reason of the incompressibility of the fluid. There will remain the attraction of the fluid between the curve surface and the tangent plane. This produces a pressure directed to the centres of curvature of the point where the canal ends, and, as the calculation shows, proportional to the sum of the greatest and least curvatures at that point; for, in fact, the quantity of matter between the curve surface and tangent plane, taken within the small distance λ from the point of contact, varies in the same proportion, and to this quantity of matter the total attracting force is proportional. Opposed to the pressure thus arising is the effect of gravity on the whole canal in producing pressure in the direction of its length, which effect, it is known from the common principles of hydrostatics, is equal to the weight of a column of the fluid of the same transverse section as the canal, and whose height is the elevation of one end of the canal above the horizontal plane in which the other is situated. Calling this elevation z, the greatest and least radii of curvature at the point of the curved surface under consideration R and R′, and the density of the fluid ϱ, we shall have

$$\frac{H}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)=g\varrho z.$$

This is the fundamental equation spoken of above. It does not contain, as we perceive, the quantity K, which Laplace supposes to be expressive of the force that causes the suspension before mentioned (p. 256) of mercury in the tube of a barometer to a height two or three times greater than that due to the atmospheric pressure. He thinks also that on this quantity depends the forces which produce cohesion and chemical affinities. The left side of the equation, expressing the pressure that arises from the action of the small quantity of matter situated between the curve surface and the tangent plane, and circumscribed by the surface of the sphere of activity whose centre is the point of contact and radius λ, must be exceedingly small compared to K.

The above equation cannot be generally integrated; but in the case in which it belongs to a surface of revolution the axis of which is vertical, as, for instance, when the capillary tube is cylindrical with a circular base, an integral is obtained which

conducts to the inference, that the surface of the fluid approaches so much the nearer to that of a sphere as the diameter of the tube is smaller. Hence it immediately follows that the surfaces in tubes of different small diameters will be similar portions of spherical surfaces, if at their juncture with the interior surfaces of the tubes they make with them the same angle. This angle will appear to be independent of the diameter of the tube, from the consideration that the extent of the sphere of activity of the attraction of the tube is altogether imperceptible; so that even in a tube of very small bore, we may regard the action of the cylindrical surface on the superficial fluid elements contiguous to it, the same as if it were a plane*. Hence if $b =$ the radius of the fluid surface, and h its mean altitude above the horizontal level of the exterior fluid, then $R = R' = b$, and $\frac{H}{b} = g\,\varrho\,h$. But in different tubes the surfaces of the fluid being similar segments of spherical surfaces, b evidently varies as the diameter of the tube. Therefore h varies inversely as this diameter. Such is the explanation according to Laplace's theory of what may be looked upon as the principal phænomenon of capillary attraction.

If the surface of the interior fluid were *convex*, as it is known to be when a capillary glass tube is dipped in mercury, then if we suppose for a moment all below the tangent plane at any point to be fluid, the effect of the attraction on a canal terminating at this point perpendicularly to the surface, will only be just equal to the action of the fluid on the other extremity, where it terminates at the exterior horizontal level. If now we subtract the fluid between the tangent plane and the surface, which tends to draw the canal *upwards*, the resulting inequality of action must be counterbalanced by the hydrostatic pressure arising from a *depression* of the fluid in the tube. The mean depth to which the fluid will be depressed may be shown, as in the case of concavity, to vary inversely as the diameter of the tube.

From all this reasoning Laplace concludes that the attraction of capillary tubes influences the elevation or depression of the fluids they inclose, only by determining the inclination of the fluid surface to the contiguous surface of the tube, on which inclination the concavity or convexity of the fluid surface depends, as well as the magnitude of its radius. He consequently speaks

* M. Gauss, who first remarked that the reason here assigned by Laplace for the constancy of the angle of contact is vague and insufficient, has given a more satisfactory demonstration, which we shall have occasion to speak of in a subsequent part of the Report.

of the concavity or convexity as the principal *cause* of the phænomenon of elevation or depression, a mode of speaking to which some have objected apparently because it does not explicitly point to the nature of the forces to which the observed effects are due. This manner, however, of referring capillary effects to the concavity or convexity of the fluid surface, is convenient in the explanation of phænomena; for we may say in general, that wherever the fluid is bounded by a curve surface, it is acted upon at each point by a force tending from the surface towards the centres of the curvature at that point.

In this way Laplace explains the well known fact, that a drop of water put in a slender conical tube, having both ends open and its axis horizontal, will move towards the smaller end. The surface of the drop will be concave towards both ends of the tube, but with a greater curvature on the side directed to the smaller end than on the other. The drop will therefore be urged by two forces in opposite directions; but the forces being proportional to the curvatures, the greater force will be that which urges it towards the vertex of the cone. If a drop of mercury were inserted, its surface would be convex towards both ends of the tube, and the greater curvature would again be at that part of the drop which is nearer the smaller end. Therefore, of the two forces directed from the curved surfaces to the centres of curvature, that will prevail which urges the drop towards the base of the cone. It follows from the constancy of the angle of contact, that the surfaces of the two ends of the drop, whether it be of mercury or water, are similar segments of spherical surfaces. Their curvatures are therefore inversely as their distances from the vertex of the cone; and the difference of the curvatures, to which the difference of the forces which urges the drop is proportional, will vary inversely as the product of these distances, if the length of the column into which the drop is formed be given, that is, this length being small, nearly as the square of the distance inversely of the middle of the drop from the vertex of the cone.

As the fundamental equation obtained above admits of being successfully treated whenever the surface of the fluid contained in a capillary space is one of revolution, it may be employed to determine the capillary action which takes place between two cylindrical surfaces having a common axis and distant from each other by a small interval; for the surface of the inclosed fluid will evidently be in this case a surface of revolution. The result of the analytical calculation is, that the fluid will be raised in this space to the same height as in a tube of which the radius is equal to the interval between the cylindrical surfaces. If the

radii of the two cylinders be supposed infinitely great, we have the case of fluid inclosed between two vertical and parallel planes very near each other. The same result still holds good; and thus the experimental fact cited by Newton in his 31st query receives a theoretical explanation.

The case in which the fluid is raised or depressed between vertical parallel planes admits of being treated independently; and this Laplace has also done. The upper surface of the raised or depressed fluid is that of a common cylinder when the interval between the planes is small, and the elevation or depression is directly proportional to the curvature.

These propositions being proved with respect to the action of parallel planes, we may apply to the case of a drop inserted between two planes inclined to each other at a very small angle, reasoning analogous to that applied to a drop inserted into a cone of small vertical angle. The force by which the drop is urged, is shown, as before, to be inversely proportional to the square of the distance from the juncture of the planes. We have already mentioned that this law was obtained experimentally by Hauksbee for the case in which a drop of water is inserted between planes. He arrived at it by observing the inclination the planes must have to the horizon, that the effect of gravity may just counteract the capillary action by which the drop of water is drawn to the line of their junction. The sine of the inclination, to which the resolved part of gravity is proportional, was found to vary for the same drop when in equilibrium, inversely as the square of the distance of its middle point from the line of junction. Laplace's calculation, besides verifying this experimental result, further informs us, that if the two planes form with each other an angle equal to half the vertical angle of a cone which incloses a drop of the same fluid, the inclination to the horizon of the plane which bisects the angle formed by the two planes ought to be the same as that of the axis of the cone, in order that the drop may remain in equilibrium; and that "the sine of the inclination of the axis of the cone to the horizon, is nearly equal to a fraction whose denominator is the distance of the middle of the drop from the vertex of the cone, and numerator is the height to which the fluid is raised in a cylindrical tube, the diameter of which is equal to that of the cone at the middle of the drop."

If fluid be raised by capillary action between two vertical and parallel planes, they will be drawn towards each other. The same thing will happen if the fluid be depressed between them. These two facts, known by experience, were in a great measure explained, as we have seen, by Monge. The theory of Laplace

not only accounts for the attraction of the planes towards each other, but gives the measure also of the pressures which urge them. By an exact consideration of all the forces concerned in these phænomena, he finds that when the fluid is raised between the planes, each plane experiences, from without to within, a pressure equal to that of a column of the contained fluid, of which the height is half the sum of the elevations above the ordinary level, of the points of contact of the interior and exterior surfaces of the fluid with the plane, and whose base is the part of the plane comprised between two horizontal lines drawn through these points. The value of the pressure is similarly stated when the fluid is depressed between the planes. Hence, neglecting the small exterior elevation or depression, the pressure varies as the *square* of the elevation or depression between the planes, and consequently inversely as the square of the interval between them.

Laplace also enters into a consideration of the proposition first announced by Clairaut, viz. that if the law of the attraction of the matter of the tube upon the fluid, differs only by its intensity from the law of the attraction of the fluid on itself, the fluid will be raised so long as the intensity of the former of these attractions surpasses the half of the intensity of the other. He arrives at the following conclusions*. If the one intensity be exactly half the other, there is neither elevation nor depression. If the intensity of the attraction of the tube for the fluid be insensible, the fluid will be depressed, and the depressed surface will be convex and hemispherical. If the two intensities be equal, the surface of the elevated fluid will be concave and hemispherical. When the intensity of the attraction of the tube is the greater of the two, the fluid, by attaching itself to the tube, forms an interior tube to which alone the capillary elevation is due, and which being of the same matter as the raised fluid, acts with the same intensity, and causes the surface to be still concave and that of a hemisphere. This appears to be the case with water and oils in capillary glass tubes. M. Haüy found by ex-

* These conclusions appear to be correct, but the reasoning of Laplace in this part of his theory is liable to a serious objection, first pointed out by Dr. Young. For in art. 12, he obtains the following equation,

$$Q \cos(\pi - \theta) = (2\varrho' - \varrho) K \sin \theta,$$

in which ϱ' and ϱ are respectively proportional to the intensities of the attraction of the solid for the fluid, and the fluid for itself; $2\varrho K$ is equal to the resultant of the molecular attractions on a superficial particle of all the fluid particles within the sphere of its activity; but ϱ the resultant of the attractions of only a portion included between a tangent plane and surface passing through the particle. This equation, therefore, could scarcely be true unless $2\varrho' = \varrho$. The source of the error that occurs here will be elucidated as we go on.

periment that the concave surface of these fluids differed little from that of a hemisphere*. It would seem from this theory that the intensity of the solid's attraction for the fluid must exceed that of the fluid for itself, in order that the fluid may *wet* the solid.

The preceding are the principal facts which Laplace explains in his first published Treatise on Capillary Action. The explanations of some few others are added by way of corollaries at the end of the Treatise. One of these, which serves to exhibit the effect of the convexity of the fluid surfaces, may be mentioned here.

If a capillary tube be plunged to a small depth in water, and then, with its lower extremity closed by the finger, be taken out, on withdrawing the finger the fluid will be seen to sink in the tube, and to form a drop at the lower end. But when it has ceased to descend, the height at which it rests above the extremity of the tube is always greater than the elevation due to capillary action when the tube is just dipped in the fluid. The reason of this excess is, that the effect of the convexity of the drop, which takes place in the upward direction, is added to the effect of the concave surface within the tube.

Hence it follows that if a slender siphon with unequal arms be filled with water, when the fluid is just on the point of running from the longer arm it has to overcome the capillary actions due to the concavity formed at the extremity of the shorter arm and the convexity at that of the longer arm; and unless the difference of the lengths of the arms be greater than the sum of the lengths of the fluid columns which these two actions will sustain at the respective extremities, the fluid will not run.

The theory of Dr. Young respecting the phænomena of capillary tubes is contained in an "Essay on the Cohesion of Fluids†," read before the Royal Society, December 20, 1804, and inserted in the second volume of his *Lectures on Natural Philosophy*. His views resemble those advanced by Segner and Monge. Like these two mathematicians, he considers the phænomena to be referable to the cohesive attraction of the superficial particles of the fluids, in so far as it gives rise to a uniform tension of

* It would be difficult to decide by experiment whether the surface be nearly or exactly a hemisphere, because when the angle of contact is very small, the line of contact is not readily discernible. The method of determining the angle of contact by reflection, proposed by Dr. Young in his *Lectures on Natural Philosophy*, (vol. ii. p. 666,) is preferable to measuring the sagitta of the surface, which was the method adopted by Haüy.

† *Philosophical Transactions*, 1805, p. 65.

the surface. He shows, moreover, how this uniformity of tension may be a consequence of ulterior principles.

The course of reasoning Dr. Young pursues in his essay is as follows. He begins with making two assumptions: first, that the tension of the fluid surface is uniform; secondly, that at the juncture of a fluid surface with the surface of a solid, there is an appropriate angle of contact between the two surfaces. "This angle," he says, "for glass and water, and in all cases where a solid is perfectly wetted by a fluid, is evanescent: for glass and mercury, it is about 140° for common temperatures, and when the mercury is moderately clean." He shows next that a theory founded on these two hypotheses will explain various capillary phænomena. And lastly, at the end of the essay, derives the hypotheses from ulterior physical principles. It is in this last part that Dr. Young's theory contains views not to be found in any previous theory. Following the order which the author adopts, I will endeavour first to exhibit the way in which his theory accounts for phænomena.

It is known from mechanical principles that if a curve line be uniformly stretched, the normal force it exerts at any point in a direction tending to the centre of curvature is directly as the curvature. The same will be the case with a *surface*, if it be cylindrical, and therefore curved only in one direction. If the surface be spherical or like that about the vertex of an elliptic paraboloid, the curvatures in directions at right angles to each other will have independent effects. Consequently the normal force in this case will vary as the sum of the curvatures: and as, from a known property of curve surfaces, this sum is the same for all perpendicular directions, the normal force will be proportional to the sum of the greatest and least curvatures. Hence because this force, applied at the surface, is employed in depressing the fluid when the surface is convex, and elevating it when concave, (for it is always directed to the centres of curvature,) it may be shown in the usual manner, that by reason of the action of gravity, the force at each point is proportional to the distance of that point from the ordinary level of the fluid. By reasoning of this kind, Dr. Young is conducted to the relation between the vertical ordinate and curvature of the surface, which is expressed by the fundamental equation of Laplace's theory. As both theories also admit the constancy of the angle made by the fluid surface with that of a given solid at the juncture of the two, it is plain that the explanation of phænomena must be virtually the same in both. In fact, before the publication of Laplace's theory Dr. Young had accounted for most of

the facts whose explanations according to that theory have already been exhibited, and his mode of accounting for them differs not in any essential respect from that of Laplace, but chiefly in a scrupulous avoidance of the use of mathematical symbols. It will therefore be unnecessary to adduce the explanations of any of these facts given by Dr. Young; we will only advert to some applications contained in his treatise which do not occur in the other.

Having first considered the rise of water in capillary tubes, he proceeds to find the weight of water raised by the horizontal surface of a solid elevated from the horizontal surface of a fluid, and to determine the relation between the height of ascent in a given tube to the *height of adhesion*; that is, the height of elevation above the ordinary level just when the fluid detaches itself from the horizontal surface of the solid. The fluid is supposed to wet the solid. Hence as the fluid surface, being horizontal where it is in contact with the solid, has no tendency by its tension to depress, the weight of water raised is very nearly equal to the hydrostatic pressure of a column of water standing on the raising surface and equal in height to the height of adhesion. This pressure he finds to be $50\frac{1}{8}$ grains on a square inch, agreeing very nearly with the result of experiments by Taylor. If the raising surface be small, for instance a disc of an inch in diameter, the curvature of the horizontal sections of the raised fluid, which are convex outwards, will have a contrary effect to the curvature of the vertical sections which are concave, and will consequently diminish the weight of fluid raised. This also is confirmed by experiment. "The height of ascent in a tube of given bore varies in the duplicate ratio of the height of adhesion."

The depression of mercury in capillary tubes is next considered, and the author does not confine himself to the case in which the surface of the mercury is spherical, which is true only when the diameter of the tube is very small. In tubes less than half an inch in diameter, the surface is very nearly that of an oblate spheroid. The depressions for different tubes calculated theoretically, are compared with a table of experiments made by Lord Charles Cavendish to ascertain the depression of mercury in different barometer tubes.

The height of adhesion of mercury to glass, and to substances which it is capable of wetting, such as gold, silver, tin, &c., as well as the thickness at which a portion of mercury will spread out on glass and on substances wholly incapable of attracting it, are quantities all determinable by this theory, and being cal-

culated are found to be sufficiently accordant with the same quantities determined experimentally.

The theory conducts also to the following result: "The linear dimensions of similar drops of different fluids depending from a horizontal surface vary in the same ratio as the heights of ascent of the respective fluids against a vertical surface, or as the square roots of ascent in a given tube."

In explaining the instances of apparent attractions and repulsions treated of by Monge, Dr. Young shows that in the two cases of attraction, the force which urges the planes towards each other varies inversely as the square of the interval between them, because it varies once inversely as the distance on account of the increase of curvature of the fluid surface as the interval diminishes, and again inversely as the distance by reason of the increase, proportional to the ascent of the fluid, of the surface on which the capillary action is exerted. With respect to the third case, that of repulsion, which is omitted in Laplace's treatise, he remarks, that "the repulsion of a wet and dry body does not appear to follow the same proportion, for it by no means approaches to infinity on the supposition of perfect contact: its maximum is measured by half the sum of the elevation and depression on the remote sides of the substances, and as the distance increases, this maximum is only diminished by a quantity which is initially as the square of the distance."

The strong cohesion of two solids produced by the interposition of a small quantity of a fluid, which wets them, between their plane surfaces, is sufficiently accounted for by the great curvature, arising from the proximity of the surfaces, of the outer boundary of the interposed fluid, which is everywhere concave like the rim of a pulley. This curvature corresponds to a force which would be capable of sustaining a great elevation of the fluid; but in this instance the force is not exerted in supporting the fluid, but acts by reason of the mutual attraction between the solid and fluid, on the plane surfaces of the solids, drawing them together. If the solids were wholly immersed in the fluid, no such cohesion would take place. On the same principle, if fluid be interposed between two solids which it is quite incapable of wetting, so that its boundary is everywhere convex, the force due to the convexity, being directed *inwards*, would present a strong opposition to any force tending to make the planes approach each other.

After showing, in the manner exhibited above, that a theory founded on the two assumptions of a uniform tension of curved fluid surfaces, and a constant angle of contact of the surface of

a given fluid with that of a given solid, will explain various capillary phænomena, Dr. Young proceeds to derive these laws from ulterior physical principles. To account for the first he reasons as follows. The repulsive force which appears to act uncontrolled in aeriform bodies, exists also in fluids and solids. In these it is counteracted by a cohesive force. These forces in fluids are so balanced, that they allow the particles to move freely in all directions. In solids the cohesion is accompanied by a force opposed in greater or less degree to all lateral motion, and independent, as he supposes, of the cohesive force. He considers it simplest to regard the cohesive force as nearly or perfectly constant in its magnitude throughout the minute distance to which it extends, and its apparent variation to be owing to the variation of the repulsive force, which diminishes with the increase of the distance. In the internal parts of a fluid, the two forces hold the particles in equilibrium; but wherever the surface is curved or angular, it would be found by collecting the effect produced on a given particle at the surface, by all the particles contiguous to it and lying within the sphere of its proper activity, that on the above hypothesis respecting the relative variation of these forces, the cohesion must necessarily prevail over the repulsion. The particle will consequently be urged in the normal direction towards the centres of curvature of the point at which it is situated, whether the surface be concave or convex; and reasons are adduced by the author for concluding that the force which urges it is proportional to the curvature, if single, or to the sum of the curvatures in rectangular directions, and consequently indicates, as is known from Mechanics, a uniform superficial tension. The reasoning by which he shows this need not be introduced here. Suffice it to say, that this result might be obtained by an analysis precisely the same as that which Laplace has employed at the beginning of his capillary theory, in calculating the attraction experienced by a superficial particle on the supposition of forces sensible only at insensible distances. Laplace makes no hypothesis respecting the law of force, excepting that it is wholly attractive. The mathematical calculation would remain the same, if the force were supposed partly attractive and partly repulsive, provided the attractive force decreased less rapidly with the increase of distance than the repulsive, and became the greater of the two before it ceased to be of sensible magnitude. With this alteration* in Laplace's theory, it would

* This modification of the theory is pointed out by its author in a note at p. 122 of the *Bulletin de la Société Philomatique*, An 1819.

differ in no essential respect from that of Dr. Young, as far as regards the principles on which the equation relative to the capillary fluid surface is obtained; and neither possesses an advantage over the other in the explanation of phænomena. But the way in which the latter mathematician accounts on physical principles for the constancy of the angle of contact, (which is the other hypothesis of his theory,) though incomplete, is more satisfactory than anything we meet with on the same subject in Laplace's treatise. The following is his reasoning on this point.

When the surface of a fluid is free, or exposed to a gas, the superficial tension arising from the cohesive power of the particles acts with full force to produce pressure directed inwards, from which, in fact, arises the tendency observable in small fluid masses to assume a globular form. This contractile power is altered by contact with a solid surface. For instance, if a cube of water had one of its halves congealed, its other properties remaining the same, the other half would retain its form, because the tendency to contract at the edges contiguous to the solid, would be just counteracted by an equal and contrary action of the solid; and at all other points of contact, the contractile force would vanish for the same reason as at any point in the interior of the fluid. If the solid were of smaller attractive power than the fluid, the tendency to contract, and consequently the tension of the surface in contact, would be proportional only to the *difference* of the attractive forces, or to the difference of the densities of the solid and fluid, if the law of the forces be the same for both*. The portion of the solid surface which is contiguous to, but not touched by, the fluid, will act on the fluid particles situated at the angle of contact, just as the fluid superficies itself does, but in a different degree according to the difference of density. Hence, the conditions of equilibrium are to be sought of three forces acting on a particle at the angle of contact, one in the direction of the surface of the fluid only, another in that of the common surface of the fluid and solid, and the third in the opposite direction along the exposed surface of the solid. If ϱ and ϱ' be the densities respectively of the fluid and solid, and the first force be $k\varrho$, the second will be $k(\varrho - \varrho')$, and the third $k\varrho'$. If then θ be the angle of contact which the free surface of the fluid makes with its surface of contact, the first force re-

* Dr. Young adduces some facts relating to the spreading of the drops of oil on water in support of the proportion here assigned. (*Lectures on Nat. Phil.*, vol. ii. p. 659.)

solved in the direction of the solid surface, will give $k\varrho \cos\theta$, which is counteracted by $k\varrho' - k(\varrho - \varrho')$, the difference of the other two forces. Hence it follows that

$$\varrho' = \varrho \cos^2 \frac{\theta}{2}.$$

When $\theta = 90°$, $2\varrho' = \varrho$, as Clairaut found. The preceding theory is deficient in not informing us how the other resolved portion of the tension of the fluid, viz. $k\varrho \sin\theta$, is counteracted.

The essay of Dr. Young, as it appears in the second volume of his *Natural Philosophy*, contains by way of appendix some remarks and strictures on Laplace's theory, the results of which are shown to be readily derivable from his own theory; but an objection is raised against its principles on the ground that no account is taken of a repulsive molecular force. We have already seen in what way this objection may be obviated without affecting the results of Laplace's theory, or materially altering the analysis. Another objection urged by him, to which allusion has been made before (p. 266), lies against the reasoning, and not the principles, of Laplace's theory. In determining the conditions of equilibrium of a fluid particle situated at the angle of contact, Laplace takes account only of the attractions of the solid and fluid upon it, omitting the consideration of the variation of *pressure* occasioned by these forces as well near the free surface of the fluid as near that in contact with the solid. The error to which this omission leads will be understood by reverting to the reasoning by which it was shown (p. 258), according to Clairaut's method, that when the attractive force of the fluid is double that of the solid, the capillary surface will be horizontal. The same kind of reasoning as that employed to show that the horizontal attractions in this case counterbalance each other, would also prove that the particle at the angle of contact is urged vertically downwards, by only *half* the force with which another particle at the fluid surface situated beyond the sphere of the attraction of the solid, is urged in the same direction. The horizontality of the fluid surface may nevertheless be maintained if we consider that the variation of pressure near the surface, due to the molecular attractions, will not be the same at the surface in contact with the solid as at the free surface, by reason of the solid's attraction. Had Laplace taken account of this circumstance, as the principles of his theory required him to do, notwithstanding the supposition of incompressibility, he would have obtained an equation equivalent to that which expresses above the relation between ϱ' and ϱ, instead of the faulty equation of art. 12 of his treatise. In fact, M. Gauss and M. Poisson, as

we shall afterwards see, have obtained in different manners such an equation on the supposition of absolute incompressibility.

The addition to Dr. Young's essay contains also a more careful investigation than he had given before of the depression of mercury by capillarity in barometer tubes of diameters not exceeding half an inch. He obtains two formulæ, one for the central depression, the other for the difference between the central and marginal depressions. The diameter in inches being d, and e being put for $\frac{\cdot 015\, d}{d^2 + \cdot 16}$,

the first formula is $\frac{\cdot 015}{d} - \frac{3}{4}\, e - 14 \,.\, 5\, e^3$,

and the other, $\frac{5\, d + 100\, d^3}{15\, (5\, d + 100\, d^3) + 18}$.

The next work that we have to notice is Laplace's *Supplement to the Theory of Capillary Action*, in which the object of the author is to perfect the theory and extend its applications, to confirm it by additional comparisons with experiments, and to present it under a new point of view. This work is prefaced by a discussion relative to the fundamental equation of the theory, which is shown to be derivable as well from the condition of the perpendicularity of the resultant of the forces to the fluid surface as from that of the equilibrium of canals, the equation obtained by the former method being the differential of that given by the other. Here also is deduced, from the fundamental equation, an expression for the weight of fluid raised in a cylindrical tube, the transverse section of which is any continuous and reentering curve. If c be the contour of the horizontal section of the tube, H and ϱ the same as in the fundamental equation, and θ the angle of contact as before, the weight of the fluid column is found to be $\frac{\mathrm{H}\,\varrho\, c}{2} \cos\, \theta$. It is to be observed that this result is obtained by *assuming* the angle of contact to be constant for the same solid and fluid, of which law Laplace has failed to give a satisfactory proof.

After these preliminaries, the author proceeds to consider capillary action in a manner different from that of his former treatise *. It may be proper to remark that the subject admits of this other method of treatment for the same reason that in

* In the observations with which the *Supplement* concludes, the author remarks that this second method resembles that of Jurin, while the other may be classed with the method of Segner and Young; and that the reasoning by which Jurin proved the elevation in a capillary tube to be inversely as the diameter, is correct when the tube is completely wetted by the fluid, and when in

common statical problems there are two kinds of equations of equilibrium, those in which pressures and tensions are involved, and those which result when forces of this kind are eliminated. The force corresponding to the convexity or concavity of the fluid surface in capillary tubes is of the nature of a tension, and may be kept out of view in finding the conditions of the equilibrium of the forces which suspend or depress the fluid column. For this purpose it will be necessary to pay regard only to the action of the tube on the parts of the fluid contiguous to it, and to the action exerted on the raised or depressed column contained within the tube by the rest of the fluid. The former, as Laplace shows, resolves itself into the attraction of a ring of the tube immediately above the extreme upper edge of the fluid, and an equal attraction of a like ring at the lower extremity of the tube; and the latter, into the attraction of the upper extremity of a tube of the fluid, supposed to be a continuation of the solid tube, and attracting with the proper action of the fluid on itself. The first two forces tend to raise the fluid, the other to depress it. If the total upward force be called $2q'\varrho c$, and the downward force $q\varrho c$, both being proportional to c the contour, it will hence appear that $(2q' - q)\,\varrho c =$ the weight of fluid raised. If $2q' = q$, there is no elevation; a result which, as we know, may be obtained in a very different manner.

The preceding expression for the weight of the elevated column being equated to that previously obtained, gives

$$2q' - q = \frac{H}{2}\cos\theta.$$

Now it is shown satisfactorily in the former treatise*, that when $q' = q$, $\theta = 0$: so that $q = \frac{H}{2}$. This equality is also proved by Laplace in an independent manner. It hence follows that $\frac{q'}{q} = \cos\frac{2\theta}{2}$, which equation, if q' and q be assumed to be in the proportion of the densities of the sold and fluid, is the same as that first obtained by Dr. Young.

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consequence the elevated column may be conceived to be contained in an aqueous tube. Leslie, in a paper containing some original and ingenious views on Capillary Attraction, (*Phil. Mag.*, vol. xiv. 1802, p. 193,) objects to the principle of Jurin's explanation, and attributes the rise of the fluid solely to its perpendicular pressure against the surface of the tube occasioned by the tube's attraction. By this consideration he finds the height to be inversely as the diameter. No doubt, as Leslie appears to have first observed, the rise is dependent on a particular state of pressure at the surface of contact of the solid and fluid, and according as this is greater or less than a certain pressure, there will be a rise or depression.

* Art. 12. p. 48.

As it appears that the weight of the elevated column varies as the periphery of the transverse section of the tube; and as, for tubes of like peripheries, the weights are as the products of the heights and the squares of homologous lines, it follows that the heights are inversely as homologous lines. This proportion, says Laplace, is true also if the contour be not continuous, but of the form, for instance, of a rectilinear polygon; for the error that would be occasioned by the angles of the polygon would be of insensible magnitude, by reason of the small extent of the sphere of activity of the particles. Gellert has made some experiments on the elevation of water in glass prismatic tubes, with rectangular and triangular bases *. They confirm the law according to which the heights are inversely as the homologous lines of like bases. He thinks also that the elevation is the same for a rectangular as for an equal triangular base; but the experiments do not appear to be decisive of this point. Laplace calculates theoretically that the difference would be one eighth between the elevations in a prism with a square base, and in one whose base is an equilateral triangle of equal area.

One of the most interesting of the questions considered by Laplace in the *Supplement*, relates to the capillary action which takes place when two or more fluids are contained in the same tube. Suppose a prismatic tube to be plunged vertically in a vessel containing any number of fluids lying horizontally one above another; then "the excess of the weight of the fluids contained in the tube above the weight it would have contained without capillary action, is equal to the weight of fluid which would have been raised above the exterior level, in case the vessel contained only that fluid in which the lower extremity of the tube is immersed." For, in fact, the action of the prism and of this fluid on the column of it in the tube, is plainly the same as in this case, the action of the tube on each of the other fluids being equal in opposite directions, and the mutual action of the fluids being destroyed just as if they were supposed to form a solid mass. The surface of the uppermost fluid is the same as if that alone were in the tube.

When only two fluids are contained in a cylindrical tube, the theory determines the common surface of their junction to be spherical, and gives for the angle of contact† of this surface a

* *Memoirs of the Academy of Petersburgh*, vol. xii. p. 302.

† It ought to be observed that the angle which is here and elsewhere called the angle of contact is, strictly speaking, not the angle which the fluid surface makes with the surface of the solid at the points of their junction, but the angle which a tangent plane to the surface of the fluid at the limit of the sphere of activity of the solid's attraction makes with the surface of the solid.

formula involving the angles of contact proper to the fluids when separately contained in the tube.

When water and mercury are put in the same cylindrical glass tube, and the water completely wets the tube, the mercury may be considered to be contained in an aqueous tube, the action of which on the mercury being small, the angle of contact is nearly 180° instead of 136°·8, which, according to Laplace, is that between glass and mercury. This result, which is also deduced from the above-mentioned formula, is confirmed by the observations of Gay-Lussac. Another deduction from the theory also receives confirmation from the same eminent experimenter, viz. that mercury is less depressed in a capillary tube when its upper surface is covered with a small portion of water, than when covered by alcohol. For the capillary action of water on itself exceeds that of alcohol on itself, and would therefore be likely to have a greater action than alcohol on mercury.

Various other interesting results, which it would be long to enumerate here, are readily deduced by the method of considering capillary effects exhibited by the author in his *Supplement*. This method, in some applications, leads to results more rapidly than that of the *Treatise*; while at the same time the latter has advantages peculiar to itself in all questions relating to the surfaces of fluid inclosed in capillary spaces, or subject in any way to capillary action. Three such questions, which had been either omitted or partially treated in the first work, are handled at considerable length in the *Supplement*; and it will be proper now to advert to their solutions. These problems, which for the most part had previously engaged the attention of Dr. Young, are: (1.) The apparent attraction and repulsion of small bodies swimming on the surfaces of fluids. (2.) The adhesion of discs to the surfaces of fluids. (3.) The figure of a large drop of mercury, and the depression of mercury in a glass tube of a large diameter.

(1.) By considering generally the capillary action between two vertical and parallel planes of different matters plunged with the lower extremities in the same fluid, the following theorem is obtained: Whatever be the substances of which the planes are formed, the tendency of each towards the other is equal to the weight of a fluid prism whose height is the difference of elevation of the extreme points of contact of the fluid on the opposite sides of the plane, whose depth is half the sum of these elevations, and breadth, that of the planes. The elevation is to be taken negatively when it changes into a depression; and if the product of these three dimensions be negative, the apparent attraction of the planes becomes a repulsion. These tendencies

are shown to be the same for the two planes, and their actions on each other by the intervention of the fluid to be equal and opposite. The theory leads to the singular result that the repulsion will change into attraction by making the planes approach very near each other, and experiments by M. Haüy show that such is the fact.

(2.) When a disc is applied to the surface of standing water, on being raised it draws, by capillary action, a portion of the fluid with it, which detaches itself when the disc is raised above a certain elevation. At this limit the suspending force must plainly be equal to the weight of the disc and of the portion of fluid raised above the horizontal level of the water. As this force may be accurately determined by experiment, we are furnished with means of putting the theory to a test, because the surface, and consequently the volume of the raised column, can be found from the fundamental equation of the theory. When the calculation is made, the expression for the volume involves a quantity which Laplace calls $\frac{2}{\alpha}$, and which is, in fact, the same as $\frac{2\,\mathrm{H}}{g}$. Now, as was said above, the weight of fluid raised in a capillary tube is $\frac{\mathrm{H}\,\varrho\,c}{2}\cos\theta$, or $\frac{\mathrm{H}\,\varrho\,c}{2}$ if the fluid perfectly wets the tube; and this weight in a cylindrical tube is also equal to $\frac{1}{2}\,r\,c\,h\,g\,\varrho$, if r be the radius of the cylinder, and h the mean elevation of the fluid; so that $\frac{2\,\mathrm{H}}{g} = 2\,r\,h =$ diameter of the tube $\times$ mean elevation of the fluid *. If, then, the diameter of a cylindrical capillary tube, and the elevation in it of a fluid which perfectly wets it, be observed, the value of $\frac{2}{\alpha}$ for that fluid is found experimentally. The following numerical values of $\frac{2}{\alpha}$ are deduced by Laplace from the experiments of Gay-Lussac, after correcting for the temperature †, which was 8°·5 of the centigrade thermometer, and for the difference of one sixth of the diameter between the mean elevation, and the observed elevation of the lowest point of the

* This equality is also readily deduced from the fundamental equation.

† "The elevation of a fluid which wets completely the sides of a capillary tube is, at different temperatures, in the direct ratio of the density of the fluid, and in the inverse ratio of the interior diameter of the tube." This is shown at p. 38 of the *Supplement*. An increase of temperature diminishes the elevation both by diminishing the density of the fluid and increasing the capacity of the tube. Admitting that H varies as ϱ, it will be seen from the equation above that h varies as $\frac{\varrho}{r}$.

surface. The diameter of the tube was in each case $1^{mi}\cdot29441$.*, or in English measure, ·05096 of an inch.

		Mi.mi.		Sq. Inch.
For water, ...	$\frac{2}{\alpha}$ =	30·2621	=	·0469
For alcohol, sp. gr. ·81961, ...	$\frac{2}{\alpha}$ =	12·1649	=	·0188
For oil of turpentine, sp. gr. ·86946, . .	$\frac{2}{\alpha}$ =	13·1606	=	·0204

By means of these values the weights of the columns of fluid raised by a disc of white glass, $118^{mi}\cdot366$, or 4·66 inches in diameter, just when the fluid detaches itself from the disc, are determined by the theory to be respectively

Grammes.	Grammes.	Grammes.
59·5873	31·1435	34·350,

and the experimental determinations † are,

Grammes.	Grammes.	Grammes.
59·40	31·08	34·104.

The nearness of these to the theoretical results not only confirms the theory, but shows also the correctness of the values of $\frac{2}{\alpha}$ deduced from the experiments on capillary tubes. Different experimenters ‡ have determined differently the heights of

* The ^{mi} attached stands for *millimetre*, and $^{mi.mi.}$ for *square millimetre*.

† These weights expressed in English grains are respectively 917·14, 479·87, and 526·56, which being divided by the number of square inches in the surface of the disc, give 53¾ grains corresponding to each square inch for water, 28 grains for alcohol, and 31 grains for oil of turpentine. Achard obtains for water 39½ grains, for alcohol 23·4 grains: Dutour finds 44·1 grains, and 25·6 grains. In the experiment of Taylor (*Phil. Trans.* 1721) on the attraction of wood to water, the raising force was 50 grains to each square inch.

‡ The following Table is given in the Art. CAPILLARY ATTRACTION of the *Edinb. Encycl.*: the heights are reduced to a tube whose diameter is ·01 of an inch.

Height of the water.	Height × diameter.	Observers.
Inches.	Sq. Inch.	
2·1	0·021	Haüy and Tremery.
2·6	0·026	Hällström.
3·27	0·0327	Dr. Brewster.
3·92	0·0392	Musschenbroek.
4·0	0·04	Average assumed by Dr. Young.
4·2	0·042	Monge.
4·28	0·0428	Weitbrecht.
4·6	0·046	From Morveau's experiments.
4·8	0·048	Martin.
5·3	0·053	Atwood.

The experiments of Sir David Brewster were made with much care, and em-

elevation in glass tubes, probably by reason of different degrees of humidity of the interior of the tubes. When the tubes were well wetted, Gay-Lussac found the elevations to be always very nearly the same in different experiments. As the weights required to detach discs from fluid surfaces can be measured with considerable precision, the accordance of the preceding experimental and theoretical values serves to verify the experimental values of $\frac{2}{\alpha}$.

Equal discs of different substances perfectly wetted by a fluid, ought to raise columns of the same weight, because the resistance to the separation of the disc is, in each case, produced by the adhesion of the fluid to itself, that is, to the stratum of fluid that lines the inferior surface of the disc.

As the angle of contact of mercury with glass under water is nearly 180°, a glass disc applied under water to the surface of mercury would not be capable of drawing up any portion of mercury on being raised from the surface.

These two conclusions from the theory have been confirmed experimentally by Gay-Lussac.

(3.) The analytical calculation for determining the form of a large drop of mercury serves also to find the depression of this fluid in a glass tube of large diameter. Gay-Lussac ascertained by experiment that the height at which a large drop of mercury stands on a horizontal plane of glass is $3^{mi}\cdot378$ (= ·133 of an inch), agreeing very nearly with the result of a like experiment by Segner. The drop was circular and one decimetre, or 3·937 inches in diameter, and the temperature at 12°·8 of the centigrade thermometer. To calculate the height theoretically, it is necessary to know the value of $\frac{2}{\alpha}$ for mercury in a glass tube, and its angle of contact with glass. Laplace takes for the former $13^{mi.mi}$, (= ·0201 square inch), and for the latter, 152° (= 136°·8). These values, he says, are mean results obtained by comparing several observed capillary phænomena with the theory, and may be further rectified by more numerous experiments. The height of a large drop of mercury given by the theory by means of these data is $3^{mi}\cdot39664$.

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brace a great variety of fluids; but the tube does not appear to have been moistened in its whole extent. The results are tabulated in the article above referred to, as well as those from the experiments of Mr. B. Martin, which, for the same fluids, uniformly give a higher value of the constant. The values of the constant for alcohol and oil of turpentine, as found by Gay-Lussac, do not differ materially from the determinations of others. Sir David Brewster finds for alcohol ·0178, for oil of turpentine ·0187; Martin, for the same fluids, ·018 and ·022; Musschenbroek for alcohol ·021.

Gay-Lussac also observed in a large glass vessel, containing mercury and having its sides vertical, the difference between the extreme elevation of the fluid and the elevation at the points of contact with the sides of the vessel, and found it to be $1^{mi}\cdot455$ (= ·057 in.). The theory gives $1^{mi}\cdot432$.

Laplace concludes his work with some general observations respecting the interior constitution of bodies, and the nature of molecular forces. The *viscosity* of fluids, he remarks, is a disturbing cause in capillary phænomena, which can be strictly explained by the theory only when the condition of perfect fluidity is fulfilled. To that cause and the friction against the sides of the tube, he considers the differences between the elevations of water in capillary tubes as determined by different observers, to be attributable. With respect to the variation of pressure from nothing to the quantity K, which according to the calculations of the theory ought to take place within a space extending from the free surface of the fluid to a small depth below, Laplace observes that it may be attended with a sensible variation of density, and have a perceptible effect on capillary phænomena. The modification that his theory must undergo if this circumstance be taken into account, has been fully discussed, as we shall presently see, by M. Poisson.

There is a good exposition of the leading propositions of Laplace's theory by M. Petit in the Journal of the Polytechnic School*.

The work of M. Gauss, entitled *Principia generalia Theoriæ Figuræ Fluidorum in statu Æquilibrii*†, has for its main object the correction of the defect already pointed out in Laplace's theory, with regard to the proof of the constancy of the angle of contact. To form the equations of equilibrium, M. Gauss employs the principle of virtual velocities, which he applies to the whole mass of the fluid, and not, as Lagrange has done, to a differential element. This elegant method, which has the peculiar advantage of evolving at once the equation of the free surface of the fluid, and that relative to the contour, conducts to a sextuple integral, which extends to the whole mass, and is to be a minimum. By supposing the fluid to be homogeneous and incompressible, the integral becomes quadruple. By further supposing the only forces that act to be gravity and the molecular attractions of the fluid and containing solid, and the sphere of activity of these attractions to be insensible, the quantity (W) to be a minimum, is found to be expressed by

$$\int z\,ds + \alpha^2\,U + (\alpha^2 - 2\beta^2)\,T,$$

* tom. ix. cah. xvi. p. 1.

† Gottingen, 1830.

in which the first term is equivalent to the volume of the contained fluid multiplied by the vertical ordinate of its centre of gravity, U is the area of its free surface, and α^2 * a constant depending on the intensity of the attraction of the fluid particles for each other, T is the area of the solid surface with which the fluid is in contact, and β^2 a constant depending on the intensity of the attraction to which the fluid is subject from the particles of the solid. By making $\delta W = 0$, and in the variation supposing U constant, it is readily found that the mean elevation in a capillary tube varies inversely as its diameter. By again putting $\delta W = 0$, and making the free surface subject to variation, M. Gauss arrives at two equations, one of which, relating to the free surface, is the fundamental equation of Laplace's theory, and the other, relative to the angle of contact, is equivalent to that which Dr. Young obtained. It is not the object of the author to trace the consequences to be derived from these equations in explaining phænomena, as this was satisfactorily done by Laplace.

The first published ideas of M. Poisson on the theory of capillary action are contained in a memoir on the Equilibrium of Fluids, read before the Paris Academy November 24, 1828†. His object in this memoir is to form the equations of the equilibrium of fluids on physical principles, that is, by considering them as composed of distinct molecules, separated from one another by spaces void of ponderable matter. He commences with the following preliminary notions.

The dimensions of the molecules and of the spaces between them are so small, that a line which may be supposed a great multiple of them is of insensible magnitude. The molecules attract each other; at the same time they mutually repel by their proper heat. For each of these forces the action is equal to the reaction: both decrease with great rapidity as the distances increase, and are sensible only at insensible distances. The radii of activity of these forces are nevertheless supposed to be extremely great compared with the intervals between the molecules, and the rapid decrease to commence only at distances which are great multiples of these small intervals. Without this supposition, in bodies whose molecules are not regularly distributed, the resultant of the molecular forces, that is, the total force which acts on each molecule, might be very different in magnitude and direction for two consecutive molecules, and consequently would not be subject to the law of continuity. It seems necessary therefore to make the above supposition.

* $4\alpha^2$ in M. Gauss's work is the same as the $\frac{2}{\varpi}$ of Laplace.

† *Mémoires de l'Académie des Sciences*, tom. ix. Paris 1830.

By *molecular action* M. Poisson understands the excess of the repulsion above the attraction between the molecules. This force he supposes to be different for different points of the two molecules. Its mean value he calls the *principal* force, and the variation from this normal value according as different points of the molecules are directed towards each other, the *secondary* force. This latter plays an important part in solids, giving rise to their rigidity and resistance to the lateral motion of their molecules. Its absence from fluids is the occasion of the perfect mobility of their particles. The characteristic of fluids that distinguishes them from solids is thus stated: If a point be taken anywhere in the interior of a fluid mass, and a straight line of insensible length but a great multiple of the mean interval between the molecules, be drawn in *any* direction from that point, the mean interval between the molecules that lie on the line is constant, though the particles may be irregularly disposed along it. The constancy of this mean interval is considered to be owing to the absence of the secondary force above mentioned, by reason of which the molecules can readily take positions that satisfy this condition*.

Setting out with these principles M. Poisson arrives at equations relative to the pressure in the interior of a fluid mass, which are the same as those usually obtained on the supposition of equality of pressure in all directions from any given point. The reasoning by which these equations are deduced is not immediately founded on any observed fact, and as it conducts to the same equations as those deducible from the known law of equal pressure, it may be said to account theoretically for the existence of this law. This is an instance of mathematics applied in the manner spoken of at the commencement of the Report. The bases of the reasoning are hypotheses; and it leads to the explanation of a known fact. It would not be right to conclude from this one explanation that the hypotheses must necessarily be true. They can be considered to be satisfactorily established, only when they have been successfully employed in accounting for all the facts that are known to depend on the intimate constitution of fluids, and when they are found to require for this purpose no modification.

After calculating, on the above-mentioned hypotheses, the

* Dr. Young has also speculated on the interesting subject of the immediate cause of *fluidity*. He remarks in his Lecture on Cohesion, (*Lectures*, vol. i. p. 620,) that "the apparent weakness of the cohesion of fluids is entirely owing to the mobility of their particles." It would be perhaps more correct to say, that a weak cohesive power is a condition necessary to the existence of a great degree of mobility.

pressures in the interior of a fluid, M. Poisson finds equations of equilibrium relative to the surface of separation of two fluids incumbent one on the other, and thence, by supposing one of the fluids suppressed, the equation of the free surface of a single fluid. The principal conclusion arrived at in the memoir is thus stated in Art. 31 : "Capillary phænomena are due to the molecular action resulting from the calorific repulsion and an attractive force, and modified not only by the form of the surfaces as in Laplace's theory, but moreover by a particular state of compression* of the fluid at its superficies." The variation of density near the surface, it is shown, must be extremely rapid. Also, "the molecular attraction in fluids as well as solids extends further than the calorific repulsion." (Art. 30.)

The above conclusion is confirmed in various ways, and the consequences that flow from it with reference to capillary phænomena are fully discussed in the work of M. Poisson entitled *Nouvelle Théorie de l'Action Capillaire*†. The object of this treatise is to bring the theory of capillary phænomena to the greatest degree of perfection that the power of analysis and the existing knowledge of facts will permit. In the first chapter the author proves that if the rapid variation of density near the surface were neglected, the fluid within the tube would remain horizontal, and there would be neither elevation nor depression. He shows also the necessity of having regard to the variable compression experienced by the fluid near the surface of the tube, and reaching to the extent of the action due to the solid. Whence it follows that the principles of Laplace's theory are defective, notwithstanding it is so successful in the explanation of phænomena. In Arts. 18 and 19, M. Poisson obtains, on the supposition of incompressibility, the equation of Dr. Young relative to the angle of contact, which, as we have seen, was also

* It is not perhaps difficult to see, without the aid of analytical calculation, that if bodies be assumed to be composed of *isolated* atoms held in places of equilibrium by attractive and repulsive forces, the sphere of whose sensible influence is very small, there must be a rapid change of density at their surfaces. If experiment should ever be able to detect such a change, this assumed constitution of bodies would be rendered extremely probable.

† Paris 1831. Extracts from this work, with Remarks by H. F. Link, will be found in Poggendorff's *Annalen der Physik und Chemie*, bd. xxv. 1832, p. 270, and bd. xxvii. 1833, p. 193.

M. Poisson states in the preamble of his forthcoming treatise on "The Mathematical Theory of Heat," which appeared recently in a Paris weekly scientific Journal (*L'Institut*, No. for May 24, 1834,) that that theory forms the *second* part of a "Treatise on Mathematical Physics," in which he proposes to treat, without restriction to any predetermined order, different physical questions which admit of the application of analysis. The "New Theory of Capillary Action" is the first part.

found by M. Gauss without deviating from the principles of Laplace's theory. But this equation is no longer true if it be necessary to take account of the variation of density at the fluid surface; nor in the same case can the argument hold good by which Clairaut showed that the fluid surface is horizontal when its attractive power is double that of the solid*.

In the next chapter, the equation of the free surface of fluid in equilibrium in a capillary space is obtained by an analysis which takes into account any variation of density that may exist at the fluid surface, although the exact law of variation be unknown. This equation is of the same form as the fundamental equation of Laplace, and involves an analogous quantity H. As M. Poisson infers from it, by assuming an angle ω, which is the supplement of that we have hitherto called the angle contact, to be constant, that the weight of fluid raised in a capillary tube is $-\frac{H\varrho c}{2}\cos\omega$, which is the expression for the same weight obtained by Laplace, it follows that H in the two theories is the same in magnitude, though differently represented by the analytical formulæ.

The third chapter is employed in finding on the same principles the equation relative to the contour of the capillary surface. The angle of contact, which is found as in preceding theories to be constant, is assigned by the equation

$$F = H \cos \omega.$$

It may be useful to give some idea of the nature and composition of the constants F and H. The following formulæ will serve to do this, when the significations of the letters they contain have been explained:

$$\frac{H}{2} = q + q_{\prime}, \qquad \frac{F}{2} = q \quad \varpi$$

$$q = -\frac{\pi}{8}\int_0^\infty R\, r^4\, dr$$

$$q_{\prime} = -\frac{\pi}{2}\int_0^\infty\int_0^l\int_0^l R_{\prime}\,\frac{u^3}{r}\, du\, ds\, ds'$$

$$\varpi = \frac{\pi}{2}\int_0^\infty\int_0^l\int_0^l R'\,\frac{u^3}{r}\, du\, ds\, ds'$$

Conceive the fluid mass to be divided by a curve surface passing through any point M into two parts A and B, and through the sides of a rectangular element of the surface at the point M, let normal planes be erected inclosing a prismoidal element of

* M. Poisson's theory cannot inform us how far that equation is erroneous, nor whether it is, or not, very approximately true.

the portion A. Let m and m' be two molecules of the fluid, one in the prismoid, the other in some part of A. Then r is the line joining them, u is its projection on the tangent plane at M, and s s' are the perpendiculars from the molecules on the same plane, so that $r^2 = u^2 + (s - s')^2$. R, $R_{\prime}$, and R′ are functions of r, which are insensible for every sensible value of this variable, and express the mutual action referred to the units of volume of the fluid molecules at the distance r from each other. R relates to the interior of the fluid, $R_{\prime}$ to its superficial stratum, and R′ to the stratum adjoining the side of the tube. With respect to $R_{\prime}$ the surface through M is parallel to the free surface of the fluid at a distance l from it, and A answers to the fluid contained between these two surfaces. So with respect to R′, the surface through M is parallel to the surface of the tube at a distance which may also be called l, and A answers to the fluid contained between this surface and the tube. $R_{\prime}$ and R′ vary very rapidly with s and s', and confound themselves with R so soon as these variables exceed the radius of the molecular activity. The quantity l, which is the limit of the integrals with respect to s and s', is greater than this radius, yet of insensible magnitude. It is shown that $q_{\prime}$ and ϖ do not change sensibly with the magnitude of l.

As the forms of the functions R, $R_{\prime}$, and R′ are quite unknown, the values of q, $q_{\prime}$, and ϖ cannot be calculated *à priori*. Neither are there any means known at present of determining them experimentally. But experiment can assign the numerical values of H and ω, and consequently that of F. Hence if the ratio of $q_{\prime}$ to q should become known, the three quantities would be determined. The knowledge of this ratio is the chief desideratum of the theory. M. Poisson has shown that if there be no variation of density near the surface of the tube, which will happen when the molecular action of the tube is the same as that of the solid, $\varpi = 2q$. In this case $\cos\omega = -\frac{q}{q + q_{\prime}}$. He shows also, according to what might be expected from experience, that $\cos\omega = -1$, when the molecular attraction of the tube exceeds that of the fluid, but assigns no limiting value of the excess at which this value of $\cos\omega$ begins. It seems, therefore, reasonable to conclude that when the forces are equal, $\cos\omega$ is nearly equal to -1, and consequently that $q_{\prime}$ is a small quantity compared to q. Also, as the density in the thin superficial stratum of the fluid varies from 0 to the density of the interior, the state of dilatation near the surface would naturally lead us, as M. Poisson observes, to make this inference with respect to the value of $q_{\prime}$.

Enough has perhaps been said to give an idea of the *physical* part of M. Poisson's theory; it remains to notice some of the mathematical deductions obtained from the two principal equations in the succeeding chapters of the work. These equations being the same in form as in Laplace's theory lead to like results. In several instances M. Poisson has carried the mathematical calculation to a greater degree of approximation, and by this means obtained numerical results more nearly agreeing with experiment. Thus, the elevation of the lowest point of the capillary surface in a tube $1^{mi}\cdot90381$ (= ·075 in.) in diameter, by the experiments of Gay-Lussac is $15^{mi}\cdot5861$; by M. Poisson's calculations $15^{mi}\cdot5829$, by those of Laplace $15^{mi}\cdot5787$.

After extending the analysis to the case in which the interior surface of the tube instead of being cylindrical is any surface of revolution with its axis vertical and its diameter small in the whole extent, M. Poisson considers what will take place when the fluid rises to the upper extremity of the tube, and finds, contrary to an opinion expressed by Laplace*, that the invariability of the angle of contact is still maintained under these circumstances, because the radius of curvature of the edge which terminates the interior surface of the tube is always exceedingly greater than the radius of the molecular action. This circumstance ought to be taken into account in determining the weight necessary to detach a solid disc from the surface of a fluid.

The weight of a drop of water suspended at the lower extremity of a capillary tube and spreading to the exterior edge, is calculated by the theory for the case in which it is just ready to detach itself, and found to be something less than the mean weight of drops falling in succession from the same tube, as inferred from an experiment by Gay-Lussac.

In considering the case of two fluids superincumbent one on the other in the same tube, M. Poisson obtains the same formulæ as Laplace, and employs them to explain a singular phænomenon observed by Dr. Young, and supposed by him to present an objection to Laplace's theory. Into a capillary tube containing water, Dr. Young inserted a small drop of oil, and then saw the superior surface of the oil depress itself below the original height of the water. This depression, no doubt, refers to the centre of the capillary surface, where it cuts the axis of the tube. If it may be supposed that the oil in descending moistened the tube, and that the water did not wet it originally in its whole extent, the fact is accounted for by the theory.

The pressure of fluids as modified by capillary action is treated

* *Supplement à la Théorie de l'Action Capillaire*, p. 25.

at considerable length: both the vertical and the horizontal pressures on a solid partly immersed in a fluid are determined, and from the calculation of the latter it appears, that if a plate, the two parallel faces of which are of different substances, be the solid immersed, the horizontal pressures on the opposite faces exactly counterbalance, and consequently the solid can have no motion of translation. It appears from a remark made by Laplace at the beginning of page 43 of the Supplement to his Capillary Theory, that his reasoning led him to suppose there would be some difference of pressure, but so small that it might be neglected. However small it might be, a motion of translation would be the consequence, and this it seems difficult to admit. Dr. Young advanced this objection to Laplace's theory in a letter to M. Poisson, against whose more exact theory, as we see, the same objection does not hold good.

Various problems which had been handled by preceding mathematicians, receive solutions in chapter vi. more exact than had hitherto been given them, and more carefully compared with experiments. The following are some of the results.

When two plates are immersed with parallel faces in a fluid which rises against the surface of one and is depressed near that of the other, it is found that the fluid surface between them may assume two different forms when the plates are near each other. In one there is a point of inflection which is retained however near the plates be brought to each other, and in this case they constantly repel with a force independent of the interval between them; the other is the form remarked by Laplace, which contains no inflection, and when it subsists the repulsion changes to an attraction on making the plates approximate. M. Poisson is of opinion, that the first of these forms obtains when the plates, being originally at a great distance, are gradually brought near each other, and the latter when, one plate being previously immersed, the other is inserted into the curved portion of the fluid contiguous to it.

The values of the two constants of the theory, viz. $2a^2$, the product of the diameter of the capillary tube by the mean elevation of the fluid in it, and $\pi - \omega$, the angle of contact, are found with reference to mercury and glass, by comparing the theory with the experiments of Gay-Lussac on the height of a large drop of mercury on a horizontal glass plane, to be as follows:

$2a^2 = 2 \times 6{\cdot}5262$ sq. millimetres ($= {\cdot}02023$ sq. in.)
the angle of contact $= 154^{\circ}{\cdot}30'$ ($= 138^{\circ}{\cdot}52'$).

In Art. 116 the weights of fluid raised by circular discs are calculated, and compared very satisfactorily with the experi-

ments of Gay-Lussac cited by Laplace for the same purpose. The heights of the disc above the horizontal level of the fluid, at the instants when the weights of the elevated columns are at a maximum, are determined at the same time by the theory, but these were not measured in the experiments.

Besides the usual problems in the capillary theory, M. Poisson has solved two others, not previously attempted, one relating to the form of fluid poured upon another fluid of greater specific gravity; the other relative to the adhesion of the base of a capillary solid cylinder to a fluid from which it is raised with its axis vertical. This question is similar to that of the adhesion of a disc, but requires to be treated by a different analytical process.

The concluding chapter of the treatise contains notes and additions, in which some points of the theory are further developed, and new experiments compared. One section is devoted to a full exposition of the author's views respecting "the interior constitution of bodies, particularly of fluids, and the nature of molecular forces;" another treats of "the general equations of the equilibrium of fluids." It results from the complete equation of the free surface of a fluid, obtained on the hypothesis of disjoined molecules, held in equilibrium by attractive and repulsive forces, that the resultant of the extraneous forces acting on the fluid, is not exactly perpendicular to its surface, unless it be perfectly plane. The views advanced in these sections are for the most part those we have had occasion to adduce in speaking of the *Memoir on the Equilibrium of Fluids*. Some of the other subjects of this chapter ought not to be passed over without notice.

The depression of mercury in the barometer cannot be conveniently calculated by the theory except the ratio of the radius of the tube to the constant a be either small or great. In other cases it is necessary to recur to the method of quadratures. A table of depressions calculated in this way by M. Bouvard, and inserted in the *Connaissance des Tems* for 1812, is cited by M. Poisson, and placed in comparison with a like table from Lord Charles Cavendish's experiments, with which it is found to agree as nearly as could be expected from the nature of the observations. It is desirable, he remarks, that the calculations should be repeated with the more exact values of the constant a and the angle of contact determined by himself.

Casbois, Professor of Physic at Metz, pointed out a method of constructing barometers with plane or even concave surfaces, having observed that by boiling mercury the convexity of its capillary surface is diminished, and by continuing the boiling

a sufficient length of time, might be changed to concavity. M. Poisson adduces a communication from M. Dulong containing the following satisfactory explanation of this phænomenon. In the operation of boiling, a thin layer of the mercury in contact with the air is oxidized, and then mingling with the whole mass, changes its properties in such a manner, that the action of the mercury on its own particles and on those of the tube, or rather on the particles of a thin coating of water which is always interposed between the mercury and the tube, is not the same as before, the change being greater in proportion to the greater quantity of metal oxidized, that is, in proportion to the duration of the boiling.

A formula obtained in a previous part of the work (Art. 53,) applicable to the rise in a capillary tube of a fluid consisting of two fluids mixed in given proportions, is here compared with experiments made a long time since by Gay-Lussac, but not before published. This formula is founded on the supposition that the loss of heat which takes place in mixing, has no influence, when the temperature has become the same as before, on the integral which determines the value of H, and on which the phænomena of capillarity depend, an hypothesis favoured by the fact, that in the case of a single fluid, the decrement of elevation at different temperatures is proportional to the augmentation of density. The theoretical heights agree much less exactly with the experimental for a mixture of water and alcohol, than for a mixture of water and nitric acid; which shows that the above hypothesis is more true for one mixture than the other.

M. Poisson lastly applies his theory to the explanation of the remarkable phænomenon of endosmose. He conceives that the two fluids meet *without mixing* in the capillary tubes which permeate the membrane, and by the relation of the molecular forces at their common surface of separation, one prevails over the other, and so passes through to the opposite side of the membrane. It has been objected to this theory that it does not account for the phænomenon of exosmose*. An abstract of Mr. Power's views on this subject having been inserted in the *Third Report of the British Association*, it will be only necessary to state that in the paper on Residuo-Capillary Attraction by the same author, subsequently published in the

* Admitting, as suggested by Professor Henslow, that each fluid tends to spread into the other through the capillary communications, may not effects such as are observed, be expected to result merely from the state of compression of the fluid stratum in contact with the substance of the membrane? This will vary with the varying density of the fluid from one point to another of the surface of contact, being greatest where the density is least.

*Transactions of the Cambridge Philosophical Society**, his theory has undergone some modification, the phænomena both of endosmose and exosmose, and the variation of the maximum difference of the heights of the fluids according to the difference of their densities, being accounted for independently of any particular mode of communication of the fluids in the capillary spaces.

I beg leave to close this Report with proposing a query suggested by the existing state of the theory of capillary attraction. How does it happen that the principles with which Laplace's theory sets out conduct to two fundamental equations the same in form as those of M. Poisson's theory? Ought not the latter, seeing that the law of the molecular forces is quite arbitrary, to embrace every possible method of arriving at these equations; and should we not expect that the method of Laplace is not inconsistent with the other, but a particular case of it? The most probable supposition respecting the molecular forces of fluids is, that the attractive force is comparatively small, decreases much more slowly with the distance than the repulsive, and is sensible to a much greater distance from the centre to which it is directed. The hypothesis of incompressibility corresponds to the limiting case, when the repulsive force being infinitely great at first, decreases by very large gradations as the distance from the centre increases, and within a very small space becomes less than the attractive force. As the above law of the forces as well as the limiting case of it are embraced by M. Poisson's theory, we may perhaps hence see why the supposition of incompressibility conducts to the same form of the principal equations. In some objections that have been made to the principles of Laplace's theory, it does not appear to have been sufficiently considered that by supposing the fluid to be incompressible, he does in fact take account of a molecular repulsion. It remains to be determined whether the variation of density, which on the hypothesis of a disjoined molecular constitution of bodies must obtain at their surfaces, be such as to admit of the supposition of incompressibility as a near *approximation* to the truth. But this there are at present no experimental means of determining. The experiments of Hassenfratz†, from which he inferred that glass by being pounded became specifically lighter, are not confirmed by those of Gay-Lussac‡. As no variation of density has been hitherto detected, we have a sort of negative evidence that the depth of the super-

* vol. v. part ii. † *Ann. Ch. Gilb.* I. p. 515.
‡ See *Nouvelle Théorie de l'Action Capillaire*, p. 6.

ficial stratum in which there is any sensible variation must be exceedingly minute. If that depth may be neglected in comparison of the radius of sensible activity of the *attractive* force, Laplace's principles suffice for a theory of Capillary Attraction, without being inconsistent with those of M. Poisson. We may add as a theoretical reason for the supposition of a rapidly decreasing repulsive force united with a feeble and slowly decreasing attractive force, that we may thus understand how the fluid particles will move readily among each other, retaining the same mean interval; for there will be a small obstacle to any change of their relative positions by separation, but a great obstacle to any approach within a certain limit*. Perhaps experiments with light, which appears to be the most successful instrument for searching into the intimate constitution of bodies, offer the best chance of getting at something satisfactory on the delicate point we have been speaking of. In the mean time, while M. Poisson's theory will engage the attention of the speculative philosopher, there appears no reason why the simpler theory of Laplace should not be made the vehicle for conveying to the younger students of science, in an elementary form, the explanations of a numerous and interesting class of phænomena.

Various causes, which it would be useless to detail, prevented me having a sight of the Number of Poggendorff's *Annalen* containing the "New Experiments on Capillarity," by H. F. Link†, (mentioned by Professor Moll at the Meeting of the Association,) till within a short interval before the revision of this Report for the press. I fear that, from want of time and sufficient acquaintance with the German language, the following notice of them will not be such as their importance demands.

The object of M. Link is to ascertain the comparative ascents of different fluids by capillary attraction, in a manner that would be free from the sources of error to which the methods of former experiments had been liable. For this purpose he observes the ascent between two glass plates inclined at a small angle with the line of junction vertical, in which case, as we know, the suspended fluid takes the form of a rectangular hyperbola. The instrument he made use of provided for the adjustment of the

* It will readily be seen, that under these circumstances the fluid would be susceptible of division by a thin plate by the application of a very small force, and we might thus account for a characteristic property of fluids, which, as was mentioned in my former Report (p. 133), has been employed as the basis of their mathematical treatment. I was in error in supposing that this method has only been recently proposed; it appears to have been thought of by Pascal.

† *Annalen der Physik und Chemie*, bd. xxix. 1833, p. 401.

angle of inclination to any required magnitude, and was convenient for dipping the plates very frequently in the fluids. The peculiar advantage of this method was, that the ascents of different fluids could be observed under exactly the same circumstances: for all the observations could be taken at the same parts of the plates and with the same interval of separation; and after experiments made with one fluid, the plates could be conveniently cleared of all remaining moisture, before experiments were made with another. When the same capillary *tube* is used, it is difficult to get rid of the moisture adhering to the interior; and when different tubes are used, the experiments cannot well be under like circumstances by reason of superficial inequalities in the glass surfaces, besides that the exact proportion of the diameters is not readily ascertained. The principal result that M. Link arrives at is, that all the fluids rose to the *same* height. The fluids employed were, distilled water, nitric acid, a solution of kali causticum (one oz. to six of water), spirit of wine (very rectified), sulphuric æther, and rectified sulphuric acid (sp. gr. 1·84). The æther stood lower at first, but after repeated dippings rose to the same height as the water. The sulphuric acid was at first higher than the water, but afterwards sunk to the same level as the rest. Previous experiments have uniformly assigned a less ascent to æther and spirit of wine than to water, and a greater ascent to sulphuric acid. M. Link is of opinion, that the experiments were not carried far enough, and that the different results of his own experiments are attributable to the repeated wetting of the plates by dipping them in the fluids. In another set of experiments the plates were of various substances; viz. glass, copper, zinc, copper and zinc plates soldered together, first, with the zinc surface opposed to the copper, next, the zinc surfaces opposed to each other, and then the copper surfaces opposed; lastly, wooden surfaces smeared over with tallow. The heights of ascent were very nearly the same for all these, excepting that the tallow plates did not give quite so high a column. It would seem, then, that the heights of ascent under similar circumstances are alike independent of the fluids and solids. It is remarkable that this result might have been looked for from either Laplace's or Poisson's theory: for by either theory the height of ascent in a given capillary tube, or between parallel plates separated by a given small interval, varies as $\frac{H}{\rho}$, when the fluid completely wets the solid*; and as H, in this case, depends only on the

* See p. 263.

molecular action of the parts of the fluid on each other*, the simplest supposition respecting it is, that it varies as ρ; whence it would follow that the height of ascent is the same for different fluids.

In the applications of the theories of Laplace and Poisson to Gay-Lussac's experiments on the ascents of fluids in capillary tubes, and the weights of the fluid columns raised by circular discs, the values of the constant $\frac{2}{\alpha}$ for the different fluids, were borrowed from the first class of experiments, and being employed in the theoretical formulæ, gave results according with the other class. If those values were incorrectly determined by the experiments, this accordance can only be explained by supposing the cause of error to be of the same kind and to act in the same degree in the two classes of experiments.

M. Link remarks that there is an essential difference between the ascent of fluids against solid surfaces not previously wetted, and the remaining height of suspension after the wetting. Tallow, for instance, will scarcely allow water to ascend at all in the first instance; but after being moistened, will sustain a suspended column, of nearly the same height, according to the experiment mentioned above, as when other substances are employed. The theory of the *first ascents*, must be of a very complicated nature, on account of the difficulty of estimating the amount of various retarding causes, such as greasiness, and the inequalities of the solid surfaces. But the theory of the *remaining suspensions* that result from wetting the surfaces, is of a more simple nature. M. Link adduces an explanation of this fact, founded on a theory of fluidity developed in the first part of his paper, in which, setting out with Newton's definition of a fluid, he is led to regard it as composed of solid particles in an extreme state of pulverization, and aggregated like the grains of a heap of sand. Whatever in other respects may be the comparative merits of this view of the nature of fluidity, and that adopted by Young and Poisson, the latter has the advantage of being more readily made a basis of calculation.

* pp. 275 and 285.

Report on the Progress and Present State of Physical Optics. By the Rev. Humphrey Lloyd, *A.M., M.R.I.A., Fellow of Trinity College, and Professor of Natural and Experimental Philosophy in the University of Dublin.*

In the Report which I have the honour to submit to the Association, I have attempted to consider in some detail the present state of our knowledge with regard to the physical theory of light, and the successive advances which have, in late years, been made towards its establishment. The method which I have thought it expedient to adopt in this review has been to take, in the first instance, a rapid survey of the several leading classes of optical phenomena, which the labours of experimental philosophers have wrought out in such rich profusion, and afterwards to examine how far they are reducible to one or other of the two rival theories which have alone advanced any claim to our consideration. This is, in fact, the only way in which the truth of a physical theory can be established; and the argument in its favour is essentially *cumulative*.

But in making this comparison it is not enough to rest in vague explanations which may be moulded to suit any theory. Whatever be the apparent simplicity of an hypothesis,—whatever its analogy to known laws,—it is only when it admits of mathematical expression, and when its mathematical consequences can be numerically compared with established facts, that its truth can be fully and finally ascertained *. Considered in this point of view, the wave-theory of light seems now to have reached a point almost, if not entirely, as advanced as that to which the theory of universal gravitation was pushed by the single-handed efforts of Newton. Varied and comprehensive classes of phnomena have been embraced in its deductions; and where its progress has been arrested, it has been owing in a great degree to the imperfections of that intricate branch of analysis by which it was to be unfolded. The principles of the theory of emission, on the other hand, have, in comparatively

* C'est en tirant des formules les conséquences les plus subtiles et les plus eloignées des principes, puis allant les vérifier par l'expérience, que l'on peut réellement s'assurer si une théorie est vraie ou fausse, et si l'on doit s'y confier comme à un guide fidèle, ou la rejeter comme un systéme trompeur.—*Biot, Traité de Physique*, tom. i. p. xiv.

few instances, been mathematically expressed and developed; and accordingly this theory presents but rarely those points of contact with experimental truth by which alone it can be judged.

This signal difference in the present state of the two theories has been by some ascribed to a difference in the intellectual power by which they have been worked; and it has been said that had the Newtonian theory been cultivated with the same zeal and talent as the Huygenian, it might have had equal triumphs to boast of. This position, I confess, appears to me altogether untenable. With respect to the implied fact, it may be enough to observe that Newton and Laplace were both engaged on one side of the question; and I believe I may add that among the supporters of the wave-theory of light there are few who have not had to encounter early predilections in favour of the theory of emission. The nature and laws of projectile movement are far more familiar to every lover of mechanical philosophy than those of vibratory propagation; and the triumphant career of the former branch of this science, in its application to the movements of the heavenly bodies, is in itself sufficient to induce every one to lean to a theory which proposes to account for the phenomena of light on similar principles. As to the opinion itself, it seems highly improbable, to say the least, that two theories so widely separated should run hand in hand in their explanation of phenomena. There is indeed *one* case, and that a striking one, of this kind:—The fundamental laws of reflexion and refraction are exact and necessary consequences of each of these theories; but I believe their history affords no parallel instance.

An unfruitful theory may, however, be fertilized by the addition of new hypotheses. By such subsidiary principles it may be brought up to the level of experimental science, and appear to meet the accumulating weight of evidence furnished by new phenomena. But a theory thus overloaded does not merit the name. It is a union of unconnected principles, which can at best be considered but as supplying the materials for a higher generalization. Its very complexity furnishes a presumption against its truth; for the higher we are permitted to ascend in the scale of physical induction, the more we perceive of that harmony, and unity, and order, which must reign in the works of One Supreme Author. The theory of emission, in its present state, exhibits all these symptoms of unsoundness; but there is something stronger than mere presumption against it. It will appear, I think, upon a fair review, that in almost every instance in which it has been developed, its consequences are *at*

variance with facts; and the proof of its insufficiency seems even stronger than the positive evidence in favour of the rival theory.

In proceeding to the consideration of these arguments, I have found it necessary to deviate from the arrangement which a strictly theoretical view of the subject would naturally suggest. The relation of theory to phenomena, which I propose to consider, obliges me to examine the latter in the groups in which they have been usually brought together, and under which their laws have been investigated. I propose, therefore, to divide the following Report into two parts; of which the first will treat of *unpolarized*, and the second of *polarized*, light. In the former I shall consider separately,

1. The propagation of light, and the principle of interference;
2. The reflexion and refraction of light;
3. Diffraction;
4. The colours of thin and thick plates.

The second part will comprise,

1. The polarization of light, and the principle of transversal vibrations;
2. The reflexion and refraction of polarized light;
3. Double refraction;
4. The colours of crystallized plates.

Many subjects of high interest are omitted in this arrangement, as being but remotely connected with the leading object of the present Report. I have left wholly untouched, for this reason, that branch of optical science which is sometimes denominated "mathematical optics," or the development of the fundamental laws of reflexion and refraction. The phenomena of vision have been in like manner omitted, as involving also the science of physiology; and the relations of light to other agents, as heat, electricity, and magnetism, because these relations are as yet little understood, and in the present state of the kindred sciences, the science of light can hope to derive little aid from their examination. These interesting subjects would, each of them, well merit a separate consideration.

Part I. Unpolarized Light.

I. *Propagation of Light. Principle of Interference.*

The first property of light which claims our notice is its progressive movement. Light, we know, travels from one point

of space to another *in time*, with a velocity of about 195,000 miles in a second. The inquiry concerning the mode of this propagation involves that respecting the nature of light itself.

There are two distinct and intelligible ways of conceiving such a motion. Either it is the self-same body which is found at different times in distant points of space; or there are a multitude of moving bodies, occupying the entire interval, each of which vibrates continually within certain limits, while the vibratory motion is communicated from one to another, and so advances uniformly. Nature affords numerous examples of each of these modes of propagated movement; and in adopting one or other to account for the phenomena of light, we fall upon one or other of the two rival systems,—the theories of Newton and of Huygens.

The Newtonian theory, in the shape in which it is usually presented, is undoubtedly simpler in conception than its rival; but this simplicity is only apparent. Newton himself was far too clear-sighted to suppose that the forces of attraction and repulsion, by which the molecules of light were supposed to be refracted and reflected, were adequate to account for all the phenomena; and it is remarkable that, when he proceeds to speculate on the physical theory of light, he has found it necessary to admit all the apparatus required in the theory of waves. In fact, Newton felt, and distinctly stated, that the vibrations of an ethereal medium were necessary in his hypothesis *, although he denied that these vibrations constituted light. He has even gone further, and asserted that they were the chief and *essential* parts of that hypothesis, the molecules emitted from luminous bodies only performing the office of exciting these vibrations, as stones flung into water produce waves †. On the other hand, the molecules themselves are supposed to be emitted by a vibratory motion of the parts of the luminous body ‡;—the same vibratory movement, though acting with a different energy, in which he supposes heat to consist. It would appear, then, that Newton assumed *too much*, and that he erred against his own valuable rule: "*Causas rerum naturalium non plures admitti debere*," &c. Had he simply left out the molecular part of his hypothesis, and supposed that the vibrations of his ethereal

* *Phil. Trans.* 1672.

† "Were I to assume an hypothesis, it should be this, if propounded more generally,—so as not to determine what light is, further than that it is *something or other capable of exciting vibrations in the ether*; for thus it will become so general, and comprehensive of other hypotheses, as to leave little room for new ones to be invented."—*Birch's History of the Royal Society*, vol. iii. p. 249.

‡ *Optics*, Query 8.

medium were *directly* excited by those of the luminous body, his theory would have resolved itself into that of Huygens and of Hooke. It may be observed, in connexion with this subject, that Newton seems actually to have admitted the wave-theory with respect to *radiant heat*; and that he supposed it to be propagated, not by the translation of material particles, but by the vibrations of an ethereal medium *.

The peculiar part of the theory of emission—the supposition that the rays of light are bodies projected with a great velocity—would seem to offer an easy criterion of its truth. If the weight of a molecule of light amounted to one grain, its momentum would equal that of a cannon ball 150 pounds in weight, and moving with the velocity of 1000 feet in a second. The weight of a single molecule may be supposed many millions of times less than this; but, on the other hand, millions of such molecules may be made to act together, by concentrating them in the foci of lenses or mirrors, and the effects of their impulse might, it was expected, be thus rendered sensible. This easy test of the materiality of light was long since appealed to. The experiments of Homberg seemed to have established the existence of a sensible impulsive effect; but when these experiments were repeated with more caution by Mairan and Dufay, they conducted to the opposite conclusion. The results obtained by Michell at a later period, and with the aid of a more sensible apparatus than any before employed, seemed to be decisive in favour of the materiality of light †. The effects observed in these experiments, however, have been with much probability referred to aerial currents, produced by unequal temperature, or even to a difference in the elastic force of the air in contact with the opposite surfaces of the body acted on ‡. The subsequent experiments of Mr. Bennet were made under circumstances far more favourable; and in particular, having been repeated in vacuum, they are independent of the sources of error now alluded to. Their result was conclusive as to the non-existence of a sensible effect §.

The objection to the materiality of light, arising from its want of sensible momentum, was first urged by Franklin. Horsley attempted to remove the difficulty ||; but his laborious arithmetical calculations only go to prove that the particles of light, if material, must be of extreme minuteness. It must at the same time be confessed that objections of this nature are entitled to

* *Optics*, Query 18.
† Priestley's *History of Optics*, p. 387.
‡ Young "On the Theory of Light and Colours," *Phil. Trans.* 1801.
§ *Phil. Trans.* 1792. || *Ib.* 1770. I.

little weight. It is easy to attribute to the molecules of light a minuteness sufficient to evade any means that we possess of detecting their inertia by their effects upon *other bodies*; and in whatever point of view we regard the phenomena of optics, we are forced to contemplate quantities immeasurably smaller than any to which the imagination has been accustomed.

The aberration of the light of the fixed stars, resulting from the motion of the earth and that of light, is an easy consequence of the theory of emission, in which these motions are conceived to subsist independently. In order to account for the phenomenon in the theory of waves, it seems necessary to assume that the ether which encompasses our globe does not participate in its motion; so that the ethereal current produced by this relative motion must be supposed to have a free passage through the solid mass of the earth; or that, in the words of Young, "the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees*. Fresnel has maintained the same opinion, and, startling as the position seems at first, he has very clearly shown that no fair argument can be advanced against it, founded on the opacity of the mass which the ether is supposed to permeate †.

The discoveries of Bradley and Roemer, when compared together, have led to a further and most important conclusion respecting light,—namely, that its velocity is one and the same, whatever be the luminous origin; the light of the sun, the fixed stars, the planets and their satellites, being all propagated with the same swiftness. This conclusion must be allowed to present a formidable difficulty in the theory of emission. Laplace has shown that if the diameter of a fixed star were 250 times as great as that of our sun, its density being the same, its attraction would be sufficient to destroy the whole momentum of the emitted molecules, and the star would be invisible at great distances ‡. With a smaller mass there will be a corresponding retardation; so that the final velocities will be different, whatever be the initial. The suggestion of M. Arago seems to offer the only means of avoiding this difficulty. It may be supposed that the molecules of light are originally projected with very different velocities; but that among these velocities there is but one which is adapted to our organs of vision, and which

* "Experiments and Calculations relative to Physical Optics," *Phil. Trans.* 1803.

† "Sur l'Influence du Mouvement terrestre dans quelques Phénomènes d'Optique," *Annales de Chimie*, tom. ix.

‡ Zach, *Ephem.*, iv. 1.

produces the sensation of light. This supposition seems to be supported by the discoveries of Herschel, Wollaston, and Ritter, respecting the invisible rays of the spectrum; but it does not appear to be easily reconciled with any hypothesis which we are able to frame respecting the nature of vision. This uniformity of velocity, on the other hand, is a necessary consequence of the principles of the wave-theory. The velocity with which vibratory movement is propagated in an elastic medium depends solely on the elasticity of that medium and on its density; and if these be uniform in the vast spaces which intervene between the material bodies of the universe, (and it is not easy to suppose it otherwise,) the velocity must be the same, whatever be the originating source.

The rectilinear motion of light has long been urged in favour of the theory of emission, and against the theory of waves. If light consists in the undulations of an elastic fluid, (it has been said,) it should be propagated in all directions from every new centre, and so bend round interposed obstacles. Thus luminous objects should be visible, even when an opake body is between them and the eye, just as sounding bodies are heard, though a dense body intervene between them and the ear. To this objection, which was first insisted on by Newton*, a full answer has been given. The phenomena of diffraction, and especially the interior fringes in the shadow of narrow opake bodies, prove that light *does bend round obstacles*, and deviate perceptibly from the rectilinear course. When the obstacle is of considerable dimensions, the intensity of the light decreases, indeed, very rapidly within the edge of the geometric shadow; so that at a very small distance from that edge, it is no longer perceptible. But the darkness does not arise from the absence of luminiferous waves, but from the mutual destruction of those sent there. In fact, if the surface of the wave when it reaches the obstacle be divided into any number of small portions, the motion of the ether at any point behind it is, by the principle of Huygens, the sum of all the motions produced there by these several portions, considered as separate centres of disturbance; and it is easy to show, that, when the distance of the point in question from the obstacle is a large multiple of the length of a wave, the magnitude of this resultant must *diminish rapidly* within the shadow, and the light become insensible when the line drawn from that point to the edge of the screen is inclined at a small angle to the normal to the front of the wave. The accurate calculation of the intensity, in this

* *Optics*, Query 28.

and other similar cases, has been made by Fresnel by the aid of the principle of interference, and the result is found to agree in the most complete manner with observation*.

The same principles apply to the aerial waves which constitute sound, and these too should present analogous phenomena. But the scale is widely different. The length of an aerial wave is more than 10,000 times greater than that of an ethereal undulation; and the distance of the ear from the obstacle must be augmented in the same proportion, in order that the same conclusions may be applicable to the two cases.

According to this account, then, the right-lined propagation of the rays of light is a consequence of the principle of interference, combined with the principle of Huygens. A very different view of the subject, however, has been presented by M. Poisson, in a memoir on the propagation of motion in elastic fluids, read before the French Academy in the year 1823†. The elasticity of the fluid being supposed the same in all directions, the velocity of propagation will be also the same, and consequently the waves spherical. The absolute velocities of the molecules themselves, however, will be very different. M. Poisson finds that when the original disturbance takes place only in one direction, the velocity of the molecules will be indefinitely small in all directions inclined to it at finite angles, so that the motion will not be *sensibly* propagated except in that direction. This diminution of intensity, he finds, will be greater the more rapid the velocity of propagation; and it is in this manner only, he concludes, that we can account for the rectilinear motion of light in the wave-theory. This conclusion however, M. Fresnel has shown, is contradicted by the ordinary phenomena of diffraction; and he has adduced theoretical reasons, drawn from the principle of the coexistence of small motions, to prove that it cannot hold in any fluid whatever, but that the molecules are in all cases disturbed in a sensible manner, in directions very much inclined to that of the original vibrations‡.

The principle of the *superposition of small motions*, which has been more than once adverted to, is an immediate consequence of the *linearity* of the original equation of partial differences which determines the law of vibration of an ethereal particle. The complete integral of this equation will contain, in general, a term for every distinct original disturbance; and the total disturbance will be the sum of all the partial disturbances due to each cause acting separately. The partial disturbances may, however, con-

* "Mémoire sur la Diffraction," *Mémoires de l'Institut*, tom. v.

† *Annales de Chimie*, tom. xxii.

‡ *Ibid.*, tom. xxiii.

spire, or be opposed; so that in the case of two such disturbances, for example, the second may have the effect either of augmenting or diminishing the first, and the absolute velocity of the ethereal molecules may be increased, or lessened, or even wholly destroyed by the union. In fact, if the form of the function which expresses the wave-disturbance, be assumed to be that by which the law of vibration of the cycloidal pendulum is represented, the sum of two coexisting disturbances will be a single disturbance of the same form, provided the component undulations have the same length; and the effect of two such coexisting undulations will be a single undulation of the same length, but differing in the position and magnitude of the space of greatest vibration from either of the components. The magnitude of the resulting vibration may be the sum, or difference of those of the component vibrations, or it may have any value intermediate to these limits. When the component vibrations are equal, the resultant may even vanish altogether; and two lights of equal intensity when added together will produce darkness, provided that the interval of retardation of one wave on the other is an odd multiple of the length of half a wave.

This important consequence of the theory of waves—the *principle of interference* of the rays of light—was first distinctly stated and established by Dr. Thomas Young, although some of the facts by which its truth is experimentally confirmed were known to Grimaldi*. The general calculation of the intensity of the resulting light, for any relative position of the interfering waves, is due to Fresnel; and has been followed out and developed by Sir John Herschel in his valuable *Essay on Light*. When a beam of homogeneous light is transmitted through two small apertures in a card, or plate of metal, the light will diverge from each as from a new centre. If the two apertures are close together, and the diverging pencils received on a reflecting surface, a series of parallel straight bands is observed, perpendicular to the line connecting the apertures, and separated by intervals absolutely dark. That these alternations of light and darkness are produced by the mutual action of the two pencils, Young proved by the fact, that when one of the beams is intercepted, the whole system of fringes instantly disappears, and the dark intervals recover their former brightness.

The experiment of Fresnel is still more satisfactory. In this important and instructive experiment, the fact of interference is placed beyond all question. The two pencils proceed from one

* This ingenious philosopher even stated explicitly that an illuminated body may be rendered darker by the *addition of light*, and adduced a simple experiment in proof of it. *Physico-Mathesis de Lumine.* Bologna, 1665.

common origin, and are separated simply by reflexion at plane surfaces, without any attending circumstance which can, by possibility, be supposed to influence the result. The phenomenon is thus divested of everything nonessential, and it becomes impossible to hesitate about its nature. But the accordance of theory and experiment is maintained, not only in the general features of the phenomenon, but even in its minutest details. The distances of the points of each fringe from the two foci of reflected rays should, according to theory, differ by a constant quantity,—that constant being an odd multiple of the length of half a wave for the dark fringes, and an even multiple of the same quantity for the bright ones. Hence the fringes should be propagated in hyperbolic lines, whose foci are the foci of the reflected pencils;—and the most accurate measurements have shown that it is so. The constant differences just alluded to are far too minute to be directly measured; but they can be calculated with great accuracy, when the distances of the successive bands from the central one have been obtained. The latter distances have been determined by Fresnel with much nicety by micrometrical measurements; and the lengths of the waves of each species of simple light, thence computed, agree in the most satisfactory manner with the values of the same quantities as deduced from the observation of Newton's rings.

The central fringe is formed at those points in arriving at which the two pencils have traversed equal paths; and as its position is therefore independent of the length of a wave, the rays of all colours will be united there, and the fringe itself will be *white*, or colourless. Such is the fact, as described by Fresnel himself, and by most observers who have repeated the experiment. Mr. Potter states, however, as the result of his observations, that the central fringe may be seen *both black and white*, although more frequently the former; and he urges the fact in opposition to the wave-theory*. But it seems premature to draw any inference from such experiments, until the circumstances which have occasioned the variation in the results have been fully investigated and understood.

The interference of the rays of light has, since the decisive experiment of Fresnel, been admitted on all hands; and the phenomena which were previously explained on the Newtonian hypothesis of the "fits of easy reflexion and transmission," are now, by most of the advocates of the Newtonian theory, referred to this simpler and more fertile principle. This principle is, it has been stated, an immediate and necessary consequence of the wave-theory, and its experimental establishment must be

* *Phil. Mag.*, (3rd Series,) vol. ii. p. 280.

regarded as a weighty argument in favour of that theory. It now remains to inquire whether any account can be given of it in the theory of emission.

The molecules of light cannot be supposed to exert any mutual influence; for the regularity of the laws of reflexion and refraction compels us to consider them as independent, and each, separately, the subject of those forces from which, in the theory of emission, these laws are derived. The phenomenon of interference may, however, be plausibly accounted for by the vibrations of the optic nerve, produced by the impulse of the rays of light upon the retina; and by the accordance or discordance of these vibrations when caused by two interfering pencils. On this supposition, which was suggested by Dr. Young himself, the intensity of the light will depend on the relation between the time of vibration of the optic nerve, and the interval of the impulses of the succeeding particles. If this interval be equal to the time of vibration, or to any multiple of it, the second impulse will add its effect to that of the first, and the motion be accumulated. It will, on the other hand, be destroyed, if the second impulse follows the first at an interval equal to half that time.

It is here assumed that the emitted particles succeed one another at equal intervals, as will be the case if their emission be owing (as Newton supposed it to be) to a vibratory motion of the parts of the luminous body. But we must assume further that the intervals of emission vary with the nature of the particles, in the light of different colours; or that all the *red-making* particles (to use an expression of Newton) are emitted at one certain interval, all the *blue-making* at another; and so for each different species of simple light. Hence the vibrations of the parts of the luminous body must be of different periods for the light of different colours. This is, in truth, a part, and a necessary part, of the theory of waves; but it has no connexion whatever with the principles of the rival theory.

II. *Reflexion and Refraction of Light.*

To account for the phenomena of reflexion and refraction it is supposed, in the Newtonian theory, that the particles of bodies and those of light exert a mutual action;—that, when they are nearly in contact, this action is *attractive*,—that, at a distance a little greater, the attractive force is changed into a *repulsive* one,—and that these attractive and repulsive forces succeed one another probably for many alternations. The absolute values, or intensities, of these forces are different in different bodies;

but the form of the law, or the function of the distance by which they are expressed, is assumed to be the same for all*. From these postulates Newton has rigorously deduced the laws of reflexion and refraction. The problem is the first in which the effects of that important class of forces acting only at insensible distances have been submitted to calculation; and the solution is regarded by M. Poisson as forming an era in the history of science.

The reflexion of light at the exterior surface of dense media is ascribed to the repulsive force; refraction and internal reflexion, to that inner attractive force which extends up to actual contact. The outermost sphere of action of every body, in this theory, is necessarily attractive, as well as the inmost; for, were it otherwise, no ray could enter, or emerge from, the medium at an extreme incidence. Sir David Brewster has made an ingenious use of this principle to explain the remarkable fact noticed by Bouguer, that water is more reflective than glass at oblique incidences.

But though the theory of emission is perfectly successful in explaining the laws of reflexion and refraction, considered as distinct phenomena, yet it is by no means equally so in accounting for their connexion and mutual dependence. When a beam of light is incident on the surface of any transparent medium, part is, in all cases, transmitted, and part reflected. The intensity of the reflexion is in general less, the less the difference of the refractive indices of the two media; and accordingly the reflective and refractive forces (if such be the cause of the phenomena,) are related to one another in all media, so that one increases or diminishes along with the other†. But how is it that some of the molecules obey the influence of the repulsive force, and are reflected; while others yield to the attractive force, and are refracted? To account for this, Newton was obliged to have recourse to a new hypothesis. The molecules of light are supposed to pass through certain periodical states, called "fits of easy reflexion and transmission," which modify the effects of the attractive and repulsive forces, and in which they are dis-

* This assumption is tacitly made by Newton, when he takes the function $\frac{\mu^2 - 1}{s}$ as the measure of the refractive power. See Herschel's "Essay on Light," *Encyc. Met.*

† The reader will find much novel and interesting matter connected with this subject in a paper by Sir David Brewster, "On the Reflexion and Decomposition of Light at the separating surface of media of the same and of different refractive powers," *Phil. Trans.* 1829.

posed to yield alternately to one or the other. The actual determination of the particle will depend, partly on the phase of the fit, and partly on the obliquity under which it meets the bounding surface. Now the molecules composing a beam of light are supposed to be in every possible phase of their fits, when they reach the surface: some of them consequently will be reflected, and others refracted; and the proportion of the former to the latter will depend on the incidence.

As to the fits themselves, Newton thought they must be referred to a vibratory motion in the ether, excited by the rays themselves; just as a stone flung into water raises waves on its surface. This vibratory motion is supposed to be propagated faster than light itself, and thus to overtake the molecules, and impress upon them the disposition in question by conspiring with or opposing their progressive motion. In one of his queries Newton has even calculated the lesser limit of the elasticity of the ether, as compared with that of air, in order that it should have so great a velocity of propagation*. The hypothesis of Mr. Melville and M. Biot is more in accordance with the spirit of the theory of emission. The molecules of light are supposed, in this hypothesis, to have a rotatory motion round their centres of gravity which continues along with the progressive motion, and in virtue of which they present attracting and repelling poles alternately during their progress in space†. Boscovich imagined a vibratory motion in the parts of the ray itself, which it received at the moment of emission, and retained in its progress‡.

The theory of the fits has now lost much of its credit, since the phenomena of the colours of thin plates, phenomena which first suggested it to the mind of Newton, have been shown to be irreconcileable with it. The explanation which it gives of the facts now under consideration is, as was observed by Young and Fresnel, inconsistent with the regularity of refraction. In fact, the molecules which are transmitted, are not all in the *maximum* of the fit of transmission, but are supposed to reach the surface in very different phases of this, which may be denominated the *positive* fit. Now as a change of the fit from positive to *negative* is, in general, sufficient to overcome altogether the effect of the attractive force, and subject the molecule to the repulsive, it is obvious that the phase of the fit must modify the effects of these forces in every intermediate degree; and that the molecules which do obey the attractive force must have their velocities augmented in different degrees, depending on the

* *Optics*, Query 21. † *Phil. Trans.* 1753. *Traité de Physique*, iv. p. 245.
‡ *Philosophiæ Naturalis Theoria.*

phase. Consequently, as the direction of the refracted ray depends on its velocity, the transmitted beam will consist of rays refracted in widely different angles, and will be scattered and irregular.

In some of his writings Newton attributes the reflexion and refraction of light to a difference in the density of the ether within and without bodies; or rather he refers the attractive and repulsive forces to this, as to a more general principle. The ether is supposed to be rarer within dense bodies than without, and the rays of light, in crossing the bounding surface, are pushed from the side of the denser ether; so that their motion is accelerated if they pass from the rarer to the denser body, and retarded in the opposite case. Reflexion at the surface of the rarer medium is explained on the same suppositions; but, to account for the ordinary reflexion by a denser medium, Newton was obliged to introduce new and gratuitous hypotheses respecting the constitution of the ether at the confines of two media in which its density is different*.

The velocity of propagation, in the wave-theory of light, depends solely on the elasticity of the vibrating medium as compared with its density. If, then, a plane wave be incident obliquely on the bounding surface of two media, it is obvious that its several portions will reach that surface at different moments of time; and each of these portions will become the centre of two spherical waves, one of which will be propagated in the first medium with the original velocity, while the other will be propagated in the new medium, and with the velocity which belongs to it. But, by the principle of the coexistence of small motions, the agitation of any particle of either medium is the sum of the agitations sent there at the same instant from these several centres of disturbance; and the surfaces on which they are accumulated at any instant will be the reflected and refracted waves. These surfaces are those which touch all the small spherical waves at any instant. It is easy to see that they are both plane; and that the *reflected* wave is inclined to the surface at the same angle as the incident wave, while the sine of the angle of inclination of the *refracted* wave is to that of the incident in the constant ratio of the velocities of propagation in the two media.

Such is the demonstration of the laws of reflexion and refraction given by Huygens †. The composition of the grand, or primary wave, by the union of the several secondary or partial waves, in this demonstration, has been denominated the *principle of Huygens*; and it is obviously a case of the more general

* Birch's *History of the Royal Society*, vol. iii. p. 247. *Optics*, Query 19.
† *Traité de la Lumière.*

principle of the coexistence of small motions. It easily follows from this mode of composition, that the surface of the primary wave must mark the extreme limits to which the vibratory movement is propagated in any direction, in any given time; so that light, according to this theory, is propagated from any one point to another in the least possible time. This is the well-known law of Fermat, the *law of swiftest propagation*, and it will readily appear that it holds, whatever be the number of modifications which the course of the light may undergo by reflexion or refraction; as, likewise, whatever be the form of the elementary wave.

The demonstration of Huygens has been thrown into an analytical form by Lagrange*, but he has added nothing to its rigour or perspicuity. An important supplement to the demonstration was however given by Fresnel. From the reasoning of Huygens it did not appear what became of those portions of the secondary waves which did not conspire in the formation of the grand wave. The crossing of these in all directions ought to give rise to a weak diffused light, filling the entire space between the grand wave and the reflecting or refracting surface; and, in fact, Huygens supposed that such a light did actually exist, but was too feeble to affect the eye. Fresnel has shown, however, that all those portions which do not conspire in the formation of the grand wave, are destroyed by interference †; so that the formation of one grand wave, by the union of an indefinite number of lesser waves, becomes a precise and definite effect.

The total reflexion of light at the surface of a rarer medium has been urged by Newton against the wave-theory, and the apparent difficulty seems to have had much weight in inducing him to reject that theory. It is, in fact, not easy to perceive at first view why the disturbance of the ether within the denser medium should not be communicated to the external ether, and a wave be thus propagated to the eye, whatever be the obliquity of the incident wave. To this it may be enough to reply, that the law of refraction itself, in all its generality, is a necessary consequence of the wave-theory; and therefore that the phenomenon of total reflexion, which is a particular case of that law, is likewise accounted for. But the principle of interference furnishes a direct answer to the difficulty. It can be shown that the elementary waves, which are propagated into the rarer medium from the several points of the bounding surface,

* "Sur la Théorie de la Lumière d'Huygens," *Annales de Chim.*, tom. xxi.

† "Explication de la Réfraction dans la Système des Ondes," *Annales de Chimie*, tom. xxi.

destroy one another by interference, when the sine of the angle of incidence is greater than the ratio of the velocities of propagation in the two media, or the angle itself greater than the limiting angle of total reflexion*. It is here supposed that the distance from the refracting surface is a large multiple of the length of a wave. The conclusion does not apply to points very near that surface; and for such points, there is reason to think, the law of refraction is more complicated. Experience shows, in fact, that light may issue from the denser medium, to an appreciable distance, when the incidence exceeds the limiting angle of total reflexion. If two prisms, whose bases are slightly convex, be put together, and the inclination of these bases gradually changed while we look through them, it will be observed that, *beyond the limiting angle*, the light will still be transmitted in the neighbourhood of the parts in contact. By measuring the breadth of this space, and comparing it to the diameters of the coloured rings, Fresnel found that the interval of the glasses, through which this deviation from the ordinary law of refraction occurred, exceeded the length of a wave†. The analysis of M. Poisson points also to the same result, and it is proved that the second medium will be agitated in the part immediately in contact with the first, this agitation decreasing rapidly and becoming insensible at a very minute distance from the surface.

The laws of reflexion and refraction, then, follow from the theory of waves, whether we suppose the vibrating medium, in dense bodies, to be the body itself, the ether within it, or both conjointly. Euler maintained the first of these opinions, and believed that light was propagated through the gross particles alone, in the same manner as sound. But this hypothesis is contradicted by the most obvious facts; and according to it, as Dr. Young has observed, the refraction of the rays of light in our atmosphere should be a million times greater than it is. Of the other two opinions, Young seems to have held the latter, and to have thought that the molecules of the body formed, together with those of the ether within it, a compound vibrating medium, which was denser than the ether alone, but not more elastic. Others, lastly, attribute the propagation of light in transparent bodies to the vibrations of the ether alone, that fluid being retained by the attraction of the body in a state of greater density within it than in free space.

A very different view of this subject has been recently main-

* See Fresnel "Sur le Système des Vibrations lumineuses," *Bibliothèque Universelle*, tom. xxii.

† *Ibid.*

tained by Mr. Challis. Assuming that the density of the ether is the same in solid media as in free space, (an assumption which he seems to think required by the phenomenon of aberration,) this mathematician conceives that the reflexion of light, and its retardation in the denser medium, may be both accounted for by the reflexions which the ethereal waves undergo from the solid particles of the medium which they encounter in their progress *. He shows, in fact, that the absolute velocities impressed upon the ethereal particles by such reflexion may be resolved into two parts, one of which is propagated uniformly, and is accompanied by a change of density; while the other is propagated instantaneously, without change of density †. The former of these, he thinks, will account for the reflexion of light, the latter for the diminished velocity of transmission ‡. This ingenious theory has the advantage of connecting the velocity of propagation in dense bodies directly with their constitution, and so of advancing a step in the process of physical induction. On the other hand, it requires us to admit that the particles of ether and those of gross bodies exert no mutual action of any kind. We know too little of the ether, or of its properties, to deny this, simply because it is unsupported by any of the properties of matter hitherto revealed; but it must at the same time be admitted that the violation of such analogies furnishes an argument of some weight against the theory which demands them.

Whatever supposition we may frame respecting the constitution of bodies, or of the ether within them, in the wave-theory, it must be such that the velocity of propagation is less in the denser medium. In the theory of emission, on the other hand, it is the reverse; so that although it conducts to the same result, it does so by an opposite route. Here, then, the rival theories are at issue upon a matter of fact; and we have only to ascertain

* This manner of conceiving the reflexion of light, in the wave-theory, was that originally entertained by Fresnel, and was put forward in a memoir read to the French Academy in 1819.

† *Phil. Mag.*, New Series, vol. xi.

‡ The mean effect of these reflexions, Mr. Challis shows, is equivalent to that of a retarding force; and, by a certain supposition respecting its value, he has arrived at the following simple formula for the determination of the ratio of the velocities of propagation in free space and in the medium

$$\mu^2 - 1 = \mu\delta H;$$

in which δ denotes the density of the medium, and H a constant proportional to the mean retarding effect of a given number of its molecules. For the gases, then, the quantity $\frac{\mu^2 - 1}{\delta}$ is nearly constant, whatever be the compression. This result is a very simple consequence of the theory of emission; its experimental truth has been established by MM. Biot and Arago. *Phil. Mag.*, New Series, vol. vii.

this fact, in order to be able to decide between them. This seemed to be accomplished by the reasonings of Young. From the laws of interference it appears that homogeneous light, in its progress in space, passes through certain periodically returning states, the intervals of which are constant in the same medium; while in different media they are proportional to the velocities of propagation, since the *number* of such intervals in a given quantity of light cannot be supposed to vary. Now it followed from the experiments of Newton that the intervals, by which he explained the phenomena of thin plates, were *diminished* in the denser medium; and as these intervals have been shown by Young to be identical with those deduced from the law of interference, it followed that the velocity of light was *slower* in the denser medium *. Newton had even found the ratio of the magnitudes of the intervals to be the same with that of the sines of incidence and refraction; and this is precisely as it should be on the principles of the wave-theory.

But the retardation of light in the denser medium has been directly established by M. Arago. If two pencils be made to interfere and produce fringes, as in the experiment of Fresnel, and if a thin plate of a denser medium be interposed in the path of one of them, the whole system of fringes will be shifted to one side or the other, according as the light has been accelerated or retarded within the plate. The result of this important and decisive experiment was in favour of the theory of waves †.

The refractive index being equal to the ratio of the velocities of light in the two media, direct or inverse, it follows, whichever theory we adopt, that any change in the velocity of the incident ray must cause a variation in the amount of refraction, unless the velocity of the refracted ray be altered proportionally. Now the relative velocity of the light of a star is altered by the earth's motion; and the amount of the change is obviously the resolved part of the earth's velocity in the direction of the star. It was therefore a matter of much interest to determine how, and in what degree, this change affected the refraction. By the observation of this effect, it was hoped, we should have an easy and accurate method of determining the constant of aberration; we should be enabled to compare the light of different stars, and detect any difference which might exist in their velocities; and lastly, we might compare these velocities with that of light

* "Experiments and Calculations relative to Physical Optics," *Phil. Trans.* 1803.

† *Annales de Chimie*, tom. i. See also the account of Mr. Potter's repetition of this experiment, *Phil. Mag.*, vol. iii. p. 333.

emanating from other sources. The experiment was undertaken by M. Arago, at the request of Laplace*. An achromatic prism was attached in front of the object-glass of the telescope of a repeating circle, so as to cover only a portion of the lens. The star being then observed directly through the uncovered part of the lens, and afterwards in the direction in which its light was deviated by the prism, the difference of the angles read off gave the deviation. The stars selected for observation were those in the ecliptic, which passed the meridian nearly at 6 A.M. and 6 P.M., the velocity of the earth being added to that of the star in the former case, and subtracted from it in the latter. No difference whatever was observed in the deviations; and the result was the same whatever was the origin of light †. Fraunhofer has likewise compared the light of several of the fixed stars with respect to its refrangibility. No difference whatever was observed, although the method employed was adequate to the detection of a difference so small as the 10,000th part of the whole refraction nearly ‡.

This remarkable and unexpected result can be reconciled to the theory of emission §, as M. Arago has observed, only by the hypothesis already adverted to, namely, that the molecules are emitted from the luminous body with various velocities; but that among these velocities there is but one which is adapted to our organs of vision, and which produces the sensation of light. The wave-theory has been more successful in its explanation. If the ether which encompasses our globe were like its atmosphere, and partook of its motion, the refraction would be precisely the same as if the whole were at rest. This however, we have seen, cannot be the case; and the phenomena of aberration compel us to admit that the ethereal medium which encompasses the earth is not displaced by its motion. This being assumed, it follows that the ether which is carried along by the refracting medium, is that which constitutes the excess of its density above

* The idea of detecting a difference in the velocity of the light of the fixed stars, by its effect upon the amount of refraction, seems to have first occurred to Mr. Michell. Such a difference of velocity, he conceived, must necessarily arise from the different attractions of the stars upon the emitted molecules; and he has computed the diminution of the original velocity of emission arising from this cause. *Phil. Trans.* 1784.

† Biot, *Astronomie Physique*, vol. iii.

‡ *Edinb. Journ. of Science*, viii. p. 7.

§ M. Prevost has endeavoured to reconcile the experimental result of M. Arago with the ordinary suppositions of the theory of emission, and to show that a change in the *relative* velocity of the light of the stars, caused by the motion of the refracting plane, does not affect the refraction in the same manner as an equal change of the absolute velocity,—"De l'Effet du Mouvement d'un plan refringent sur la Refraction," *Geneva Memoirs*, vol. i. His reasonings do not appear to be conclusive.

that of the surrounding ether. On this supposition Fresnel has calculated the length of a wave in the moving medium, and thence also the actual change in the direction of the refracted ray produced by the earth's motion *. This change is found to be opposite, and exactly equal to that produced by the same cause in the apparent direction of the ray; so that the ray is actually seen in the same direction as if the earth were at rest, and the *apparent* refraction is unaltered by the earth's motion. These results, it may be observed, are precisely the same for terrestrial objects, the velocity of wave-propagation being independent of the motion of the luminous body.

Newton thought that the different refrangibility of the rays of light could be explained by supposing simply that they were bodies of different sizes, the red being greatest and the violet least. It is obvious, however, that this supposition can have no reference to the simple projectile hypothesis held by his followers, or to the demonstration of the law of refraction given in the *Principia*. It is connected with that more complex theory, in which the molecules of light are supposed to excite the vibrations of the ether in the bodies which they meet.

M. de Courtivron and Mr. Melville proposed to account for the dispersion of light by a difference in the initial velocity of the molecules, the red being swiftest and the violet slowest. But were such the cause of the phenomenon, the dispersion should be proportionate to the mean refraction. Indeed the hypothesis was abandoned almost as soon as proposed. Its authors had foreseen the consequence that, in the eclipses of Jupiter's satellites, the colour of the light should vary just before immersion, and after emersion; and the existence of such an effect, in the degree indicated by theory †, was completely disproved by the observations of Mr. Short ‡. Another consequence of such a difference in the initial velocities of the light of different colours is, that the aberration of the fixed stars should also vary with the nature of the light, and each star appear as a

* The sine of the change is to the sine of the total deviation of the ray in the ratio of the velocity of the earth to that of light. Fresnel's result is much more complicated, but it will be easily seen to reduce itself to this.—"Sur l'Influence du Mouvement terrestre dans quelques Phénomènes d'Optique," *Annales de Chimie*, tom. ix.

† The duration of this change, according to Mr. Melville, should amount to thirty-two seconds, the velocity of the light of different colours being inversely as their refractive indices.—(*Phil. Trans.* 1753.) This principle, however, as M. Clairaut has shown (*Phil. Trans.* 1754), is obviously incorrect. It will easily appear that the initial velocities must vary inversely as the quantity $\sqrt{\mu^2 - 1}$, in order to account for dispersion; and that the duration of the expected phenomenon must be even greater than that assigned by Mr. Melville.

‡ *Phil. Trans.* 1753.

coloured spectrum, whose length is parallel to the direction of the earth's motion.

According to the modern advocates of the theory of emission, the molecules of light are *heterogeneous*; and the attractions exerted on them by bodies vary with their nature, and are, in this respect, analogous to chemical affinities. This supposition, however, as Dr. Young has justly observed, is but veiling our inability to assign a mechanical cause for the phenomenon.

It is remarkable that Newton himself was the first to suggest that part of the wave-theory, in which the colour of the light is supposed to be determined by the frequency of the ethereal vibrations, or by the length of the wave *; and the addition has been received by all its supporters. But observation proves that the refractive index, or the ratio of the velocities of propagation, in the two media, is different for the light of different colours. The advocates of the wave-theory, therefore, are forced to conclude that the velocity of propagation in refracting media *varies with the length of the wave.* Here, then, we encounter a difficulty in this theory, which has been regarded as the most formidable obstacle to its reception. Theory indicates that the velocity of wave-propagation is constant in the same medium, depending solely on the elasticity of the medium as compared with its density. That velocity, therefore, should be the same for light of all colours, as it is found to be for sound of all notes.

Various attempts have been made to solve this difficulty †. Euler thought that the successive waves underwent an increase of velocity arising from their mutual action; and this increase he supposed to vary with their length, the waves of greatest length undergoing the least augmentation of velocity, and being therefore most refracted ‡. But the phenomena of coloured rings, as Euler perceived, compel us, on the contrary, to suppose that the lengths of the waves *diminish* as the refrangibility increases; and he seems himself to have abandoned his first conjecture.

Dr. Young accounted for dispersion by the supposition that the solid particles of the refracting substance vibrate, as well as the particles of the ether within it; and that the former vibrations affect the latter, and affect them differently according to

* *Phil. Trans.* 1672.

† It is scarcely necessary to advert here to the law proposed by M. Rudberg, to connect the lengths of an undulation, or the velocities of propagation, in different media;—for this law is purely hypothetical, and its apparent consistency with observation has arisen solely from the adaptation of the arbitrary constants which enter the expression.—*Annales de Chimie*, tom. xxxvi. xxxvii.

‡ *Opuscula varii Argumenti*, tom. i. p. 217.

their frequency. Mr. Challis has adopted and developed this hypothesis. According to this author, it has been already observed, the diminished velocity of transmission in the denser medium may be explained by the obstacle which the solid particles of the medium offer to the free movement of the ethereal particles. If the former be supposed to be immoveable, the ratio of the velocities of propagation, in free space and in the medium, will be a simple function of the density of the latter, and in a given medium its value will be constant; but when the particles of the medium vibrate, the value of this ratio will depend also on the length of the wave, and will therefore vary with the colour of the light *.

The solution suggested by Professor Airy is more closely connected with received principles. It is now generally admitted that part of the velocity of sound depends on a *change of elasticity*, which the air undergoes during its vibrations, in consequence of the development of latent heat by compression. If this heat required *time* for its development, the quantity developed, and therefore the elastic force, must vary with the time of vibration. Consequently the velocity of propagation should also vary with the time, and be different for waves of different lengths. Professor Airy imagines something similar to this in the case of light; and conceives that the elasticity of the ether, in refracting media, may consequently undergo a change, whose amount depends on the time of vibration.

But the explanation offered by Fresnel seems to be the simplest and most natural. The conclusion of analysis—that the velocity of wave-propagation is constant in the same homogeneous medium,—is deduced on the particular supposition that the sphere of action of the molecules of the medium is indefinitely small compared with the length of a wave. If this restriction be removed, we have no longer any ground for concluding that waves of different lengths will be propagated with the same velocity. Fresnel states that he has demonstrated, that when the mutual action of the ethereal molecules extends to a sensible distance as compared with the length of a wave, the waves of different lengths will be propagated with different velocities; the elasticity of the medium, and therefore also the velocity, increasing with the length of the wave †. Here then

* "An Attempt to explain theoretically the different Refrangibility of the Rays of Light, according to the hypothesis of Undulations," *Phil. Mag.*, New Series, vol. viii.

† This demonstration is more than once referred to by the author, as contained in a note appended to his memoir on double refraction. The note however, probably by some oversight, has never been printed.

the constancy of the velocity of wave-propagation is regarded but as the approximate result of an incomplete analysis. The problem presented itself to M. Cauchy in a similar point of view. In the profound researches of this mathematician relating to light, the ether is considered as a system of particles solicited by mutual attractions or repulsions; and from the partial differential equations which represent their movement, he had deduced the laws of propagation in crystallized as well as homogeneous media. These equations however were but approximate, and derived from others of greater generality by the omission of the terms containing the higher powers of the displacements, and of their derivatives with respect to the coordinates. Resuming the problem of the propagation of a plane wave, with the aid of the more general equations, he has finally demonstrated the existence of a relation between the velocity of propagation and the length of the wave *.

The *opacity* of bodies is ascribed by Newton to the discontinuity of their parts, and to the multitude of internal reflexions which the rays of light undergo within them †. We have many reasons for believing this to be the case; but as yet we are far from a complete account of the phenomenon. If the reflexions and refractions, which thus arise at each new bounding surface, be similar to those which take place at the outer surfaces of bodies, the molecules of light will indeed be scattered in every direction, but they should undergo no diminution of velocity. How, then, is it that they do not emerge finally from the body as readily as they entered it, and thus render it visible in all directions,—not by a superficial reflexion, but by a secondary emission? To account for the *extinction* of light, in the theory of emission, we must suppose it united to the body which it enters; and the simplest mode in which we can conceive this union to be brought about, is by the direct impact of the molecules of light on those of bodies, whereby they are brought within the sphere of those interior attractive forces to which chemical combinations are referred. This appears to have been the opinion of Newton. "Are not gross bodies and light," says he, "convertible into one another, and may not bodies receive much of their activity from the particles of light which enter their composition? For all fixed bodies being heated emit light, so long as they continue sufficiently hot, and light mutually stops in bodies as often as its rays strike upon their parts‡."

* *Mémoire sur la Dispersion de la Lumière.*—The attention of the Mathematical Section of the British Association was drawn to this theory by Professor Powell, at the last meeting, chiefly in reference to a limitation which seemed to be required in the physical hypothesis.—See *Report of Proceedings.*

† *Optics*, book 2, part 3. ‡ *Optics*, Query 30.

When from the simple fact of absorption we proceed to consider its law, as depending on the nature of the light, the difficulties increase at every step. The intensity of the transmitted light considered as a function of its refrangibility appears to be subject to no law, or to a law so complicated as completely to baffle all attempts to embrace it in an empirical rule. The maxima and minima are often actually numberless; and the variable does not reach them gradually, but by what seems to be an abrupt violation of the law of continuity. These apparently capricious changes were observed long since by Dr. Young, in the light transmitted through the common smalt-blue glass. Sir David Brewster has recently directed his attention to the same subject, and examined a great number of coloured bodies with reference to their absorptive properties. He has found, in particular, that a very remarkable definite action is exercised upon the rays of the spectrum by the green liquids obtained by extracting the colouring-matter of the leaves of plants in alcohol; and this action does not cease altogether even when the liquid has become perfectly colourless *. But the absorbing properties of nitrous acid gas, observed by the same author, are by far the most remarkable ever noticed. When the light transmitted through this gas is analysed by a prism, it is found that about two thousand portions of the beam are stopped, and two thousand dark spaces, or abrupt deficiencies of light, appear in the spectrum. These increase in number and magnitude with the temperature of the gas, until, by a sufficient elevation of temperature, this rare body becomes perfectly opaque, and refuses to transmit a single ray of the brightest sunshine †. Prof. Miller and Prof. Daniell have found some analogous properties in other gases. In the spectrum produced by the light transmitted through the vapours of *bromine* and *iodine*, more than one hundred dark lines are visible, disposed at equal distances ‡.

To account for the selection of certain classes of rays by coloured media, in the theory of emission, it seems necessary to suppose that an attractive force is exerted *at a distance* between the molecules of the body and those of light, and that the absolute value of this force varies with the colour. It does not seem easy to reconcile these suppositions to the Newtonian account of refraction; and the difficulty is still further increased when we proceed to apply the same considerations to the absorption of definite rays; and introduce the hypothesis of

* "On the Colours of Natural Bodies," *Edin. Trans.*, vol. xii.

† "On the Lines of the Solar Spectrum," &c., *Edin. Trans.*, vol. xii.

‡ French translation of Herschel's *Essay on Light*, Supplement, p. 455.

specific actions, varying in the most abrupt and irregular manner with the refrangibility of the ray *.

The absorption of light, and the opacity of bodies, were long since urged by Halley as difficulties in the wave-theory. The ether is supposed to penetrate all bodies freely, and why not also the undulatory motion in which light consists? To this difficulty we find a full and complete solution in the principle of interference. When a wave enters a discontinuous substance, it will be broken up, and its parts undergo continued subdivision by internal reflexions; so that when these parts reach the second surface of the body, they are found in *every possible phase*, and must destroy one another by interference. The phenomenon, as has been observed by Sir John Herschel, is analogous to the impeded propagation of sound in a mixture of gases differing much in elasticity as compared with their density.

The same writer has given an ingenious and natural account of the absorption of specific rays on the principles of the wave-theory, in a paper read before the Association last year †. He considers the molecules of the body and those of the ether as forming, conjointly, compound vibrating systems, which are more disposed to transmit vibrations of some determinate period than others. Other vibrations, however, not in unison with these systems, may be propagated through them. These *forced* vibrations, as he calls them, will be obstructed in their progress, and their amplitudes diminished by the mutual influence of the motions of the parts of the systems; and he shows that it is possible to conceive systems, which will be wholly impervious to a vibration of a particular period, while they freely transmit others not differing from them materially in their frequency ‡. But these important and interesting speculations, it must be remembered, are advanced by their author solely with the view of removing an imagined inconsistency between the phenomena of absorption and the mechanical laws of vibratory movement.

* See Sir David Brewster's Report on Optics.

† "On the Absorption of Light by coloured Media, viewed in connexion with the Undulatory Theory," *Phil. Mag.*, Third Series, vol. iii.

‡ An interesting interference experiment, similar in some respects to that indicated by Sir John Herschel in this paper, has been recently made by Mr. Kane. A compound tube, whose branches of 9 and 13½ inches united at the two extremities, was made to sound by the languette of an organ pipe. Each of the tubes, separately, gave its own fundamental note, and all its harmonics; and when a free communication was opened between them, the system gave all the notes of the two series, with the exception of those whose waves were in complete discordance. Thus the fundamental note of the short tube was *stopped altogether*, while its octave was given with remarkable clearness; the two waves being in complete *discordance* in the former case, and in complete *accordance* in the latter.

We are still far from a precise theory of absorption. When such a theory shall have been established, there seems reason to believe that it will bring with it also an insight into the internal constitution of bodies even yet more close than that afforded by the affections of polarized light; and that the laws of molecular action may perhaps, at some future day, be studied in the phenomena of transmitted light.

The properties of solar phosphori, which attracted so much of the attention of experimental philosophers of the last century, seem at first view to favour the account of absorption suggested by the theory of emission, and to arise from the disengagement of the light which had become united to the body. Canton observed that light may remain in these bodies, as it were in a latent state, for several months, until its re-emission is determined by the action of heat. But it must be observed, in the first place, that the feeble light emitted from the phosphori bears a very small proportion to that which they are supposed to receive by absorption. Dessaignes has remarked that most of these substances emit the *same kind of light*, whatever be the species of light to which they have been exposed*. The same fact has been observed by M. Grotthouss† and other subsequent inquirers; and in some of the diamonds possessing the property of phosphorescence, the most efficacious exciting light is of a *different colour* from that excited. These facts seem to be inexplicable in the theory of emission. In the wave-theory, on the other hand, the phenomenon is easily comprehended. As the vibrations of the air excite those of sounding bodies, and communicate to them a motion which continues for some time after the exciting cause has ceased to act; so it must also be with the undulations of the ether. When the body is *in unison* with the incident light, their vibrations will continue isochronous, and the undulations of the ether excited by the body will be of the same length as those by which it is itself excited. In the other case, the period of vibration, and consequently the length of the wave, will be altered, and the excited and exciting lights will be of different colours. The fact observed by Canton is indeed not so easily explained. Young supposed that the vibrations of the body may be abruptly suspended by cold, and may proceed anew when released from this

* *Mém. Inst.* tom. xi.

† *Schweigger's Journal*, 1815.—The same observer discovered the curious fact, that the electric current restored the property of phosphorescence, in many cases where it appeared to have been destroyed by the action of violent heat.

restraint, like a string which has been stopped and detained in any part of its vibration on either side of the centre.

The fixed lines in the solar spectrum first noticed by Wollaston, and afterwards more minutely traced by Fraunhofer, have lately been examined with great care, and with his usual success, by Sir David Brewster; and he has observed a remarkable coincidence between these lines and the dark bands of the spectrum of the nitrous acid gas *. Sir David Brewster has also studied, in connexion with the same subject, the definite absorbing effects of the earth's atmosphere. This has been effected by examining the solar spectrum, when the sun was near the horizon; and it has been found that most of the dark bands thus developed belonged to the fixed lines of Fraunhofer, which were thus, as it were, widened, and brought out, by the absorptive action of the atmosphere. A similar result has been arrived at in other cases, and it has been found that the points of the spectrum on which absorbing bodies exert the strongest specific actions are generally coincident with the deficient rays of solar light †. This singular connexion gives considerable weight to the speculations of Sir David Brewster respecting the latter phenomena ‡.

The observation of the fixed lines in the solar spectrum led Fraunhofer to examine the optical characters of the lights emanating from other sources. He thus arrived at the interesting discovery, that the system of bands in the different species of light which he examined, varied with the source; while it was constantly the same in the number of the bands, and their relation to the coloured spaces, in the light of the same source, however modified. In the light of Sirius there are three broad bands which have no resemblance to those of solar light. The light of the electric spark, on the other hand, when analysed by the prism is found to have several *bright* lines, of which that in the green is remarkably brilliant. Similar phenomena were observed in the light of artificial flames,—the flame of an oil lamp, for example, exhibiting a well-defined bright band between the red and yellow, and another not so distinct in the green §. This however is not universally the case. In the red flame of *strontia*, as was observed by Dr. Faraday and Mr. Talbot, there are a number of red rays separated from each other by *dark* bands; and in the flame of *cyanogen*, when similarly analysed, the violet is found to be divided into three distinct portions with broad dark intervals §.

* "On the Lines of the Solar Spectrum," *Edin. Trans.*, vol. xii.
† "On the Colours of Natural Bodies," *Edin. Trans.*, vol. xii.
‡ Report on Optics. § *Munich Memoirs.*
‖ *Phil. Mag.*, Third Series, vol. iv. p. 114.

It is easy to account for the general fact of the deficiency of certain *classes of rays* in certain lights. When a body violently heated begins to shine, the phenomenon is simply accounted for, in the wave-theory, by an increase in the frequency of its vibrations. In the same manner it seems natural to suppose, generally, that the mechanical agencies at work during combustion accelerate or retard, in various ways, the rate of vibration, and so alter the character of the emitted lights. The light emitted in weak or incipient combustion is generally blue. Sir John Herschel observed that when sulphur burns with a feeble flame, its light contains all the rays of the spectrum, and particularly the blue and violet; while, in vivid combustion, these disappear entirely, and the light is a yellow of almost perfect homogeneity *. The various shades of colour in the flame of a common candle,—from the deep blue of the lower part (which is found by prismatic analysis to consist of five distinct portions,) to the yellowish white in the centre, and thence to the dusky red at the apex of the flame,—seem to be referrible to the same principle. Fraunhofer and Sir David Brewster have both remarked that the flame of oil, urged by the blowpipe, consists chiefly or wholly of yellow rays. The same fact was long since observed by Mr. Melville with respect to the flame of alcohol, into which nitre, muriate of soda and other salts had been introduced †; and Sir David Brewster has found that the quantity of yellow light given out by burning bodies increases with their humidity, the flame of alcohol diluted with water being nearly a homogeneous yellow ‡. It is more important to remark however, in illustration of the undulatory view of the phenomenon of emission, that the *colour* of flames is often found to depend on the presence of something which is itself unaltered in the process of combustion. Thus Mr. Talbot has remarked that when a small quantity of muriate of lime was placed on the wick of a spirit lamp, it gave out red and green rays during an entire evening, though the salt was not sensibly diminished §. The absence of *definite rays* in certain lights, and the fixed lines of the solar spectrum, have been referred by Sir John Herschel to the same principle by which he has explained the absorption of specific rays ‖.

In what has preceded we have assumed the truth of the received theory with respect to the composition of solar light, and the connexion between the colour of a ray and its refrangibility. This theory however has been recently opposed by Sir

* "On Absorption of Light in coloured Media," *Edin. Trans.*, vol. ix.
† *Edinb. Essays.* ‡ On a Monochromatic Lamp. *Ibid.*
§ *Edinb. Journ. of Science*, v. 77. ‖ *Phil. Mag.*, Third Series, vol. iii. p. 407.

David Brewster. According to this philosopher, white light consists of but *three* simple colours,—red, yellow, and blue; and the solar spectrum is composed of three overlapping spectra of these colours, the intensity of each of which is greatest at the point where that colour is strongest in the compound spectrum. According to this view, then, all the colours in the solar spectrum are *compound*, and consist of red, yellow, and blue light, in different proportions. These compound colours cannot be analysed by the prism, in as much as the rays of which they consist at any point of the spectrum have the same refrangibility; and it is only by the different action of absorbing media on their constituent elements that their compound nature can be detected. Each of them may be conceived to consist of a certain quantity of *white* light, and of an *excess* of the light of two of the simple colours; and if this excess be absorbed, a white light will be the result, which will be indecomposable by the prism. This result of his hypothesis has been experimentally confirmed by Sir David Brewster *.

These views, if finally established, sever the connexion between the colour of a ray and its refrangibility, laid down by Newton; and the former must be supposed to depend,—not on the length of the wave,—but on some other element of the vibratory movement.

III. *Diffraction.*

It has been already stated that Newton considered the undulations of an ethereal medium to be a necessary part of his theory, and that that theory as maintained by its author differed from the theory of Huygens and of Hooke, only by the *addition* of a new hypothesis. The necessity of something extraneous to the undulations of the ether seems to have been admitted by Newton mainly to account for the right-lined propagation of the rays of light; and a careful consideration of his optical writings leaves the impression, that had the wave-theory alone appeared to explain this fact, Newton would not have hesitated to embrace it. This explanation has been spoken of in another place, and it has been shown to follow from that theory, that the light which encounters an obstacle must diminish rapidly in intensity within the edge of the geometric shadow. It now remains to consider the other phenomena which arise under these circumstances; and it will be found that the same theory affords the most complete account, not only of their general characters, but even of their numerical details.

In order to understand the theory of *shadows*, it is necessary

* "On a New Analysis of Solar Light," *Edin. Trans.* 1831.

to investigate their laws in the simple case in which the magnitude of the luminous body is reduced to a point. The effects thus presented were first observed by Grimaldi, and they have been since studied, as a separate branch of optical science, under the title of *diffraction* or *inflexion*. Grimaldi found that when a small opaque body was placed in the cone of light, admitted into a dark chamber through a very small aperture, its shadow was much larger than its geometric projection, so that the light suffered some deviation from its rectilinear course in passing by the edge. Observing these shadows more attentively, he found that they were bordered with three iris-coloured fringes, which decreased in breadth and intensity in the order of their distances from the edge of the shadow, preserving the same distance from the edge throughout its entire extent, unless where the body terminated in a sharp angle. Similar fringes were observed under favourable circumstances within the shadows of narrow bodies*.

The phenomena of diffraction were subsequently examined by Hooke and by Newton. The first observations of Newton were but repetitions of those of Grimaldi; and it is remarkable that he altogether overlooked the important phenomenon of the *interior fringes* noticed by the Italian philosopher. But to Newton we owe the analysis of the phenomena, so far as they depended on the nature of the light. When the different species of simple light into which the sun's rays were divided by a prism were cast in succession on the diffracting body, Newton observed that the fringes formed were broadest in red light, narrowest in the violet, and of intermediate magnitude in the light of mean refrangibility, so that the iris-coloured fringes which are formed in white light are but the fringes of different colours superposed. But the observations of Newton most closely connected with his physical theory are those in which the light is made to pass between two near knife-edges, whether parallel or inclined. From these observations Newton concluded that the light of the first fringe passed by the edge, at a distance greater than the 800th of an inch, that of the second and third fringes passing at still greater distances. These distances, however, were not the same wherever the fringes were formed; and it appeared to follow from the experiment, that the light of the same fringe was not the same light at all distances, but that each fringe was, as it were, a caustic formed by the intersection of the rays passing at different distances from the edge; the portion of the fringe near the knives being formed of light which passed nearest to the edge and was most bent†.

* *Physico-Mathesis de Lumine*, Bologna, 1665.

† *Optics*, Book iii.

To account for these phenomena Newton supposed the rays of light to be *inflected* in passing by the edges of bodies, by the operation of the attractive and repulsive forces which the molecules of bodies were conceived to exert on those of light at sensible distances. Thus, the rays passing by the edges of a narrow opaque body are supposed to be turned aside by its repulsion; and as this force decreases rapidly as the distance increases, the rays which pass at a distance from the body will be less deflected than those which pass close to it. The caustic formed by the intersection of these deflected rays will be concave inwards; and as none of the rays pass within it, it will form the boundary of the visible shadow. To explain the alternations of darkness and light beyond this, Newton appears to have supposed that the attractive and repulsive forces succeed one another for some alternations; and that the molecules composing each ray, in their passage by the body, are bent to and fro by these forces "with a motion like that of an eel," and are finally thrown off at one or other of the points of contrary flexure. The separation of white light into its elements is explained, by supposing that the rays which differ in refrangibility differ also in *inflexibility*; the body acting alike upon the less refrangible rays at a greater distance, and upon the more refrangible at a less distance*. In one of his letters to Oldenburgh †, Newton advances a more refined theory of diffraction. The bending of the ray near the edge of the obstacle he conceived to arise from a variation in the density of the ether in the neighbourhood of the body; and, following the analogy of thin plates, he endeavoured to account for the coloured fringes by the vibrations of the ether which are propagated faster than the rays themselves, and overtake them at the middle of the curved portion of the trajectory they describe.

It is needless to comment upon the vagueness of these explanations. Newton himself was dissatisfied with them, and the subject fell from his hands unfinished. Still, however, the mere guesses of such a mind as that of Newton must possess a high interest, and we are not to wonder that among his followers more weight should be attached to these explanations than he himself ever gave them. It seems necessary therefore to advert to some of the circumstances of these phenomena, which are not only unexplained by this theory, but which seem moreover irreconcileably at variance with it.

If the phenomena of inflexion be the effects of attractive and repulsive forces emanating from the interposed body, and if these forces are the same, or even analogous to those to which

* *Optics*, Book iii. Queries 1, 2, 3, 4.

† December 21, 1675.—*Birch's History of Royal Society*, vol. iii.

the reflexion and refraction of light are ascribed in the theory of emission, it will follow that they must exist in different bodies in very different degrees; so that the amount of bending of the rays, and therefore the position of the diffracted fringes, should vary with the *mass*, the *nature* and the *form* of the inflecting body. Now it is clearly ascertained, on the contrary, that *all bodies*, whatever be their nature or the form of their edge, produce under the same circumstances fringes *identically the same*; and in fact the partial interception of light, caused by the interposition of an obstacle of any kind, seems to be the only condition on which the character of the phenomenon depends. Gravesende seems to have first observed that the nature or density of the body had no effect upon the magnitude of the diffracted fringes; and the fact has since been confirmed in the fullest manner by almost every inquirer in this branch of experimental science. One of the ablest supporters of the theory of emission has admitted that the inflecting forces, if such exist, must be independent of the chemical nature of the inflecting body, and altogether different in their nature from those to which, in the same theory, the phenomena of reflexion and refraction are ascribed*. To ascertain whether the form of the edge had any effect upon the fringes, Fresnel took two plates of steel, the edge of each of which was rounded in one half of its length and sharp in the remaining half, and placed the rounded portion of one edge opposite the angular part of the other, and *vice versâ*. If, then, the position of the fringes depended on the form of the surface, the effect would thus be doubled, and the fringes appear broken in the middle. They were found, on the contrary, to be perfectly straight throughout their entire length†.

Again, the inflecting forces (though they must be supposed to vary in intensity, with the form and mass of the body, and with the distance of the luminous molecule from the edge) cannot be conceived to depend in any way upon the distance previously traversed by the molecule before it arrives in the neighbourhood of that edge; so that the magnitude and position of

* Biot, *Précis élémentaire*, vol. ii. p. 473, 3me Edit.

† *Mémoire sur la Diffraction*, p. 370. The *Bulletin Universel* for February 1828 contains some animadversions on this part of Fresnel's optical labours, in a paper signed by the secretary of the Academy of Sciences of St. Petersburgh, and purporting to be an official reply to some remarks in a former number of the *Bulletin* on the programme of the prize questions proposed by the Academy. The writers have confounded two experiments of Fresnel which were instituted with different views, and differently reasoned upon. Fresnel's object in this experiment was simply to show that the form of the edge produced no effect upon the fringes, as it ought to do if diffraction arose from attractive or repulsive forces extending to sensible distances from bodies. Most of the objections urged in the same paper against the wave-theory arise, in like manner, in misconception.

the fringes, in this hypothesis, cannot vary in any way with the distance of the inflecting edge from the luminous point. But this conclusion is the reverse of fact: the fringes *dilate in breadth*, and their mutual inclination is increased, as the screen approaches the luminous origin. There seems to be but one way of avoiding the inference drawn from this fact against the theory of emission. It may be supposed that the bands have their origin at some sensible distance from the edge of the body, and thus that the obliquity of the incident ray varies as the edge approaches the luminous point. Such was the conjecture of Dutour, who noticed the fact. Fresnel has calculated the breadth of the fringes according to this supposition, and found that the computed and experimental results do not agree*. But, in point of fact, the bands may be supposed without sensible error to have their origin at the edge itself. Fresnel found by direct measurement that the distance of the third band from the edge of the shadow at its origin was less than the 100th part of a millimetre.

The objections just considered seem to apply equally to the hypothesis of Mairan and Dutour, in which the phenomena of diffraction are referred to the reflexions and refractions of an atmosphere supposed to encompass all bodies. For if such an atmosphere be retained around the body by its attraction, (and this seems to be the only mode of accounting for its presence,) its density and its form must vary with those of the body itself, and consequently its effects upon the rays of light must vary also. But the experiments of M. Haldat seem to leave no tenable ground for these hypotheses. Every agent has been tried which could be conceived capable of modifying the attractive force of the body, or the density of the imagined atmosphere, and without effect. The metallic wires and plates which produced the fringes were heated to redness, and cooled down below the freezing-point; they were traversed by voltaic currents, and the charges of powerful batteries transmitted through them; but in whatever manner the condition of the diffracting body was varied, no change whatever was perceived either in the intensity or dimensions of the diffracted fringes†.

Although the phenomena of diffraction were studied by many diligent observers ‡ after the publication of the *Optics*, no ma-

* "Mémoire sur la Diffraction de la Lumière," *Mém. de l'Institut*, tom. v. p. 353.

† "Sur les Causes de la Diffraction," *Annales de Chimie*, tom. xli. Similar experiments had been made some time before by Mayer, and with the same result. *Gottingen Memoirs*, vol. iv.

‡ Maraldi (*Mém. Acad. Par.* 1723.), Mairan (*Ibid.* 1738.), Dutour (*Mémoires présentes*, tom. v.), Mr. Brougham (*Phil. Trans.*, 1796—7.), and Mr. Jordan (*New Observations concerning the Inflexion of Light.* London 1795).

terial accession was made to the knowledge of their laws until the principles of the wave-theory were applied to their explanation by Young. The *exterior* fringes, formed without the shadows of bodies, were ascribed by Young to the *interference* of two portions of light, one of which passed by the body and was more or less inflected, while the other was obliquely reflected from its edge, the latter losing half an undulation at the instant of reflexion*. The fringes formed by narrow apertures were, in like manner, supposed to arise from the interference of the two pencils reflected from the opposite edges; while the *interior* fringes, within the shadows of narrow bodies, were accounted for by the interference of the pencils which passed on either side of the body at an insensible distance, and were inflected into the shadow. The observed facts closely correspond with the calculated results of this theory; and in the case last mentioned Young proved that the phænomena admitted of no other explanation. Placing a small opaque screen on either side of the diffracting body, so as to intercept the portion of light which passed by one of its edges, the bands immediately disappeared, although the light passing by the other edge was unmodified. The same effect was produced, and by the same means, upon the *crested fringes* of Grimaldi, formed within the shadows of bodies having a rectangular termination†. Thus the phenomena of the fringes, or the alternations of light and darkness, were shown to be cases of the more general principle of interference; and the connexion is now admitted by some of the warmest advocates of the Newtonian theory‡. The bending of the light into the shadow, or the fact of inflexion itself, was at first ascribed by Young to the refraction of an ethereal atmosphere encompassing bodies and decreasing in density with the distance. He afterwards, however, adopted the simpler doctrine of Huygens and Grimaldi, and referred the phenomenon to the fundamental property of waves.

But perhaps the most important of the labours of Young on his subject is that in which he descends into numerical details, and, taking the observations of Newton, as well as his own, calculates the differences of the lengths of the paths traversed by the two pencils, when they destroy or reinforce one another by interference. These intervals he found to constitute an arithmetical progression for the successive bands; the first term of which was the same in the same species of light, whatever be

* "On the Theory of Light and Colours," *Phil. Trans.* 1802.

† "Experiments and Calculations relative to Physical Optics," *Phil. Trans.* 1804.

‡ Biot, *Précis élémentaire*, vol. ii. p. 472., 3me Edit.

the distance at which the fringes are received, or the other conditions of the experiment. And, finally, comparing these constants with the similar intervals of the two pencils reflected by the surfaces of a thin plate, as deduced from the experiments of Newton, he found that their difference was within the limits of error to which such observations are liable, and that we are warranted in concluding that the two classes of phenomena are to be referred to one simple principle*. It is true, that in these calculations, Young starts from an erroneous principle respecting the lights which form the diffracted fringes by their interference, and he has remarked some discordances in his results which have, no doubt, their origin in that circumstance; but the results of the exact theory are not greatly different from that which he adopted, and the more complete analysis of Fresnel has only tended to confirm the conclusion obtained by Young.

The important experiment of Young, on the disappearance of the fringes in the shadow of a narrow opaque body, when the light passing by one of its edges was intercepted, was that which first led him to the principle of interference. An instructive variation in this experiment was made by M. Arago. The interior fringes were found to disappear likewise when the light passing by one of the edges was transmitted through a plate of some transparent substance; and by varying the thickness of the interposed plate, M. Arago discovered that the disappearance of the fringes in this case arose from their *displacement*, the bands being always transferred to the side on which the plate was interposed. From this it followed, that the light was *retarded* in the denser medium †. M. Arago afterwards produced the same modification in the interference bands formed by two mirrors; and the experiment, in this form, is a complete *crucial instance*, as applied to the two theories of light. The amount of the displacement determines the velocity of light in the medium, and therefore the refractive index, with an accuracy unattainable by any other method. Professor Powell has suggested a very elegant modification of this experiment, which at once establishes the truth of the law, that the velocity of light is inversely as the refractive index of the medium traversed ‡.

The experimental laws of the diffracted fringes were next examined by MM. Biot and Pouillet. In the case of a narrow rectilinear aperture, (which was that chiefly studied,) they found

* "Experiments and Calculations relative to Physical Optics," *Phil. Trans.*

† "Sur un Phénomène remarquable qui s'observe dans la Diffraction de la Lumière," *Annales de Chimie*, tom. i.

‡ *Phil. Mag.*, Second Series, vol. xi. p. 6.

that the deviations produced in the different species of simple light, or the distances of the bands from the axis of the pencil, were in all cases *proportional to the lengths of the fits*; the magnitude of the aperture remaining the same. The same analogy was preserved in different media, the deviations varying in the inverse ratio of the refractive indices of the media, or in the direct ratio of the fits*. M. Pouillet adds, that they were unable to explain these laws, having adopted the theory of emission†. They are all simple consequences of the wave-theory. The interval of the fits is exactly half the length of a wave, and the true connexion between the place of the fringes and the latter quantity had been already pointed out by Young.

Mayer afterwards studied the phenomena of diffraction, but without adding any new facts to those already known. As to the theory, he adopted that of Newton, with some modifications. With Newton, he ascribed the inflexion of light into the shadow to the operation of an attractive force; but, unwilling to admit the existence of a repulsive force, he attempted to account for *deflexion* by the impact of the molecules reflected from the edge against those which passed by it‡.

Fresnel at first adopted and developed Young's theory of diffraction, and found that the general laws of the fringes,—the dependence of their magnitude upon the length of a wave, and upon the distances of the luminous origin and of the screen,—were thus fully explained. It was shown, that as the position of the screen is varied, the successive points at which the same fringe is formed are not in a right line, but constitute an *hyperbola*; and that when the distance of the luminous origin is lessened, the inclination of these hyperbolic branches considered as coincident with their asymptots, augments, and the fringes dilate in breadth§. Fresnel, however, was soon dissatisfied with this theory. If the exterior bands had their origin in the interference of the direct and reflected light, their intensity should depend on the curvature of the edge; it is found, on the contrary, that the fringes formed by the back and by the edge of a razor are precisely alike in every respect. As to the other cases of diffraction, there were many phenomena, and especially those exhibited in Newton's experiment with the two knife-edges, which proved that the rays grazing the edges of the body were not the only rays concerned in the production of

* Biot, *Traité de Physique*, tom. iv., Supplement à l'Optique.
† *Elemens de Physique*, tom. ii. p. 437.
‡ *Comm. Soc. Gottingensis Recentiores*, vol. iv. p. 49.
§ *Annales de Chimie*, tom. i. p. 239.

the fringes, but that the light which passed by those edges at sensible distances was also deviated, and concurred in their formation*.

Fresnel was thus led to seek a broader foundation for his theory, and the result of his investigations is given in the able memoir which was crowned by the French Academy in 1819. In this memoir the laws of diffraction are derived from the two principles to which the laws of reflexion and refraction are themselves referred,—the principle of interference and the principle of Huygens. To apply these principles to the present case, Fresnel supposes the surface of the wave when it reaches the obstacle to be subdivided into an indefinite number of equal portions, and he applies the mathematical laws of interference, unfolded in this memoir, to determine the resultant of all the elementary waves sent by them at the same instant to any point. This resultant is expressed by means of two integrals, which are to be taken within limits determined by the particular nature of the problem. Its square is the measure of the intensity of the light; and it is found that its value has several maxima and minima which correspond to the intensities of the light in the bright and dark bands.

The problem of diffraction was thus completely solved, and it only remained to apply the solution to the principal cases, and to compare the results with those of observation. The cases of diffraction selected by Fresnel are: 1st, The phenomena produced by a single straight edge; 2nd, By an aperture terminated by parallel straight edges; and 3rd, By a narrow opaque body of the same form. The agreement of observation and theory is so complete, that the computed places of the several bands seldom differ from those observed by more than the 100th part of a millimetre, the case of diffraction by narrow apertures alone excepted. The small differences between observation and theory, in this case, Fresnel ascribes to a false judgment of the eye as to the position of the centre of the dark bands, occasioned by the different intensities of the bright bands on either side; the minimum always appearing nearer to the brighter light than it really is. The computed places of the bands, in the first case of diffraction, were found to differ from those deduced from the hypothesis of Young by a small numerical quantity, the distance of the first dark band being less in the former theory, in the ratio of ·936 to unity; but small as the difference is, the measures of Fresnel completely decide the question†.

* *Mémoire sur la Diffraction de la Lumière*, p. 368. † *Ibid.*, p. 420.

M. Poisson applied Fresnel's integral to the case of diffraction by an opaque circular disc, and arrived at the singular result, that the intensity of the light in the centre of the shadow is precisely the same as if the disc were removed. This remarkable anticipation of theory has been verified by the observation of M. Arago*. Fresnel has himself solved the problem in the analogous case of a circular aperture, and arrived at the result, that the intensity of the light of any simple colour, at the central spot, will be the same as that reflected by a plate of air, whose thickness bears a certain simple relation to the radius of the aperture, and its distances from the luminous origin and from the eye. With homogeneous light, therefore, the illumination of the central spot vanishes periodically, as the distance of the eye from the aperture is varied; and in white light it assumes in succession the most vivid and beautiful hues, coinciding with those of the reflected rings of thin plates. These interesting phenomena were observed about the same time by Sir John Herschel, and their laws deduced, independently, from observation†.

With the exception of the observations now referred to, no attempt has been made to verify the theory, by comparing the *intensity* of the light in the fringes with that deduced from the formulæ; and indeed it is obvious that a comparison of this nature is ill calculated to afford any conclusive evidence on the question. Fresnel thought, however, that the expression for the intensity might be *indirectly* verified, by superposing two sets of fringes (such as the interior and exterior fringes of a narrow opaque body,) by means of double refraction, and then examining the *position* of the new maxima and minima. This ingenious suggestion does not appear to have been acted on.

The intensity of the light in the partial waves sent from each point of the primary wave, considered as a distinct centre of disturbance, will necessarily be different in different directions, depending on the angle which these directions form with the front of the original wave; and to solve the problem of diffraction in its most general form, it would be necessary to know the law of this variation. Fresnel has shown, however, that the rays whose directions are inclined at sensible angles to the normal to the front of the primary wave, destroy one another by interference; so that the actual effect is produced by rays indefinitely near that normal, and which therefore may be regarded as of equal intensity. The truth of this assumption, however, is

* *Mémoire sur la Diffraction*, p. 460. † *Essay on Light*, Art. 729.

disputed by M. Poisson. From his theory of the propagation of motion in fluid media, this mathematician inferred that the absolute velocities of the molecules are insensible in directions making finite angles with the direction of the original vibrations. He concludes, therefore, that these velocities, or the intensity of the light in the partial waves, cannot be regarded as sensibly equal in directions inclined to it at very small angles*. Fresnel's reply to this part of M. Poisson's theory has been already referred to. The principle of Huygens itself, which forms the basis of Fresnel's theory, though not denied by M. Poisson, is yet objected to, as introducing a needless complication into the question; and indeed it does not seem easy to understand, at first view, why each point of the primary wave in this mode of composition should not give rise to a *retrograde* as well as to a direct wave †.

An objection of a different nature has been raised against Fresnel's theory, derived from its supposed discordance with phenomena. It is a consequence of that theory, when applied to the case of diffraction by a narrow aperture bounded by parallel straight edges, that if a point be taken in the axis of the pencil, whose distances measured from the centre and edge of the aperture differ by half a wave, that point will be the limit within which all the *interior* fringes are confined; and beyond that point the centre of the image will be *always white*. This result is confirmed by the previous experiments of M. Biot, by the observations of Fresnel himself, and by those of Professors Airy and Powell, by whom they have been since repeated. M. Biot found that the central band was dark and white alternately, to a certain distance from the aperture; after which it was always white. He remarks that when this limit is attained, we may diminish the breadth of the aperture, and even bring its sides into actual contact, without any change in the central band except its enlargement and consequent diminution of intensity ‡.

Newton's celebrated experiment with the two knife-edges has

* It may be necessary to state that it was part of M. Poisson's theory, that the vibrations are normal to the wave.

† See *Annales de Chimie*, tom. xxii. p. 270, tom. xxiii.; and Airy's *Math. Tracts*, p. 267.

‡ *Traité de Physique*, tom. iv. pp. 749, 760. The description of the phenomenon given by Mayer is very similar: "Prout illa distantia acierum semper magis magisque imminuitur, fasciæ adeo evanescunt, ita ut denique non nisi fascia media remaneat; sed ad dextram atque sinistram adeo in latitudinem extensa, ut non nisi lumen languidum, a medio spectri initialis utrinque instar caudæ cometæ sese dilatans, representet." *Gottingen Memoirs*, vol. iv. p. 61.

been adduced in opposition to these results. Newton found that when the distance of these edges was the 400th part of an inch, the light which passed between the knives parted in the middle, and left a *dark space* in the centre*. The experiment has been repeated by Mr. Barton, and with a similar result†. These experiments, however, were made with *curved edges*; and as Professor Powell has observed, we have no ground for supposing that the phenomenon may not be modified by this change in the conditions under which it is presented. The theory of Fresnel has not been applied to the more complex problem of an aperture with curvilinear edges, and the analytical difficulties of the problem seem to be insuperable. There seems to be some uncertainty, however, with respect to the phenomenon itself. Professor Powell repeated the experiment with edges of various curvatures, and always found that the centre was a point of relative brightness, as compared with other points in the line perpendicular to the length of the aperture‡. As to Newton's experiment, it seems certain, as the same writer has observed, that we are not acquainted with all its conditions; and it is apparent from many passages that the illustrious observer himself was far from being assured with respect to the real nature and circumstances of these phenomena§.

But there is another essential circumstance to be taken into account, in comparing the experiments of Newton with the results of Fresnel's theory. In that theory the origin of light is supposed to be a point, and this condition is practically fulfilled by making the light to diverge from the focus of a lens of high power; the origin of the light in that case being (by the principles of the wave-theory) the minute image of the sun in the focus. In Newton's experiments, however, the sun's light was made to pass through a hole of sensible magnitude; and in the remarkable experiment now referred to, that hole was a quarter of an inch in diameter. The problem of diffraction in this case is one of much greater complexity. It is necessary to determine the joint effect produced at any point of the diffracting aperture by the several indefinitely small portions of a wave

* *Optics*, Book iii., Obs. vi. and vii.

† *Phil. Mag.*, vol. ii. p. 268.

‡ *Ibid.*, p. 429, &c.

§ "The subject of the third book I have also left imperfect, not having tried all the experiments which I intended when I was about these matters, nor repeated some of those I did try until I had satisfied myself about all their circumstances. To communicate what I have tried, and leave the rest to others for further inquiry, is all my design in publishing these papers." *Optics*, Advertisement 1. See also latter part of Obs. 11. Book iii.

transmitted through the external hole; and, considering each of these as a new centre of disturbance, to find their total resultant at any point of the screen on which the fringes are received. The method of solution has been pointed out by Professor Airy; and he has shown that when the external hole is a rectangular parallelogram, and the diffracting aperture of the same form and similarly placed, the law of illumination at any point of a screen will be similar to that produced by a rhomboidal aperture, in Fresnel's method of observation; the dimensions and distances in the two cases being connected by certain relations*. From these investigations Professor Airy concludes that the size of the external hole could not account for the dark central shadow mentioned by Newton in the sixth observation. He has confirmed this conclusion by experiment; and employing holes of various magnitudes, he found the central band in all cases bright. The effect recorded by Newton is ascribed by Professor Airy to the influence of contrast on the retina.

A remarkable class of phenomena arise when a lens is placed close to an aperture of any form, and the light received on a screen at its focus, or on an eyeglass at its own focal distance from it. In fact, the phenomena of diffraction are in this manner produced with holes of considerable dimensions, and were observed by Sir W. Herschel, with the undiminished apertures of his great telescopes; the stars being seen encompassed by several dark and bright rings, succeeding one another at equal intervals, when a high magnifying power was employed. But the phenomena become more distinct when the aperture is limited by a diaphragm of moderate size, the diameters of the rings varying inversely as those of the apertures. The effects produced by diaphragms of different sizes and forms have been examined in much detail by Sir John Herschel and M. Arago†.

The phenomena produced by *minute* apertures, when combined with a lens in the manner now spoken of, have been studied with much zeal and success by Fraunhofer. The most remarkable of these phenomena are those produced by a fine grating, such as may be formed by stretching a fine wire between two parallel screws of equal thread. When such a grating is placed before the object-glass of a telescope, and a narrow slit whose length is parallel to the wires of the grating, viewed through it, the direct image of the slit is bordered on either side by a succes-

* "On the Calculation of Newton's Experiments on Diffraction," *Cambridge Trans.*, vol. v. part 2.

† Professor Amici has also noticed some phenomena of the same class. See *Edin. Journal of Science*, vol. iv. p. 306.

sion of richly coloured diffracted images, which increase in breadth and diminish in brightness, as they recede from the centre. The first pair of spectra are separated from the central image by a space absolutely black, and a similar interval occurs between the first and second pair. Fraunhofer observed, under favourable circumstances, 13 such spectra on either side of the central image. He has measured with great accuracy the angular deviations of the rays of each colour from the axis; and he has found that the experimental laws thus deduced agree in the most complete manner with the results of the principle of interference*. The results are the same, both by theory and experiment, in the case of reflexion from ruled surfaces†.

The optical phenomena of gratings are interesting in many points of view. The appearance of lateral spectra, produced by simply *intercepting a part of the light*, proves that the light actually diverges in all directions from the front of the grand wave where it meets the lens, and that it is to the interference of this light with that intercepted by the grating that we are to ascribe its want of sensible effect under ordinary circumstances ‡. Another very remarkable circumstance of these phenomena is the purity of the light of each simple colour, which is such that the fixed lines may be discerned in the spectra. The distances of these lines, in the diffracted spectrum, are *always proportional*, whatever be the diffracting substance; while the ratio of their intervals, or the breadths of the coloured spaces, in the spectra formed by refraction, *vary* with the nature of the

* The angular deviation, θ_n, of any ray from the axis is expressed by the formula

$$\sin \theta_n = \frac{n\,\lambda}{\varepsilon},$$

in which n denotes the *order* of the spectrum, λ the length of an undulation, and ε the interval of the axes of the wires. The value of ε is obtained with great precision, so that the measurement of the angular deviations of the rays of each simple colour affords the most exact data for the determination of the length of their waves. Fraunhofer has in this manner computed the lengths of the waves, corresponding to the seven principal fixed lines in the spectrum; and the resulting values are perhaps the most exact optical *constants* we possess. It is a remarkable consequence of the expression above given, that when ε is less than λ, the angle θ_n will be imaginary. In this case, then, there can be no coloured spectra; and it follows that scratches or inequalities on any polished surface, whose interval is less than the length of a wave, do not disturb the regularity of reflexion and refraction.

† Fraunhofer's researches on diffraction are published in the Memoirs of the Bavarian Academy of Sciences, vol. viii. A very full analysis of them is given in the *Edinburgh Encyclopædia*, art. Optics; and in Sir J. Herschel's "Essay on Light," *Encyc. Metrop.*

‡ Airy's *Math. Tracts*, p. 331.

prism. This fact appears to be decisive against the Newtonian theory of inflexion, in which inflexion and refraction are referred to the same cause.

The analytical investigation of the problem of diffraction in the cases last alluded to,—those, namely, in which a lens is combined with the aperture, and the intensity of the light is sought at any point of a parallel plane passing through the focus, —is far more manageable than in most other cases. The general expression of the displacement is at once integrated with respect to one of the variables, and the complete integral can, in many cases, be exactly found. Professor Airy has given the solution of this problem in his valuable tract on the Undulatory Theory*, and in applying it to the phenomenon last mentioned has deduced all the appearances observed by Fraunhofer. The remarkable appearance of the six-rayed star, observed by Sir John Herschel, when a triangular diaphragm was placed before the object-glass of a telescope, has been likewise deduced as another case of the same problem.

The same effects, Fraunhofer observed, were produced by reflexion from grooved surfaces; and their theory is to be referred to the same principles, the light reflected from the surfaces between the grooves interfering in a manner precisely analogous to that admitted through the apertures of the gratings. The colours exhibited by such surfaces under ordinary circumstances were observed by Boyle and Grimaldi; Young showed that they were consequences of the principle of interference, and determined the law of their recurrence depending on the incidence†; and Sir David Brewster seems to have been the first to observe that the spectra formed in these cases of multiplied diffraction approached the solar spectrum in purity, far more nearly than the ordinary diffracted bands, or the coloured rings of Newton. These phenomena indicate the superficial structure more unerringly, perhaps, than the most powerful microscopes. Among the most important and beautiful instances of this application of optical science may be ranked the analysis of the colours of mother-of-pearl‡, and the investigation of the structure of the crystalline lenses of the eyes of fishes and other animals, by Sir David Brewster§. The same author has also described a new series of periodical colours, which are exhibited by some of the plates of grooved steel constructed by Mr. Barton, and which suc-

* *Math. Tracts*, p. 321, &c.

† "On the Theory of Light and Colours," *Phil. Trans.* 1801.

‡ *Phil. Trans.* 1814.

§ *Ibid.* 1833.

ceed one another in a plane at right angles to that in which the usual spectra are developed*. The theory of this phenomenon remains yet to be developed. In the solution of the analogous problem, given by Professor Airy, a periodical variation in the intensity of the light in the direction of the apertures of the grating is indeed pointed out; but that variation, it is easily seen, will not account for the facts last mentioned.

IV. *Colours of thin Plates.*

The earliest observations on record, in which the colours of thin plates were made the subject of experimental research, are those of Boyle†. This diligent observer remarked the fact, that most transparent substances exhibit colour by reflected light when sufficiently reduced in thickness; and that these tints varied in the same substance, and therefore did not depend essentially upon its chemical nature. The observations of Boyle were made on the bubbles of various liquids, and he even succeeded in blowing glass sufficiently thin to exhibit similar phenomena.

The vivid and varying colours of the soap bubble also engaged the attention of Hooke‡; but the most important of the observations of this philosopher, connected with the subject of thin plates, are those recorded in his *Micrographia*, which was published in the year 1665. In this work he shows, that the colours of laminæ of mica are dependent on their thickness, and appear only when that thickness is comprised within certain limits; that when the tint exhibited by a given plate is uniform over its entire surface, the plate is also uniformly thick; and that the colour presented by two plates superposed is different from those of either separately. Hooke has also the merit of producing the phenomena of thin plates in the instructive form in which their laws have since been studied, namely, by placing two object-glasses in contact; and he found that any transparent fluid introduced between the lenses furnished a succession of colours as well as air;—the colour, however, being more vivid, the more the refractive power of the plate differed from that of the glasses within which it was inclosed.

The attention of Newton was soon after directed to the same subject; and his investigations, which ended in the complete

* *Phil. Trans.* 1829.
† *Experiments and Observations upon Colours*, 1663.
‡ *Birch's History of the Royal Society*, vol. iii. p. 29.

discovery of the laws of the phenomena, will ever be considered as a model of experimental inquiry. A convex lens of glass being laid upon a plane surface of the same material, after the manner of Hooke, the bands of the same colour are arranged round the point of nearest approach in concentric circles; and the diameters of these circles will be obviously as the square roots of the thicknesses of the plate of air at the points at which they are exhibited. In order to investigate the relation between the colour and the thickness, then, it was only necessary to measure the diameters of these rings in the different species of simple light; and taking similar measurements when the other circumstances of the phenomena were varied, Newton deduced their laws, as they depended on the substance of the reflecting plate, and on the obliquity of the incident pencil. Newton observed, moreover, that there was a second system of rings formed by *transmission*. The transmitted rings were found to observe the same laws,—with this remarkable exception, that the colour transmitted at any particular thickness of the plate was always complementary to that reflected at the same thickness; so that in homogeneous light, the bright transmitted ring is always found at the same distance from the centre as the corresponding dark one of the reflected system.

The observations of Mariotte*, Mazeas†, and Dutour‡ have added nothing essential to the laws discovered by Newton. Most of these observations, in fact, related to the colours exhibited by the plate of air inclosed between two *plane* glasses; and in circumstances, therefore, much less favourable to the analysis of the phenomenon than those selected by Newton. Perhaps the most interesting of the facts noticed by Mazeas are the effects produced on the coloured bands by the application of heat to the glasses, the colours retreating to the edges of the plates, and the bands diminishing in breadth as the temperature was increased. The same author also found, that no sensible change took place in the phenomenon when the air was withdrawn by the air-pump.

In the observations of Dutour, the reflected and the transmitted tints were observed at the same time, the latter being reflected from the second surface of the lower glass, and returning to the eye through the entire system. This latter set of rings is rendered more distinct, when the shadow of an opaque body is passed over the upper surface. In this manner the phenomenon was observed by Sir William Herschel; and it was found that ad-

* *Traité de la Lumière et des Couleurs.*
† *Mémoires présentés*, tom. ii. ‡ *Ibid.*, tom. iv. v. vi.

ditional sets of rings became visible by increasing the number of reflecting faces. Sir William Herschel observed, likewise, that the primary reflected system was produced when a lens was laid upon a metallic reflector; and he remarks, that in this case the transmitted system must be conceived to be absorbed by the metal. The same author has described a remarkable set of coloured bands adjacent to the *iris*, at the limit of total reflexion, when a prism is in contact with a plane surface *. The analysis of this phenomenon has been given by Sir John Herschel in his Essay on Light†.

The important observations of M. Arago are the next to demand our notice‡. Viewing the rings through a rhomboid of Iceland spar, whose principal section was parallel or perpendicular to the plane of incidence, this philosopher observed that the intensity of the light in one of the images varied with the incidence, and that it vanished altogether when the rays made an angle of 35° with the surface. It was further observed, that the same image vanished, and at the same angle, whether the rings were formed by reflexion or transmission. Thus, the light of the transmitted, as well as of the reflected rings, was wholly polarized *in the plane of incidence*, and at the usual angle for glass. M. Arago has further shown, that the colours of the reflected and transmitted rings are not only complementary, but that their intensities are also precisely the same; for, when the two systems are superposed, they completely neutralize each other.

But the most remarkable of the results obtained by this author relate to the rings formed by the plate of air inclosed between a lens of glass and a metallic reflector. When these were observed in the manner already alluded to, one of the images vanished, as before, *at the polarizing angle of glass*; while its appearance, at angles above and below the polarizing angle, presented a remarkable contrast. When the incidence was less than this angle, the two images seen through the double refracting crystal differed only in intensity; the dimensions and colours of the rings were the same in both. Beyond the polarizing angle, however, the rings in the two images were of *complementary colours*; so that if the series in one commenced from a black centre, in the other it began from a white one. The dimensions of the rings of the same order in the two images were also different. Similar phenomena were produced when the thin

* "Experiments for investigating the Cause of the coloured Rings," &c., *Phil. Trans.* 1807, 1809, 1810.

† Articles 641, 642.

‡ "Sur les Couleurs des Lames minces," *Mémoires d'Arcueil*, tom. iii.

plate was of a density intermediate to those of the two substances between which it was contained. I shall hereafter have occasion to refer to the observations and deductions of Professor Airy connected with these phenomena.

When the metallic reflector was slightly tarnished, a second system of rings was visible to the naked eye. The formation of these rings depended on the light irregularly dispersed at the surface of the metal; and they were visible, in whatever manner the eye was placed with respect to the incident light. Their tints were complementary to those of the regular series.

It was soon felt that the phenomena of thin plates were closely connected with some new and fundamental property of light*, and that it was in their application to these phenomena that all theories of light were to be judged. For their explanation, it has been already stated, Newton invented his celebrated doctrine of the "fits of easy reflexion and transmission," a doctrine which will always hold a prominent place in the page of philosophical history. Its application is obvious. The ray is in a fit of easy transmission in its passage through the first surface; this is succeeded by a fit of easy reflexion, and so alternately. On arriving at the second surface, then, the ray will be in a fit of easy transmission or easy reflexion, according as the interval of the surfaces, or the thickness of the plate, is an even or an odd multiple of the length of the fit. Thus the alternate succession of bright and dark rings in homogeneous light, and the arithmetical progression of the thicknesses at which they are exhibited, are satisfactorily explained. To explain the variation in the dimensions of the rings depending on the nature of the light, it is necessary to suppose that the length of the fits varies with the colour,—being greatest in red light, least in violet, and of intermediate magnitude for the rays of intermediate refrangibility. Newton determined the absolute lengths of these fits for the rays of each simple colour, and found that they bore a remarkable numerical relation to the lengths of the chords sounding the octave. These results are even yet referred to as fundamental data in optical inquiries.

To account for the remaining laws Newton was constrained to make new suppositions, and to attribute properties to the fits which seem inconsistent with every physical account which has been given of them. Thus, to explain the dilatation of the rings

* It is unnecessary to refer to the theories of Sir William Herschel or of M. Parrot, in both of which the laws of thin plates have been referred to those of reflexion and refraction; or to that of Mayer, who attempted to reduce them to inflexion. None of these theories have had supporters, and they are all of them inconsistent with obvious facts.

with the increasing *obliquity* of the incident pencil, he assumed that the length of the fits augmented with the incidence, and according to a complicated law. This assumption is at entire variance with the physical theory. If the fits are produced by the vibrations of the ether, which are propagated faster than the rays, and which alternately conspire with and oppose their progressive motion, their lengths should continue the same in the same medium, whatever be the incidence. No attempt, that I am aware of, has been made to reconcile this law with the physical hypothesis of Mr. Melville and M. Biot.

The same may be said of the variation of the dimensions of the rings with the *substance* of the reflecting plate. Newton found that when a drop of water was introduced between the glasses, the rings contracted; and by comparing their diameters in air and in water, he found that the corresponding thicknesses of the plate were as 4 to 3, or in the inverse ratio of the refractive indices. It was necessary to suppose, therefore, that in different media, the lengths of the fits varied in the same proportion; and, since in the Newtonian theory the refractive indices are directly as the velocities of propagation, it followed that as the velocity augmented, the spaces traversed by the ray in the interval of its periodical states, must *diminish*, and in the same ratio.

But the facts observed by M. Arago and Professor Airy seem to overturn altogether this part of the theory of emission. The rings formed by a plate of air, inclosed between a lens of glass and a metallic reflector, vanish altogether when the light is polarized perpendicularly to the plane of incidence, and is incident at the *polarizing angle of glass*. Under these circumstances, no light is reflected from the upper surface of the plate; but as it is abundantly reflected from the lower, the disappearance of the rings proves that the light reflected from the *upper surface* is essential to their production. That the light reflected from the *lower* surface also concurs in their formation, appears from the effects observed by M. Arago, when the metallic plate was tarnished; and we are thus driven to the conclusion that the phenomena arise from the union and mutual influence of the pencils reflected from the two surfaces.

This mode of explaining the colours of thin plates was pointed out by Hooke, in a remarkable passage in his *Micrographia*, some years before the subject was taken up by Newton. In this passage he very clearly describes the manner in which the rings of successive orders depend on the interval of retardation of the second "pulse," or wave, on the first; and therefore on the thickness of the plate. But he does not seem to have had

any distinct idea of the principle of interference itself; and his conception of the mode in which the colours resulted from this "duplicated pulse" is entirely erroneous. Euler was the next who attempted to connect the phenomena of thin plates with the wave-theory of light; but the attempt, like all the physical speculations of this great mathematician, was signally unsuccessful. Euler thought, in fact, that the colours of thin plates, as well as those of natural bodies, arose from *emitted*, and not from reflected light. The incident light was supposed to excite the vibrations of the plate, the frequency of which depended on its thickness, in the same manner as the frequency of the vibrations of the column of air in a tube depends on its length. These vibrations again were believed to excite those of the luminiferous ether, and thus to produce the sensation of various colours, the red corresponding to the less frequent vibrations, and the violet to the most frequent*.

The subject remained in this unsatisfactory state until the principle of interference was discovered by Young. When this principle was combined with the suggestion of Hooke, the whole mystery vanished. The application was made by Young himself, and all the principal laws of the reflected rings were readily and simply explained by the interference of the two portions of light which are reflected at the two surfaces of the plate†. In applying this principle, however, Young perceived that the interval of retardation was not simply that due to the difference of the paths traversed by the two pencils; but that one of them must be supposed to undergo a *change of phase*, amounting to half an undulation, at the instant of reflexion. Young clearly pointed out the accordance of this effect with mechanical principles; and the connexion has been fully confirmed by the more complete investigations of Fresnel. In fact, the two reflexions take place under opposite circumstances, one of the portions being reflected at the surface of a rarer, and the other at that of a denser medium; and the laws of impact of elastic bodies indicate that the direction of the vibratory movement must be reversed by reflexion in the one case, while in the other it is unchanged. Young had the satisfaction of putting this principle to the test in a remarkable manner. It followed from it that if the thin plate were of a refractive density intermediate to those of the two media within which it was inclosed, the laws of the phenomenon would be determined by the difference of the paths alone, the reflexion being of the same kind at the two surfaces.

* *Mém. Acad. Berlin*, 1752.

† "On the Theory of Light and Colours," *Phil. Trans.* 1802.

Young accordingly predicted that in this case the rings should commence from a *white centre*, instead of a black one, and the prediction was soon after verified on trial *.

The transmitted rings are accounted for, in the wave-theory, by the interference of the direct light with that which has undergone two reflexions within the plate; and it follows from the preceding considerations that their colours must be complementary to those of the reflected system. This origin at once shows the reason of the fact observed by M. Arago, that the light of the transmitted rings is polarized *in the plane of reflexion*. M. Biot has laboured to reconcile this fact to the theory of emission, with which it appears, at first view, at utter variance. The account which he has given of the phenomenon will, I think, be hardly deemed satisfactory †.

The theory of thin plates, as it came from the hands of Young, was however incomplete. It is obvious that the intensity of the two portions of light reflected from the upper and under surfaces of the plate can never be the same, the light incident on the second surface being already weakened by partial reflexion at the first. These two portions therefore cannot wholly destroy one another by interference; and the intensity of the light in the dark rings should never entirely vanish, as it appears to do when homogeneous light is employed. M. Poisson was the first to point out and to remedy this defect of the theory. It is evident, in fact, that there must be an infinite number of partial reflexions within the plate, at each of which a portion is transmitted; and that it is the sum of all these portions, and not the two first terms of the series only, which is to be considered in the calculation of the effect. Taking up the problem in this more general form, and employing the formula obtained by himself and Young for the intensity of the light reflected and transmitted at a perpendicular incidence, M. Poisson has proved that—at this incidence, and at points for which the thickness of the plate is an exact multiple of the length of half a wave,—the intensity of the reflected and transmitted lights will be the same as if the plate were suppressed altogether, and the bounding media in absolute contact; so that when these media are of the same refractive power, the reflected light must vanish altogether, and the transmitted light be equal to the incident ‡. Fresnel after-

* "Account of some Cases of the Production of Colours," *Phil. Trans.* 1802.

† See Biot's "*Traité de Physique*," tom. iv. p. 308, *et seq.*

‡ "Sur le Phénomène des Anneaux colorés," *Annales de Chimie*, tom. xxii. p. 337. M. Poisson has further shown that rings absolutely black will be formed at points corresponding to the bright rings in the ordinary case, when the velo-

wards showed that the result was independent of the expression of the intensity of the reflected light; and by the aid of the property discovered by M. Arago, namely,—that the light is reflected in the same proportion at the first and second surfaces of a transparent plate,—he extended the conclusion to all incidences*. The general expression of the intensity of the light in any part of the reflected or transmitted rings has been given by Professor Airy †.

Here, then, we have reached a point with respect to which the two theories are completely opposed. According to both, a certain portion of light is reflected from the first surface of the plate. This in the Newtonian theory is left in all cases to produce its full effect; while in the wave-theory it is, at certain intervals, wholly destroyed by the interference of the other pencil; and the dark rings should be *absolutely black* in homogeneous light. The latter of these conclusions seems to accord with phenomena, while the former is obviously at variance with them. This is clearly shown by an experiment of Fresnel. A prism was laid upon a lens having its lower surface blackened, a portion of the base of the prism being suffered to extend beyond the lens. The light reflected from this portion, according to the Newtonian theory, should not surpass in intensity that of the dark rings. The roughest trial is sufficient to show that the intensity of the light in the two cases is widely different, and to prove that the dark rings cannot arise (as they are supposed to do in the theory of fits,) from the suppression of the second reflexion‡.

Mr. Potter has applied a new method of "photometry by comparison" to determine the relative intensities of the light in the bright and dark rings of the transmitted system. In this method the ratio of the intensities of the light reflected from two plane glasses is varied, by varying the incidence, until it is judged to be equal to the ratio of the light in the bright and dark rings. The former ratio is then deduced from the incidence by means of an empirical formula. In this manner Mr. Potter concludes that the ratio of the light in the rings, at a perpendicular incidence, is 2·48 for green light, and 3·49 for red §. The ratio deduced from the principles of the wave-theory is about 1·20 in

city of propagation within the plate is a mean proportional to the velocities in the bounding media.

* *Annales de Chimie*, tom. xxiii. p. 129.

† *Math. Tracts*, p. 302, &c.

‡ *Mémoire sur la Diffraction*, p. 347.

§ *Lond. & Edin. Phil. Mag.*, 3rd Series, vol. i. p. 174.

the case of crown glass. But, independently of the uncertainty connected with the empirical law which is taken by Mr. Potter as the basis of his computation in these deductions, the photometrical method itself seems to be open to objection. It appears to be assumed, in the application of that method, that where the quantity of light incident upon an *irregularly* reflecting surface is given, the quantity of reflected light will be the same in its entire amount, and in all directions, whatever be the incidence. This seems to be contradicted by obvious facts. There is yet another difficulty in the application of this method which appears to leave room for some uncertainty in the results. Where luminous objects are so small that the eye cannot readily distinguish parts, the absolute quantity and the intensity of the light are confounded. I am not aware how far this may have been the case in Mr. Potter's instrument; but it is remarkable that if we suppose the *quantities* of light reflected from the two glasses to have been taken as the terms of comparison, the calculated results will accord very closely with theory*.

When a beam of light falls upon two plates superposed, some of the many portions into which it is divided by partial reflexion at the bounding surfaces are often in a condition to interfere and exhibit colour. Thus, when light is transmitted through two parallel plates, slightly differing in thickness, the colour produced will be that corresponding to the difference, and will be independent of the interval of the plates. This phenomenon was observed by Mr. Nicholson†, and was shown by Dr. Young to arise from the interference of two pencils, one of which is twice reflected within the first glass, and the other twice reflected in the second. Sir David Brewster observed a similar case of interference produced by two plates of *equal* thickness, slightly inclined, the thickness traversed in the two plates being altered by their inclination. In both these cases, however, the interfering pencils are mixed up with, and overpowered by, the light directly transmitted; and some contrivance is necessary to make the fringes visible. The phenomena are much more obvious in the light reflected by both plates, and which, on account of their inclination, is separated from the direct light. It is obvious, in fact, that the direct image of a luminous object seen through the glasses, will be accompanied by several lateral images, formed by 2, 4, 6, etc. reflexions. These images Sir David Brewster observed to be richly coloured. The bands are parallel to the line of junction of the two glasses, and their breadth is greater

* See *Phil. Mag.* vol. v. p. 441.

† *Nicholson's Journal*, vol. ii. p. 312.

the less the inclination of the plates *. The colours in the first lateral image are produced by the interference of the pencils which have undergone two reflexions,—one of them being reflected internally by the first plate, and externally by the second, while the other is reflected internally by the second, and externally by the first. The routes of these portions differ only by reason of the different inclinations at which they traverse the intervals of the surfaces. M. Pouillet has observed a phenomenon of the same kind, when a thick plate of glass is placed above a metallic mirror, and in a direction nearly parallel to its surface †. The interfering rays in this case appear to be those which have undergone two reflexions within the plate, and one at the surface of the mirror; the reflexion from the mirror preceding the others in the case of one pencil, and following them for the other. The routes of two such pencils will slightly differ, owing to the different obliquity under which they traverse the plate.

The remarkable phenomena observed by Mr. Knox when a double-convex lens was combined with two plane glasses, one adjacent to each surface, have been explained by Young on the same principles. In addition to the rings exhibited by each plate of air, a third system of concentric rings is formed in this case, the dimensions of which are greater than those of either of the primary systems. The diameters of these rings increase indefinitely as those of the primary systems approach to equality; until finally the circles become straight lines when these are equal ‡. It is easily seen, in fact, that each ring is the locus of the points for which the difference of the thicknesses of the two plates of air is constant; and that this locus is a circle, whose diameter will depend on the curvatures of the surfaces, and on the interval of the centres of the two primary systems. The fringes formed by "double plates" have been observed under another form by Mr. Talbot, when two films of thin blown glass were superposed.

The "colours of thick plates" are perhaps of too unusual occurrence to entitle them to be studied as a separate class of optical phenomena: the attention which they have received is owing to the investigations of Newton. In the experiment of Newton a beam of light is admitted through a small aperture, and received on a concavo-convex mirror with parallel surfaces, the second of which is silvered. When a screen of white paper is then held at the centre of the mirror, having a hole in the middle to allow the beam to pass and repass, a set of broad

* *Edin. Trans.*, vol. vii. p. 435.

† *Elemens de Physique*, tom. ii. p. 478.

‡ *Phil. Trans.* 1815, p. 161.

coloured rings will be depicted on it, similar to the transmitted rings of thin plates, the diameters of the rings varying inversely as the square roots of the thicknesses of the mirrors. The Duke de Chaulnes observed that similar phenomena were produced when a metallic mirror was substituted for the glass one, and the rays transmitted through a semi-transparent plate of any kind, or even through a screen of gauze placed at a short distance in front of the mirror *. Sir W. Herschel found that the rings could be produced by scattering fine powder in the air before the mirror†; and M. Pouillet has ascertained that similar rings are formed when the light incident on the mirror is simply transmitted through an aperture of any form in an *opake screen* ‡. More recently Mr. Whewell and M. Quetelet have observed a set of coloured bands, which are formed when the image of a candle is viewed in a *plane* glass mirror; the candle being held at a short distance in front of the eye, so that the incident and reflected rays may make a small angle §. M. Quetelet appears to think, however, that this phenomenon is to be referred to a different class from those last considered.

Newton very ingeniously accounted for the colours observed in his experiments by the fits of easy reflexion and transmission of that portion of light which is scattered in all directions at the first surface of the glass; and M. Biot has extended the explanation to the analogous phenomena observed by the Duke de Chaulnes. Young showed that they could be explained by the interference of the two portions of light which are scattered in the passing and repassing of the ray through the refracting surface ||. The complete investigation, as far as relates to the dimensions of the successive rings, is given by Sir John Herschel; and the formula obtained is found to agree precisely with Newton's measures ¶.

When the interval between two glasses is filled with different substances, such as water and air, or water and oil, in a finely subdivided state, the portions of light which have traversed them are in a condition to interfere, the interval of retardation depending on the difference of the velocities of light in the two media. Accordingly, coloured rings will be seen when a luminous object is viewed through the glasses; the rings being similar to those usually seen by transmission, but much larger.

* *Mém. Acad. Par.* 1755. † *Phil. Trans.* 1807.

‡ *Elemens de Physique*, tom. ii. p. 476.

§ *Correspondance Mathématique*, tom. v. p. 6, et tom. vi. p. 1.

|| "On the Theory of Light and Colours," *Phil. Trans.*; and *Encycl. Brit.*, Art. CHROMATICS.

¶ *Essay on Light*, Art. 679, *et seq.*

But when a dark object is behind the lenses, and the incident light somewhat oblique, the rings immediately change their character, and resemble those of the ordinary reflected system; one of the portions in this case being reflected, and therefore suffering a loss of half an undulation. These phenomena were observed and explained by Young*, and have been denominated by him the "colours of mixed plates." Young also observed some similar phenomena of colour in an unconfined medium. Thus, when the dust of the lycoperdon is mixed with water, the mixture exhibits a green tint by direct light, and a purple tint when the light is indirect; and the colours rise in the series when the difference of the refractive densities is lessened by adding salt to the water. The interval of retardation in this case depends also on the magnitude of the transparent particle†.

In closing the review of this part of the subject, I would observe that any well-imagined theory may be accommodated to phenomena, and seem to explain them, if only we increase the number of its *postulates*, so as still to embrace each new class of phenomena as it arises. In a certain sense, and to a certain extent, such a theory may be said to be true, so far as it is the mere expression of known laws. But it is no longer a *physical theory*, whose very essence it is to connect these laws together, and to demonstrate their dependence on some higher principle: —it is an aggregate of separate principles, whose mutual relations are unknown. Thus the cycles and epicycles of the Ptolemaic system represented with fidelity the more obvious movements of the planetary bodies; but when the refinements of astronomical research laid bare new laws, new epicycles were added to the system, until at length its complication rendered it useless as a guide. Such appears to be the present state of the theory of emission; and so glaringly does this blemish show itself in that part of the theory which has been last under consideration, that one of its advocates says, "Reverà illæ vices reflexionis et transitus, cum omnibus additamentis fictitiis, mirabiliores adhuc sunt quàm phænomenon ipsum, ad cujus explicationem in usum sunt vocatæ‡." The same attribute appears in the broader divisions of the science; and the several classes of phenomena do not flow from the theory as from one common source,—but each has its separate and independent head, and its separate and independent data. In the wave-

* "Account of some Cases of the Production of Colours," *Phil. Trans.* 1802. The Abbé Mazeas noticed many facts which appear to be referable to the same principles,—*Mémoires présentés*, vol. ii.

† *Introduction to Medical Literature*, p. 556.

‡ Mayer on Newton's Rings,—*Göttingen Memoirs*, vol. v. p. 22.

theory, on the other hand, not only the individual laws, but the classes of phenomena are *related*; and to calculate, *numerically*, the laws of refraction, the varied phenomena of diffraction, and those of thin plates, we only need to borrow *one* result from experience,—the length of a wave of light in each medium. There is thus established that connexion and harmony in its parts which is the never-failing attribute of truth. But powerful as is the weight of this intrinsic evidence in favour of the wave-theory, it has yet stronger claims to our assent. These claims are grounded on the vast body of new phenomena which it explains,—and explains, (it is to be remembered,) not in a vague and general manner, but in the precise language of analysis, and with an accuracy which the refinements of modern observation have not been able to impugn. It may be confidently said that it possesses characters which no *false* theory ever possessed before.

Part II.—Polarized Light.

(1) *Polarization.—Transversal Vibrations.*

In the various phenomena which have been hitherto described as taking place when a ray of light encounters the surface of a new medium, it has been assumed that the direction and the intensity of the several portions into which it is subdivided are wholly independent of the manner in which the ray is presented to the bounding surface, the direction of the ray remaining unchanged. In other words, it was taken for granted that a ray of light had *no relation to space*, with the exception of that dependent on its direction;—that around that direction its properties were *on all sides alike*;—and that if the ray were supposed to revolve round that line as an axis, the resulting phenomena would be unaltered.

Huygens was the first to observe that this was not always the case. In the course of his researches on the law of double refraction, he found that when a ray of solar light is received upon a rhomb of Iceland crystal in any but one direction, it is always subdivided into two of equal intensity. But on transmitting these rays through a second rhomb, he was surprised to observe that the two portions into which each of them was subdivided were no longer equally intense;—that their relative brightness depended on the position of the second rhomb with regard to the first;—and that there were two such positions in which one of the rays vanished altogether.

From this "wonderful phenomenon," as Huygens justly called it, it appeared that each of the rays refracted by the first rhomb had acquired properties which distinguished it altogether from solar light. It had, in fact, acquired *sides*; and it was evident that the phenomena of refraction depended, in some unknown manner, on the relation of these sides to certain planes within the crystal. Such was the conclusion of Newton: "This argues," says he, "a virtue or disposition in those sides of the rays, which answers to, and sympathizes with, that virtue or disposition of the crystal, as the poles of two magnets answer to one another."

This conception was followed out by Malus, whose varied and important discoveries respecting the nature and laws of polarized light have justly placed him in the rank of founder in this most interesting branch of science. The molecules of a polarized ray were supposed by him to have all their homologous sides turned in the same directions. He adopted the term "polarization" to express the phenomenon, and compared the effect to that of a magnet which turns the poles of a series of needles all to the same side. M. Biot has modified the hypothesis of Malus in order to embrace the other phenomena of light, and assumed that there was *one line*, or axis, similarly placed in each molecule, and that these axes in a polarized ray were all turned in the same direction. The molecules, however, are at liberty to revolve round these axes, and thus to assume different dispositions with respect to the attracting or repelling forces to which they are exposed when they encounter the surface of a new medium.

The phenomenon of polarization seems to have had much weight with Newton in forcing him to reject the theory proposed by Huygens: "It is difficult," he says, "to conceive how the rays of light, unless they be bodies, can have a permanent virtue in two of their sides, which is not in their other sides, and this without any regard to their position to the space or medium through which they pass *." "Are not all hypotheses erroneous," he adds in another place, "in which light is supposed to consist in pression or motion, propagated through a fluid medium? . . . . Pressions or motions, propagated from a shining body through an uniform medium, must be on all sides alike; whereas by those experiments it appears that the rays of light have different properties in their different sides †." In this objection Newton seems to have fixed his thoughts upon that species of undulatory propagation whose laws he himself had

* *Optics*, book iii. Query 29. † Query 28.

so sagaciously divined. When *sound* is propagated through *air* or *water*, the vibrations of the particles of the fluid are performed in the direction in which the wave advances; and if the vibrations of the ether, which are supposed to constitute light, were of the same kind, the objection would seem to be insuperable. But the case is altered, if, as is now assumed, the vibrations of the ethereal particles be *transverse* to that of the ray's progress. And though we were unable to render any account of this hypothesis, or even to show that it is consistent with mechanical principles, yet the numerous classes of phenomena which it has explained, and the striking and exact manner in which its predictions have been verified on trial, compel us to admit, that if the law to which we have thus reduced so various and such complicated facts be not itself a law of nature, it is at least coordinate with it, in such a sense that we may take it as the representative of actual existence, and reason from it as we would from an established physical law.

The hypothesis of transversal vibrations first occurred to Dr. Thomas Young, who illustrated it by the propagation of undulations along a stretched cord agitated at one of its extremities. Young seems to have been led to this principle while considering the results arrived at by Sir David Brewster, in his researches on the laws of double refraction in biaxal crystals. The principle was soon after raised above the rank of a mere hypothesis, and shown to be a necessary consequence of the laws of interference of polarized light, if the theory of waves be admitted at all. It follows, in fact, from the laws of composition of vibrations, that the intensity of the light resulting from the union of two rays oppositely polarized will be *constant*, and independent of the phase (as was proved to be the case in the experimental researches of MM. Arago and Fresnel,) only when the vibrations normal to the wave are evanescent. It appears from the same investigation that the actual vibrations are either parallel or perpendicular to the plane of polarization. As far as the phenomena of interference are concerned, it is indifferent which of these results be assumed to be the fact. But the theory of transversal vibrations itself, when applied to the laws of double refraction, leads to the conclusion that the vibrations which constitute the ordinary ray in uniaxal crystals are perpendicular to the *principal plane*; and this being its plane of polarization, Fresnel concluded that the vibrations of a polarized ray are on the surface of the wave, and *perpendicular to the plane of polarization* *.

* "Mémoire sur la Double Refraction," *Mém. Inst.*, tom. vii.

The principle of transversal vibrations, thus deduced from the phenomena of interference of polarized light, is easily extended to the case of common or unpolarized light. For when a ray of such light falls perpendicularly upon a double-refracting crystal, it is divided into two polarized pencils, neither of which, it appears from the preceding, can contain vibrations normal to the surface of the wave. If, then, there were any such in the incident ray, they would be destroyed by refraction, and there would ensue a loss of *vis viva*, and consequently a diminution in the intensity of the light; in other words, the sum of the intensities of the two refracted pencils would be less than that of the incident, which is contrary to observation. In unpolarized light therefore, as in polarized, the vibrations are only on the surface of the waves; and we must conceive such light to consist of a rapid succession of systems of waves polarized in every possible plane passing through the normal to the front of the wave. The phenomenon of polarization then, in this theory, consists simply in the resolution of the vibrations into two sets, in two rectangular directions, and the subsequent separation of the two systems of waves thus produced.

The erroneous views of mathematicians on this subject, according to Fresnel, have arisen from the imperfect physical conceptions which they have made the basis of their reasoning. Elastic fluids have been represented as composed of particles in contact, capable only of condensation and dilatation; and accordingly the accelerating forces have been conceived to arise solely from the difference of density of the consecutive shells of the fluid. In this case, it is evident that if any row of particles is displaced in the direction of the connecting line, this row will slide upon the succeeding one, and the motion will be resisted by no elastic force. But when we regard these bodies as they really are, composed of molecules separated by intervals which are probably considerable as compared with their magnitude, and acting on one another according to some law varying with the distance, the whole question is altered. When any row or line of such molecules is similarly displaced, and through a space which is small compared with the separating intervals, the molecules of the succeeding row will be moved in the same direction by the forces which are thus developed with the change of distance; so that the vibrations of the particles composing the first row will be communicated to those of the second, and thus the vibratory motion will be propagated in a direction perpendicular to that in which it takes place*. The rapidity of the

* The existence of transversal vibrations has been fully established in other cases of vibratory motion. M. Savart and Mr. Wheatstone have shown that in

propagation will depend on the magnitude of the force developed by the displacement. To account for the fact that there are no sensible vibrations in a direction normal to the wave, we have only to suppose the repulsive force between the molecules to be very great, or the resistance to compression very considerable; for in this case, it will be seen, the force which resists the approach of two strata of the fluid is much greater than that which opposes their sliding on one another. Fresnel's views on this subject are contained in a short paper, entitled, "Considerations Mécaniques sur la Polarisation de la Lumière *," and in his celebrated memoir on double refraction †.

The principle of transversal vibrations, however, has not been received without much discussion; and even to this hour, the opinion of the mathematical world is not entirely at rest upon the subject. In a memoir on the propagation of motion in elastic fluids, read before the Academy of Sciences in the year 1823, M. Poisson arrived at the conclusion that the vibratory motions of the particles finally become *normal to the wave*, whatever be the direction of the original disturbance ‡. To this Fresnel replied that the equations of motion of elastic fluids employed by M. Poisson are but a mathematical abstraction, which do not apply to anything actually existing. That in fact these fluids are assumed to be composed of *contiguous* elements, capable of compression in a degree proportionate to the pressure exerted; that this hypothesis is untrue; and that although it may accord with the statical properties of these fluids, it can never lead to the discovery of their *dynamical* laws§.

M. Poisson seems to have felt the full force of this objection; for in his memoirs on the same subject, read to the Academy in the years 1828 and 1830, he has resumed the whole theory, and reared it upon its firmer basis. In the former of these memoirs he has formed the differential equations of equilibrium and motion of elastic bodies, these bodies being supposed to consist of molecules attracting or repelling one another according to some function of the distance ‖. In the latter he proceeds to integrate these equations generally, and to deduce the laws of propagation of waves at a considerable distance from the origin of disturbance ¶. In the case of fluids he arrives at the con-

many instances the elementary motions of the molecules of bodies which transmit sound are transverse to the direction of the propagation.

* *Bulletin de la Soc. Philom.* 1824. † *Mémoires de l'Institut*, tom. vii.

‡ *Annales de Chimie*, tom. xxii. § *Ibid.*, tom. xxiii.

‖ "Mémoire sur l'Equilibre et le Mouvement des Corps Elastiques," *Mém. Inst.*, tom. viii.

¶ "Mémoire sur la Propagation du Mouvement dans les Milieux Elastiques," *Mém. Inst.*, tom. x.

clusion which he had before obtained, namely,—that when the distance from the origin of disturbance is very great compared with the length of a wave, the motion of the particles, in any fluid, is normal to the surface of the wave, whatever be the initial motions. He admits, however, that the fundamental equations of the motion of fluids, and therefore also the consequences deduced from them, will probably require modification in the case of very rapid motions, such as those of the luminiferous ether; there being a finite interval of time, whose magnitude depends on the nature of the fluid, during which the pressure is not the same in all directions. In the case of very rapid motions this time must be taken into account, and the equations of motion of fluids will no longer be those furnished by the principle of D'Alembert *.

M. Poisson has shown also that a disturbance produced in a limited portion of a *solid body* will give rise to *two waves*, which will be propagated with different velocities. He proves further that whatever be the initial motions of the disturbed particles, the vibrations in one of these waves will finally be *radial*, or in the direction of the motion propagated; while those of the other are perpendicular to that direction, or *transversal*. The first are attended with dilatations proportionate to the absolute velocities of the molecules, and the waves thus propagated are similar to those which take place in fluids. The transversal vibrations, on the other hand, are unaccompanied by any change of density in the medium. M. Poisson does not seem to think that this result can justify the hypothesis of transversal vibrations in the ethereal fluid; though he admits that the properties attributed to the ether are in some respects analogous to those of a solid body.

The propagation of transversal vibrations appears to be now established as a necessary consequence of dynamical principles by the able researches of M. Cauchy †. I shall shortly have occasion to allude more particularly to the important conclusions arrived at by this mathematician, on applying the general laws of the propagation of motion in elastic media to the case of light. For the present it will be sufficient to observe that the form of the wave-surface, obtained in the course of these investigations, is a curved surface of three sheets; and that consequently a ray of light on entering any medium will be, in general, subdivided into *three rays*, the directions of the vibrations being determined in each. When the elasticity of the ether, in this medium, is the same in all directions, these three

* *Annales de Chimie*, tom. xliv.

† "Mémoire sur la Théorie de la Lumière," *Mém. Inst.*, tom. x.

rays will have a common direction, and two of them a common velocity. They are thus reduced to two, a *single* and a *double* ray, coincident in direction; the vibrations of the former being parallel to that direction, and those of the latter perpendicular to it. If the initial vibrations in the system in question are contained in a plane perpendicular to the direction of the rays, the single ray will vanish, and the vibrations of the molecules of the double ray will be constantly parallel to the direction of the initial displacements. This condition therefore reduces the three rays to *one*, which is *unpolarized*; and as this is known by experience to be the case in media in which the light is propagated in all directions with the same velocity, it follows that the propagation of transversal vibrations is a necessary consequence of the general theory.

Thus the theory of Young and Fresnel has received the strongest possible confirmation; and when we consider the numerous and important conclusions which have been reproduced and confirmed by M. Cauchy in the development of his analysis, it is scarcely possible to believe that there is anything defective in its principle. There is one important and fundamental difference, however, between the theories of M. Cauchy and Fresnel; a difference which seems to mark the limits to which we have attained in this branch of mathematical physics. According to the latter author, it has been already stated, the vibrations are perpendicular to the plane of polarization, as it is usually defined: according to M. Cauchy they are parallel to that plane. I am inclined to think that the field on which this question between the two theories must be decided, is their application to the laws of reflexion of polarized light; and if so, there seems already reason for believing that the hypothesis of Fresnel is the true one.

II. *Reflexion and Refraction of Polarized Light.*

Although the phenomenon discovered by Huygens was one of the highest interest in itself, and in its bearings of such importance, in the mind of Newton, as to force him to admit the existence of properties in the rays of light which until then had never been imagined; yet the result remained for more than one hundred years a *unique fact* in science, and the kindred phenomena,—the properties which light acquires in a greater or less degree in almost every modification which it undergoes,—remained unnoticed until the beginning of the present century. In the year 1808, while Malus was engaged in his experimental researches on the Huygenian law of double refraction, he dis-

covered the important fact, that when a ray of light is reflected from the surface of glass or water at certain angles, the reflected ray acquires all the characters which had been found to belong to one of the pencils produced by double refraction. When received upon a rhomb of Iceland spar, one of the two pencils into which it is generally divided vanished in two positions of the principal section with respect to the plane of reflexion; while in intermediate positions these pencils varied in intensity through every possible gradation*. The same variations were observed when it underwent a second reflexion at the same angle at which the effect was produced by the first; the twice reflected light being a maximum when the plane of the second reflexion coincided with that of the first, and vanishing altogether when it was perpendicular to it,—the whole light in that case passing into the refracted pencil. To represent the intensity of the reflected light, in any position of the plane of the second reflexion with regard to the first, Malus assumed it to vary as the square of the cosine of the angle which these planes formed with one another †. The accuracy of this law has since been verified by the observations of M. Arago and others.

From this law it follows that a beam of common light may be represented as composed of two polarized beams of equal intensity, whose planes of polarization are at right angles; for when such a compound beam is received upon a reflecting surface at the polarizing angle, the intensity of the reflected light will be constant, and independent of the position of the plane of reflexion. But though this compound beam so far exhibits the character of common or unpolarized light, it must not be regarded (as it seems to be by many writers,) as its physical representative. It appears, in fact, from the theory of the composition of vibrations, that two rays of equal intensity polarized at right angles compound a single ray *polarized in a single plane*, when the difference of their phases is nothing or equal to any integer number of semiundulations; while in intermediate cases the polarization of the resulting light is either *circular* or *elliptic*. These indications of theory have been confirmed in the fullest manner by a beautiful experiment of Fresnel.

On pursuing his inquiries Malus found that all other transparent substances impressed upon the reflected light the same modification; and that the angle of incidence at which this effect was produced, and which he called the angle of polarization, was in general different for every different substance. He ascertained, moreover, the relation between the angles of polarization at the first and second surfaces of the same transparent medium,

* *Mémoires d'Arcueil*, tom. ii. p. 143. † *Ibid.*, p. 254.

and found that their sines were in the ratio of the sines of incidence and refraction;—so that when the medium is bounded by parallel surfaces, and the light incident on the first at its polarizing angle, the transmitted portion will meet the second surface also at *its* polarizing angle, and the light reflected from both be wholly polarized *. Malus was unable, however, to discover any connexion between the polarizing angle and the other properties of the substances; and he concluded that the power of polarizing light by reflexion, which different bodies possessed at different angles, was wholly independent of their other modes of action upon light.

Sir David Brewster commenced, not long after, an extensive series of experiments, with the view of determining the angles of polarization of different media, and of connecting them by a law. These researches terminated in the discovery of the law,—perhaps the most beautiful in the whole range of this interesting science,—that "the tangent of the angle of polarization is equal to the refractive index." This law, when translated into geometrical language, declares, that when the ray is wholly polarized by reflexion, the angles of incidence and refraction are complementary; so that the reflected and refracted rays form a right angle. The law applies to the case of reflexion from the surface of the rarer as well as that of the denser medium; and it follows from it that the two angles of polarization at the bounding surface of the same two media are complementary †.

Malus observed that when the angle of incidence was either greater or less than the polarizing angle, the properties already described were only in part developed in the reflected pencil. Neither of the two pencils into which it was divided by a rhomb of Iceland spar ever wholly vanished; but they varied in intensity between certain limits, these limits being closer the more remote the incidence from the angle of complete polarization. From this he naturally concluded that in these circumstances *a portion* only of the reflected pencil had received the modification to which he had given the name of polarization,—that portion increasing as the incidence approached the polarizing angle;—and that the remaining portion was unmodified, or in the state of common light. In this supposition Malus has been followed by most subsequent philosophers. A different view of

* *Mémoires d'Arcueil*, tom. ii. p. 152. M. Arago has extended the same law to the case of partial polarization, and has found that the sines of the angles at which the first and second surfaces of a transparent medium polarize light by reflexion *in an equal degree*, are to one another in the ratio of the sines of incidence and refraction; so that the pencils reflected from the two surfaces of a parallel plate, at any incidence, contain the same proportion of polarized light.

† "On the Laws which regulate the Polarization of Light by Reflexion from transparent Bodies," *Phil. Trans.* 1815.

the phenomenon of *partial polarization* has been taken by Sir David Brewster, to which I shall have occasion presently to allude; and he has employed his theory to explain a phenomenon which he seems to have been the first to observe,—namely, that common light may be polarized by a sufficient number of reflexions at any angle, the number of reflexions required to produce the effect being greater, the more remote the incidence is from the polarizing angle *.

Examining the *transmitted* pencil, Malus found that it was *partially polarized*; and that its plane of polarization was not, like that of the reflected pencil, coincident with the plane of reflexion, but perpendicular to it †. The two portions of light thus polarized in opposite planes he observed to be intimately connected; and in a subsequent memoir he announced the fact that whenever we produce by any contrivance a ray polarized in any plane, there is produced at the same time a second ray polarized in the opposite plane. These two polarized rays follow separate paths, and their quantities are always proportionate. The connexion, however, is still more strict than was supposed by Malus; for the quantities of polarized light in the reflected and transmitted pencils are not only proportionate, but absolutely equal. This remarkable law was discovered by M. Arago.

When a ray, which is partially polarized by transmission through a plate of glass, is received upon a second plate at the same angle, the portion of common light which it contains undergoes a new subdivision; and so continually, whatever be the number of plates. Hence when that number is sufficiently great, the transmitted light will be, as to sense, completely polarized; and the whole light is thus subdivided into two pencils oppositely polarized, one of which is reflected from, and the other transmitted through, the pile. These facts were also observed by Malus. The laws of the phenomena have since been investigated, in much detail, by Sir David Brewster; and he has arrived at the conclusion, that when a ray of light is transmitted successively through any number of parallel plates, the tangent of the angle at which the polarization of the refracted pencil appears complete is inversely as their number ‡.

I may now proceed to consider these phenomena in their relation to the two theories of light.

Newton proved that the fundamental laws of reflexion and refraction could be derived from the operation of attractive and

* *Phil. Trans.* 1815. † *Mém. Inst.* 1810.

‡ "On the Polarization of Light by oblique Transmission," &c., *Phil. Trans.* 1814.

repulsive forces exerted by the molecules of body on those of light. The phenomena of polarization, however, show that these forces are exerted in very different degrees, according to the position of the sides of the ray with respect to the plane of reflexion or refraction; and we are now to consider the additional hypotheses which become necessary in the theory of emission in order to render an account of these new facts.

It has been already mentioned that, in the theory of M. Biot, a polarized ray was one in which certain axes (called the *axes of polarization*) of all the molecules were turned in the same direction. This effect is ascribed to the operation of certain forces emanating from the molecules of the body. These forces M. Biot denominates *polarizing forces*; and he considers them as distinct from the reflecting and refracting forces, although intimately connected with them. The effect of a polarizing force is to give a rotation to the axes of the molecules; and that which impresses the property of polarization upon the reflected ray is assumed to act in the plane of reflexion. This being supposed, since a ray of common light is polarized by reflexion when incident at a certain angle, we are obliged to admit that, *at this angle*, the polarizing force turns the axes of polarization of *all the molecules*, and brings them into the plane of reflexion; and, since this takes place for all the molecules of the reflected ray, such an arrangement of the axes is conceived to be a necessary condition of reflexion at that incidence.

Now let such a polarized ray fall upon a second reflecting surface at the polarizing angle, and let the plane of the second reflexion be perpendicular to that of the first. Then the axes of polarization of the molecules, in their incidence on the second plate, are perpendicular to the plane of reflexion; consequently the polarizing force acting in that plane affects equally the two halves of the axis, and cannot therefore turn it into the plane of reflexion,—a condition which is assumed to be necessary to reflexion at that angle. No light therefore is reflected. But when the plane of the second reflexion is inclined to that of the first at any angle less than 90°, the polarizing force of the second plate no longer acts symmetrically on the two halves of the axes of the molecules: it may therefore turn these axes so as to make them coincide with the plane of reflexion, and thus subject the molecules to the action of the reflecting force. The effect of the polarizing force increases as the inclination of the two planes of reflexion diminishes; and consequently the number of molecules reflected by the second plate increases likewise.

But here it is necessary to make another supposition. In any position of the plane of the second reflexion with respect to

the first, except the perpendicular one, experience proves that a portion of the light is reflected and another portion refracted. According to this theory, then, some of the molecules obey the polarizing force and have their axes brought into the plane of reflexion, while others do not. To account for this diversity of effect there must be some diversity of condition in the molecules themselves. The theory of M. Biot supplies this by attributing to them an oscillatory movement round their axes of polarization, the molecules yielding to the polarizing force or not, according to the phase of the oscillation in which they are found at the moment they reach the surface.

The force which impresses the property of polarization upon the *refracted* pencil is supposed by M. Biot to act also in the plane of incidence, its operation however being to turn the axes of polarization of the luminous molecules in a direction *perpendicular* to that plane. Thus, when a ray of light traverses the surface of a plate of glass at the polarizing angle, it is subjected to the action of two forces, one tending to bring the axes of polarization of the molecules into the plane of incidence, the other to turn them at right angles to it; and the molecules themselves yield to one or other of these forces according to the phases of their fits. For the manner in which this may be supposed to take place we must refer to the *Traité de Physique**. The whole quantities of light oppositely polarized by the two forces, M. Biot supposes to be equal; but he conceives that the force which polarizes the reflected pencil is exerted on a much greater number of molecules than those which actually undergo reflexion. These molecules, thus polarized in the plane of incidence, enter into the transmitted beam,—neutralize an equal number of molecules polarized by refraction in the opposite plane,—and compound with them a beam of common light. The whole quantities of light polarized by the two forces being then equal, the remaining portions *effectively* polarized will still be equal, conformably to the law discovered by M. Arago.

I have endeavoured to present the theory of M. Biot as fully as the limits of the present paper will permit, because it appears to me that the *number* and the *nature* of the hypotheses required, in order to render any account of the phenomena of polarization in the theory of emission, furnish in themselves a sufficient argument against it. But let all these be admitted, and how far can we be said to have advanced towards an explanation of the phenomena? The assumed forces and the known laws have not been connected, in any one instance, by the

* Book vi. chap. i. vol. iv.

sure processes of mathematical deduction; and we are therefore unable to state how far the explanation offered is competent to express even the general facts,—far less can we calculate them numerically, and compare the results with those of observation.

The first attempt to connect the modifications of reflected light with the theory of waves, was made by Dr. Thomas Young. This sagacious philosopher succeeded in solving the problem of reflexion in the case of perpendicular incidence, and showed that the intensity of the reflected light in that case was represented by a simple function of the refractive index *. This formula was afterwards reproduced as the result of a more refined analysis by M. Poisson, in a memoir on the simultaneous motions of two elastic fluids in contact, read to the French Academy in 1817 †. In that memoir, however, the author had considered only the case of perpendicular incidence; or the law of propagation of a plane wave parallel to the bounding surface of the two media. In a subsequent memoir, to which I have already alluded, and which was read to the Academy in the year 1823 ‡, he has resumed the problem generally, and examined the modifications produced in the intensity as well as the direction of a wave, or series of waves, in passing from one fluid to another of the same elasticity but of a different density. The expressions obtained for the intensity of the reflected and refracted waves, are functions of the angle of incidence and of the ratio of the velocities of propagation in the two media. When the wave is incident upon the surface of the denser medium, the expression for the intensity of the reflected wave *vanishes* at a certain angle, whose tangent is equal to the ratio of the velocities of propagation. At this angle, which is the angle of complete polarization, objects should therefore cease to be visible by reflected light;—a result which is contradicted by all experience, and is only true when the light is polarized in a plane perpendicular to the plane of reflexion. When the wave is reflected at the surface of the rarer medium, there are two expressions for the intensity, for incidences above and below the limiting angle of total reflexion respectively; there are also in this case two angles of evanescence. These conclusions, which apply to the case of sound as well as light, are sufficient to show the physical inapplicability of the theory.

* *Encyc. Brit., Supp.*, Art. CHROMATICS.

† *Mém. Inst.*, tom. ii.

‡ Only a portion of this memoir has been printed in the Memoirs of the Institute, under the title "Mémoire sur le Mouvement de deux Fluides elastiques superposés," tom. x.

The theory of waves, however, when combined with the principle of transversal vibrations, has afforded the complete solution of the problem we have been considering. In this development of his theory the character of Fresnel's genius is strongly marked. Our imperfect knowledge of the precise physical conditions of the question is supplied by bold, but highly probable assumptions: the meaning of analysis is, as it were, intuitively discerned, where its language has failed to guide; and the conclusions thus sagaciously reached are finally confirmed by experiments chosen in such a manner as to force Nature to bear testimony to the truth or falsehood of the theory *.

It is evident that the strata of ether in the two media, which are adjacent to the bounding surface, must undergo equal displacements parallel to that surface, in as much as one of them cannot slide on the other. Consequently the amplitude of the vibration, resolved in a direction parallel to the surface, must be the same in the two media. Fresnel assumes that this equality at the bounding surface is maintained at all distances; and this furnishes him with one relation among the amplitudes of vibration of the incident, reflected, and refracted waves. A second relation among the same quantities is afforded by the law of the *vis viva*; but to apply this it is necessary to know the relative densities of the ether in the two media. Here Fresnel assumes that the elasticity of the ether in these media is the same†, but the density different; and this being taken for granted, it follows that the two densities are to one another inversely as the squares of the velocities of propagation, and that therefore their ratio is given when the refractive index is known. The amplitudes of the reflected and refracted vibrations, and therefore also the intensities of the light in the two pencils, are obtained by simple elimination between the equations just mentioned.

The expressions for the intensity of the light in the reflected ray are different, according as the incident light is polarized in

* Fresnel's theory of reflexion is contained in a memoir read to the Academy of Sciences in the year 1823, entitled, "Mémoire sur la Loi des Modifications que la Reflexion imprime à la Lumière polarisée." An incomplete extract of this memoir was published in the *Annales de Chimie*, 1825. The original paper was mislaid, and for a time supposed to be lost; it has lately, however, been recovered among the papers of M. Fourier, and has been printed in the 11th vol. of the Memoirs of the Institute.

† Fresnel states that he had solved the problem of reflexion in the general supposition that the two media differ in elasticity as well as density,—in the case of rays polarized in the plane of reflexion; and that the resulting formula was the same as that to which he had already arrived on the more limited hypothesis. *An. Chim.*, tom. xxiii.

the plane of reflexion or in the perpendicular plane*. The intensity of the reflected light in the latter case vanishes when the sum of the angles of incidence and refraction is a right angle; and thus was solved the difficulty, which,—in the opinion of Young, pronounced but three years before,—"would probably long remain, to mortify the vanity of an ambitious philosophy, completely unresolved by any theory." When common, or unpolarized light, therefore, is incident at an angle whose tangent is equal to the refractive index, the reflected light will be wholly polarized in the plane of reflexion; and the beautiful law of Brewster is among the first fruits of the theory of Fresnel. The remarkable law obtained by M. Arago is also a necessary consequence of the same formulæ; and it is easily inferred that the quantities of polarized light in the reflected and refracted pencils are equal, whatever be the incidence.

In the case of perpendicular incidence, these formulæ are both reduced to the simple expression obtained by Young and Poisson; and when the incidence is 90°, or the ray grazes the surface, the intensity of the reflected light is equal to that of the incident, or the whole of the light is reflected, whatever be the reflecting medium. The latter conclusion has been verified by the observation of the bands produced by the interference of direct light with that which is reflected at an incidence of nearly 90°. The first dark band appears to be *perfectly black*; and therefore the two lights are, as to sense, of equal intensity †.

We are thus furnished with the solution of a problem which has long baffled the labours of experimentalists,—namely, the determination of the law of intensity of reflected light as dependent on the incidence. The formulæ obtained have not been compared with experiment by Fresnel except in the case of two observations of M. Arago; the observations of Bouguer and Lambert being confessedly inaccurate. The result of the comparison alluded to has been given in the *Annales de Chimie* ‡, and the agreement is as satisfactory as can be expected in observations of the kind.

Mr. Potter has recently examined the intensity of the light reflected from *diamond* and *glass of antimony*, at various incidences §. The photometrical method employed in these observations consisted in comparing the light reflected at any incidence from the substance examined with that reflected from a piece of

* These two formulæ were first published in the *Annales de Chimie*, 1821; the second without demonstration.

† "On a New Case of Interference," *Trans. Royal Irish Academy*, vol. xvii.

‡ tom. xvii. p. 190.

§ *Phil. Mag.*, Third Series, vol. i. p. 179; vol. iv. p. 6.

crown-glass, and then varying the incidence on the latter until the intensities are observed to be equal. The intensity of the light reflected from crown-glass at various incidences had been already obtained from a detailed series of experiments; and the results were embodied in an empirical law, in which the intensity is represented by the ordinate of a rectangular hyperbola, the corresponding abscissa being the sine of incidence. This formula then gives the intensity of the light reflected from crown-glass, and therefore also from the substance examined, at the corresponding incidences. Mr. Potter concludes in this manner, that the intensity of the light reflected from diamond at a perpendicular incidence is 9·3, and that from glass of antimony 8·2; the intensity of the incident light being represented by 100. The intensities calculated from the refractive indices, by the formulæ of Young, Poisson, and Fresnel, are 18·36, and 13·33, respectively. This variance in the results of theory and experiment is undoubtedly beyond the limits of the errors of observation; and, were it otherwise, the partial results obtained by Mr. Potter, in these and other experiments of the same nature, agree too closely to permit us to refer the discrepancy to such a source. The principle of the method however, appears, to say the least, uncertain; and it cannot but be wished that some of the various photometrical methods recently proposed should be applied to the examination of this interesting question.

The formulæ of Fresnel supply the account of the remarkable phenomenon observed by M. Arago;—namely, that when Newton's rings are formed between a lens of glass and a metallic reflector, one of the two images into which they are divided by a double-refracting crystal whose principal section is parallel or perpendicular to the plane of reflexion, changes its character as the incidence passes the polarizing angle of the glass; the colours being the same as in the other image when the incidence is less than the polarizing angle, but *complementary* to them when it is greater. In fact, when the incident light * is polarized perpendicularly to the plane of reflexion, the amplitude of the reflected vibration (which vanishes at the angle whose tangent is equal to the refractive index,) *changes sign* in passing through zero; being negative when the incidence is less than that angle, and positive when it is greater. Consequently, if the wave reflected from the glass, at the central spot, is in complete discordance with that reflected from the metal in the former case, it will be in complete accordance with it in the latter; and the centre, which before was black, will then be

* The effect is the same whether the light be polarized before or after reflexion.

white. For the same reason the whole system will be complementary to that which it was before. Professor Airy was led to anticipate this result from the consideration of Fresnel's expressions, and afterwards verified it on trial*,—apparently without any knowledge of the facts observed by M. Arago. A similar confirmation of the same principles may be obtained by combining, in Fresnel's experiment, a metallic reflector with one of glass. The light being polarized perpendicularly to the plane of reflexion, the central band will be *white* when the angle of incidence is below the polarizing angle of the glass; at the polarizing angle the interference bars will vanish altogether; and beyond that incidence they will reappear with a *dark* centre, instead of a white one. This method of observation would seem to be peculiarly adapted to the investigation of the change of phase produced by metallic reflexion at various incidences.

By the same considerations Professor Airy was led to expect that when Newton's rings were formed between two transparent substances of different refractive powers,—the light being polarized perpendicularly to the plane of incidence,—the rings should be *black-centred*, when the incidence was less than the polarizing angle of the low-refracting substance, or greater than that of the high-refracting substance; while they should appear with a *white* centre, when it was intermediate to these angles;—the vibrations of the waves reflected from the two surfaces being of opposite signs in the former case, and of the same sign in the latter. All these expectations were fully confirmed by experiment†. The substances selected by Professor Airy for these observations were plate-glass and diamond,—these substances differing very widely in their refractive powers; and in the course of his experiments he has noticed certain peculiarities in the phenomena, from which he has drawn some highly interesting conclusions respecting the nature of reflexion from diamond. Had this been subjected to the ordinary laws, the reflexion should cease, and the rings disappear, at the polarizing angle of both substances. This however was not the case. The rings did not vanish at the polarizing angle of the diamond; but the *first black ring contracted*, as the incidence was gradually increased, and finally usurped the place of the central white spot. A portion of the light is therefore still reflected at the maximum polarizing angle of diamond; and it is evident from the phenomenon that the transition from a white to a black centre is owing to a *gradual change of phase* of the reflected vibration,

* "On a Remarkable Modification of Newton's Rings," *Cambridge Trans.* 1832.

† "On the Phenomena of Newton's Rings, when formed between two transparent substances of different refractive powers," *Cambridge Trans.* 1832.

amounting to nearly 180°, while the coefficient of the vibration itself is not much altered. The diamond therefore has no angle of complete polarization; and Professor Airy concludes that the nature of the reflexion from this singular substance, in the neighbourhood of the angle of maximum polarization, is different from any that has been hitherto described.

Fresnel's theory of reflexion has received experimental confirmation of a different kind, and to an extent which leaves little ground to doubt of its truth. When a ray polarized in any plane falls upon a reflecting surface at any angle, the reflected ray is still polarized, but its plane of polarization is changed,—the amount of the change depending on the incidence. The law of this change is at once furnished by the theory of Fresnel; for the tangent of the inclination of the plane of polarization of the reflected ray to the plane of incidence, is equal to the ratio of the displacements in the plane of incidence and in the perpendicular plane. The formula thus deduced has been verified in the most complete manner by the observations of Fresnel himself, and more fully since by those of M. Arago and Sir David Brewster*.

The views of the latter philosopher respecting the nature of partially polarized light are founded upon the phenomenon of the change of the plane of polarization by reflexion. If common light be conceived to consist of two pencils oppositely polarized, in planes inclined 45° on either side of the plane of reflexion, the effect of reflexion, it is obvious, will be to bring each of these planes nearer to the plane of incidence; so that the planes of polarization of the two pencils will approach each other, and form an acute angle after reflexion. *Partially polarized light*, then, according to Sir D. Brewster, consists of two polarized pencils, whose planes of polarization form an *acute angle*; and no portion of it is in the condition of ordinary light†. This hypothesis receives some support from the explanation which it affords of the effects of *successive reflexions*. When light thus constituted is received upon a second reflecting surface in the same plane of incidence, the planes of polarization of the two pencils will be brought nearer, and so continually; until by a sufficient number of reflexions, these planes will, as to sense, coincide with the plane of incidence, and the resulting light will appear to be wholly polarized in that plane.

* *Annales de Chimie*, tom. xvii.; *Phil. Trans.* 1830.

† Sir David Brewster has computed, on these principles, the quantity of light apparently polarized in the plane of incidence, by a single reflexion at any angle; adopting Fresnel's expression for the intensity of the reflected ray. The agreement of the formula with the observations of M. Arago is found to be as near as can be expected in such comparisons. "On the Law of Partial Polarization of Light by Reflexion," *Phil. Trans.* 1830.

This ingenious theory seems open to an objection already noticed,—namely, that the light resulting from the union of two oppositely polarized pencils cannot, in all respects, be taken as the physical representative of common or unpolarized light. It also involves this further difficulty, that the positions of the planes of polarization of the two oppositely polarized portions are entirely arbitrary; and that if they be differently assumed, the results will be physically different. Thus, for example, if the two planes be taken, one coincident with the plane of reflexion itself, and the other with the perpendicular plane, neither of these planes will be changed by reflexion, although the intensities of the corresponding pencils will.

Sir David Brewster has also investigated experimentally the effect of refraction upon the plane of polarization of the refracted ray; and he has found that the law of the change may be expressed by a very simple and elegant formula *. This formula is a necessary consequence of Fresnel's theory, although he does not seem himself to have observed it. Its discovery by Sir David Brewster adds one to the many instances of rare sagacity by which this philosopher is guided in his experimental inquiries. The partial polarization of light by refraction has been considered by Sir David Brewster in the same memoir. In the investigation of the quantity of polarized light in the refracted pencil, he employs a principle similar to that which he had already applied to the reflected ray; and he arrives at the result that the quantities of polarized light in the reflected and refracted pencils are precisely equal, whatever be the incidence, conformably to the law of M. Arago. The effects produced by successive refractions are accounted for on the same principles.

Sir David Brewster seems to have been the first who studied the effects produced by *total reflexion* upon polarized light, and he observed in particular the complementary colours which the light thus reflected furnished when analysed with a rhomb of Iceland spar †. At this time both he and Dr. Young concurred in thinking that these phenomena arose from the interference of two portions of light which were reflected at unequal depths; one portion, according to Dr. Young, beginning to be refracted, and being then turned back by the continued exercise of the same power ‡.

* a and a' being the azimuths of the planes of polarization of the incident and refracted rays, estimated from the plane of reflexion, and i and i' the angles of incidence and refraction,

$$\cot a' = \cot a \cos (i - i').$$

"On the Laws of the Polarization of Light by Refraction," *Phil. Trans.* 1830.

† *Journ. Royal Inst.*, vol. iii.

‡ *Suppl. Encyc. Brit.*, Art. CHROMATICS.

Fresnel had likewise observed, at an early period of his inquiries, that when a ray polarized in a plane inclined at an angle of 45° to the plane of incidence undergoes total reflexion, it is in part *depolarized*; and that this depolarization is rendered complete by two total reflexions at an incidence of about 50°. The reflected light being then *circularly* polarized, is, according to theory, composed of two equal pencils, one polarized in the plane of incidence, and the other in the perpendicular plane, and differing in their origin by a quarter of a wave. From this it followed that the two pencils into which the incident light may be resolved, polarized in these two planes, are not reflected at the same depth; or that they have undergone unequal changes of *phase* at the moment of reflexion, so that after reflexion one of them is in advance of the other. After many ineffectual attempts to discover in what manner this difference of phase depended on the incidence, Fresnel was at length conducted to the solution of the problem by the discussion of the formulæ for the intensity of the reflected light already noticed.

When the angle of incidence exceeds the angle of total reflexion,—the light passing from the denser into the rarer medium,—these formulæ become imaginary. It is evident, however, from the law of the *vis viva*, that the intensity of the reflected light in this case is simply equal to that of the incident. How, then, are the imaginary expressions to be interpreted? They signify, according to Fresnel, that the periods of vibration of the incident and reflected waves, which had been assumed to coincide at the reflecting surface, no longer coincide there when the reflexion is total; or in other words, that the ray undergoes a *change of phase* at the moment of reflexion. The amount of this change is deduced, by a train of the most ingenious reasoning, from the general expressions. Now when a ray, polarized in any azimuth, is incident upon the reflecting surface at an angle greater than the angle of total reflexion, it may be resolved into two: one polarized in the plane of incidence, and the other in the perpendicular plane. The *intensities* of these two portions will not be altered by reflexion; but their *phases* will, and each by a different amount. The reflected vibration, therefore, will be the resultant of two rectangular vibrations differing in phase. This vibration, consequently, will be *elliptic*, and the reflected light will be *elliptically-polarized*. When the azimuth of the plane of polarization of the incident ray is 45°, the intensities of the resolved portions are equal; and if, moreover, their difference of phase, after reflexion, is equal to a quarter of an undulation, the ellipse will become a circle, and the light will be *circularly-polarized*.

Reducing his formulæ to numbers, in the case of St. Gobain glass, Fresnel found that the difference of phase of the two portions of the reflected light amounted exactly to one eighth of an undulation, when the angle of incidence was 54° 37′. Polishing, therefore, a parallelopiped of this glass, whose faces of incidence and emergence were inclined to the other sides at these angles, it followed that a ray incident perpendicularly on one of these faces, and once reflected at each of the sides, would emerge perpendicularly at the opposite face,—the difference of phase in the two portions of the twice-reflected ray amounting to a quarter of an undulation. If, then, the incident ray be polarized in a plane inclined at an angle of 45° to the plane of reflexion, the emergent light will be *circularly-polarized*. This was found to be the case on trial; and the parallelopiped thus constructed, and which is known under the name of *Fresnel's rhomb*, is of essential service in experiments on circular and elliptic polarization. The results of this remarkable theory have been confirmed by Fresnel by other well-chosen experiments; so that although the reasoning on which it is based is far from rigorous, there can remain little doubt of its general truth. Fresnel was himself fully aware of the incompleteness of his solution, considered in an analytical point of view. In his memoir he has adverted to the method to be adopted in order to obtain an exact solution of the problem, unlimited by any arbitrary hypothesis; and he proposed himself to resume the question. But his brilliant career of discovery was cut short by an untimely death.

The problem of the reflexion and refraction of polarized light has also engaged the attention of M. Cauchy*. The solution given by this mathematician is derived from a consideration of the conditions which must be fulfilled at the separating surface of the two media; and it assumes that the density of the ether is the same in both. The expressions obtained for the amplitudes of the vibrations in the reflected wave agree with those of Fresnel. The corresponding quantities for the refracted wave differ from those deduced from Fresnel's theory, by the simple inversion of the ratio of the sines of incidence and refraction, which occurs as a factor in both cases; and, thus, though the formulæ are different, their consequences agree in many instances,—as, for example, in the determination of the plane of polarization of the refracted pencil. It is important to observe, however, that according to the formulæ of M. Cauchy, the velocities of the ethereal molecules in the refracted wave are greater than in the incident; so that the law of the *vis viva* is

* *Bulletin Universel*, tom. xiv. p. 6.

violated. This is not the case in Fresnel's results, which are in fact derived from that law.

The phenomena of *metallic reflexion* remain yet to be noticed in connexion with this division of the science of light.

The effects produced upon light by reflexion at the surfaces of metals did not escape the scrutiny of Malus. From his first experiments upon the subject, Malus concluded that metals had no effect in polarizing the light. He soon, however, modified this opinion, and found that the phenomenon of polarization was partially produced, the effect increasing to a maximum as the incidence approached a certain angle. But the most instructive mode of studying these phenomena, is to let fall upon the metallic reflector a ray polarized in a plane inclined at an angle of 45° to the plane of reflexion, and to analyse the reflected pencil by a double refracting prism. Proceeding in this manner Malus found that when the incidence was very small or very great, the reflected ray was still polarized; while at moderate incidences it was *depolarized*, and the pencil was divided into two in every position of the rhomb. From these facts Malus concluded that the difference between metals and transparent bodies consisted in this, that the latter reflect all the light which is polarized in one plane, and refract all the light polarized in the opposite plane; while metals on the other hand reflect light which is polarized in both planes.

The subject of metallic polarization was next examined by Sir David Brewster; and his labours on this subject constitute the most important addition which has been recently made to our knowledge of the laws of polarized light*. When light reflected at a metallic surface is analysed by a double-refracting crystal, it is observed to be partially polarized in the plane of reflexion. The effect is greatest in galena, and least in silver; and the angle at which it is a maximum is about 74°, but varies with the metal. By successive reflexions in the same plane Sir David Brewster found that the proportion of polarized light was increased; and that by a sufficient number of reflexions the light became, as to sense, wholly polarized in the plane of incidence. The number of reflexions required to produce this effect varied widely in the different metals.

In order to determine the nature and laws of this phenomenon, it is necessary to examine the effect produced upon polarized light. Adopting, then, the method of Malus, Sir David Brewster found that when a ray of light polarized in the azimuth of 45°

* "On the Phenomena and Laws of Elliptic Polarization, as exhibited in the Action of Metals upon Light," *Phil. Trans.* 1830.

was received upon a metallic reflector at an incidence greater than 40° and less than 86°, the reflected light was partly depolarized. The effect produced was greatest at an angle of about 74°; and when the light underwent a second reflexion in the same plane and at the same angle, it was restored to light polarized in a single plane. This new plane lies always on the other side of the plane of reflexion; and its azimuth varies within the limits 0° and 45°, being greatest for *silver* and least for *galena*. It is evident, then, that the light produced by a single reflexion cannot be common light. Neither is it plane-polarized light, because it does not vanish in any position of the analysing rhomb. Sir David Brewster concludes, that this light has received a species of polarization hitherto unrecognised, intermediate between plane and circular polarization. He calls it *elliptic polarization*, because the angles of reflexion at which this light is restored to plane-polarized light, in any azimuth of the plane of the second reflexion with regard to the first, may be represented by the variable radii of an ellipse; while these angles are equal in all azimuths in the case of light circularly-polarized.

Sir David Brewster seems to have been led to employ the term "elliptic polarization" in this manner, in his desire to avoid as much as possible all reference to theory. The laws which he has obtained, however, belong to elliptically-polarized light, in the sense in which the term was introduced by Fresnel. It appears, in fact, from the theory of the composition of vibrations, as laid down by this author, that the vibration resulting from the union of two rectilinear and rectangular vibrations, will be in general *elliptic*; so that two oppositely polarized pencils compound in general a pencil elliptically-polarized,—the ellipse becoming a right line, when the difference of phase of the two portions is an integer multiple of 180°. When, therefore, by the effect of reflexion, two such pencils are made to differ 90° *in phase* (as Sir David Brewster has shown to be the case when a ray polarized in the azimuth 45° is incident at the maximum polarizing angle of the metal), a second reflexion in the same plane, and at the same angle, will raise the difference to 180°, and the resulting light will be *plane polarized*. In other parts of his memoir, however, Sir David Brewster seems to acknowledge that theory; for he speaks of elliptic polarization as produced by the interference of two unequal portions of oppositely polarized light, and even calculates their difference of phase for any incidence.

The identity of the light produced by metallic reflexion, with the elliptically-polarized light of the wave-theory, seems to be placed beyond all doubt by an observation of Professor Airy.

When Newton's rings are formed between glass and metal,—the incident light being polarized, and the angle of incidence exceeding the polarizing angle of the glass,—it is found that the rings *dilate,* as the azimuth of the plane of polarization with respect to the plane of reflexion is increased; the dilatation being a maximum when these two planes become perpendicular. In order to account for this fact, Professor Airy has shown, that if the vibrations of the incident pencil be resolved into two, one in the plane of incidence, and the other in the perpendicular plane, it is necessary to assume that their *phases* are *unequally changed by reflexion*; the phases of the vibrations in the plane of reflexion being *more retarded* than in the perpendicular plane. The two oppositely polarized portions, therefore, will differ in phase after reflexion, and will therefore compound a pencil *elliptically-polarized.* Professor Airy has observed a similar phenomenon when Newton's rings were formed between diamond and plate-glass, the angle of incidence being a few degrees less than the maximum polarizing angle of diamond; and he concludes that, *for such incidences,* the nature of reflexion from diamond is analogous to metallic reflexion.

Sir David Brewster has extended his researches on the subject of metallic reflexion to a great variety of cases, and has traced the effects of *successive* reflexions in the same, or in different planes; and at the same, or different angles. When the light which has been restored to plane-polarized light, by two reflexions in the same plane and at the maximum polarizing angle, undergoes a third reflexion under the same circumstances, it becomes again elliptically-polarized. By a fourth reflexion it is again restored to plane-polarized light, the plane of polarization being, however, brought nearer to the plane of reflexion. This continued approach of the plane of polarization to the plane of reflexion, enables the author to explain, according to his peculiar views, the effect of successive reflexions upon common light.

It remains, further, to extend the theory of Fresnel to reflexion at the surface of a medium in which the elasticity of the ether is different in different directions. All that we know on this interesting subject we owe to the unwearied zeal of Sir David Brewster. It had been supposed by Malus, and the opinion seems to have passed current with succeeding philosophers, that the exterior surfaces of crystallized substances acted upon the reflected light exactly in the same manner as the surfaces of ordinary media; or, in the language of the theory of emission, that the *reflecting forces* extended beyond the limits of the *polarizing forces* of the crystal. Sir David Brewster was led to doubt this opinion; and in the year 1819 he undertook an ex-

tensive series of experiments on the subject of crystalline reflexion. One of the first results at which he arrived was, that the angle of complete polarization on the *same surface*, varies with the inclination of the plane of reflexion to the principal section of the crystal; being least when the plane of reflexion coincides with the principal section, and greatest when it is perpendicular to it;—and that with *different surfaces* the variation depended on the inclination of the surface to the axis of the crystal. The difference of the greatest and least angles in the case of Iceland spar, and on one of the cleavage planes of the crystal, was found to amount to more than 2°.

But the effects produced upon the plane of polarization are still more remarkable. On weakening the reflecting force, by causing the reflexion to take place at the surface of contact of the crystal and some fluid, such as oil of cassia, which had nearly the same refractive power, Sir David Brewster found that the ray was no longer polarized in the plane of reflexion; and that the deviation of the plane of polarization from the plane of reflexion depended on the angle which the incident ray formed with the axis of the crystal. This relation Sir David Brewster found to be expressed by the law,—that the sine of half the deviation varied as the square-root of the sine of the inclination of the incident ray to the axis*.

It is much to be desired that the attention of analysts should be directed to the problem of reflexion at the surface of extraordinary media. It is one of the very few important provinces of the science of light, which has not yet yielded its tribute to the wave-theory; and we can hardly conceive a finer subject for the exercise of mathematical and physical skill †.

* "On the action of Crystallized Surfaces upon Light," *Phil. Trans.* 1819.

† Since the preceding was written, Mr. M'Cullagh has arrived at an expression for the angle of polarization at the surface of crystallized media, in the case in which the plane of reflexion coincides with one of the principal sections of Fresnel's ellipsoid; and he has found that the law, which he has extended by analogy to all cases, represents with much exactness the observations of Sir David Brewster. If a and b denote the semiaxes of the elliptic section formed by the intersection of the plane of reflexion with the *ellipsoid of indices*, (or the ellipsoid whose axes coincide in direction with the axes of elasticity of the medium, and are equal to its three principal indices,) and r the radius of the same section coinciding with the face of the crystal; the angle of polarization, ϖ, will be the same at whichsoever side of the perpendicular the ray is incident, its value being given by the formula,

$$\operatorname{Sin}^2 \varpi = \frac{1 - \frac{1}{r^2}}{1 - \frac{1}{a^2 b^2}}$$

III. *Double Refraction.*

The phenomenon of double refraction was first discovered by Erasmus Bartholinus, in Iceland spar. After a long series of observations, he found that one of the rays within the crystal observed the known law of refraction discovered by Snellius, while the other was bent according to a new and extraordinary law. An account of these experiments was published at Copenhagen in the year 1669, under the title "*Experimenta Crystalli Islandici Disdiaclastici, quibus mira et insolita refractio detegitur.*"

The success of Huygens in deriving the laws of ordinary refraction from the hypothesis of waves, naturally led him to examine whether these new phenomena could be reconciled to the same theory; and in his desire to assimilate the two classes of phenomena, he was happily led to assign the true law of *extraordinary refraction.* Huygens had already shown that the direction of the refracted ray, in glass and other uncrystallized substances, could be deduced from the supposition, that the ethereal wave within the substance was a *sphere*; or, in other words, that the velocity of undulatory propagation was the same in all directions. One of the rays in Iceland crystal, too, was found to obey the same law; and judging that the law which governed the other, though not so simple, was yet next in simplicity, he assumed the form of its wave to be the *spheroid of revolution*, the greater and lesser axis of the generating ellipse being in the ratio of the greatest and least index of refraction. The form of the wave being known, the law of refraction is derived from the principle of the superposition of small motions. Conceive three surfaces having their common centre at the point of incidence, and representing respectively the simultaneous positions of three waves diverging from that point,—the first in air, the other two within the crystal. Let the incident ray be produced to meet the *air wave*, and at the point of intersection let a tangent plane be drawn: through the line of intersection of this plane with the refracting surface, let planes be drawn touching the two refracted waves;—the lines connecting the centre with the points of contact are the directions of the two refracted rays. This beautiful construction, and the other speculations of Huygens on the subject of extraordinary refraction, are contained in the fifth chapter of his *Traité de la Lumière.*

Huygens was unable to reconcile the existence of a double wave within the crystal with the supposition of a single vibrating medium; and he was accordingly forced to assume the existence of two such media, the spherical wave being propagated by the

vibrations of the ether alone, while the spheroidal wave arose from the vibrations of the crystal and the ether jointly.

For the construction of Huygens, Newton substituted another, without stating the theoretical grounds on which he formed it, or even advancing a single experiment in its confirmation*. In this unsatisfactory position the problem of double refraction was suffered to rest for nearly a century; and it was not until the period of the revival of physical optics in the hands of Young, that any new light was thrown upon the question. This sagacious philosopher was led by the theory of waves to assume the truth of the law of Huygens; and it was by his advice that Dr. Wollaston undertook the experimental examination † which recalled to it the attention of the scientific world, and ended in its universal admission. The French Institute soon after proposed the question of double refraction as the subject of their prize essay, and the successful memoir of Malus left no doubt remaining as to the accuracy of the Huygenian law‡.

The examination of Malus was chiefly directed to the case of *Iceland spar*; but he made a few similar measurements, also, in *quartz*, *sulphate of barytes* and *arragonite*. In the first of these crystals he mistook the ordinary for the extraordinary ray; and the faces which he chose for examination in the two latter not happening to be well adapted to the discovery of their properties, he was satisfied with a hasty generalization of the law observed in Iceland spar, and concluded that it belonged to all double refracting bodies. Malus entered largely, in the same memoir, into several questions connected with the problem of double refraction; and he showed, in particular, that the laws of *extraordinary reflexion* at the second surfaces of crystals are deducible from the law of Huygens. In a memoir presented to the Institute, in the following year §, he extended considerably the list of bodies possessing the property of double refraction; and arrived at the conclusion that this property belonged to all crystals, excepting those whose primitive form was the *cube* or *regular octohedron*. Most organized substances, whether vegetable or animal, were found to possess the same properties.

In Iceland spar the extraordinary refractive index is less than the ordinary. The extraordinary ray consequently is always refracted *from the axis* of the crystal; and the same law had been supposed to belong to all double-refracting substances.

* *Optics*, book iii., query 25.

† "On the Oblique Refraction of Iceland Crystal," *Phil. Trans.* 1802.

‡ "Théorie de la Double Refraction," *Mém. Inst.*

§ "Sur l'Axe de Refraction des Cristaux et des Substances organisées," *Mém. Inst.* 1811.

M. Biot made the important discovery that in many crystals the extraordinary index was greater than the ordinary, and the extraordinary ray therefore refracted *towards the axis*. Crystals of the latter kind he called *attractive*, while those of the former were called *repulsive*; the extraordinary refraction being ascribed, in the theory of emission, to attractive or repulsive forces which act as if they emanated from the axis*. These crystals are now generally distinguished by the denominations *positive* and *negative*. The Huygenian law applies to positive as well as to negative crystals; the spheroid being *prolate* in the former case and *oblate* in the latter.

The construction given by Huygens for the direction of the two refracted rays is, it has been stated, an immediate consequence of the assumed form of the wave-surface. It easily appears, from the principle of Huygens already adverted to, that the same construction will apply in all cases, whatever be the form of the wave or the law of the velocity of propagation within the crystal;—so that the law of direction is determined when that of velocity is known. A similar connexion between the velocity of the molecule and its path is established, in the theory of emission, by the law of *least action*. This principle, we know, holds generally in the motion of a point subjected to the action of attracting or repelling forces; and in applying it to the case of a luminous molecule acted on by forces emanating from the particles of the body which it meets, we may leave out of consideration the insensible curvilinear portion of the trajectory described in the passage from one medium into another of different density,—provided we assume, with Newton, that the forces exerted by the molecules of body on those of light are sensible only at insensible distances. In this simplification of the problem we have to deal only with straight lines and uniform velocities; and when the dependence of these velocities on the directions is assumed or given, the principle in question furnishes a relation between the directions of the two portions of the trajectory. Such was the problem whose solution was given by Laplace, in his memoir on the motion of light in transparent media†; and he has arrived at two equations, in which that solution is completely contained. Laplace applied these results to two cases;—one in which the difference of the squares of the velocities of the incident and refracted rays is constant,—and the other in which that difference is equal to a constant quantity, plus another varying as the square of the cosine of the inclination of the refracted ray to the optic axis. In the former of these cases he obtained the known law of

* *Mém. Inst.* 1814. † *Ibid.* 1809.

Snellius; and the formulæ of refraction to which he arrived in the latter were found to be identical with those furnished by the construction of Huygens.

The velocity of the extraordinary ray, assumed by Laplace, is the reciprocal of the radius-vector of the ellipsoid of Huygens, and therefore the *inverse* of the assumed velocity in the wave-theory. But Laplace himself has shown that the construction suggested by that theory, and employed by Huygens for the determination of the direction of the refracted ray, resolves itself into the principle of *least time*,—and *that* whatever be the form of the wave-surface; and as the law of least action and that of least time are identical, provided the assumed velocities be reciprocal, it ceases to be strange that two such very different methods should lead precisely to the same result. The difference between Huygens and Laplace, as to the mode of deducing the law of extraordinary refraction, is in fact precisely the same as that which existed formerly between Fermat and Maupertuis with regard to the ordinary law of the sines.

This identity of the results afforded by the two theories has since been more distinctly pointed out by M. Ampère. By means of the principle of least action he has arrived at the following general conclusion, whatever be the assumed law of the velocities,—that if from the point of incidence on any extraordinary medium, as centre, two surfaces be described whose radii-vectores are inversely as the velocities of the incident and refracted rays in their directions; and if the incident and refracted rays be produced to meet these surfaces, and tangent planes be drawn at the points of meeting, the line of intersection of these planes will be on the separating surface of the two media*. Hence the position of the refracted ray is determined when that of the incident ray is known; and the construction thus supplied for its determination is obviously the generalization of the construction of Huygens already alluded to, if only the radii-vectores be taken in the direct ratio of the velocities, instead of the inverse.

It is obvious, then, that the problem of double refraction, considered as a physical question, resolves itself into the determination of the law of velocities. Newton showed that the constant ratio of the velocities in ordinary media, and therefore the law of the sines, could be explained on the supposition that the luminous molecules are solicited by attracting forces emanating from the molecules of the refracting body, and sensible only at very small distances. The phenomenon of extraordinary refraction, in like manner, was ascribed by Laplace to the operation

* *Mém. Inst.* 1815.

of similar forces emanating from the molecules of the crystal; but modified by the form of these molecules and those of light, and by the manner in which they are presented to each other. No attempt, however, has been made in the theory of emission to advance beyond the point to which Newton arrived, and to deduce the velocity of the extraordinary ray in crystallized media from any assumed constitution of the molecular forces*; and, indeed, when the condition of *polarity* is to be superadded to the laws of such forces, the theory seems embarrassed in inextricable difficulties. The refraction which a polarized ray undergoes in a crystal depends upon its plane of polarization, and, by a simple change of that plane, the refracted ray may be converted from an extraordinary to an ordinary ray. The extraordinary force then, it appears from the phenomena, exerts no effect upon a ray polarized parallel to the principal plane; its effect is greatest upon a ray polarized in the perpendicular plane; and it must be supposed to act in every intermediate degree upon rays polarized in intermediate planes. Now a ray of common light, in the theory of emission, is composed of molecules whose planes of polarization are turned in all azimuths; and these molecules, consequently, should feel the influence of the extraordinary force in every possible degree. Instead, therefore, of two refracted rays, such a ray should be divided into an infinite number, inclined in every possible angle between the limiting directions of the ordinary and extraordinary rays.

It had been hitherto assumed, that no crystal had more than one optic axis. While examining the rings which surround these axes in polarized light, Sir David Brewster made the important discovery that the greater number of crystals possess *two optic axes*; and he soon after discovered the connexion between these diversities of optical character and the crystalline form†.

The optic axes, however, as Sir David Brewster has shown, cannot be regarded in general as the fundamental axes of the double-refracting medium. He calls them *apparent axes*; and considers them as the resultants of others, which he denominates

* Fresnel states, in the commencement of his memoir on double refraction, that Laplace had derived the velocity of the extraordinary ray, in uniaxal crystals, from the hypothesis of a *resultant force* acting in a direction perpendicular to the optic axis, and varying as the square of the sine of the angle which the ray makes with that line. I have not been able to discover, in any of Laplace's writings, the discussion thus adverted to.

† The important relations here alluded to have been already brought under the attention of the Association, in the able Report on Mineralogy, by Mr. Whewell.

true or *polarizing axes*, and from which the forces which produce the phenomena of polarization and double refraction are conceived to emanate. The polarizing force proceeding from a single axis, is measured by the difference of the squares of the velocities of the ordinary and extraordinary rays, and is supposed to vary as the square of the sine of the angle which the direction of the ray within the crystal contains with it; and when two such axes cooperate, it is assumed that the increment of the square of the velocity, arising from their joint action, is equal to the diagonal of a parallelogram whose sides are the increments of the square of the velocity produced by each separately, and whose angle is double of that formed by the two planes passing through the ray and the axes*. From this hypothesis it followed that two rectangular polarizing axes of equal intensity, and both positive or both negative, compound a *single resultant axis* at right angles to both. This axis is of the same intensity as the component axes, but of an opposite character; and, accordingly, three equal rectangular axes of the same character balance each other's effects, and have *no resultant*. Thus, then, the laws of uniaxal crystals, as well as of singly-refracting media, are embraced in this hypothesis. The case of *two resultant axes* is reducible to that of two unequal polarizing axes; and it has been shown to be a consequence of the rule that the difference of the squares of the velocities of the ordinary and extraordinary rays within the crystal, is proportional to the product of the sines of the angles which the latter makes with the resultant axes. M. Biot was led to the discovery of this beautiful law by analogy†, and he afterwards observed that it was implicitly contained in the law proposed by Sir David Brewster.

The term "polarizing force" seems to have been adopted by Sir David Brewster without any reference to the law which governed the planes of polarization of the two pencils,—a law which, in biaxal crystals, still remained unknown. In the case of uniaxal crystals, it could not fail to be observed, the plane of polarization of one of the pencils contained the direction of the ray and the axis; while that of the other was a plane passing through the ray at right angles to the former. Conceiving that these planes, in biaxal crystals, must be symmetrically placed with respect to the planes passing through the ray and the two axes, M. Biot was led to the simple and elegant law—that the plane of polarization of one of the pencils was that passing

* "On the laws of Polarization and Double Refraction in regularly crystallized Bodies," *Phil. Trans.* 1818.

† "Mémoire sur les Lois générales de la Double Refraction, &c.," *Mém. Inst.*, tom. iii.

through the ray, and bisecting the dihedral angle contained by these planes; while that of the other was perpendicular to the former, or bisected the supplemental dihedral angle*.

When a ray of light enters a crystal, the component molecules are supposed, in the theory of M. Biot, to receive different motions round their centres of gravity, dependent on the nature of the forces exerted upon them by the particles of the body. Sometimes the molecules of the ray are turned by the operation of these forces, so as to have certain lines in each, denominated axes of polarization, all in the same direction; and this arrangement of the molecules is maintained throughout the whole of their future progress. There are other cases, however, according to this author, in which the molecules *oscillate* round their centres of gravity in certain periods, during their entire progress through the crystal; while in others, finally, they receive a motion of *continued rotation*. To the two latter cases I shall have occasion to advert hereafter.

The phenomena of *fixed* polarization are ascribed by M. Biot to the operation of certain forces, which he denominates polarizing forces. In the case of uniaxal crystals these forces are supposed to act in the planes containing the two rays and the axis of the crystal,—the ordinary polarizing force tending to arrange the axes of the molecules in the plane containing the ray and the axis, while the extraordinary polarizing force draws them towards the perpendicular plane. If the molecules were similarly circumstanced in every respect, they would necessarily obey the stronger of these forces, and there would be but one plane of polarization. This, however, is supposed not to be the case. Owing to the different phases of their fits, at their incidence upon the crystal, the molecules are disposed to yield more readily to one or other of these forces; so that when a polarized ray meets a double refracting medium, some of the molecules fall under the influence of the ordinary polarizing force, and have their axes of polarization turned into the plane containing the ray and the axis of the crystal, while others are actuated by the extraordinary force, and have their axes arranged in the perpendicular plane. The number of molecules which yield to one or other of these forces, or the intensity of the two polarized rays, is supposed to depend on the angle which the plane of primitive polarization makes with the two planes just mentioned. When the plane of polarization coincides with the former, the extraordinary force has no effect, and the ray receives only the ordinary polarization; the converse takes place when the plane of polarization coincides with the perpendicular plane. Similar

* *Ibid.*

suppositions were made to account for the phenomena of polarization in biaxal crystals.

Such was the state of the theory of double refraction when the subject was taken up by Fresnel. The law of refraction, we have seen, whether in the theory of emission or in that of waves, was intimately connected with and dependent on the law of velocities; so that, considered as a physical question, the problem resolved itself into the determination of the latter. With the exception, however, of the reasonings of Young respecting the form of the wave-surface in a medium compressed or dilated in a given direction*, no attempt had been made to deduce the velocity of the extraordinary ray from the principles of either theory. Indeed, the general law of the velocities was itself unknown, even as an experimental fact, although an important relation between the velocities of the two pencils had been discovered by the labours of Sir David Brewster and M. Biot. But this was not all. It was evident that no physical theory of double refraction could be regarded as complete, which did not at the same time account for the attendant phenomenon of *polarization*. In this branch of the subject, however, nothing had been accomplished; and all that had been said in explanation of the phenomenon of polarization did not go further than some vague speculations as to its cause. The theory of Fresnel to which I now proceed,—and which not only embraces all the known phenomena, but has even outstripped observation, and predicted consequences which were afterwards fully verified,—will, I am persuaded, be regarded as the finest generalization in physical science which has been made since the discovery of universal gravitation.

Fresnel† sets out from the supposition that the elastic force of the vibrating medium is, in general, different in different directions. This is, in fact, the most general supposition that can be made; and whether we suppose that the vibrating medium is the ether within the crystal, or that the molecules of the body itself partake of the vibratory movement, there will be obviously such a connexion and mutual dependence of the parts of the solid and those of the medium in question, that we cannot hesitate to admit for the one what has been already established on the clearest evidence for the other‡. Now if a disturbance be produced in a medium so constituted, and

* *Quarterly Review*, vol. ii.

† "Mémoire sur la Double Refraction," *Mém. Inst.*, tom. vii.

‡ M. Savart has shown that the elasticity of crystals, determined by means of their sonorous vibrations, is, in general, different in different directions. The optic axis of Iceland spar is the axis of least elasticity: that of rock crystal is the axis of greatest elasticity.

any particle displaced from its position of rest, the resultant of the elastic forces which resist the displacement will not, in general, act in the direction of that displacement (as in the case of a medium uniformly elastic), and therefore will not drive the displaced particle directly back to its position of equilibrium. Fresnel has shown, however, that there are *three* directions at right angles to each other, in any of which, if the particles are displaced, the elastic forces *do* act in the direction of the displacement whatever be the nature or laws of the molecular action; and the only assumption which he makes is—that these three directions are *parallel* all throughout the crystal*. These directions Fresnel denominates *axes of elasticity*. He conceives that they ought also to be axes of symmetry with respect to the crystalline form; but observes that M. Mitscherlich has noticed some crystals in which this does not hold †. If on each of these axes, and on every line diverging from the same origin, portions be taken which are as the square roots of the elastic forces in their direction, the locus of the extremities of these portions will be a surface which Fresnel calls the *surface of elasticity*. This surface determines the velocity of propagation of the *wave*, when the direction of its vibrations is given. For the velocity of undulatory propagation in an elastic medium, being as the square root of the elastic force, must be represented by the radius-vector of the surface of elasticity in the direction of the vibrations.

Now let us conceive a plane wave advancing within the crystal. By the principle of transversal vibrations the movements of the ethereal molecules are all parallel to the wave. But the motion of each displaced particle is resisted by the elastic force of the medium, and that force is, in general, oblique to the direction of the displacement. Fresnel shows, however, that the displacement may be resolved in *two directions* in the plane of the wave, such that the elastic force called into action by each component will be the resultant of two forces, one of which acts in the direction of the displacement itself, while the other is normal to the wave. The latter, by the principle of transversal vibrations, can produce no effect; and the former will give rise to a wave propagated with a constant velocity. These two directions, he

* This will be the case, if the homologous lines of the groups of particles are all parallel; an arrangement at once the simplest and most natural, and which appears to be observed in most crystallized bodies. Fresnel admits, however, the possibility of other regular arrangements; and he conceives that the phenomena of circular polarization in rock crystal oblige us to suppose that its molecules are arranged according to some less simple law.

† See *Bulletin de la Société Philomathique*, March 1824.

finds, are those of the greatest and least diameters of the section of the surface of elasticity made by the plane of the wave; and if the original displacement be resolved into two, parallel to them, each component will give rise to a plane wave whose velocity of propagation is represented by that diameter, and the vibrations in each wave will preserve constantly the same direction.

Thus it appears that a polarized plane wave will be resolved into two within the crystal; and these will be propagated with different velocities, and consequently follow different paths. The amplitudes of the component vibrations are as the cosines of the angles which the direction of the original vibration contains with the two fixed rectangular directions; and, as the squares of these amplitudes represent the intensities of the two pencils, the law of Malus respecting these intensities follows as an immediate consequence*. Again, the planes perpendicular to these two directions are the planes of polarization of the two pencils. And it is easily inferred that one of them must bisect the dihedral angle contained by the two planes passing through the normal to the wave, and the normals to the circular sections of the surface of elasticity; while the other is perpendicular to it. This conclusion does not coincide mathematically with the experimental law of M. Biot: but the differences are much within the limits of the errors of observation, and the results of experiment must be regarded as confirmatory of the theory.

The velocity of propagation of a plane wave in any direction being known, the form of the *wave-surface* diverging from any point within the crystal may be found. For if we conceive an indefinite number of plane waves, which, at the commencement of the time, all pass through the point which is considered as the centre of disturbance, the wave-surface will be that touched by all these planes at any instant. This surface is of the fourth order. Fresnel has deduced its equation, although in an indirect manner; and he has shown that it may be geometrically constructed by means of an ellipsoid whose semiaxes are the same as those of the surface of elasticity. The form of the wave-surface being known, the directions of the two refracted rays are given by the construction of Huygens.

* Young seems to have been the first to observe that the law of the square of the cosine could be derived from the hypothesis of transversal vibrations, (*Ency. Brit.* Chromatics, p. 161.) The subject of the experimental confirmation of this important law has been recently brought before the French Academy by M. Arago, and he has indicated the practical results which may be derived from this law in its application to photometry.—*Herschel's Essay on Light: French Translation, Suppl.*, p. 590.

From the construction now alluded to it appears that there are two directions,—the normals, namely, to the two circular sections of the ellipsoid,—in which the velocity of the two rays is the same. These directions are called by Fresnel the *optic axes*; although he sometimes applies this term to the normals to the circular sections of the surface of elasticity, or the directions in which a plane *wave* is propagated with a single velocity. It thus appears that crystals have in general two optic axes, and can have no more. When *two* of the three principal elasticities are equal, the two optic axes unite, and the wave-surface resolves itself into the sphere and spheroid of revolution. Thus the form of the wave in *uniaxal* crystals, which Huygens assumed as the most natural, comes out as a simple corollary from the general theory of Fresnel. When, lastly, the *three* elasticities are all equal, the wave-surface becomes a sphere; the velocity is accordingly the same in all directions, and the law of refraction is reduced to the known law of Snellius.

It was easily shown to follow from the general construction, that the difference of the squares of the reciprocal velocities of the two rays, in *biaxal* crystals, is proportional to the product of the sines of the angles which their common direction within the crystal contains with the two axes; so that the remarkable law of Sir David Brewster and M. Biot is brought under the same theory. But it appeared further, from that theory, that the velocity of *neither* of the rays is constant, and that the refraction of both is performed according to a new law. This conclusion was at variance with all the received notions upon the subject; and indeed the experiments of M. Biot on limpid topaz* seemed to warrant his assumption that the refraction of one of the rays followed the ordinary law of the sines. It became, therefore, a matter of much interest to decide this question by accurate experiment. This has been done by Fresnel himself by the ordinary method of prismatic refraction, as well as by the nicer means afforded by the displacement of the diffracted fringes; and the result in both cases has been conclusive in favour of his theory. The numerical data afforded by the observations of M. Biot on topaz enabled Fresnel to compute, according to the principles of that theory, the velocity of the ray in different directions; and the observed variation was found to agree with that deduced.

The phenomenon of dispersion, in singly-refracting substances, proves that the elasticity of the vibrating medium varies with the length of the wave. The same thing must take place in

* *Mém. Inst.*, tom. iii.

double-refracting media, in which the elasticity is different in different directions; and as we have no reason for supposing that the elasticities should vary in the same proportion in the direction of the three axes of elasticity, it will follow that in general each refractive index will have its appropriate dispersive ratio. Sir David Brewster first showed that this was actually the case, and that Iceland spar and other double-refracting substances had two dispersive powers*. M. Rudberg has recently examined the laws of dispersion in double-refracting media with much care, following the accurate method of Fraunhofer. He has in this manner determined the greatest and least refractive index corresponding to the seven principal dark lines of the spectrum in Iceland spar and rock crystal, and the three principal indices in arragonite and topaz; and has found, in accordance with the discovery of Sir David Brewster, that the ratio of these indices increased with the refrangibility of the light†. The experiments of M. Rudberg confirm also the fundamental position of Fresnel's theory, namely, that the velocity of a ray in a given medium is the same as long as its plane of polarization is unchanged.

The angle contained by the optic axes, in biaxal crystals, is a simple function of the three principal elasticities; and if their ratio vary with the colour of the light, the inclination of the axes must likewise vary. Such a variation has been established by the observations of Sir John Herschel; and it has been found that the inclination of the axes is greater in red than in violet light for some crystals, while in others it is less‡. In the case of Rochelle salt, the angle between the optic axes of the red and violet rays amounts to 10°. Generally the position of the three axes of elasticity is invariable, and the optic axes for all colours are confined to one plane; but Sir John Herschel has lately observed, that in borax the optic axes belonging to different colours lie in different planes; and we are compelled to conclude that the direction of the axes of elasticity in this, and probably in many other crystals, varies with the colour.

The first addition to the theory of Fresnel was made by

* *Treatise on New Philosophical Instruments*, Edin. 1813.

† *Annales de Chimie*, tom. xlviii. For the calculation of the phenomena of double refraction in biaxal crystals, according to Fresnel's theory, it is necessary to know the three principal refractive indices, or the velocities of propagation of rays whose vibrations are parallel to the three axes of elasticity. Beside the researches of M. Rudberg, I do not know that we possess any other in which all these data have been directly determined. It is true that if we know the greatest and least index, and the angle contained by the optic axes, the mean index can be deduced. But the inclination of the optic axes cannot be determined experimentally with the same precision as the other elements.

‡ *Phil. Trans.* 1820.

M. Ampère. The results alluded to are contained in two short papers read to the French Academy in the year 1828, and since embodied into one, and published in the *Annales de Chimie**. Fresnel had arrived at the equation which belongs to all the tangent planes of the wave-surface, and had shown in what manner the equation of the surface itself might be thence deduced by differentiation and elimination. This direct process, however, he seemed to think would involve complicated and embarrassing calculations. The method which he substituted for it consisted in *verifying* the equation, to which he was led by reasonings not altogether rigorous, and proving (by calculations which he found too tedious to transcribe), that it satisfied the conditions already assigned. M. Ampère has supplied the direct demonstration, and deduced the equation of the wave-surface in the manner originally pointed out by Fresnel. From this equation he has derived also the beautiful geometrical construction given by Fresnel, and which the latter had obtained indirectly.

A very concise demonstration of the same theorem, and of the other principal points of Fresnel's theory, was given not long after by Mr. M'Cullagh†. This writer has shown that both the magnitude and direction of the resultant elastic force, called into action by any displacement, may be represented by means of an ellipsoid whose semiaxes are the three principal refractive indices of the medium; and from this ellipsoid, by the aid of a few geometrical lemmas, he has deduced in a clear and simple manner the leading results arrived at by Fresnel. The axes of this ellipsoid coincide in direction with, and are inversely proportional to, the axes of Fresnel's generating ellipsoid; and Mr. M'Cullagh has demonstrated the truth of Fresnel's construction for the wave-surface, by means of a simple geometrical relation between its tangent planes and the sections of the two ellipsoids.

In the third supplement to his "*Essay on the Theory of Systems of Rays*‡," Professor Hamilton has presented that portion of Fresnel's theory, which relates to the fundamental problem of the determination of the velocity and polarization of a plane wave, in a very elegant analytical form; and from the

* "Mémoire sur la Détermination de la Surface courbe des Ondes lumineuses," &c., tom. xxxix.

† "On the Double Refraction of Light in a crystallized medium according to the principles of Fresnel," *Transactions of the Royal Irish Academy*, vol. xvi. A further development of the principles of this memoir has been recently given by the author in the 17th vol. of the same Transactions, under the title "Geometrical Propositions applied to the Wave-theory of Light."

‡ *Transactions of the Royal Irish Academy*, vol. xvii.

velocity and direction of the *wave* he deduces those of *the ray*, and therefore the form of the wave-surface, by means of the general relations suggested by his view of mathematical optics.

In this system, of which the author gave a brief sketch at the late meeting of the Association, the laws of reflexion and refraction, ordinary or extraordinary, are comprised in two fundamental expressions, which state that the partial differential coefficients of the first order of a certain function,—taken with respect to two final coordinates in the plane which touches the reflecting or refracting surface at the point of incidence,—are not altered by reflexion or refraction. The function here considered is the *characteristic function* of the author, whose particular form may be considered as characterizing the optical system, and on whose properties, he finds, all the problems of mathematical optics may be made to depend. On the principles of the wave-theory, this function is equal to the undulatory time of propagation of light, from any one assumed point to another, in the same or in a different medium; and the expressions just alluded to, signify simply that the components of *normal slowness* of the wave parallel to the bounding surface, or the reciprocal of the velocity of wave-propagation resolved in the direction of that surface, are not changed by reflexion or refraction. The normal slowness of wave-propagation is, then, of fundamental importance in this theory; and if it be represented in magnitude by a line taken in its direction, there is obtained for its expression a curved surface which, on the principles of Fresnel, is found to be a surface of two sheets, connected with the wave-surface by a remarkable relation of reciprocity. When this relation is combined with the laws of reflexion and refraction just alluded to, they lead to a very elegant construction for the reflected or refracted ray which is, in most cases, more convenient than that of Huygens. Thus, when a ray proceeds from air into any crystal, we have only to construct the *surfaces of wave-slowness* belonging to the two media, and having their common centre at the point of incidence. Let the incident ray be then produced to meet the sphere, which represents the normal slowness of the wave in air; and from the point of intersection let a perpendicular be drawn to the reflecting or refracting surface. This will cut the surface of slowness of the reflected or refracted waves in general in two points. The lines connecting these points with the centre, will represent the direction and normal slowness of the *waves*; while the perpendiculars from the centre on the tangent planes at the same points, will represent the direction and slowness of the *rays* themselves.

This important curved surface presented itself also to M. Cau-

chy in his able researches on the propagation of waves in elastic media, although he does not seem to have been aware of all its properties. The properties of the same surface, and its use in constructing the direction of a reflected or a refracted ray, were also discovered, independently, by Mr. M'Cullagh, who has recently applied them to the geometrical development of the theory of double refraction*.

The relations between the surface of wave-slowness and that of the wave have led Professor Hamilton to the discovery of some new geometrical properties of the latter. These properties are demonstrated by means of certain transformations of the equation of the wave-surface; and it is shown that this surface has *four conoidal cusps*, at the extremities of the lines of *single ray-velocity*, at each of which the wave is touched, not by two planes as Fresnel supposed, but by an infinite number forming a tangent cone of the second degree; while, at the extremities of the lines of *single wave-velocity*, there are *four circles of plane contact*, in every point of each of which the wave-surface is touched by a single plane. These singular properties have led Professor Hamilton to anticipate two new laws of refraction—called by him *conical refraction*, because in each case a single ray is refracted into an infinite number forming a species of cone. *External* conical refraction corresponds to the cusp on the wave-surface; and takes place without, when a single internal ray coincides with either of the lines of single ray-velocity. *Internal* conical refraction, on the other hand, takes place within the crystal, when a single ray is incident externally at an angle corresponding to the line of single wave-velocity within. In this latter case, if the crystal be bounded by parallel planes, all the rays of the cone will emerge at the second surface parallel to the ray incident on the first, so as to form a small elliptic cylinder, whose magnitude will depend upon the angle of the cone and the thickness of the crystal. All these remarkable conclusions have been verified in the fullest manner by experiment†.

I shall now proceed to give a brief account of the labours of M. Cauchy in this interesting department of analysis. The researches of this eminent mathematician, on the propagation of motion in elastic media, are scattered through various *livraisons* of the *Exercices de Mathematiques*; and he has given a valuable summary of the results of these investigations,

* "Geometrical Propositions applied to the Wave-theory of Light," *Transactions of the Royal Irish Academy*, vol. xvii.

† "On the Phenomena presented by Light in its passage along the axes of biaxal Crystals," *Ibid.*

as applied to the wave-theory of light, in a memoir read to the French Academy in the year 1830 *.

Having assigned the general equations of motion of a system of molecules, acting on one another by attracting or repelling forces which vary according to any function of the distance, M. Cauchy observes that it is not necessary to have recourse to their general integrals in order to determine the laws of undulatory propagation. It is sufficient, in fact, to determine the law of propagation of a *plane wave*. For if we consider a great number of plane waves inclined to one another at small angles, and which are at first superposed in the neighbourhood of the point which is considered as the origin of the disturbance, the vibrations in the elementary waves, to which each of these gives rise, may be supposed too small to affect the sense separately, and these waves become efficacious only by superposition. Consequently the general wave-surface will be the locus of all the points in which the elementary plane waves are superposed; and will therefore be the surface touched by them all at any instant†. Hence the problem is reduced to the determination of the law of propagation of a plane wave.

M. Cauchy then shows that a disturbance, confined originally to a given plane, will in general give rise to *three pairs* of plane waves parallel to the original plane, and propagated with uniform velocities,—the two waves of each pair moving with equal velocities in opposite directions. The velocities of propagation of the separate pairs, he proves, may be represented by the reciprocals of the axes of a certain ellipsoid, whose form depends upon the position of the plane wave and upon the nature of the system; and the absolute displacements of the molecules will be parallel to the directions of these axes. Accordingly, a system of plane waves, superposed at first at the point of original disturbance, will be subdivided into three corresponding systems; and these, by their superposition, will generate a *curved surface of three sheets*, each sheet being touched by all the plane waves of the same system. From these principles it follows that a single ray of light will be, in general, subdivided into *three polarized rays*;—a ray being said, in this theory, to be polarized parallel to a certain line or plane, when the vibrations of the ethereal molecules are parallel to that line or plane. M. Cauchy

* "Mémoire sur la Théorie de la Lumière," *Mém. Inst.*, tom. x.

† M. Poisson does not admit the legitimacy of this conception of the wave-surface; and he thinks that an assemblage of indefinite plane waves, having a small part in common at the origin of the motion, cannot represent the initial condition of a medium disturbed at that point.

does not state the precise physical condition on which the existence of the third ray depends. It would seem, however, that it must arise from the circumstance that the vibration normal to the wave is not absolutely insensible, or that the actual vibrations are not accurately in the plane of the wave. He states that the intensity of this ray will be in all cases very small, and that its observation therefore will be a matter of difficulty; but he promises in a future communication to point out the means of manifesting its existence.

The formulæ, on which the solution of the general problem depends, may be reduced to contain nine constant coefficients depending on the law of distribution of the molecules in space. Three of them represent the pressures sustained in the natural condition of the medium by any three planes parallel to those of the three coordinates; and these, M. Cauchy afterwards concludes, vanish of themselves. When the general theory is applied to the case in which the elasticity is the same in all directions round any line parallel to one of the axes of coordinates, M. Cauchy finds that the nine coefficients are reduced to five; and that two sheets of the wave-surface become the *sphere* and *spheroid* of the Huygenian law, provided that the remaining constants fulfill two assigned equations of condition. In the general case, in which the elasticity is unequal in all directions, he investigates the sections of the wave-surface made by the planes of the three coordinates; and he finds that,—for two sheets of that surface,—they are reduced to the *circle* and *ellipse* of Fresnel's theory, provided that the constants fulfill three assigned equations of condition. The wave-surface itself differs a little from the surface of the fourth order obtained by Fresnel; but is reducible to it when the excentricities of the ellipses just mentioned are small, as is the case in all known crystals.

Thus the results obtained by M. Cauchy embrace and confirm those of Fresnel; and the mathematical laws of the propagation of light are shown to be particular cases of the more general laws of the propagation of vibratory motion in any elastic medium composed of attracting and repelling molecules. Considered, however, simply with reference to the theory of light, the solution given by M. Cauchy cannot, I conceive, be considered as a complete *physical* solution. In other words, the phenomena of light are not connected *directly* with any given physical hypothesis; but are shown to be comprehended in the results of the general theory, in virtue of *certain assumed relations* among the constants which that theory involves. If, indeed, we were able to assign the precise physical meaning of these equations of condition, we should have nothing more to desire in the

general theory of light; for these equations must necessarily express the characteristic properties of the vibrating medium. In this point of view their discussion becomes a subject of the highest interest; and it is probable that the important conclusions of which we have yet to speak may in this manner be confirmed and extended.

These conclusions are contained in a memoir presented to the French Academy by M. Lamé, in the spring of the present year*; and in which the author has proposed to determine the laws according to which the molecules of bodies act on those of the ether, and the molecules of the ether on one another. Setting out from the existence of transversal vibrations, as established by the fact of the non-interference of rays oppositely polarized, the author supposes a disturbance of the ether to take place in *vacuum*,—that is, in a space devoid of all ponderable matter,—and proceeds to consider what will be the result when that disturbance reaches the ether contained in a *transparent body*. Assuming the property of transversal vibrations noticed by Fresnel, and more explicitly stated by M. Poisson,—namely, that they are propagated *without any attendant change of density*,—M. Lamé then seeks the conditions to be satisfied by the function, which represents the mutual action of the molecules of the ether and those of the solid body, in order that this property may subsist. Introducing, accordingly, this principle into the partial differential equations, which express the laws of the vibratory movement generally, he arrives finally at an equation of condition, from which he concludes, that "the action of ponderable matter on the ether varies in the inverse ratio of the square of the distance; and that the elasticity of the ether itself is proportional to its density."

In order to determine the *sign* of this action,—that is to say, whether it is attractive or repulsive,—it is necessary to integrate the differential equations. After certain transformations of these equations tending to facilitate their examination, he obtains their integral in the case of a single spherical and homogeneous molecule of the body, around which the ether is distributed in spherical shells. The conclusions deduced from this case being combined with the established fact—that the velocity of light is less in transparent bodies than in vacuum, he arrives at the result, that the mean density of the ether is *less* in the former; or that the action of the molecules of these bodies on those of

* "Mémoire sur les Lois de l'Equilibre de l'Ether dans les Corps diaphanes." A full account of this paper is given in the *Annales de Chimie* for March. The memoir itself is not yet published.

the ether is *repulsive*. M. Lamé concludes also from the examination of the same case, that the retardation of the vibratory motion, in penetrating into a dense body, will be greater, the less the length of an undulation; so that the refraction will be greater for waves of shorter length. This he conceives to be the true explanation of the phenomenon of dispersion.

M. Lamé has likewise endeavoured to connect the phenomena of double refraction with an assumed constitution of the ethereal fluid. He takes the case in which the ether is supposed to be distributed round the molecules of the body in confocal ellipsoidal shells; and he concludes that a vibratory movement, propagated from vacuum into a body so constituted, will be separated at its entrance into two component movements, which will advance with different velocities. The two component vibrations, he finds, will be at right angles, and parallel to the lines of greatest and least curvature of the elementary ellipsoids. Thus, the bifurcation of a ray of light on entering a crystallized medium, and the opposite polarization of the two pencils, are found to be consistent with a molecular constitution such as that described.

These results are of the highest interest; and will, no doubt, receive an early examination from those engaged in the same department of analysis. Their author seems to be persuaded that his methods will lead him to the mathematical laws of other phenomena, which he conceives to depend, in like manner, on the motions of the ethereal fluid*.

I cannot close this division of the present Report without referring to the phenomena of absorption by crystallized media, although the laws of these phenomena are as yet wholly without the pale of theory. Dr. Wollaston seems to have been the first who noticed any facts connected with this interesting subject. The absorbing properties of crystals were found to vary with the direction; certain crystals of palladium, for example, appearing of a deep red colour when viewed along the axis, and of a yellowish green in a transverse direction. Tourmalines were observed also to possess analogous properties†. Similar observa-

* In a continuation of this memoir, recently read to the French Academy, M. Lamé has considered particularly the mode of vibration of the particles of the ether which are disposed round the ponderable particles of body in concentric spherical shells of decreasing density. Transparent homogeneous bodies are supposed to consist of a multitude of such particles distributed uniformly in space, and at distances incomparably greater than their diameters; and he conceives that the waves propagated from the particles adjoining to the surface of emergence will, by their interference, give rise to phenomena resembling the fixed lines in the spectrum. *Ann. Chim.*, tom. lvii.

† *Phil. Trans.* 1804.

tions were afterwards made by M. Cordier and the Count de Bournon.

The next step of any importance in this new field of research was made by Sir David Brewster. This philosopher observed that in some double-refracting crystals, as carbonate of barytes, the two pencils were differently coloured*; while in others their intensity was widely different†. The unequal absorption of the two pencils is most remarkable in tourmaline, in which it was observed, nearly at the same time, by M. Biot and Dr. Seebeck; and the former philosopher inferred from the phenomena that the more refrangible rays of the spectrum are more easily absorbed by the mineral, when polarized parallel to its axis, than when perpendicularly‡.

Sir David Brewster, to whom we owe the greater part of our knowledge on this subject, has shown§ that similar properties belong, in a greater or less degree, to most coloured crystals which possess double refraction; and that the absorption of light by such media varies, in general, both with the colour of the light and with the position of the plane of polarization. When a ray of common light therefore enters a plate of such a crystal, the two pencils into which it is divided will be unequally absorbed, and the emergent light will be partially polarized;—the difference of the intensities of the oppositely polarized portions increasing with the thickness of the medium traversed. But the two pencils differ, in general, in colour as well as in intensity; and this difference, in uniaxal crystals, Sir David Brewster found to depend on the inclination of the ray to the axis,—vanishing when the ray coincided with the axis, and becoming a maximum when it was perpendicular to it. A ray of common light, therefore, transmitted perpendicularly through a plate of such a crystal, will emerge coloured; and the resulting colour will, in general, vary with the inclination of the surface to the axis. Thus the phenomena of *dichroism*, observed by Wollaston and others, are reduced to the more general laws of absorption. Analogous properties belong to biaxal crystals, and depend in like manner on the planes of polarization of the two pencils, or on the direction of the ray. These properties, Sir David Brewster found, could be modified by heat; and were even communicated by such influences to crystals in which they did not naturally reside.

* *Edin. Trans.*, vol. vii. † *Phil. Trans.* 1814.

‡ *Traité de Physique*, tom. iv. 313.

§ "On the Laws which regulate the Absorption of Polarized Light by double-refracting Crystals," *Phil. Trans.* 1819.

Notwithstanding the important labours of Sir David Brewster, much remains to be done connected with this subject. Sir John Herschel has proposed empirical formulæ to represent the intensity of the transmitted light as dependent on its direction; and the results of the formulæ present a general accordance with observed facts*. It is much to be desired that these laws should be placed beyond doubt by an extensive series of experiments directed to this specific object. Although the laws of absorption by crystallized media are necessarily more complicated than those of ordinary media, yet they bear an evident and close relation to the well-known laws of double-refraction, which seems to hold out a clue to their discovery; and I feel persuaded that it is in the phenomena of dichroism that the physical theory of absorption will first take its rise, and seek its confirmation.

IV. *Colours of Crystalline Plates.*

If a beam of light, polarized by reflexion, be received upon a second reflecting plate at the polarizing angle, it is wholly transmitted when the second plane of incidence is perpendicular to the first. But if between the *polarizing* and *analysing* plates, as they are termed, there be interposed a plate of any double-refracting crystal, a portion of the light is reflected, whose quantity depends on the position of the interposed crystal. In order to analyse the phenomenon, the crystalline plate may be placed so as to receive the polarized beam perpendicularly, and then turned round in its own plane. It is then observed that there are two positions of the plate in which the reflected light totally disappears, just as if no crystal had been interposed. These two positions are those in which the *principal* and the *perpendicular sections* of the crystal coincide with the plane of the first reflexion. When the plate is turned round from either of these positions, the light gradually increases; and it becomes a *maximum* when the principal section is inclined at an angle of 45° to the plane of the first reflexion. These phenomena were observed by Malus.

The reflected light in these experiments was in all cases *white*. But M. Arago observed that when the interposed plate is sufficiently thin,—such as the laminæ into which *mica* or *sulphate of lime* may be readily divided by cleavage,—the most gorgeous colours appear, which vary with every change of incli-

* *Essay on Light*, p. 554, &c.

nation of the plate to the polarized beam. When the plate is perpendicular to the transmitted pencil, and then turned round in its own plane, the tint does not change, but only varies in intensity,—being a maximum when the principal section of the crystal is inclined at an angle of 45° to the plane of primitive polarization, and vanishing altogether when it coincides with that plane, or is perpendicular to it. On the other hand, when the crystal is fixed, and the analysing plate turned so as to vary the inclination of the plane of the second reflexion to that of the first, the colours change in the most striking manner; and it is found that the colour reflected, in any one position of the plane of the second reflexion, is always *complementary* to that reflected in the perpendicular position. The colours disappear altogether when the thickness of the crystalline plate is reduced below a certain limit *.

The experimental laws of these phenomena were investigated with unwearied zeal by M. Biot †. When the light was incident perpendicularly on plates of the same substance, of different thicknesses, the tints were observed to follow the same law as the colours of thin plates; the thicknesses of the crystal at which each tint was developed in perfection being proportional to the thicknesses of the plate of air which gave the same tint in Newton's scale. These thicknesses vary with the nature of the crystal; and are always much greater than the corresponding thicknesses of the uncrystallized plate which exhibit the same tint. Pursuing the same inquiry, afterwards, for oblique incidences, M. Biot found that, in uniaxal crystals, the tint developed was determined by the length of the path traversed by the light within the crystal, and by the square of the sine of the angle which its direction made with the optic axis, jointly. From this law it followed, that if a crystalline plate of moderate thickness be cut perpendicularly to the axis, and a converging or diverging pencil transmitted through it, the lines of equal tint,—or the *isochromatic* lines, as they are sometimes called,—will be disposed in concentric circles similar to Newton's rings. This phenomenon was observed, under different circumstances, by Sir David Brewster, Dr. Wollaston, M. Biot, and M. Seebeck.

The researches of M. Biot were followed by those of Sir David Brewster. In investigating the law of the tints in *biaxal* crystals, Sir David Brewster considers the optic axes as the resultants of others which he denominates *polarizing* axes. The tint developed by a single axis is taken as the measure of its

* *Mém. Inst.* 1811. † *Ibid.* 1812.

polarizing force, and is assumed to vary as the square of the sine of the angle contained by the ray with it; and when two such axes cooperate, the tint resulting from their joint action is measured by the diagonal of a parallelogram whose sides represent the tints produced by each axis separately, and whose angle is double the angle contained by the two planes passing through them and the ray. This law Sir David Brewster has verified by comparison with the observations of M. Biot on sulphate of lime, and its agreement with phenomena was complete *. When analytically developed by M. Biot, it was found to accord with the beautiful law to which he was himself conducted by analogy; namely, that the tint is measured by the product of the sines of the angles which the direction of the ray within the crystal makes with the optic axes †. From this law it easily followed that the isochromatic lines, in biaxal crystals, will be *lemniscates*, whose poles are in the apparent direction of the optic axes ‡. This phenomenon was first discovered by Sir David Brewster in topaz. The law has been established in the most complete manner by Sir John Herschel; and he has found that the constant parameter, or the product of the radii-vectores drawn from any point to the two poles, varies inversely as the thickness of the plate, for different plates of the same substance, and increases from one curve to another in the same plate, in the ratio of the numbers of the natural series.

To account for these varied phenomena in the hypothesis of emission, M. Biot has imagined his ingenious and beautiful theory of *moveable polarization*. When a polarized ray of any simple colour enters a crystalline plate, the component molecules are supposed, in this theory, to penetrate at first to a certain depth without losing their primitive polarization; and then to commence a series of regular oscillations round their centres of

* "On the Laws of Polarization and Double Refraction in regularly crystallized Bodies," *Phil. Trans.* 1818.

† From the researches of M. Biot it appeared that the measure of the tint, in uniaxal crystals, observed the same law as that attributed to the difference of the squares of the velocities of the two rays in the theory of emission. The same relation was assumed to hold generally; and thus from the law of the tints in biaxal crystals the relation of the velocities of the two pencils, noticed in the preceding section, was inferred.

‡ M. Biot has observed an apparent exception to this law in the *diopside* of the Tyrol, in which the rings are in general unsymmetrical with respect to the two axes. One of the axes presents the ordinary phenomena; but in the neighbourhood of the other there is a remarkable distortion of the rings near their centre, when the crystalline plate is turned in its own plane. These distortions seemed to observe a regular law, and were the same in all the specimens examined. It may be remarked that the optic axes of this crystal are unsymmetrically placed with respect to the crystalline form, *Mém. Inst.*, tom. x.

gravity, the axes of polarization being carried alternately to one side or other of the axis of the crystal, or of the perpendicular line. These oscillations being isochronous, the thickness traversed by the molecule in its motion of translation during each of them is constant, and is assumed to be equal to double the depth to which it has penetrated before commencing its vibrations. The oscillatory movement is supposed to stop, when the molecules repass into air through the second face of the crystal; and the emergent ray has a *fixed polarization*, the same as if the last oscillation of the molecules had been actually completed at the instant of emergence. Thus a polarized ray which has traversed a thin crystalline plate is ultimately polarized either in the primitive plane, or in a plane inclined to it at an angle equal to double the angle which it forms with the principal section, according as the thickness of the crystal is an odd or an even multiple of a certain length *. The formulæ deduced from these postulates is found to represent all the more obvious laws of the tints with much fidelity.

This assumed difference between the effects produced by thick and thin crystals has however been completely disproved by the decisive experiments of Fresnel. When two mirrors, slightly inclined, are placed so as to receive the incident light at the polarizing angle, and two laminæ of sulphate of lime of the same thickness are interposed,—one in the path of each of the reflected pencils, and so that their principal sections are inclined at angles of 45° on either side of the plane of primitive polarization, —the phenomena of the interference bands prove in the clearest manner that the light emergent from each consists of two pencils polarized respectively in the principal section and in the perpendicular section of the crystals; and that the results differ from those produced by thick crystals only in this, that the two pencils are superposed †. The light resulting from the union of these oppositely polarized pencils has, in certain cases, the properties ascribed to it in the theory of M. Biot; but these properties are immediate and necessary consequences of the laws of interference of polarized light, and of the theory of transversal vibrations.

* "Sur un nouveau genre d'Oscillation," &c., *Mém. Inst.* 1812.

† See Report made to the Academy of Sciences, in 1821, on the memoir of Fresnel relative to the colours of crystallized plates, *Annales de Chimie*, tom. xvii. Indeed, a more obvious objection to M. Biot's theory may be drawn from the fact which he has himself observed;—namely, that the phenomena of colour may be produced by *crossing* two thick plates of nearly the same thickness, although the thickness in each was sufficient to furnish two images sensibly separated, and therefore having a *fixed* polarization.

Let us now inquire what account the wave-theory furnishes of the same phenomena.—A ray of light on entering a crystalline plate is divided into two, or, in the language of the wave-theory, a series of waves incident upon the crystal is resolved into two within it, which traverse it in different directions and with different velocities. One of these sets of waves, therefore, will lag behind the other, and they will be in *different phases* of vibration at emergence. When the plate is thin, the emergent waves are superposed; and as the retardation will then amount only to a few undulations and parts of an undulation, it would appear that we have here all the conditions necessary for their interference, and the consequent production of colour. Such was the sagacious conjecture of Young. And indeed, shortly after the publication of the first researches of M. Biot on the laws of the tints for different thicknesses, it was observed by Young that these tints corresponded accurately to the interval of retardation of the two pencils; so that they were manifestly due to interference *. This correspondence is now made out in the fullest manner. It is an easy consequence of Fresnel's theory of double refraction, that the interval of retardation of the two pencils, in traversing a crystalline plate, is nearly proportional to the length of their path within the crystal multiplied by the product of the sines of the angles which its direction makes with the two optic axes; and as this has been found to be the general measure of the tint, it follows that the forms of the isochromatic curves,—the lemniscates and the circles,—are all necessary consequences of the wave-theory.

But in the first application of the principle of interference to the colours of crystalline plates there arose a difficulty to which the known laws afforded no answer. So far as this explanation went, the phenomena of interference and of colour should be produced by the crystal alone, and in common light, without either polarizing plate or analysing plate. Such, however, is not the fact; and the real difficulty seemed to be, not to explain how the phenomena are produced, but to show why they are *not always* produced. It occurred to MM. Arago and Fresnel to inquire how far the state of polarization of the two pencils might modify the known laws of interference; and the results of this inquiry † have happily furnished an account of the difficulty, and completed the solution of the problem. It was found that two rays of light polarized in the same plane, inter-

* *Quarterly Review*, vol. xi.

† "Mémoire sur l'Action que les Rayons de la Lumière polarisée exercent les uns sur les autres," *Annales de Chimie*, tom. x.

fere and produce fringes under the same circumstances as two rays of common light;—that, when the planes of polarization are inclined, the interference is diminished and the fringes decrease in intensity;—and that, finally, when the angle between these planes is a right angle, the rays no longer interfere at all. Hence the two rays which emerge from a crystalline plate, being oppositely polarized, cannot interfere; and, to produce the phenomena of colour in perfection, their planes of polarization must be brought to coincidence by the analyser.

The non-interference of rays oppositely polarized is a necessary result of the mechanical theory of transversal vibrations. Fresnel has shown, on the principles of that theory, that the intensity of the light resulting from the union of two such rays is *constant*, and equal to the sum of the intensities of the components, whatever be the phases of vibration in which they meet. But though the *intensity* of the light does not vary with the phase of the component vibrations, the character of the resulting vibration will. It appears from theory that two rectilinear and rectangular vibrations compound a single vibration, which will be also *rectilinear* when the phases of the component vibrations differ by an exact number of semiundulations; that, in all other cases, the resulting vibration will be *elliptic*; and that the ellipse will become a *circle*, when the component vibrations have equal amplitudes, and the difference of their phases is an odd multiple of a quarter of a wave. These results of theory have been completely confirmed by experiment. When a polarized beam diverging from a luminous origin is transmitted through two rhomboids of Iceland spar of equal thickness, having their principal sections inclined 45° on either side of the plane of primitive polarization, the emergent light will diverge as if from two near points, and the two portions will be oppositely polarized. It was found by Fresnel and Arago that the light resulting from the union of these pencils was *plane*, *circularly*, or *elliptically* polarized, according to the difference of the paths traversed when they met.

Here, then, we have an account of the facts which seem to have suggested the theory of moveable polarization; and we learn moreover that they are but particular cases of the general result. The light arising from the union of the ordinary and extraordinary pencils which emerge from the crystalline plate, will be polarized in the primitive plane, or in a plane inclined to it at an angle equal to double the angle which it makes with the principal section, according as the interval of retardation of the two pencils is an even or an odd multiple of half a wave. In all other cases, the thickness of the crystal

having any other value than those which exactly answer to these intervals, the resulting light will be *elliptically-polarized*. The ellipse will become a *circle*, and the light will appear to be completely depolarized, when the two pencils are of equal intensity, and the interval of retardation is an odd multiple of a quarter of a wave. Here, then, is suggested an easy method of putting the theory of moveable polarization to the test. If a plate of sulphate of lime, whose thickness corresponds to such an interval, be placed in a beam of polarized light of some simple colour, so that its principal section is inclined at an angle of 45° to the plane of primitive polarization, the emergent light should, according to the theory of waves, be *circularly-polarized*; and the two pencils into which it is divided by the analysing rhomb should not vary in intensity during its revolution. According to the theory of moveable polarization, on the other hand, the light should be *plane-polarized*; and one of the images should vanish when the principal section of the rhomb coincided either with the primitive plane, or the plane perpendicular to it. This *experimentum crucis* was tried by MM. Fresnel and Arago, and the result was just as had been predicted by the wave-theory *.

In the prosecution of their researches on the laws of interference of polarized light, MM. Fresnel and Arago discovered further that two oppositely polarized rays will not interfere, even when their planes of polarization are brought to coincidence, unless they belong to a pencil, the whole of which was originally polarized in one plane;—and that, in the interference of rays which have undergone double refraction, half an undulation must be supposed to be lost or gained, in passing from the ordinary to the extraordinary system. The latter principle is a beautiful and simple consequence of the theory of transversal vibrations. When a vibration in any given direction is resolved into two at right angles, and each of these again into a second pair, in two fixed directions which are also perpendicular, it will easily appear that, of the four components into which the original vibration is thus resolved, the two in one of the final directions *conspire*, while those in the other are *opposed*. The tint produced by the interference of the former, therefore, corresponds to the actual difference of routes of the two polarized rays in the plate; while that arising from the latter is that due to the same difference augmented or diminished by half an undulation.

The former of the two laws now mentioned explains the office

* *Annales de Chimie*, tom. xvii.

of the polarizing plate in these phenomena. To account mechanically for the fact of the non-interference of the two pencils, when the light incident upon the crystal is *unpolarized*, it is necessary to consider such light as a rapid succession of systems of waves polarized in all azimuths; so that if any two planes be assumed at right angles, there will be an equal quantity of light actually polarized in each. Each of these portions, when resolved into two within the crystal, and these afterwards reduced to the same plane of polarization by the analysing plate, will exhibit the phenomena of interference. But the interval of retardation differs by half a wave in the two cases; the tints produced therefore will be complementary, and the light resulting from their union will be of a uniform whiteness.

We are obliged to admit, therefore, that common light consists of a rapid succession of systems of waves, in each of which the vibrations are different. But the phenomena of interference, (which are exhibited by common light) compel us also to admit, as Professor Airy has observed*, that the vibrations do not change *continually*; and that in each system of waves there are probably several hundred successive vibrations, which are all similar,—although the vibrations of one system bear no relation to those of another, and the different systems succeed one another with such rapidity as to obliterate all trace of polarization. This *per saltum* transition from one system of waves, to another in which the vibrations are wholly different, seems a complication in the machinery of light, for which the elegant simplicity of the parts better known has not prepared us; and I cannot but indulge the hope that the hypothesis, which now stands as the representative of experimental laws, may be found to merge in some simpler physical principle.

The laws of interference of polarized light have thus supplied the defective link in the explanation of the colours of crystallized plates first suggested by Young. The magnitudes of the resolved vibrations are known, when the planes of polarization of the two pencils are given with respect to the plane of primitive polarization, and the plane of analysation; and as the laws of double refraction enable us to find the interval of retardation of these pencils, we have all the data necessary for the computation of the intensity and colour of the light resulting from their interference. This computation has been given by Fresnel, not only for a single plate, but likewise for two plates superposed†; and the theory has been since more fully developed by Professor Airy‡. The results are found to be, in all cases, in

* *Mathematical Tracts*, p. 407. † *Annales de Chimie*, tom. xvii.
‡ *Cambridge Transactions* 1831, and *Math. Tracts.*

exact accordance with the observed facts; and all the circumstances of the coloured rings in uniaxal and biaxal crystals are completely explained.

The *form* of the rings, or isochromatic curves, depends upon the interval of retardation alone; and the value of this interval had been deduced but approximately. Mr. M'Cullagh has recently given a general and exact method for its calculation, and for the determination of the forms of the rings for any plate of a double-refracting crystal bounded by parallel planes. This method is made to depend upon the properties of the *surface of wave-slowness*, of which I have spoken in another place; and it is found that if the incident ray be produced to meet the sphere, (which is the surface of wave-slowness for air,) and through the point of intersection a perpendicular drawn to the refracting surface, meeting the two sheets of the surface of wave-slowness for the crystal, the intervals of retardation of the rays at emergence will be measured by the thickness of the crystal multiplied by the difference of the corresponding ordinates*. By the aid of an expressive notation for the path of a ray, the author has extended his conclusions to the case of a ray which has undergone any number of internal reflexions.

If the double-refracting energy of the crystal were the same for the light of every colour, the colours of the rings should follow exactly the Newtonian scale of tints, and their magnitudes should observe the same laws as those of the rings formed between two object-glasses. This is the case in carbonate of lime, beryl, and some other crystals; and in these, therefore, the colours are similar to those of thin plates. But many remarkable deviations from this law have been observed by Sir John Herschel and Sir David Brewster. Thus, in the common uniaxal apophyllite, it was observed by the former writer, the diameters of the rings are very nearly *the same* for all the colours of the spectrum; so that the rings of different colours are superposed, and form a succession alternately black and white, which may be traced through a great number of orders †. In this remarkable case, then, the double-refracting energy of the crystal varies very nearly in the subduplicate ratio of the lengths of the waves for the rays of different colours. A very remarkable case

* If $y_i\ y_o\ y_e$ represent the corresponding ordinates of the sphere, and of the two sheets of the surface of wave-slowness for the medium, and θ the thickness of the crystal, $\theta\ (y_o - y_i)$, $\theta\ (y_e - y_i)$ will be the retardations of the two refracted waves at emergence, and $\theta\ (y_o - y_e)$ will be the interval between them.—"Geometrical Propositions applied to the Wave-theory of Light," *Trans. R. I. Academy*, vol. xvii.

† *Phil. Trans*, 1820.

of the *inversion* of the Newtonian scale of tints was observed by Sir John Herschel in some rare varieties of the same mineral. The diameters of the rings, instead of contracting as the refrangibility increases, *enlarge*, and actually become infinite for rays of mean refrangibility. Having passed through infinity, they again acquire a finite value; and diminish as the refrangibility increases up to the extremity of the spectrum. Here, then, for rays of a certain mean refrangibility the crystal is *singly refractive*; and as the double refraction changes its character in passing through zero, the crystal is *positive* for the rays of one end of the spectrum, and *negative* for those of the other *. This singular phenomenon is accounted for on the principles of Fresnel's theory by supposing that the elasticity increases, with the length of the wave, faster in the direction of the axis of the crystal than in the perpendicular direction; so that the difference of these elasticities is positive for the rays of one end of the spectrum, negative for those of the other, and vanishes at some intermediate point.

In biaxal crystals similar deviations take place in the magnitude of the lemniscates corresponding to the different simple colours. But there is here another source of irregularity which is not found in uniaxal crystals. The optic axes vary, in general, with the colour; so that the lemniscates differ also in the position of their poles, and the colours are not the same in different parts of the same ring. Where the optic axes belonging to different colours are in *different planes*, as Sir John Herschel has observed to be the case in borax, the irregularity produced in the coloured curves is yet more striking.

In all the preceding cases, the laws of double-refraction are dependent only on the direction, and are the same all throughout the mass. It is otherwise, however, in many crystals,—such as analcime and some varieties of apophyllite. The complicated arrangement of the coloured bands which these substances display in polarized light, proves them to consist of several distinct portions, possessing different optical properties; and the phenomena indicate relations among the molecular forces, and principles of aggregation, of which it is difficult in some cases even to form a conception. These remarkable phenomena, and their laws, were discovered by Sir David Brewster†.

* *Cambridge Trans.* 1821. Similar properties have been observed by the same author in other crystals, as hyposulphate of lime and vesuvian. From the table of tints exhibited by the latter substance it appears that the most refracted of the two images is the least dispersed.

† *Edin. Trans.*, vol. ix. & x.

When a polarized ray traverses a plate of Iceland spar, beryl, or almost any other uniaxal crystal, in the direction of its axis, it suffers no change of any kind; so that when the emergent ray is analysed by a double-refracting prism, the two pencils into which it is divided are colourless, and one of them vanishes when the principal section of the prism is parallel, or perpendicular to the plane of primitive polarization. But when a ray passes in the same manner through a plate of *rock crystal*, the phenomena are very different. Two images are given in every position of the prism; these images are of complementary colours; and the colours change in the most beautiful manner as the prism is turned round in its cell. These phenomena indicate that the plane of polarization has been changed, and differently for the different rays of the spectrum. They were first observed by M. Arago; and he has given an account of his observations in his memoir on the colours of crystalline plates, read to the Institute in the year 1811.

The subject was then taken up by M. Biot, in a paper published in the *Mémoires de l'Institut*, in the year 1812; and the analysis of the phenomenon was completed in a second memoir read in the year 1818 *. When a polarized ray of any simple colour passes through a plate of rock crystal in the direction of the optic axis, it is still polarized after emergence; but its plane of polarization is changed. The angle through which the plane is made to revolve, varies with the colour of the light, and with the thickness of the plate,—being proportional to that thickness divided by the square of the length of the fit or wave. In some crystals the plane of polarization revolves from left to right, while in others it is turned in an opposite direction; and the crystals themselves are denominated *right-handed* or *left-handed*, according as they produce one or other of these effects. When two plates are superposed, the effect produced is, very nearly, the same as that due to a single plate whose thickness is the *sum* or *difference* of the thicknesses of the two plates, according as they are of the same or of opposite denominations.

This curious distinction between plates cut from different crystals has been connected by Sir John Herschel with a corresponding diversity in the crystalline form. The ordinary form of the crystal of quartz is the six-sided prism terminated by the six-sided pyramid. The solid angles formed at the junction of the pyramid and the prism are sometimes replaced by small secondary planes, which in the same crystal lean all in the

* "Mémoire sur les Rotations que certaines substances impriment aux axes de polarisation des Rayons lumineux."

same direction; and it is found that when that direction is *to the right*, (the apex of the pyramid being uppermost,) the crystal is *right-handed*; and that on the contrary it is *left-handed*, when the planes lean in the opposite way *. Sir David Brewster has shown that the amethyst, or violet quartz, is actually composed of alternate layers of right-handed and left-handed quartz. It is to the *cropping out* of the edges of these layers, that the undulating appearance peculiar to the fracture of this mineral is owing. The structure itself is displayed in the most beautiful manner in polarized light †.

Some liquids, and even gases, have been found by MM. Biot and Seebeck to possess the same property as quartz, though in a much feebler degree; and to impress a rotation on the plane of polarization of the intromitted ray, which is proportional to the thickness of the substance traversed. These liquids do not lose their rotatory power by dilution with other liquids not possessing the property. They retain it even when raised to the state of vapour; and, in general, the rotatory power is independent of the mode of aggregation, provided the molecular constitution is unchanged. Lastly, when two or more liquids possessing this property are mixed together, the rotation produced by the mixture is the sum of the rotations produced by the ingredients, in thicknesses proportional to the volumes in which they are combined. From these and other facts M. Biot concludes that the property of rotatory polarization is inherent in the ultimate particles of bodies, and does not depend on their mutual distance or arrangement ‡. On the other hand, quartz is found to lose the property when deprived of its crystalline structure. Thus Sir John Herschel observed that quartz held in solution by potash did not possess the property; and the same thing has been remarked by Sir David Brewster with respect to fused quartz.

The phenomena of rotatory polarization in rock crystal, M. Biot ascribed to a continued rotation of the molecules of light round their centres of gravity, produced by the operation of some unknown forces. Fresnel has proved that they arise from the interference of *two circularly-polarized pencils* which are propagated along the axis with unequal velocities, one revolving

* *Cambridge Trans.*, vol. i. † *Edin. Trans.*, vol. ix.

‡ M. Biot has recently extended his researches on this subject to a great variety of substances, *Annales du Museum d'Histoire Naturelle*, tom. ii. In a memoir read to the French Academy last year he has applied the laws of circular polarization to the analysis of the process of vegetation in the grasses; and he has shown, in general, the importance of the indications drawn from these phenomena in the researches of organic chemistry. *Institut*, Nos. 1 & 9.

from left to right, and the other in the opposite direction. A plane-polarized ray, in fact, is equivalent to two circularly-polarized rays of half the intensity, in one of which the vibrations are from left to right, and in the other in the opposite direction. When a plane-polarized ray, therefore, is incident perpendicularly upon a plate of rock crystal, cut perpendicularly to the axis, it may be resolved into two such circularly-polarized rays. These are supposed to be transmitted with different velocities; so that when they assume a common velocity at emergence, one of them is in advance of the other. They then compound a single ray polarized in a single plane; and this plane, it can be shown, is removed from the plane of primitive polarization through an angle proportional to the interval of retardation of the two pencils, and therefore measured by the thickness of the crystal. But this interval varies also with the colour of the light; and we are obliged to suppose that it is the same for a *given number* of waves, whatever be their length,—so that, for a given thickness of the crystal, it varies inversely as the length of a wave. From this supposition it will follow that the deviation of the plane of polarization of the emergent ray is inversely as the square of that length, agreeably to the experimental results of M. Biot*.

The laws of rotatory polarization are then completely explained; and it only remained to prove the truth of the hypothesis,—that two circularly-polarized pencils, whose vibrations are in opposite directions, will actually be transmitted along the axis of quartz with different velocities. This supposition is easily put to the test of experiment; since such a difference of velocities must give rise to a difference of refraction, when the surface of emergence is oblique to the direction of the ray. According to the hypothesis, therefore, a plane-polarized ray, transmitted through a prism of rock crystal in the direction of the optic axis, should undergo *double refraction* at emergence; and the two pencils into which it is divided should be *circularly-polarized*. This has been completely verified by Fresnel, by an achromatic combination of right-handed and left-handed prisms arranged so as to double the separation; and he has shown that the two pencils are neither common nor plane-polarized light, but possess all the characters which are impressed upon a polarized ray by two total reflexions from glass at an angle of about 50°.

The refraction of quartz, then, in the direction of its axis is wholly different from that of every other known crystal. In

* *Annales de Chimie*, tom. xxviii. p. 147.

other directions, the two pencils into which a single ray is divided, were supposed to obey the ordinary laws, and to be plane-polarized in opposite planes. This supposition has been overturned by Professor Airy *, and it has been shown that the two pencils in quartz are, each of them, *elliptically-polarized*; the elliptical vibrations of the two rays being in opposite directions, and the greater axes of the ellipses being in the principal plane, and perpendicular to it respectively. The ratio of the axes, in these ellipses, is the same in the two rays †; but varies with their inclination to the optic axis,—being a ratio of equality when the direction of the ray coincides with the axis, and increasing indefinitely with their inclination to that line according to some unknown law. As to the course of the refracted rays, Professor Airy finds that it is still determined by the Huygenian law; but that the sphere and spheroid, which determine the velocity and direction of the two rays, *do not touch*, as in all other known uniaxal crystals, the latter surface being contained entirely within the former. This position is certainly a startling one. The two sheets of the wave-surface being thus absolutely separated, there is a complete interruption of continuity in passing from the velocity of one ray to that of the other; a result which does not hold in any other case with which we are acquainted. It is however necessary to the explanation of the phenomena; for the interval of retardation does not vanish with the inclination of the ray to the axis. Professor Airy has given an elaborate calculation founded on these hypotheses, of the forms of the rings, &c.—displayed by quartz in plane-polarized, and circularly-polarized light, and in any position of the analysing plate; and he has found the most striking agreement between the results of calculation and those of observation.

We yet want a mechanical theory which will account for the peculiar form of the wave-surface just alluded to. Fresnel seems to have thought that the difference of the velocities of the two rays in the direction of the axis might be physically explained

* "On the Nature of the Light in the two rays produced by the double refraction of Quartz," *Cambridge Transactions* 1831.

† In the Supplement to this paper Professor Airy has explained a highly ingenious method of determining experimentally the relation between the ellipticity and the direction of either of the rays. This method depends upon a remarkable effect which he had been led to expect from theory;—namely, a sudden change of half an undulation in the interval of retardation, and therefore a change of *half an order* in the rings, when the incident light is elliptically polarized. From the results of some experiments conducted in this method Professor Airy seems to think that the ratio of the axes in the ordinary ray approaches more nearly to one of equality, than in the extraordinary ray.

by an helicoidal arrangement of the molecules of the vibrating medium, which will have different properties according as the helices are right-handed or left-handed. But this hypothesis can hardly be supposed to apply to the case of fluids, in which the property of circular polarization is independent of direction; and we are driven to confess that, with respect to these important laws, physical theory is as yet wholly at fault. The singular relation between the interval of retardation and the length of the wave seems to afford the only clue to the unravelling of this difficulty.

The phenomena of depolarization and of colour, impressed by double-refracting substances upon the transmitted light, are, we have seen, the necessary results of the interference of the two pencils into which the light is divided within them. These properties, then, enable us to discover the existence, and to trace the laws of double-refraction, even in substances in which the separation of the two pencils is too minute to be directly observed. By such means the important discovery has been made, that a double-refracting structure may be communicated to bodies which do not naturally possess it, by *mechanical compression* and *dilatation*. Sir David Brewster observed that when pressure was applied to the opposite faces of a parallelopiped of glass, it developed a tint in polarized light, like a plate of a double-refracting crystal; and the tint descended in the scale as the pressure was augmented. Singly-refracting crystals, such as muriate of soda and fluor spar, acquired the properties of double refraction by the same means*. All this is in perfect accordance with the wave-theory. Owing to the connexion of the vibrating medium with the solid in which it is contained, its elasticity is rendered unequal in different directions by the effect of compression, the maximum and minimum corresponding to the directions of greatest and least pressure. Accordingly, the vibrations of the ray on entering the plate are resolved into two in these rectangular directions, and these are propagated with unequal velocities; the colour developed is determined by the interval of retardation. These results of theory were experimentally confirmed by Fresnel, by the method of interferences; and it was found that the velocity with which a ray traversed the glass was greater or less, according as it was polarized parallel or perpendicular to the axis of compression. The bifurcation of the ray at oblique incidences is a necessary consequence of this difference of velocities; but this was also shown by Fresnel by direct experiment. A series of

* *Phil. Trans.* 1815 and 1816.

glass prisms were placed together with their refracting angles alternately in opposite directions, and the ends of the alternate prisms powerfully pressed by screws. A ray transmitted through the combination was found to be divided into two oppositely polarized *.

The opposite effects of compression and dilatation may be seen in a thick plate of glass which is bent by an external force. When this body is interposed between the polarizing and analysing plates, so as to form an angle of 45° with the plane of primitive polarization, two sets of coloured bands are seen separated by a neutral line; and these vanish altogether when the compressing force is withdrawn. By crossing the glass with a plate of mica or sulphate of lime, Sir David Brewster found that the parts towards the convex, or dilated side of the neutral line, had acquired a positive double-refracting structure, and those at the concave, or compressed side, a negative one †. The intimate connexion between the double-refracting property, and the internal state of the body as to condensation or rarefaction, is likewise proved by the curious observation of M. Biot, —that glass, when in a state of sonorous vibration, possesses the power of depolarizing the light.

In these cases of induced double refraction, the phenomena are related to the form of the entire mass; and are essentially different from those produced by regular crystals, in which the law of elasticity and of double refraction depends solely on the *direction*, and is the same in all parts of the substance. Sir David Brewster has lately succeeded in communicating a *regular* double-refracting structure to a mixture of resin and white wax, by pressing it into a thin film between two plates of glass. This film had a single axis of double refraction at every point in the direction of the axis of pressure; and the tint developed depended solely on the inclination of the ray to this line. Sir David Brewster has drawn from this phenomenon some highly interesting conclusions respecting the origin of double refraction in regular crystals. He mentions several facts which seem to prove that this property is not inherent in the molecules themselves; and he conceives that it is developed by the unequal pressure caused by the forces of aggregation, which are in general different in the direction of three rectangular axes. Thus the double-refracting properties and the crystalline form are referred to the same agency ‡.

Sir David Brewster and Dr. Seebeck had before observed the

* *Annales de Chimie*, tom. xx.

† *Phil. Trans.* 1816.

‡ *Phil. Trans.* 1830.

phenomena arising from unequal condensation and rarefaction in the case of uncrystallized bodies unequally heated. These phenomena may be studied by applying a bar of hot iron to the edge of a rectangular plate of glass, and placing it in the polarizing apparatus, so that the heated edge may form an angle of 45° with the plane of primitive polarization. At the end of some time the whole surface of the plate is observed to be covered with coloured bands, the parts near the opposite edges having acquired a positive double-refracting structure, and those near the centre a negative one. The effects are reversed when a plate of glass uniformly heated is rapidly cooled at one of its edges; and all the appearances vanish when the glass acquires the same temperature throughout*. These phenomena may be endlessly varied by varying the form of the glass to which the heat is applied. If now, by any means, the glass be arrested in one of these transient states, it will have acquired a permanent double-refracting structure. This has been accomplished by M. Seebeck by raising the glass to a red heat, and then cooling it rapidly at the edges. As the outer parts, which are thus more condensed, assume a fixed form in cooling, the interior parts must accommodate themselves to that form, and therefore retain a state of unequal density. The law of the change of density, and therefore the double-refracting structure, will depend on the external form; and M. Seebeck found, accordingly, that the coloured bands and patches which such bodies display in polarized light, assume a regular arrangement which varies with the shape of the mass†. The laws of these phenomena have been completely analysed by Sir David Brewster; and he has shown that the colours are those of crystallized plates, the direction of the axes however being different in different parts of the substance.

As the double-refracting structure is communicated to bodies which do not possess it naturally, by mechanical compression or unequal temperature,—so, by the use of the same means, that structure may be altered in the bodies in which it already resides. Thus Sir David Brewster and M. Biot have found that the double refraction of regular crystals may be altered, and the tints they display made to rise or descend in the scale, by simple pressure. But the changes induced by heat are yet more remarkable. Professor Mitscherlich discovered the important fact, that, in

* *Phil. Trans.* 1816.

† The experiments of M. Seebeck are recorded in *Schweigger's Journal*, 1814. The depolarizing property of unannealed glass seems to have been first noticed by M. Arago; and was afterwards studied by Sir David Brewster in glass which had been melted and cooled in water.—*Phil. Trans.* 1814.

general, heat dilates crystals differently in different directions, and so alters their form; and their double-refracting properties have been found to undergo a corresponding change. Thus Iceland spar is dilated by heat in the direction of its axis; while it actually *contracts* by a small amount in directions perpendicular to it. The angles of the primitive form thus vary, the rhomboid becoming less obtuse*, and approaching the form of the cube. M. Mitscherlich, accordingly, conjectured that its double-refracting energy must in these circumstances be *diminished*; and the conjecture was fully verified by experiment. This inquiry has been followed up by M. Rudberg; and the effects of heat on the refractive indices of double-refracting crystals examined by the direct method of prismatic refraction. In conformity with the observations of M. Mitscherlich, it was found that the extraordinary index in Iceland spar increased considerably with the temperature, while the ordinary index underwent little or no change. In rock crystal, on the other hand, both indices *diminished* as the temperature augmented, and nearly by the same amount. In arragonite a similar effect was produced on the three principal refractive indices,—the least index, however, undergoing the smallest proportionate diminution†.

The inclination of the optic axes, in biaxal crystals, is a simple function of the elasticities of the vibrating medium in the direction of three rectangular axes; and the plane of the optic axes is that of the greatest and least elasticities. If, then, these three principal elasticities be altered by heat in different proportions, the inclination of the axes will likewise vary; and if, in the course of this change, the difference between the greatest elasticity and the mean, or between the mean and the least, should vanish and afterwards change sign, the two axes will collapse into one, and finally open out in a plane perpendicular to their former plane. All these variations have been actually observed. Professor Mitscherlich found that, in sulphate of lime, the angle between the axes (which is about 60° at the ordinary temperature) diminishes on the application of heat; that, as the temperature increases, these axes approach until they unite; and that, on a still further augmentation of heat, they again separate and open out in a perpendicular plane. The primitive form of the crystal undergoes a corresponding change, the dilatation being greater in one direction than in another at right angles to

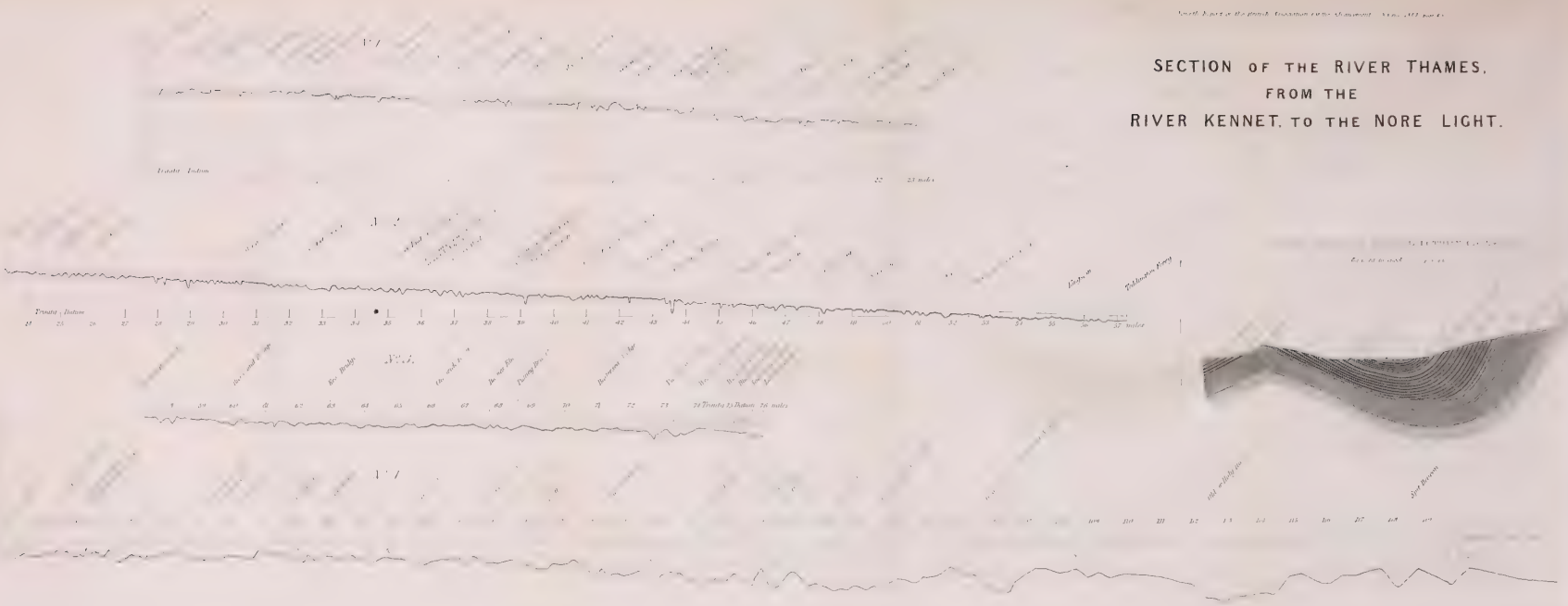
* A change of temperature, from the freezing-to the boiling-point, produced a change of $8\frac{1}{2}'$ in the dihedral angles at the extremity of the axis.—*Bull. Soc. Phil.*, March 1824.

† *Phil. Mag.*, Third Series, vol. i. 409.

it. Sir David Brewster has observed an analogous, and even yet more remarkable property, in glauberite. At the freezing temperature this crystal has two axes for all the rays of the spectrum, the inclination of the axes being greatest in red light and least in violet. As the temperature rises the two axes approach, and those of different colours unite *in succession*; and at the ordinary temperature of the atmosphere, the crystal possesses the singular property of being *uniaxal* for violet light and *biaxal* for red. When the heat is further increased, the axes which have united open out in order, and in a plane at right angles to that in which they formerly lay; and at a temperature much below that of boiling water, the planes of the axes for all colours are perpendicular to their first position*. The inclination of the optic axes in topaz, on the other hand, *augments* with the increase of temperature; and the variation, M. Marx has observed, is much greater in the coloured than in the colourless varieties of this mineral†.

* *Edin. Trans.*, vol. xi.; and *Phil. Mag.*, Third Series, vol. i. 417.
† *Jahrb. der Chemie*, vol. ix.

SECTION OF THE RIVER THAMES,
FROM THE
RIVER KENNET, TO THE NORE LIGHT.



Report on the Progress and present State of our Knowledge of Hydraulics as a Branch of Engineering. Part II. *By* George Rennie, *Esq., F.R.S., Acad. Reg. Sc. Turin. Corresp., &c. &c.*

My former paper was confined to a brief elucidation of the progress and present state of that branch of hydrodynamics which relates to the motions of fluids through orifices, tubes, and artificial channels. My object was to combine together the experiments which had been made by different philosophers (from Castelli and Galileo down to Lesbros and Poncelet,) to determine the effective expenditures from orifices of different kinds, as well as the remarkable phænomena exhibited by the motions of fluid veins. In the first instance, with regard to orifices, it was shown, That the law of Torricelli relative to bodies gravitating in free space, (and which applies to all fluids,) requires certain modifications which diminishes their expenditure as compared with the areas of the orifices nearly one half, and the diameter of the fluid vein after it issued from the orifice nearly three fourths;—That the form of the orifice (with equal areas) had little influence over the expenditure:

2ndly, That although an increase of expenditure (considerably above the expenditures by simple orifices of equal magnitude,) is found to take place through additional or cylindrical tubes, equal in length to three times the diameter of the orifice, yet very little variation exists in their coefficients;—That the same is true of conical or divergent ajutages, but with greater expenditures:

3rdly, That the expenditures by *incomplete* orifices, *i. e.* through rectangular notches in dams, (and which form a particular case of simple orifices,) follow the parabolic law, with a coefficient of two thirds of the orifice:

4thly, That the expenditures by rectilinear and curvilinear pipes follow more complicated laws, which can only be represented by a certain portion of the height or inclination of the column; and that the various formulæ which have been applied to the determination of these questions approximate very nearly.

The same may be said of the formulæ made applicable to the motions of fluids in artificial and natural channels; and to other cases of orifices when influenced by variable pressures, as in

locks and sluices, &c.; or when the motions are rendered complicated by a system of pipes, as in water-works. The influence of friction* and adhesion was briefly alluded to. Friction was stated to have been found to be nearly independent of pressure and surface, being the same when the fluid was made to run over different kinds of surfaces, such as glass, wood, and metals, &c.; and with regard to the velocity, resistances were stated to be as the squares of the velocities at moderate inclinations. Lastly, as to adhesion, reference was made to the experiments of Coulomb and others to determine the molecular action of fluids.

The theory of the motions of canals and rivers is founded on the following axioms:

1st, That the fluidity of the water, which obliges it to maintain a perfectly level surface in any close vessel containing it, allows it to move with the utmost facility in any direction, under the influence of external impulse or gravity;

2ndly, That motion cannot take place in an artificial channel without an inclination in the surface;

* From the following experiments with which I have been favoured by William Tierney Clarke, Esq. of the West Middlesex Water-works, the friction and resistance of the pipes to the free motion of the water through them have been found to be between one fourth and one fifth of the total height of the column.

Table showing the result of Experiments made at the West Middlesex Water-works upon work performed by the 64-inch cylinder Engine by Messrs. Boulton, Watt, and Co., working with coal as stated below, to Barrow Hill Reservoir, being an elevation of 188 feet above low water of the river Thames, and working through the 15-inch, 12-inch, and 9-inch Mains.

Name of Coal.	Pounds raised 1 foot high per cwt. of coal consumed, exclusive of friction and obstructions in pipes, calculated at 188 feet. 112 lbs.	Pounds raised 1 foot high per bushel of coal consumed, exclusive of friction and obstructions in pipes, calculated at 188 feet. 84 lbs.	Pounds raised 1 foot high per cwt. of coal consumed, including all resistance in pipes, calculated at 42 feet—equal to 230 feet head. 112 lbs.	Pounds raised 1 foot high per bushel of coal consumed, including all resistance in pipes, calculated at 42 feet—equal to 230 feet head. 84 lbs.
1833. Wylam Moor..	41,406,202	31,054,651	50,656,523	37,992,391
1834. Hollywell Main	44,625,373	33,469,029	54·594·871	40,946,153
— Ditto.....	45,094,274	33,820,705	55,169,560	41,377,170
— Ditto.....	44,338,876	33,254,157	54,242,241	40,681,680
— Wylam Moor..	41,046,265	30,784,698	50,213,782	37,660,33

3rdly, That when the mean velocity of a current is uniform, the accelerating force is equal to the retardations;

4thly, That as the particles of the fluid fill up the cavities of the surface and form their own fluid bed, the nature of the surface makes very little difference in the retardation.

The inclination of the surface, or the force of gravity, being the cause of motion in the whole mass, it follows that when the motion has become uniform there pass equal quantities of water through each section in equal times and velocities; hence the depth of the water will be the same in every part of the section, and the surface will be parallel to the bottom of the channel.

But the motion would never be uniform if the fluid were perfect and there were no resistance in the channel: on the contrary, it would follow the laws of acceleration. But experiment proves, even with very great inclinations, that the motion soon becomes uniform: hence the conclusion as a general principle, that when water moves uniformly in any channel or conduit, "the resistance which it experiences is equal to the force of acceleration*."

If we examine the motion of a stream of water running in an open channel, we shall find that the instant the water enters into the channel, it spreads along the bed in filaments, which continue to precede the general mass until they are retarded by the resistances of the bed and overtaken by the superior filaments as they reach the termination of the channel, and being no longer retarded by the friction of the bottom and sides, they acquire a greater velocity, and thus produce inequalities in the motions of different parts of the section of the stream; and experiment teaches us that every mass of water, whether it moves in a pipe, open channel, or river, follows more or less the same law; and since the resistance is caused by the surface of the channel, the greater the extent of surface, the greater will be the resistance†, and *vice versâ*. Hence the effect of the resistance will be in the inverse ratio of the section; and as adhesion forms a certain portion of the resistance, and has been proved by Coulomb to be simply proportional to the velocity, it follows that the resistance which water experiences in moving in an artificial channel is proportional to the quantity of wetted

* Bossut found that when an artificial canal of wood 200 metres in length was inclined in the proportion of one decimetre per metre, or one in ten, and divided the length into 33 metres, each of the spaces, with the exception of the first, was run over in the same number of seconds.

† This has been found by me to be very partial.

surface, and the square of the velocity, plus a small fraction of the velocity, and is in the inverse ratio of the section *.

In the preceding determinations we have supposed the water to have an uniform motion; that is to say, that each section of the fluid mass presents the same expenditure and velocity, and consequently the same depth of water. M. Bélanger has taken into consideration a motion in canals of a different kind; it is that in which the fluid mass gives the same expenditure through its sections, but has not the same depth throughout, nor with its surface parallel to the bottom. There are examples in canals where the length is insufficient to produce uniformity of motion at the commencement and extremity; likewise, where the breadth and inclinations are unequal: but it is essential to the theory of M. Bélanger that these variations should take place insensibly. By admitting the hypothesis of the parallelism of filaments being perpendicular to the canal, this engi-

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* *Formula of Motion.*

Let $g\,p$ be the force of acceleration,
A and B the two constant coefficients,
A' the constant multiplier,
$B\,v$ the friction of the velocity assumed,
c the wetted perimeter of the section,
$\frac{c}{s}$ the mean radius or relation of the area to the wetted perimeter of the section;

then $A'\frac{c}{s}(v^2 + B\,v)$ will be the expression of the resistance.

Then, adopting the principle of the resistance to be equal to the force of acceleration $g\,p$, we shall have the following equation, viz.

$$g\,p = A'\frac{c}{s}(v^2 + B\,v), \text{ or } p = A\,\frac{c}{s}(v^2 + B\,v),$$

by making $\frac{A'}{g} = A$ for a portion of the canal taken when the motion is uniform, of which $l =$ the length. P' being the absolute inclination, we shall have $p = \frac{p'}{l}$ and $P' = A\frac{c\,l}{s}\,(v^2 + B\,v)$; or, taking the whole extent of the canal, L being the length, P the total inclination, (from which inclination must be deducted the height due to the velocity v of the uniform motion,) the equation will then be $P - \frac{v^2}{2g} = A\,\frac{C\,L}{S}\,(v^2 + B\,v)$. We must now determine the two constant coefficients A and B.

Then, taking Eytelwein's results from 91 experiments made on different canals and rivers, in which the velocity varies from 0·124 metre to 2·42 metres, and the fluid section from 0·014 metre to 2·604 metres, it follows that

$$A' = 0{\cdot}0035855, \text{ or}$$
$$A = 0{\cdot}00036554,$$
$$B = 0{\cdot}06638.$$

So that, putting g for the numerical value, the fundamental equation of the mo-

neer* arrives at an equation which expresses the relation of the expenditure, the length, the section, and the depth of the water in the canal under consideration; by a series of very complicated calculations, M. Poncelet has arrived at similar results. But uniformity of motion cannot take place when the surface of the water is not of the same inclination as the bottom of the canal, as, for example, when the bottom is horizontal, or the declivity is contrary to the current: it was therefore very important to establish the distinction between the two kinds of regimen, and only to regard the uniform regimen as a modification of the permanent regimen; that is, it was necessary to find a general formula which should represent all the

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tion of water in canals becomes $p = 0{\cdot}00036554 \frac{c}{s} (v^2 + 0{\cdot}0664 v)$; that is, if $v = \frac{Q}{S}$, Q being the expenditure,

$$p s^3 = 0{\cdot}00036554 c (Q^2 + 0{\cdot}0664 Q S);$$

for the expression of the velocity, $v = -0{\cdot}0332 + \sqrt{2736 \frac{p s}{c} + 0{\cdot}011}$;

for the expenditure, $Q = S(-0{\cdot}0332 + \sqrt{2736 \frac{p s}{c} + 0{\cdot}0011})$;

or, what is sufficiently near, $Q = S(\sqrt{2736 \frac{p s}{c}} - 0{\cdot}0332)$.

In great velocities, where the resistance is simply proportional to their squares, we have

$$v = 51 \sqrt{\frac{p s}{c}} \text{ and } Q = 51 S \sqrt{\frac{p s}{c}}.$$

These formulæ might be illustrated in practice in the method adopted by Messrs. Prony, Girard, and d'Aubuisson. See *Traité d'Hydraulique*, 8vo, Paris, 1834, wherein several cases, such as the breadth, height and form of the channel which gives the greatest expenditure, are determined;—the circle, the semicircle, and segment of a circle; afterwards the regular half-polygons, the regular semi-hexagon, the semi-pentagon, and the semi-square.

But as many of these figures are inadmissible in practice for canals, we must adopt a trapezium, with its smallest base for the bottom, and having its sides inclined to angles of about 34°, but which form will be, however, obliterated in the angles in time by deposits, and present a concave bottom.

By preserving the slope at 2 to 1, or n,
the mean velocity, $l + n h = 2h$; l being the length,
h = depth or height;

and consequently $s = 2h^2$ and $c = 2h - n h + 2 h \sqrt{n + 1} = n' h$,

by making $2 - n + 2\sqrt{n^2} + 1 = n'$. These values will form the equation $p h^6 = 0{\cdot}00004569 n' (Q^2 h + 0{\cdot}133 Q h^3)$, from whence we can deduce h.

In the case of rectangular canals moving through an aqueduct or rock, the breadth ought to be double the depth, and consequently $\sqrt{\frac{2Q}{v}}$.

* *Essai sur la Solution Numérique de quelques Problèmes relatifs au Mouvement permanent des Eaux Courantes:* par M. Bélanger.

circumstances of the permanent motion of running streams. This M. Bélanger has done, in adopting the following hypotheses.

He first supposes the current, when in a state of permanent regimen, to be divided into plates perpendicular to the fluid filaments, and the velocity of the particles of the water to be constantly the same in the same and every plate:

Secondly, That each particle of water moves in a right line, so that the centrifugal motion generated by the curvilinear motion (if there be any) may be neglected:

Thirdly, That, although the analysis which follows cannot apply but to cases where the dimensions of the section vary in very small quantities in proportion to the length, the velocity of each particle may be considered to be perpendicular to the plate it traverses, neglecting the transversal velocities which exist as soon as the section varies from one plate to the other.

Of these hypotheses, the first, namely, the motion of the current by parallel plates, is never realized in nature, because the resistance which the periphery of the canal opposes to the motion of the current is transmitted to the adjacent filaments, and so on to the central filament, which moves the quickest; but in general the velocities of the different filaments differ very little, and may be safely represented by a mean velocity, which is the quotient, or the volume of water expended in a given time, divided by the area of the section, as before stated. As soon as this compensation is admitted, each fluid filament may be considered as retarded by a force equivalent to friction. M. Girard* has applied this idea to the formation of an equation to represent the motion of the current, but with the supposition that the two powers of the velocity are represented by the same coefficients; a supposition which cannot apply to every case. M. Prony has shown that the function which expressed the retarding force may be represented by

$g \frac{x}{w} (a v + b v^2)$, in which the metre is taken for unity and the second for the time:

$g' = 9^{m}\ 8088$ represents the accelerating force of gravity:

w = the area of the transverse section to which belongs the particle under consideration:

x = the length of the wetted perimeter of the section:

v = the mean velocity supposed to be common to all the particles which traverse the section:

* *Rapport sur le Projet général du Canal de l'Ourcq:* par P. S. Girard. Paris, 1803.

a and b, two constant quantities to be determined by experiment*.

From these, and additional notations, M. Bélanger arrives at a general expression for the *permanent* and *uniform* motion of the current similar in some respects to that of M. Prony, whose formula for canals is

$$av + bv^2 = \frac{w\varepsilon}{\lambda x}.$$

The values of a and b, which represent the constant coefficient of the first and second powers of the velocity, were determined by M. Prony from thirty experiments deduced from the mean and superficial velocities of the current:

$$\text{thus } a = 0{\cdot}000444499$$
$$b = 0{\cdot}0003093140\dagger.$$

Eytelwein has exactly followed M. Prony, but from a greater number of experiments.

$$a = 0{\cdot}000242651$$
$$b = 0{\cdot}0003655430.$$

The difference between these respective coefficients does not much affect the results for calculations in the formulæ for the relations between the velocity of the water in a canal, and its length, inclination, and section, as they appear in M. Prony's Five Tables‡.

If the form of the channel and the volume of water expended remain the same, uniformity of motion can only take place when the canal has one inclination and a proportionate depth throughout its length.

The true principle, therefore, for regulating the inclination of a canal, consists in establishing a relation between the ordinates of the height and the horizontal distances, always considering that for every volume of water contained in a given inclination, there must be a depth corresponding to the uniform regimen. M. Genieys§, who has investigated these principles with a view to render them applicable to canals and aqueducts, has endeavoured to find the velocity best suited to the nature of the soil, but always with reference to the velocity necessary to maintain the salubrity of the water, which has been determined to be 35 centimetres, or about 13¾ English inches, per second for the minimum velocity; whereas M. Girard adopted a less velocity in the canal de L'Ourcq. M. Girard at first proposed to lay out the inclination

* *Recherches Physiques et Mathématiques sur la Théorie des Eaux Courantes:* par M. de Prony. Paris, 1804.

† *Recueil de cinq Tables:* par M. Prony. Paris, 1825.

‡ *Mémoires de l'Académie de Berlin* (Années 1814 et 1815).

§ *Essais sur les Moyens de conduire, d'élever et de distribuer les Eaux:* par M. Genieys.

according to the law represented by the coordinates of a funicular curve, by which the upper part of the canal had an inclination of $0^{m}\cdot0000625$ per metre, and the lower part of $0^{m}\cdot0001236$ per metre; but these inclinations were found insufficient. M. Genieys prefers one decimetre per kilometre, or $\frac{1}{10000}$. Dubuat is inclined to think that the smallest inclination capable of maintaining the mobility of water is $\frac{1}{1000000}$, but that at $\frac{1}{300000}$ it is barely perceptible; and in an artificial channel set to $\frac{1}{9288}$ he found the mean velocity to be nearly 6 inches per second, and 7 inches per second in a drain near Condé, of which the inclination was $\frac{1}{27000}$, and 10 inches per second in the river Hayne, with an inclination of $\frac{1}{3300}$*. M. Bossut found the motion to cease entirely with pipes having less inclinations than those of Dubuat.

M. Dubuat has given the results of seventeen experiments on the mean and superficial velocities and radius of a trapezium canal set at inclinations of $\frac{1}{212}$ to $\frac{1}{432}$, also the same results from fifteen experiments made in a rectangular canal set at inclinations of from $\frac{1}{438}$ to $\frac{1}{27648}$, also of four experiments on the superficial and mean velocities of the river Hayne, all of which he finds to accord with very nearly the theory laid down by him.

The Romans inclined their canals much more than the moderns. Vitruvius fixed the inclination at $\frac{1}{200}$, Scamozzi at $\frac{1}{300}$.

From different observations made on the ancient aqueducts by M. Rondelet†, he found the mean inclination to be $1\frac{1}{3}$ line per French toise, or about $\frac{1}{650}$, towards the lower part, $1\frac{1}{2}$ line, or $\frac{1}{570}$, towards the upper part. More recent experiments have made the inclination from $\frac{1}{432}$ to $\frac{1}{648}$.

According to M. Prony, the following are some of the inclinations of the canals and rivers of the Pontine Marshes.

	Metres.	Low Water. Metres.	High Water. Metres.
Canal of Pius VI., in two lengths of	17,677	4·811000	4·603000
Inclination in unity of the length		0·000272	0·000260
Second length	9,112	0·619000	1·830000
Inclination in unity of surface		0·000068	0·000091

* The drains in Lincolnshire are inclined at 5 inches to a mile, or $\frac{1}{12672}$. The slope of the New River is 3 inches per mile, or $\frac{1}{21120}$. The slope of the Eau Brink Cut in Norfolk is 5 inches per mile. The slope of the New Cut of the Nene at Cross Keys Wash, in Lincolnshire, is about 4·9 inches per mile.

Of the inclinations of the Caer and Foss Dykes, originally constructed by the Romans, we have no positive information; but from $\frac{1}{10000}$ to $\frac{1}{12000}$ seems to be a fair average for the inclinations of the drains in low countries; and on straight canals, such as the Thames and Medway, we have seen the effects of the wind in raising the surface higher at one extremity than the other equal to $1\frac{1}{2}$ inch per mile.

† See *Commentaire de Frontinus sur les Aqueducs de Rome*, par Rondelet: Paris, 1820.

The Ninfa, which runs into the above, inclines from	0·012466	to 0·000090
The Uffente inclines from	0·000095	0·000049
	0·000410	0·000420
The Amaseno 1st length	0·001751	0·001305
The Amaseno 2nd ditto	0·000636	0·001152
The Amaseno 3rd ditto	0·000665	0·000905
Canal of Terracina, in a length of 3·728 inches, inclines	0·000197	0·000141
Canal of Botte . . . . ditto	0·000187.	

Similar examples might be quoted of the Pedicata, the Scaravazza, &c.

These inclinations vary according to circumstances, but in general they may be taken from $\frac{1}{2000}$ to $\frac{1}{8000}$.

M. Prony, in the 8th chapter of his Report, proposes to distribute the inclination of the rivers or drains so as not to corrode their channels; and this is effected by a series of planes or falls, from 0·0005035 to 0·0002879; as an example he cites the marshes of Bourgoin in France *.

M. Prony proposes to accommodate the high and low waters by forming the channel into a double set of trapeziums, so that in times of flood the waters will have liberty to spread above the lower banks; and being confined between the higher or external banks, the capacity but not the velocity will be increased. This is precisely the mode adopted in the Eau Brink and Nene Cuts by the late Mr. Rennie, and by the Italians in the embankment of the Po and other rivers.

According to Deschales†, if the depth and quantity of water in a river or canal be considerable, it will suffice in the part nearest the mouth to allow a declivity of one foot perpendicular in from 6000 to 10,000 feet in horizontal extent, above which the declivity must be slowly and gradually increased, as far as the current is made navigable, to 1 foot perpendicular in 4000 feet horizontal.

Riccioli partly confirms this statement with regard to the mouth of the river Po. The mean declivity of several of the canals and rivers in Flanders was found by the Abbé Mann to be from $\frac{1}{3280}$ to $\frac{1}{6000}$, and of the Lys, near Ghent, by M. Brisson, one in $\frac{1}{2000}$. According to the observations of the Abbé Chappe D'Auteroche, M. Nollet and MM. Cassini‡, the height of the

* *Description Hydrographique et Historique des Marais Pontins*, &c.: par M. Prony. Paris, 1818, de l'Imprimerie Royale.

† *De Fontibus et Fluviis*, Prop. 49.

‡ *Mémoires de l'Académie Royale des Sciences pour* 1730.

Seine at Paris above the level of the sea is 127 French feet, which divided over the length of that river to Havre is one in 4252. By similar observations made on the river Loire by MM. Picard and Pitot, the declivity in proportion to its length was found to be $\frac{1}{3174}$.

The Rhone gives the proportion $\frac{1}{2620}$, which is double the mean declivity of the rivers in Flanders.

On the mean Velocity of Water running in artificial Canals.

We have seen, that the resistance of the sides of the channel causes a diminution in the velocity of the water which is communicated to parts remote from the periphery; from which it follows that when the section is a semicircle the greatest velocity is in the middle of the surface; and that in a channel of any other shape this maximum velocity is in the most distant point from the periphery; and that, *vice versâ*, the velocity decreases towards the periphery. A knowledge of this progression has always been considered of great importance, and many experiments have been made for that purpose.

Dubuat has perhaps made the most accurate experiments on the subject; and having performed them on a scale of considerable magnitude, he concluded that the relation between the velocity at the surface and the bottom was independent of the depth, and greater in proportion as the velocity was smaller; he observed also, that the mean velocity is a mean proportional between the superficial and bottom velocity, that is, calling

v the velocity at the bottom,
V the velocity at the surface,
u the mean velocity,

the result of these observations may be represented by the equations

$v=(\sqrt{V}-0{\cdot}165)^2$ and $u=\frac{1}{2}(V+v)=(\sqrt{V}-0{\cdot}082)^2+0{\cdot}00677$.

M. Prony*, in discussing these observations of Dubuat, adopts $uV=\frac{V+2{\cdot}372}{V+3{\cdot}153}$; thus,

V.	u.
0·25	0·77 V.
0·5	0·79 V.
1·	0·81 V.
2·	0·85 V.

* *Jaugeage des Eaux Courantes*, 1802.

but thinks that in practice $u = 0{\cdot}8V$ may be adopted, that is, that the mean velocity of a current of water may be found by taking $\frac{4}{5}$ths of the superficial velocity. In conclusion, we may say that the resistance which water experiences in moving in a canal or channel, is proportional to the wetted perimeter, and to the square of the velocity plus a fraction of the velocity, and is in the inverse ratio of the section. This is in accordance with the experiments of Eytelwein, Funk, and Brünings, &c. And with regard to the natural phænomena of water running in regular channels, we have observed, that with the same inclination throughout the length, the water preserves the same breadth; that the section of its surface is composed of curved lines, resulting from the adhesion of the water to the sides of the channel, and the mutual reaction of each half of the section, by which a swell is produced in the middle; and, finally, that over the whole surface a series of diagonal lines, crossing each other from side to side like network, is formed, of which the obliquity or resultant of the lateral impulsion is proportional to the velocity of the water in the channel.

On the Progress and present State of our Knowledge of Rivers.

Hitherto we have confined our attention to the motions of a fluid in pipes and artificial conduits: the motions of rivers follow more complicated laws. So long as philosophers were contented to reason from experiments made under given and determined conditions, the problem was comparatively easy of solution; but the question was very much altered when they attempted to apply the results to rivers. In the former case, they could regulate the inclination and velocity of the fluid, and, by comparing the effective with the calculated expenditures, could analyse the resistances with approximate accuracy. In the latter case, they had to contend with an infinity of resistances, which were augmented or diminished at every instant of time.

These natural phænomena depend upon the physical constitution of the country and soil in which rivers derive their origin and formation. For whether we trace them to their sources among mountains, or follow their directions through the valleys, to the plains, and thence into the sea, we shall find them (although actually governed by well-defined laws,) subject to new conditions from every inequality of soil and country. In analysing, therefore, the motions of rivers, it is necessary that we should investigate not only the mechanical properties of the fluid, but the elements of resistance with which these properties are combined; that we should prove by comparison how the

sections of rivers assimilate in their inclination and magnitude, and demonstrate the law of their augmentation in volume, but decrease of velocity, as they approach the sea.

It is the office of science to unravel these mysteries; but although the attention of philosophers has been directed to the attainment of a true theory from the time of Galileo to the present, our knowledge of the laws which govern the motion of rivers is as yet very imperfect. The little success with which they have been investigated may be attributed to the difficulty of making correct observations, and to the local obstructions which generally exist in most rivers; and until we can ascertain these points correctly, by means of a series of careful experiments, we can only arrive at approximate results.

The application of the science of hydraulics to rivers may be justly said to have arisen in Italy. The peculiar physical structure of the surface of that country was well calculated to produce such a result, as it is intersected in all directions by mountains, and by numerous torrents and rivers, which carry off the superfluous waters to the Mediterranean and Adriatic seas, on either side of the Peninsula. But the lofty character of these mountains, as compared with the small extent of the country through which the rivers have to run, causes them to descend with extreme rapidity into the plains, which are frequently ravaged and desolated to an extent unknown in wider expanses of country. The evils thus generated, independently of the litigation and strife which they occasioned (and which exist at the present day), could hardly fail to excite the attention of ingenious men at an early period; hence may be dated the origin of that science which has since made such brilliant progress in Italy.

The arts of irrigation and drainage had been long known and practised by the ancients; but whatever science existed, seems to have remained dormant until the eleventh and twelfth centuries, when the Italians applied themselves to render several of their rivers navigable, such as the Brenta, the Mincio, the Arno, the Reno, the Tecino, the Adda, &c., also several canals for irrigation and drainage, such as the Muzza and others. But it was only after the invention of the lock * for transporting vessels

* Zendrini in his treatise, chap. 12, No. 20, speaking of the invention of the lock says, " Ho trovato dunque che Dionisio e Pietro Domenico, fratelli da Viterbo de fu Maestro Francesco di detta citta, ingegnere della Signoria di Venezia, acquistano del 1481 li 3 di Settembre da Signori Contarini certo sito nella Bastia di Strà, luogo ben noto verso Padova, per formare in esso un soratore del Piovego, che è quel canale, che viene da Padova al detto luogo di Strà; ed in certa supplica de'medesimi da Viterbo di detto anno resta espresso, ch'essi, che

from one level of a river or canal to another, that a new career was opened out to hydraulic architecture. By this beautiful contrivance all the difficulties attending navigation were overcome, rivers were rendered navigable, or avoided where too rapid or too dangerous, whilst the irregularities of the surface of a country were compensated.

The two canals which communicate to the Tecino and Adda rivers, and which were afterwards united at Milan by the celebrated Leonardo da Vinci about the end of the 14th century, were remarkable for the first application of a series of locks to any canal. The Naviglio Grande, made in the 13th century, from the Tecino river to Milan, was undoubtedly the first canal with a lock. In contemplating these works in the year 1827, the words of that excellent writer on hydraulics, Paul Frisi, naturally occurred to us, "Io no getto mai gli occhi sopra questi navigli senza un interno sentimento di stima verso que gl' illustre architetto chevi seppero vincere tante difficoltà*."

From this epoch may be dated the progress of Italy generally in the practice of hydraulic architecture. In the year 1516 a commission of scientific men was appointed by Francis the First to examine and consider the actual state of the canals then ex-

si chiamano *Maestri di Orologio*, faranno che le barche e i burchi protranno passare per la chiusa de Strà senza pericolo, operando in modo, che le acque usciranno con facilità, senza esser obligate a scaricare, e senza essere tirate," &c.

Antonio Lecchi, in his treatise on Navigable Canals, pronounces the invention of the lock to have taken place in the year 1420, because an early writer, Pietro Candido Decembrio, in his Life of Duke P. M. Visconti, says, "Meditatus est et aquæ rivum, per quem ab Abbiate ad Viglevanum usque sursum veheretur, aquis altiora scandentibus, machinarum arte, quas Conchas appellant."

Antonio Lecchi further says, that about the year 1188, Pitentino, an architect of Mantua, had constructed a lock at Governolo, on the river Mincio, to render it navigable, and that many remains of locks existed on several of the Italian rivers anterior to the year 1188. Paul Frisi, referring to the expression of Visconti, says, it only meant a regulator of the surface of the water, and not a lock. An anonymous Italian writer, in the year 1825, on the canal of Bologna, gives the discovery to Alberti in the year 1452. In the ten books of Alberti's Architecture the following sentence occurs, "Duplices facito clausuras, secto duobus locis flumine spatio intermedio quod navis longitudinem capiat, ut si erit navis conscensura cum eo applicuerit inferior clausura occludatur, aperiatur superior: sin autem erit descensura, contra claudatur superior aperiatur inferior, navis eo pacto cum istâ parte fluenti evehetur fluvio secundo." Lastly, Bruschetti, in his account of the progress of the internal navigation of the Milanese, says, that the first lock (*conca*) was erected at the commencement of the 15th century at Viarenna, and that the honour of this invention was due to two engineers of the Grand Duke Philip of Modena, named Orgagni and Fioravante, and not Leonardo da Vinci, who did not flourish until a century after. The name *conca* was given to the lock in consequence of its having been constructed for the purpose of transporting the stones intended for the Cathedral or Duomo of Milan.

* *De' Canali Navigabili, Trattato del P. D. Paoli Frisi:* Firenze, 1770.

isting in the Milanese, with a view to their further extension. The result was a project to join the Lake of Como with Milan by means of the river Adda and the canal of Martesana. This project was finally executed, and the difficulties of the navigation of the river Adda were overcome by means of a small cut with ten locks in it, called the canal of Paderno, which was finished in the year 1520. The next idea was to open a direct communication between Milan and the Po, but this project, with many others, such as the junction of the lakes of Como and Maggiore with the Po, the Tecino with the Po near Pavia, and the Adda near Cremona*, were postponed on account of political circumstances.

Hitherto the science of rivers had been greatly neglected, and indeed had never made much progress until after the celebrated congress of scientific men in Tuscany in the year 1665. This congress was appointed by the governments of Rome and Florence with a view to put an end to the contests which had taken place among the inhabitants bordering on the Val de Chiana, (anciently called the Chesina Palus,) and now one of the most fertile districts in Italy. It was precisely this river which gave rise to the famous controversy in the Roman senate, related by Tacitus, on the proposal for obviating the inundations of the Tiber by diverting the Chiana into the Arno. The Chiana, being situated between the Tiber and the Arno, had been alternately forced backwards and forwards by the neighbouring population until it had subsided into a noxious marsh, pouring out its surplus waters wherever they could find a vent. The result of the deliberation of the congress was a proposition by Cassini

* "Memoria sulla Navigazione interna del Milanese," dell' Ingegnere Parea, *Annal.*, lib. i. 79.

Almost the whole of the Val di Chiana has been raised by the process of *colmata*, or warping, similarly to the practice adopted in the marshes bordering on the Humber in this country.

It takes from five to six years to raise the surface as many feet.

Torricelli alone recommended the system of Colmates in 1768. The Grand Duke Leopold of Tuscany appointed a commission, at the head of which was the learned Fossombroni, to direct the operations; and on this occasion Fossombroni published a work, entitled, *Memorie Idraulico Storiche sopra la Val di Chiana*, Firenze 1769, in which the whole system is detailed.

From experiments made on the depositions of the Ombrone (a small but rapid river) at different periods, the deposits were found to be $\frac{1}{30}$, $\frac{1}{18}$, $\frac{1}{18}$, $\frac{1}{37}$ of the height of the water. It is to be wished that this system were practised over the whole of the marshes of the Tuscan Maremma, which are alone computed to amount to 300 square miles: the most considerable are the marshes of Viareggio, Grosseto, Piombino and the Pontine marshes. The works which were executed by Ximenes in the year 1767, in the marshes of Grosseto, although magnificent and effective for a time, were afterwards ruined by neglect: several attempts have since been made to renew them.

and Viviani to confine the Chiana by banks, and so conduct it to the Arno. In a subsequent meeting, at which Torricelli was present, the same system was recommended, on the ground that the rivers Arno, Tiber, and Po were confined by the same means.

Although nothing important had arisen out of the proceedings of the congress of Florence, the attention of philosophers was excited to discover the true causes of these evils. Much had been said and written on the rivers of Lombardy, Ferrara, Bologna *, Tuscany, and other provinces of Italy; but no one had undertaken to combine together the facts elicited, by a careful observation on the rivers themselves, until the meeting of a second congress at Bologna in the year 1681.

The Po and the Reno were the rivers that excited the greatest interest, on account of the absorption of the river Primaro by the Po, and the blocking up of the Reno by the depositions of the Ferrara branch, by which the Reno was raised so high as to cause the bursting of the banks, and the consequent inundation of the most fertile provinces of the Bolognese. This evil was greatly increased by the addition of five other torrents to the mass. Such a scene was well calculated to increase the interest upon this subject; hence may be dated the rise of the science of hydrometry in Italy.

The discovery of the law of falling bodies by Galileo, and the subsequent misapplication of this law to the rivers Bisenzio and Arno† in opposition to the opinion of Bartolotti, paved the way to several very important investigations by Castelli, who introduced the element of velocity, arising from pressure, into the calculation of the quantities of water which flow in the beds of rivers. Castelli proved,—

1st, That in a river reduced to a state of permanence, the quantity of water which passes through all its sections in equal spaces of time will be equal:

2ndly, That the medium velocities in the different sections will be reciprocally proportional to the amplitude of the sections:

* *Della Salveazione de' Fiume del Bolognese e della Romagna*, del M. R. P. Leonardo Ximenes e del Pietro Paolo Conti: Roma, 1776. Also, *Trattata de' Canali Navigabili* del Ab. Antonio Lecchi: Milano, 1776. Also, *De' Canali Navigabili* di P. D. Paoli Frisi, 1770.

† *Lettera di Galileo Galilei sopra il Fiume Bisenzio, a Raffaello Staccoli.* Bartolotti, an engineer, having projected to shorten the course of the river Bisenzio by means of a cut or canal, Galileo opposed it for the following reasons:—1st, That in two canals of equal height, but of unequal lengths, the velocity of the stream would be the same in both of them. 2ndly, That it is not the inclination of the bed of the canal, but the surface, that regulates the motion of the water. 3rdly, That the velocities do not follow the ratio of inclination as Bartolotti asserted, but differ in a variety of ways in similar inclinations.

3rdly, That if a river, flowing in a rectangular channel with a certain velocity, be augmented by a flood to double its height, the velocity of the water will be double; a principle subsequently adopted by Genneté, and disputed so often by the Italian philosophers. Castelli was well aware of the necessity of removing the obstacles to the free flow of rivers; but he was wrong in his supposition of the effect of sluices, and in attributing the velocity of the water near the mouths of rivers to the pressure of the superior waters. His opinions relative to the effect of rivers in purifying the air, and in preventing the increase of the sea-shore opposite Venice, were contradicted by Montanari and Guglielmini, who advised the diversion of the rivers from their ancient channels; and corrected the evil for a time.

Torricelli was the first who endeavoured to prove the analogy subsisting between spouting fluids and rivers, and their acceleration on account of the slope of the surface.

The respect of Viviani for Galileo did not prevent him from rejecting the ideas of his master as to the effect of shortening the course of the Bisenzio *. Viviani added several useful observations on the subject.

Zendrini, in his experiments with the pendulum, discovered that the velocities in the different parts of the section of the river Po were nearly proportional to the square roots of the heights, when the velocities were not very great †.

The truth of this law has been confirmed by all the experiments which have been made with the hydrometrical flask invented by the Bolognese in the year 1721, in which the quantities of water entering in a given time by a small aperture left open at the top, and collected by sinking the flask successively to different depths in stagnant as well as running waters, were at all times nearly in proportion to the square root of the heights. Independently however of these experiments, the parabolic law is sufficiently ascertained; so that in a parabola, of which the abscisses represent the depth of a river, and a corresponding semiordinate represents the velocity, all the other semiordinates will express velocities corresponding with the heights of their respective abscisses. Again, the space run

* Opinions are yet divided on the propriety of shortening the courses of rivers; but in rivers carrying gravel there can be no doubt.

Viviani had several striking examples before him of the evil consequences which had resulted from shortening the course of the Arno, both above and below Florence; and his observations upon the rising of the bed of that river are applicable to all rivers similarly situated.

† *Leggi, Fenomeni, Regolazioni ed Usi delle Acque Correnti*, di Bernardo Zendrini, (Firenze, 1770,) cap. v. part. ii. pag. 100.

through in one second, by a body floating on the surface of a river, divided by the same parameter, will give the height due to the velocity of the surface, which, added to the height of the river, will give the whole effective or equivalent height: the square root of the product of the equivalent height by the parameter will give the *velocity at the bottom of the section.*

Two thirds of the product of the velocity at the bottom by the whole equivalent height, minus two-thirds of the product of the velocity at the surface by the height added to the actual height, will give the mean velocity.

Finally, the product of the mean velocity by the actual breadth and the actual height will give the quantity of water that passes in one second through the rectangular section.

Zendrini's observations on the continual rise of the Adriatic Sea, in confirmation of the opinions of Sabbadini, Montanari, and Manfredi, and on the prolongation of the whole shore of the Po, as far as Ancona, and his Report on the diversion of the Ronco and Montone, rivers near Ravenna, together with the extension of the sandbanks at the mouths of the different rivers, are extremely interesting.

His great experience on this subject led him to conclude that a harbour ought not to have a turbid river either on its right or left side within a distance of seven or eight miles.

As early as the commencement of the eleventh century the opinions of philosophers coincided very nearly with the theory that the surface of the Adriatic Sea was continually rising, and certain indications along its shores seemed to confirm the correctness of these opinions. The cause was generally attributed to the continual accumulation of the substances brought down by the rivers and collected on the beach, and which, by prolonging the shores and contracting the outline, caused an elevation of the surface of the sea. This explanation, says Paul Frisi, would be very plausible if the Baltic did not exhibit at one and the same time an enlargement of its shores and a depression of its superficial level; and if it were not evident that as all seas must have a common level with respect to each other, the absolute height of the waters cannot be raised in one without being at the same time elevated in all the rest. In the Memoirs of the Academy of Stockholm, Celsius, Dalin, Stembeek, and others have given a long statement of facts, which prove very clearly the extension of all the shores of the Baltic Sea*. But what-

* See Mr. Lyell's *Geology* relative to the Delta of the Po, vol. i. pp. 236, 237; also a paper recently presented by that gentleman to the Royal Society on the rise of the shores of the Baltic Sea. The following examples in illustration of Manfredi's theory are mentioned by Col. Leake: Ilafonisi, an island formerly

ever may have been the opinions of philosophers on this subject, the fact was not known until Manfredi established it. Sabbadini had given his opinion in his discourse on the Lagunes of Venice.

Montanari, in his essay entitled *Il Mare Adriatico e sua Corrente esaminata*, maintained that the rise of the bottom of the Adriatic Sea was owing, not to the alluvion of the Po, as has been asserted, but to the sands of the shores of the Mediterranean brought by the current which runs from the Straits of Gibraltar along the African coast, and through the Ionian Seas into the Adriatic: his conclusions, however, are too fortuitous to be quoted.

The observations of Manfredi on the levels of floors of several ancient buildings at Ravenna, such as the Cathedral, Rotunda and Church of St. Vital, as compared with the levels of the neighbouring sea, and which Zendrini afterwards confirmed by other observations of the same nature, are curious. Zendrini observed that the rings formerly used to fasten boats to the quays at St. Mark's Place, are now below the level of the sea; that the subterranean church of St. Mark is no longer serviceable, because it is below water; that the ground plot of the Piazza is sometimes overflowed in moderately high tides, although it had been raised a foot; that in the island of Capri the whole platform of an ancient Roman edifice placed on the sea-shore was inundated; and he states that similar observations of Donati along the coasts of Dalmatia gave the same results.

The observations of Zendrini on the embouchures of rivers in the Mediterranean apply with equal correctness to all rivers which empty themselves into inland seas and lakes.

Grandi repeated the experiments of Zendrini; but although in his treatise on the motion of running water he professes to follow the principles of Galileo and Torricelli, his observations on rivers indicate that he possessed very little knowledge on that subject; his dissertations on the river Era and other rivers have merely a local interest, without adding anything to the science. The same may be said of the treatises of Cassini and Michelini, although the latter was the first to show the art of regulating rivers.

But the treatises of Guglielmini on the measure of running waters and on rivers are the greatest works of the Italian school of hydrometry. The publication of these works originated with the commissioners appointed in the year 1693 by Pope Innocent XII. to investigate the state of the provinces of Bologna, Ferrara,

a peninsula; Monemvasia, an island formerly the promontory of Minerva; the Cothon of Carthage, now a swamp separated by the port of Lecheum; Corinth, the port of Patara, and the Catacombs of Alexandria.

and Romagna, with a view to the regulation of the rivers and the drainage of those districts. Guglielmini was included in the Commission on the part of the city of Bologna; and having investigated the whole of the circumstances connected with the Po and Reno rivers, he published the result of his labours shortly afterwards. In that Report he confines himself to the subject in question, by detailing very fully the various projects which had been proposed to ameliorate the condition of the country and the rivers which flow through it, particularly the Po, the Reno, and the Panaro, and he demonstrates the method by which the difficulties could be overcome. His opinions were questioned by several engineers of that period. In his work entitled *La Misura dell' Acque Correnti,* he adopts the theorems of Castelli and Torricelli, and founds upon them a system of hydraulics inconsistent with experiment, in as much as he makes the velocity proportional to the square root of the height, and regards every point in a mass of fluid as tending to move with the same velocity with which it would issue from an orifice: and as the velocities are as the square roots of the depths of the orifices, the greatest velocity must be at the bottom of a stream and the least at the surface, besides a continual acceleration of the river as it moves. It was in vain that he attempted to reconcile these principles to facts. But the great work of Guglielmini is his *Natura de' Fiumi,* which was published with notes by Manfredi in the year 1697, and followed by a second part in the year 1712, after his death. The first three chapters contain definitions and general notions on the equilibrium of fluids, and the origin of fountains: the fourth and fifth chapters relate to the motions of rivers down inclined planes, with reference to friction and resistance, by which an equilibrium is established between the force of the current and the resistance of the bed. He states, that the inclination and velocity of rivers continually diminish in proportion as the rivers recede from their sources, and that consequently the power of transporting materials and the magnitude of the materials themselves diminish in a corresponding ratio;—that if there be two rivers of equal velocities, but of unequal masses, the river which has the greatest mass will have the least inclination: and from data not satisfactory he deduces that the greater the body of water in rivers, the less will be the inclination of their beds.

Chapter the 6th relates to the direction of rivers, and to the difficulty of restraining and regulating their courses; and in a series of propositions and corollaries the author demonstrates, That the direction of rivers is necessarily rectilinear if not in-

fluenced by external causes;—that the inequalities of the soil, together with artificial obstacles which rivers encounter in their courses, are the causes of the bends, sinuosities, and irregularities which constantly exist in them;—that rivers which carry gravel preserve their direction with great difficulty, on account of the alterations which continually take place in the time of floods by partial depositions;—that in consequence it is exceedingly difficult to regulate such rivers by artificial works, but much less so where rivers run through sand or other homogeneous beds.

Chapter the 8th contains several interesting observations relative to the junctions of rivers with each other and with the sea. In times of flood the elevation of the water is less sensible at the embouchures than above them, but a few inches of elevation at the embouchure occasions an elevation of several feet in the river. The velocity also, although stated by Guglielmini to be greater, is actually less at the embouchure than above it.

The author finishes this chapter by examining the cases of rivers joining each other perpendicularly or obliquely, and when they are subject to the flux and reflux of tides, and consequent changes in the directions of the embouchures.

Chapter the 9th treats of the effects resulting from the union of rivers with each other, and with the sea. In the 1st proposition it is stated that if two rivers similar in section and volume empty themselves separately into the sea, the sum of their sections will be greater than if they entered the sea in one united bed. The author adduces the sections made of the Reno and Tecino, affluents of the Po, in the year 1719 as proofs of this assertion. In proposition the 2nd he states, That two rivers united in one bed have greater velocity and power of corrosion of the bed than two rivers running in separate beds, and the increased effect will not only take place below, but above the confluence of the two rivers;—that the breadth and section will be less in the united than the disunited rivers;—lastly, respecting the effects of tides in keeping open the mouths of rivers, that the water of the sea, which during the flood enters into the beds of rivers, returning back with the ebb, helps to clean out the bed and to sweep away the deposits. He has repeated this doctrine elsewhere, expressing his opinion that so long as rivers could of themselves keep their mouths open on a flat shore, the agitation of the tides would prevent any shoals from forming in the trunk lying above the mouth; and with regard to the entrance of rivers into the sea, that the form of the mouth will depend upon the difference of velocity between the river and tide currents: that the sediments of the river will settle along the eddy part of the shore

and form sand-banks, which will go on gradually increasing; and the river being opposed on one or other side, according to the direction of the current of tide, will turn to the right or left as may be. Proposition 3:—Not only will the depth of the united river be increased, but the depths of all the other affluents likewise.

The remaining principles attempted to be established in this chapter are:

1st, That it is improper to unite rivers which carry gravel with rivers which carry sand;

2ndly, That the courses of gravelly rivers should not be shortened towards their embouchures;

3rdly, That the corrosions of the borders of united rivers are inevitable;

4thly, That it is better to cause a river carrying gravel to deposit its gravel by lengthening its course than to join it with another river carrying sand: that the consequences of such a junction would be to oblige the greater river to change its direction or to raise its bed in the upper parts.

Chapter the 10th relates to the increase and diminution of rivers, and the proportions in which they take place. Every river is subject to variations in the volume of its waters and in the capacity of its bed, from natural and artificial causes.

It is also affected by winds and tides. An affluent which enters into a river when its waters are at the lowest state of depression, will maintain a greater elevation of surface than when the river is highest*. A small river may enter into a larger one without augmenting the section of the latter. This apparent paradox is founded on the augmentation of the velocity of the greater river, and Guglielmini quotes the absorption of the Ferrara and Panaro branches of the Po by that river, without any sensible augmentation of its channel: this doctrine was first published by Castelli. The inutility of diverting the waters of rivers by means of side cuts for the purpose of lowering floods, is also insisted upon.

Chapter the 11th relates to natural and artificial streams, and the mode of conducting and distributing them for the purposes of drainage and irrigation. In the former case the author concludes, from a variety of reasons, that it is better to unite all the waters of a region into one grand conduit, than to allow them to run off by many separate conduits, and *vice versâ* with respect to irrigation.

* The truth of this observation seems to be generally allowed, although not satisfactorily established,—That the water rushes quicker down rivers in their high than in their low state.

Chapter the 12th treats of canals, and the precautions necessary to supply them with water from rivers and reservoirs, such as diminishing the force of the waters at their junction with the canal, fortifying the points of junction, &c. The effects of sluices, dams, regulators, aqueducts, siphons, and locks are all spoken of in detail.

Chapter the 13th relates to the drainage and warping of marshes. The first principle is to intercept and prevent the accumulation of water, by diverting it from the borders of the marsh, so that by the cessation of the cause, the effect will also cease. In this manner the whole of Lombardy was drained*.

The other principle is by raising the general surface of the soil; by allowing the water to deposit its earthy materials in times of floods: this was called *colmata*, or warping, a practice often adopted in Italy, where the rivers have not been allowed to raise their beds to an unnatural height above the general surface of the adjacent country by means of embankments.

Chapter the 14th and last, is very important in an engineering point of view, as it treats of the effects of regulating and shortening the courses of rivers. This operation ought never to be undertaken without a perfect knowledge of the soil through which it is proposed to carry the river. Cuts and shortening rivers with gravelly bottoms are rarely attended with success, but where the soil is muddy or sandy, such works are more durable. The author adduces the Po, which has established itself in the middle of its basin, as an example of the equilibrium which its course has attained by the rivers which flow into it on both sides. The work of Guglielmini contains much valuable information, although, from its numerous contradictions and errors, particularly on the formation and transportation of stones and gravel, it requires to be consulted with caution.

The next author on rivers is Zanotti: this writer endeavours to determine, by a series of observations, the position which the beds of rivers should occupy near the sea, in proportion to the superficies of their waters.

In considering the sections of the Po and Tiber, he was of opinion that the acceleration of the waters occasioned by the freeness of the outlet in these rivers, extended up the river to a considerable distance, and reached to the spot which would be struck by a horizontal line drawn from low-water mark. Finally, on comparing these observations together in detail, he disco-

* This was always the principle adopted by the late Mr. Rennie in draining the fens of Lincolnshire and Cambridgeshire, by means of catchwater drains at the bases of the surrounding hills; and by uniting the scattered waters by large drains, they were conveyed to the sea.

vered that the reduced slope of the surface in the highest floods, reckoning from the point to which the surface of the sea at low water reaches to the mouth, was equal to the reduced slope of the bottom or of the lower superficies of the river, beginning from the same point, and proceeding to the opposite direction. His observations generally on rivers are valuable; but the most estimable writer, after Guglielmini, on rivers and torrents is Paul Frisi.

The work of this author is divided into three parts; in the first the author investigates the phænomena of rivers and torrents which flow over gravel; the origin of rivers; the substances brought down by them; and the formation and rectification of their beds. The 2nd chapter treats of the velocity of water from apertures in vessels according to the theories of Torricelli, Newton, Michellotti, &c.; and the velocities of rivers and artificial canals whether united or divided; their declivities, and the distribution of them according to the principles of Galileo, Castelli, Grandi, Guglielmini, Genneté and others: and the third part relates to rivers which carry sands and mud; the states of the old and new beds of rivers, with reference to the projects which had been advanced for improving the Tiber, Arno, and other rivers of the Bolognese; the resistances, whether natural or artificial, opposed to the free flow of rivers; the doctrines of different authors upon this subject; the effects of regurgitations occasioned by dams, weirs, and other obstructions thrown across rivers; and lastly, the phænomena attendant on rivers entering into the sea. An interesting essay on navigable canals completes the work.

Frisi, after demonstrating that Guglielmini had been mistaken in supposing that the formation of the smaller gravel and sand in the beds of rivers was owing to the attrition of the larger stones in the upper parts of the courses of rivers, maintained, on the contrary, that gravel and sand are original bodies spread over the earth through which the rivers traverse; and, by experiments, determined that the formation of sand in rivers is not owing to the attrition of stones against each other, but to variations in the velocity of the current, which deposits the materials according to the greater or less intensity of its force.

Viviani and Belgrado were of the same opinion. Belgrado observed that stones torn from the mountains are precipitated down their declivities, turning for the greater part of the time on their own centres; that they continue to roll along in the same manner in the beds of torrents, until, the slopes becoming less, they afterwards slide along the bottom, rubbing against it, and are scattered to and fro by the impetuosity of the torrent;

and that in consequence of the rolling and sliding motion they acquire in their descent, there can be little or no abrasion of the surface. Grandi, in considering the dam of Era, and comparing the specific gravities of the granite in the water, and of the water itself, inferred that the transverse impetuosity of the waters was sometimes sufficient to raise the gravels from the bottom, and to throw them on the edges of the dykes.

Besides his work on rivers and torrents, Frisi particularly distinguished himself in the Bolognese and Ferrara controversy, in which his plan for the rectification of the rivers of those provinces in 1760 was approved of by all the mathematicians then present. It was just at this period while Frisi was engaged in the Bolognese controversy, that the work of M. Genneté made its appearance; and on comparing together the observations made on rivers by both parties, it appeared to Frisi that there was no sensible height even when there is a considerable augmentation of water, and therefore, that the velocity of the water increases sensibly in the same ratio as its quantity.

The propositions of Genneté were,

1st, If two rivers be added to another during the time of its flood, the river will experience no sensible rise in its surface;

2ndly, That if from the same river two branches be taken, its surface will not be sensibly lowered.

These doctrines had been partly advanced with regard to canals by Castelli, Cassini, Guglielmini, and Corradi, but Genneté was the first to apply them to rivers. It had been stated by Frisi that the river Reno received the Samoggia without any perceptible difference in the amplitude of its sections, and that therefore it might receive other torrents without any sensible augmentation. Doctrines so extraordinary, and at variance with the received opinions on this subject, excited many discussions in Italy. Genneté's experiments were tried at Ferrara in the year 1762, and at Rome in the year following, and again repeated at Ferrara in the year 1766, but with results entirely different; he, however, clearly proved that the dissimilarity was principally owing to the different modes of experimenting, although the apparatus used at Ferrara resembled Genneté's very nearly. The recipient was 199 feet in length and 7 inches in width, and the result was, that the first tributary stream (equal in section to the recipient) occasioned an augmentation in height of one half, and on introducing a second tributary of the same section, the augmentation was double; it was conceived, therefore, that Genneté had either erroneously stated his case, or the effect was due to the increase of velocity occasioned by

the pressure of the tributary waters against the water of the recipient.

But, besides the law of acceleration, there remained other elements to take into account, one of which related to the motion arising from the junction of two or more rivers.

In the Memoirs of the Academy of Sciences for the year 1738, M. Pitot has used the same principle to determine the mean direction which the waters of the two rivers will take when freely united together, and this he does according to the resultant of the collision of hard bodies, where the same quantity of motion is invariably preserved; and from this hypothesis he draws as its consequent that the common velocity of the united rivers is equal to the quantities of motion in the two separate rivers divided by the sum of those quantities of water. Grandi has endeavoured to decide by the same principles of the composition and resolution of forces, not only the direction, but the absolute velocity of the waters which either unite or divide. For this purpose he constructed a float which gave the resultant of the two confluences, from which he concluded that the course of the river would naturally take an intermediate direction; but, if the banks of the recipient remained firm, its stream would preserve the same direction as before, increasing, however, its former velocity by a part, in proportion to the velocity of the tributary stream, as the cosine of inclination of the river is to the radius: whence it would follow, that if the thread of water in the tributary stream should second by its direction the thread of water in the recipient, in making with it, as is generally the case, a very acute angle, the velocity in the common bed would be equal to the sum of the velocities of the recipient and affluent streams. If this principle were admitted, it would follow, that the sections of the receiving stream could not be considerably augmented by the junction of the tributary, for this reason, that the quantity of water augmenting, the velocities would be compounded of this augmentation, and the flow of the current be more rapid than it was before.

Guglielmini, in the seventh chapter of his work, in considering the celebrated phænomenon of the Po (of Venice), which receives the branch of the Ferrara and the Panaro without any enlargement of its bed, has stated in general that a smaller river might enter into a larger one without increasing either its breadth or height; and he was of opinion that this might happen without any lateral dispersion, because the whole of the increased body continued in motion by following the direction of the thread of the stream. On the hypothesis that all the sections were effective, and that the velocities before and after the con-

fluence of the two rivers were as the square roots of the actual heights, the cubes of the heights would be as the squares of the quantities of water which are discharged in an equal time by the sections.

Manfredi deduced that the Reno, which added $\frac{1}{31}$ part of the whole quantity to the Po, could not raise the height of the Po more than $\frac{1}{31}$ part; but, reflecting afterwards that some experiments made on adding or subtracting the water of a drain to and from the Panaro, occasioned no difference in the elevation of the surface of that river, he concluded that the elevation of the Po must be very small for any augmentation which the waters of the Reno could cause in its stream*. The fatal consequences which had arisen from dividing the Rhine into so many branches from the frequent bursting of the embankments which maintained them above the adjacent lands, and the continual expenses entailed by them, necessarily excited great interest.

The great Rhine divides itself near Emmerik into two branches, nearly equal to each other, viz., the Waal and the Rhine: the bed of each of these branches is nearly as large as that of the whole river before its division, and when the waters rise they are at an equal height in both. The second branch divides itself again towards Arnheim to form the Issel, which has nearly the same section as that of the Rhine.

The first division of all the waters of the Rhine was begun under the Roman generals Drusus and Corbulo: many subdivisions were made in subsequent ages. This great multiplicity of channels, although productive of advantages to Holland, occasioned many fatal consequences: the waters, divided into so many branches, lost the rapidity and strength necessary for them to push forward the alluvial matter, occasioned a continual rising of the bottom, rendered the draining of the waters from the adjacent lands more difficult, increased the expense of the embankments, and augmented the damages over the extensive lands when the dykes broke.

"To secure that part of Holland which lies between Rotterdam, Utrecht, Amsterdam, and the ocean, it was proposed in 1754," says Frisi, "to form a cut, with sixteeen sluices, in the Leck, which is another branch of the Rhine, by which part of the waters would be discharged into the Meruva, which is the junction between the Waal and the Meuse. M. Genneté opposed the project on the ground that it would not have diminished the height of the floods, but that it would have been pre-

* See Major-General Garstin's Translation of Paul Frisi's Work on Rivers and Torrents.

ferable to have united all the waters of the Great Rhine into the ancient branch of the Issel, and thus have conducted them by the shortest direction to the sea, because by the union of the waters their rapidity would have been increased, while the amplitude of the sections would have continued the same, and the evils complained of would have been avoided: he supports his opinion by several examples of the junction of the Mayne and Moselle rivers with the Rhine, without any sensible increase of section in the Rhine before or after the junction; but, in order to satisfy himself of this apparent anomaly, he caused an artificial river to be constructed at Leyden, in the year 1755, which was supplied with water by means of a vessel, five or six feet in height, and connected by sluices with six other small streams.

"The bottom of the recipient and of the tributaries had a slope of $\frac{1}{1200}$; and he observed all the variations that occurred either in adding the tributaries or in retrenching their streams."

The results of these experiments were, that when a stream, equal to half the water in the recipient, was added, and afterwards another stream equal to another half, the quantities of water in the recipient being successively as 1, $1\frac{1}{2}$, and 2, the height of the water in the recipient was apparently the same, while the velocities and quantities of the fluid increased in the same proportion, viz., 1, $1\frac{1}{2}$, 2. Again, when the augmentations to the quantity in the recipient were in the ratios of 3, 4, 5, 6, and 7, the increase in the height of the water in the recipient was only $\frac{1}{48}$, $\frac{1}{24}$, $\frac{1}{16}$, $\frac{1}{12}$, and $\frac{1}{9}$, respectively. By a contrary proceeding he let off the six tributary streams successively, and found the diminution of the height of the water in the recipient to prevail in the same proportion as the augmentations.

Having witnessed these apparent anomalies in the junction of rivers, it occurred to me to repeat the experiments of Genneté; and having provided a suitable apparatus, consisting of a wooden trough ten feet in length, and six inches in width and eight inches in depth, together with troughs of similar dimensions let into the sides of the inner trough at angles of 30 degrees, and furnished with suitable openings and valves, I caused one and two streams respectively of water to be let into the main stream from equal apertures and under equal and constant pressures, from a cistern of two feet internal dimensions every way, and the following Table shows the results:

Experiments made on Water, August 9th, 1834.

Position of Trough in Degrees of Inclination.	Depth of Water with one opening of ½ an inch diameter.	Depth of Water with two openings of ½ an inch diameter.	Depth of Water with three openings of ½ an inch diameter each.	Additional depth of water with two openings compared with one.	Additional depth of Water with three openings compared with one.
	Inches.	Inches.	Inches.	Inches.	Inches.
Trough level	1·25	1.75	2·375	·625	1·25
Inclined 1°	·75	1·	1·375	·25	·625
2	·625	·875	1·125	·25	·50
3	·50	·75	1·	·25	·50
4	·50	·75	1·	·25	·50
5	·50	·687	·937	·187	·437
6	·50	·625	·875	·125	·375
7	·50	·620	·812	·120	·312
8	·48	·610	·805	·130	·325
9	·44	·600	·75	·160	·31
10	·43	·590	·73	·160	·30

The results were, 1st, That when the artificial river or recipient was exactly level, it required two streams of equal magnitude to raise the main stream to double of its original height: 2ndly, That when the artificial river or recipient was set at angles of inclination of from 1 to 10 degrees, a sensible diminution took place in the altitude of the main stream, as well as in the ratio of increase in the tributaries, corroborating in some degree the experiments of Genneté.

In addition to the Italian collection, there appeared, at different intervals, a variety of works on the motions of rivers by Mariotte, Hermanus, Michelini, Michelotti, Fontana, Poleni, Statlerius, Ximenes, &c. In the year 1779 the Italian collection was first made known in this country by the Abbé Mann, in a valuable Treatise on Rivers and Canals, in the *Philosophical Transactions*. The author recapitulates the different doctrines, propounded by Torricelli and others, on the motions of rivers, from the laws of their action, to the establishment of their beds. He adopts the principles of Guglielmini in almost every instance relative to the accelerations and retardations of rivers, and shows, according to the principles laid down by Leibnitz and Euler, that, in order to render the velocity of a current everywhere equal, the bed should have the form of a curve, along which a moving body should recede from a given point, and describe spaces everywhere proportionate to the times.

The author gives several practical rules relative to the junction of and derivations from rivers, whether with each other or with the sea; and, in the fourth section of his treatise, he details a series of experiments to determine the different velocities of the same floating body, moved uniformly by an equal force in different depths of water, the results of which are, that the different velocities of the floating bodies are in an inverse ratio of the respective depths of the water in which they float with an equal impulsive force.

The author gives the declivities of several rivers in France and in Flanders, such as

The Seine, from Paris to Havre, which he states to be	$\frac{1}{4282}$.
The Loire	$\frac{1}{3174}$.
The Rhone, from Besançon to the Mediterranean, stated to be one of the most rapid in the world, or double of the mean declivity of the rivers in Flanders	$\frac{1}{2620}$.
The Ypres in Flanders to Newport	$\frac{1}{3280}$.
The Lys and Scheldt	$\frac{1}{6672}$.
The canals	$\frac{1}{10000}$.

The following Table is considered an approximation to the actual state of Rivers.

Distinctive Attributes of the various Kinds of Rivers.	Rates or Classes of Rivers and Flowing Waters.	Comparative Degrees of the Mean Velocities of Currents.	Seconds of Time in which Currents run 20 Fathoms.	Fathoms run by the Current per minute of Time.	Ratios of Declivity compared with horizontal Length.	Fathoms of Length for each one twelfth inch of Declivity.
Channels wherein the resistance from the bed, and other obstacles, equal the quantity of the current acquired from the declivity; so that the waters would stagnate therein, were it not for the compression and impulsion of the upper and back waters.	1	0	0″	0	$\frac{1}{12000}$	14
Artificial canals in the Dutch and Austrian Netherlands.	2	$\frac{2}{3}$	180	$6\frac{2}{3}$	$\frac{1}{7000}$	8
Rivers in low flat countries, full of turns and windings, and of a very slow current, subject to frequent and lasting inundations.	3	1	120	10	$\frac{1}{5200}$	6
Rivers in most countries that are a mean between flat and hilly, which have good currents but are subject to overflow; also the upper parts of rivers in flat countries.	4	$1\frac{1}{2}$	80	15	$\frac{1}{4000}$	$4\frac{2}{3}$
Rivers in hilly countries with a strong current and seldom subject to inundations; also all rivers near their sources have this declivity and velocity, and often much more.	5	$2\frac{1}{6}$	55	$21\frac{2}{3}$	$\frac{1}{3200}$	$3\frac{2}{3}$
Rivers in mountainous countries having a rapid current and straight course and very rarely overflowing.	6	3	40	30	$\frac{1}{2000}$	3
Rivers in their descent from among mountains down into the plains below, in which plains they run torrent-wise.	7	5	24	50	$\frac{1}{2000}$	$2\frac{1}{3}$
Absolute torrents among mountains.	8	8	15	80	$\frac{1}{1700}$	2

In the year 1823 a new collection, or rather continuation, of the *Trattato* was published at Bologna, in six volumes, in which the papers relating to rivers, are, first, An elegant dissertation on the Natural Phænomena of Rivers, by Count Mengiotti; and secondly, An Exposition of the Experiments which have been made by different authors to arrive at the true theory of running waters, by means of various instruments, such as the float, the pendulum, the wheel, &c. Thirdly, A confirmation of the doctrines of Castelli with regard to the ratio of Increase by Tributaries: and remarks on the inutility of diversions in rivers, as adduced by the diversions from the Po, the Rhine, and other rivers, by Guglielmini and Genneté. The effects of regurgitations in obstructing the free flow of rivers are quoted from different authors and illustrated by experiment, more or less confirmatory of the opinions of Guglielmini.

Volume the second, contains papers by De Lorgna on the Inundation of the River Adige; the prolongation of Rivers into the Sea, and the confining of their channels; the effects of Affluents and Diversions.—A paper, by Zuliani, on the advantages and disadvantages attending the expansion of rivers at their embouchures; the number and direction of the streams necessary to maintain the water in its proper channel, and to resist the opposition of winds and waves. The author quotes many examples in illustration of his theory, but concludes that the determination of the question in a mathematical point of view is beyond the reach of science.—Also, a learned paper on the motion and measure of running water, by Tadini. The author adopts the usually received theory of the velocity of running water, which he reduces to expressions, and makes the relation of the velocity at the surface and bottom of a torrent to be as $1 : + 0{\cdot}0016$; he states that in the case of a river such as the Po, of which the inclination, when the experiment was made, was as 0·000214 metre per metre, the velocity at the surface and bottom is very nearly alike, and that in similar cases the velocity is small and the surface nearly parallel to the bed. The notion, therefore, that the velocity of a river increases from the surface to the bottom as the square roots of the depths, is erroneous.

The remaining chapters of Tadini's treatise are devoted to an examination of the theory of the measurement of running water through close and open channels according to the velocity and amplitude of the sections, with due allowance for obstacles; he shows also the modes adopted by the different provinces in Italy, in the measurement of running water, and the discrepancies resulting therefrom, and concludes with a variety of experiments on the expenditures of orifices and rectangular channels,

but more particularly on the canal of Martesana, in which the approximation to the parabolic theory is very close. In allusion to the fluidity of water he states, that from accurate experiments which had been made on the inclination of the Lake of Como towards its outlet, the sensibility was found to be $\frac{1}{74337}$.

The treatise of Tadini is followed by a valuable practical paper on the measurement of running water, reduced to the provincial measures of Italy and according to an extensive parabolical table appended.

Lastly, this volume contains papers by Masetti, on the Theory and Practice of the different Instruments (*tachimetri idraulici*) which have been invented for the purpose of measuring the velocity of running water by Castelli, Guglielmini, Ceva, Grandi, Pitot, Mann, Brouckner, Woltmann, Saverien, Ximenes, Lecchi, Michelotti, Leslie, and Venturoli. He divides them into two classes, floating and fixed instruments, and demonstrates, both theoretically and practically, that the fixed instruments give the surest results; in general all of them indicated, in a greater or less degree, the diminution of velocity towards the bottom. Masetti's conclusions are, that, for measuring the velocity of the surface of rivers, the floating instrument or balls of Castelli is the simplest and best. Secondly, that of the fixed instruments, the sliding rod of Bonati, and the pendulum of Guglielmini, improved by Venturoli, are best.

A second paper, by Masetti, is devoted to the examination of the different states of running water through orifices and rectangular channels, according to the parabolic tables of Prony and Eytelwein, calculated for different latitudes. The author quotes the experiments of Newton, Borda, Bossut, Dubuat, Mariotte, Michelotti, Navier, Hachette, Venturoli, &c.

Volume the third, contains a paper by Fossombroni on the celebrated Val di Chiana, and the systems of Warping and Drainage which have been practised in it at different periods. This volume also contains papers by the same author on the distribution of Alluvions, on the Draining the Pontine Marshes, and on rendering the river Arno navigable by means of Jetties and Contractions.

Volume the fourth, contains several valuable papers on Canals, by Lecchi, Ferrari, Bruschetti, and Parea, including the original letters and reports concerning the early navigations and canals of Italy.

Volume the fifth, is principally occupied by a translation from the French of Borgnis *Sur les Machines Hydrauliques*, and two papers by Magistrini and Masetti on the action and reaction of water on hydraulic machines.

The sixth and last volume, contains the experiments and conclusions of Bonati in opposition to those of Genneté, on the Methods of Measuring the Expenditure of Rivers and of Canals of Irrigation adopted by the different provinces of Italy; and in which the author, after showing the discrepancies which exist between them, gives the preference to the Milanese method.

A paper by Morri, on the Navigation of Faenza, and some unsatisfactory experiments on the inclination, velocity, and product of the river Po, together with several observations of minor importance on the rivers Reno, Tiber, Brenta, and Velino, conclude the new collection. Such may be considered to be the present state of hydraulic science in Italy. In rendering an account of its progress it is impossible to withhold the just tribute which is due to the Italians, namely, that of having been the first to establish hydraulic science upon anything like true principles.

Progress and Present State of Hydraulics in France, Germany, and England.

The writers included under the above title may be considered to consist of two classes, viz. theoretical and practical.

The first have confined themselves to a purely speculative consideration of the subject, in extending the chain of geometrical truths without contributing anything to the real progress of the science.

The last have endeavoured by observation and experiment to arrive at practical inferences.

Mariotte belongs more properly to the latter class. His treatise on the motion of water, accompanied by an immense number of experiments, in the year 1728, has greatly contributed to perfect the science.

Pitot demonstrated, that in open channels friction diminished in proportion to the diminution of the surfaces in the inverse ratio of the homologous sides; and that the friction of water moving in tubes at equal velocities, in relation to the volume of water, is in the inverse ratio of the diameters.

Couplet illustrated this principle very clearly in his experiments, although his deductions from them were incorrect.

Varignon contented himself with reducing the opinions of Guglielmini to geometrical forms.

Belidor followed the steps of Guglielmini in his great work on Hydraulic Architecture.

Bossut was the first to follow the steps of the Italian school by combining theoretical with experimental investigation. His

admirable work on Hydrodynamics shows abundant proofs of the great sagacity with which he investigated every question relative to the motions of waters through orifices and pipes; but his experiments on artificial canals are unsatisfactory from his having omitted the consideration of the depth.

The investigations of Bernoulli seem to have formed the groundwork of the French school; for although he adopted the opinion of Guglielmini, with regard to the analogy between the motion of a river and the motion of a fluid escaping from a vessel, yet his theory of the law of the velocity, however absurd its application to the gradations of velocity in a river, is correct. Although the science of hydrodynamics had acquired a high degree of perfection at this period, it was nevertheless confined to the hypothesis of the parallelism of filaments, in which all the points of the same filament move in one and the same direction. It was desirable to express the motion from a given point in a fluid in any direction. This problem was resolved by D'Alembert, who discovered equations on two principles, namely, that a rectangular canal, taken as a fluid mass, is *in equilibrio*, and that a portion of a fluid, in changing its position, preserves the same volume when the fluid is incompressible, or dilates according to a given law when the fluid is elastic. This profound and ingenious investigation was published in his *Essai sur la Résistance des Fluides* in the year 1752, and afterwards perfected in his *Opuscules Mathématiques*.

Euler, in his *Mémoires des Académies de Berlin et de St. Petersbourg*, and La Grange, in the year 1781, exhausted all the resources of geometry for the same object, but without any applicable result. It was not until the year 1781, when M. Bossut published his *Traité Théorique et Expérimental*, that the theory of hydrodynamics was made subservient to experiment.

M. Bossut divides his work into two volumes, theoretical and experimental: the first explains the general principles of hydrostatics and hydraulics according to the previously established theory; the second contains a vast number of experiments on practical hydraulics; on the motion of water through orifices, pipes, and rectangular canals.

In the case of a rectangular canal of 105 feet in length, a considerable difference between the natural and artificial expenditures, arising from the friction of the sides of the canal and of the atmosphere, was found to prevail: also a very considerable swelling or rise of the water between the two extremities of the canal; but without any diminution of the expenditure in a given time, although the reverse is the case in pipes. He also found that with the same initial velocity of the fluid, canals which are

inclined pass off a greater quantity of water than horizontal canals: this is illustrated by a great many experiments on the velocities of water issuing from openings under variable and invariable pressures and inclinations of from three inches to eleven feet.

The experiments were repeated upon a larger scale in a canal of 600 feet in length, and with nearly similar results, namely, that the velocity augmented with the inclination. There were, however, observed two distinct velocities, viz., the velocity of projection as the fluid issued from the orifices, and the invariable velocity which established itself *in equilibrio* with the resistances. When the canal had scarcely any inclination below a tenth part of its length, there existed little or no uniformity between the primitive and permanent velocity. M. Bossut attempts to make several applications of his experiments to rivers; among others, to the Beuvronne, which he found to have an inclination of $\frac{1}{3000}$ nearly, the same as the Seine at Paris *, although the velocity of the Beuvronne, as compared with the velocity of the Seine, was as 36 to 100, and the quantity of water passed through the respective sections was as 1 to 278; from which he deduced that with equal inclinations the greatest quantities of water have the greatest velocities, but that the velocities do not augment in the ratio of the quantities of water; hence the reason, according to him, that when two rivers unite into one, the capacity of the channel of the united river is always less than the sum of the capacities of the minor rivers taken conjointly: these minor rivers may also have different inclinations and velocities than the united river. He differs in some respect from the principles of Genneté, but agrees with him in the inutility of derivations from rivers, and very properly refers to M. Dubuat for more precise information on the subject.

Inspired by the perusal of Bossut's work, Dubuat endeavoured to investigate the subject *de novo*, by considering, that if water was perfectly fluid, and received no impediment from the surface over which it moved, it would be accelerated in the same manner as bodies running down inclined planes; but as this effect was found not to take place, he concluded that there existed a certain degree of retardation arising from the friction of the channel or the viscosity of the water, and that when water ran uniformly in any channel whatever, the accelerating force was equal to the sum of the resistances. This principle, as we have seen, had been long known in Italy. Encouraged by this

* According to later observations the inclination of the Seine varies from $\frac{1}{5000}$ to $\frac{1}{7000}$.

apparent discovery, Dubuat endeavoured to render the experiments of Bossut conformable to it, and in the year 1779 published his *Principes Hydrauliques.*

Dubuat felt, however, that his theory required further elucidation, and having undertaken a more extensive series of experiments, published the result in three volumes in the year 1786*.

The first two volumes treat of the uniform and variable motions of water in rivers, canals, and pipes; the origin of rivers, the establishment of the beds, and the effects of dams, sluices, bridges, reservoirs, and fountains; the navigation of rivers and canals, and the resistance of fluids.

The last volume treats of the mechanical properties of ætherial fluids as affected by heat. Dubuat had been long sensible of the unsatisfactory state of the theory of the motions of rivers and the difficulty which surrounded the discovery of a true theory, conceiving that every river ran with an uniform velocity peculiar to itself, and that the velocity in the middle was greatest. He believed that the formation of bends in them was owing to obstacles; that the development of their curves was in proportion to the mean radius: and having traced geometrically the circumstances of his hypothesis, he had recourse to analyses, out of which he formed equations applicable to practice; and having observed frequent changes in rivers from floods and other causes, he concluded, that it was easy to find the expenditure of a river in any part of its course by calculating the annual produce of the rains which fall upon the surface of the surrounding country, deducting a certain proportion ($\frac{1}{4}$th) for filtration, evaporation, &c. Hence the total expenditure of a river is deduced by the product of its mean section and mean velocity. The author applied his principles to the river Seine; he examined different cases of the expenditures of water, and added new expressions for each to his formula of uniform motion; and in the case of great rivers which are difficult to submit to experiment, he assimilated their motions to the motions of fluids through conduit pipes.

In commenting upon the experiments of Bossut, he says, "The experiments which occasioned the greatest difficulties were those on rectangular and trapezium canals, in as much as it was found very difficult to render the motion of the current uniform; but we have been amply recompensed by the experiment which we had occasion to make on the diminution of the velocity

* *Principes d'Hydraulique vérifiés par un grand nombre d'Expériences faites par Ordre du Gouvernement; Ouvrage dans lequel on traite du Mouvement uniforme et varié de l'Eau dans les Rivières, les Canaux, et les Tuyaux de Conduite, &c.*: par M. le Chevalier Du Buat.

of a uniform current, reckoning from the surface to the bottom, and by very curious observations on the mode in which the water corrodes the bottom, according to the kinds of soil, such as gravel, sand, and clay, which constitute it."

After recapitulating the various principles laid down in his first edition relative to the effect of bridges, sluices, aqueducts, &c., he develops the fundamental principles of uniform motion, the causes which create, and the resistances which affect it, which latter he makes proportional to the squares of the velocities; he gives a formula for uniform motion in any channel, and then shows by experiment and by analyses the causes of variation, what amount is due to friction, and what to adhesion or viscosity. By this means the law of motion is developed from infinite velocities to its total cessation. These elements determined, he examines the nature of the different beds over which rivers run, whether natural or artificial; the effects of floods or the affluents of rivers, shortenings, swellings, derivations; the forms most proper for canals, the piers of bridges; and illustrates the whole by a great variety of experiments, which are extended to the resistance of fluids.

Dubuat values the effect of viscosity at 0·3 of an inch: the mobility of water he limits to $\frac{1}{1,000,000}$ of the inclination, and considers $\frac{1}{500,000}$ to be the smallest possible inclination that can be given to a canal to produce sensible motion. He cites several experiments made by him on an artificial canal with an inclination of $\frac{1}{9288}$ which gave only a velocity of 6 inches per second, whilst in a drainage canal with an inclination of $\frac{1}{27,000}$ the velocity was only 7 inches per second, and in a part of the river Hayne having an inclination of $\frac{1}{33,000}$ the velocity was 10 inches per second, so that the velocity was greatest with the least inclination. Dubuat adds, "it is impossible to reason against facts." The anomalies which prevail throughout the whole of Dubuat's work render many of his conclusions very doubtful. The principles upon which Dubuat founds his theory of uniform motion are:

1stly, That water is composed of molecules perfectly spherical, hard, and polished, but gifted with a certain degree of tenacity;

2ndly, That rivers cannot run without a certain degree of inclination in their surface;

3rdly, That when the mean velocity of a river is uniform, the accelerating force is equal to the resistance of the bed;

4thly, That it is the tendency of every mass of water to form its own bed by filling up the inequalities of the bed itself;

5thly, That the surface of this bed consists of an assemblage of molecules or globules, over which the other globules glide,

and from which results a resistance proportional to the square of the velocity with small velocities, and diminishing to nothing in high velocities, the relation between the velocity and inclination being expressed by $V = \frac{\sqrt{mg}}{\sqrt{b} - L\sqrt{b} + 1{\cdot}6}$;

6thly, That the resistance which the whole mass experiences from the friction of a part of it against the bed, is in the direct ratio of the bed, and inversely as the section;

7thly, That each molecule experiences a resistance in proportion to its distance from the bed;

8thly, That these velocities taken conjointly produce a mean velocity, which leads to the following general expression:

$$V = \frac{297\,(\sqrt{r} - {\cdot}01)}{\sqrt{b} - L\sqrt{b} + 1{\cdot}6} - 0{\cdot}3(\sqrt{r} - {\cdot}01).$$

M. Dubuat considers that the amount of friction being proportional to the extent of surface, and the circle containing the least perimeter, that figure is preferable for pipes on account of presenting less friction, but that rectangular figures are preferable for aqueducts, and trapeziums for rivers, from the nature of the channel and the velocity in all cases being sensibly proportional to the square root of the mean radius of the bed: it follows that a trapezium in which the breadth at the bottom is $\frac{2}{3}$ of the height of the water, and the slope of the sides $\frac{4}{3}$ of the depth, will give the least resistance.

The following are the results of his experiments:

	Inches.	
Fine gravel	4	per second.
Middling ditto	7	ditto.
Large ditto	12	ditto.
Gravel of the size of an egg . . .	36	ditto.

Hence the reason why in the channels of rivers there is necessarily a relation between the tenacity of the soil and the velocity of their currents; and in general, if we call q the relation to the breadth and depth of a channel, we shall have $r = \frac{l}{q+2}$ and $r = \frac{qh}{q+2}$; or if the depth be undetermined and the breadth be finite, we shall have $r = \frac{l}{2}$; and, *vice versâ*, if the depth be finite and the breadth undetermined, we shall have $r = h$. So that in rivers in which the width is very great in proportion to the depth, we may without any sensible error take the depth for the mean radius, and in this case their mean velocities for equal inclinations

are as the square roots of their depths. We have, therefore, formulæ for calculating the different cases (two of the data being given,) of the breadth, depth, mean radius, velocity, and inclination, derived from a table of experiments on trapeziums and rectangular canals, on the canal of Jard and the river Hayne. In order to facilitate the use of these tables the late Professor Robison reduced them for his *Mechanical Philosophy**: they have since been greatly enlarged by Mr. Laurie of Glasgow, but are now in a great measure superseded by the more accurate researches of Eytelwein.

For the curves and salient angles of rivers, and during permanent and periodical floods, the author endeavours to establish theories which have no relation to the actual state of things; but it results from his observations that an inclination of $\frac{1}{10200}$ only produces on account of bends a velocity due to $\frac{1}{21600}$. In applying his formula of uniform motion to the course of rivers, he compared the velocities of the Seine and Loire in their mean state: he found that the mean inclination of the Seine was 1 metre for 100 toises, or $\frac{1}{7200}$; that its mean depth was 3 feet 7 inches, and its mean velocity 25 inches per second; and as the theoretical velocity of an inclination of $\frac{1}{7200}$ gave 26 inches 10 lines instead of 25 inches, the excess was occasioned by friction and the bend of the river. In the Loire the inclination was 2 metres per 100 toises, or $\frac{1}{3600}$, the mean depth 34 inches; but the velocity due to the depth was 38 inches per second, consequently $\frac{1}{15}$ was lost by friction and adhesion; the actual velocity being 35 in 6 lines.

In regard to the velocity, Dubuat may be said to have discovered the following laws:

1st, In small velocities, the velocity in the axis is less than that at the bottom;

2nd, This ratio diminishes as the velocity increases, and in very great velocities approaches to the ratio of equality;

3rd, Neither the magnitude of the channel nor its slope has any influence in changing this proportion while the mean velocity remains the same, whatever be the nature of the bed;

4th, When the velocity in the axis is constant, the velocity at the bottom is also constant, and is not affected by the bottom of the river or the magnitude of the stream.

In some experiments the depth was thrice the width, and in others the reverse, without any change in the ratio of the velocities. Another most important fact discovered by him is, that

* See an excellent article on Hydrodynamics in Brewster's *Edinburgh Encyclopædia*.

the mean velocity in any pipe or open stream is an arithmetical mean between the velocity in the axis and the velocity at the sides of the pipe or bottom of the open channels.

Let V be the mean velocity, v the velocity at the axis, u the velocity of the bottom:

$$u = \sqrt{v - 1}^2 \text{ and } V = \frac{v + u}{2};$$

$$\text{also } v = (\sqrt{V - \tfrac{1}{4}} + \tfrac{1}{2})^2 \text{ and } v = (\sqrt{u + 1})^2$$

$$V = (\sqrt{u - \tfrac{1}{2}})^2 + \tfrac{1}{4}) \qquad \text{and } V = (\sqrt{u + \tfrac{1}{2}})^2 + \tfrac{1}{4}$$

$$u = (\sqrt{v - 1})^2 \qquad \text{and } u = (\sqrt{V - \tfrac{1}{4}} - \tfrac{1}{2})^2.$$

Also $v - u = 2\sqrt{V - \frac{1}{4}}$ and $v - V = V - u = \sqrt{V - \frac{1}{4}}$; that is, the difference between these velocities increases in the ratio of the square roots of the mean velocities diminished by a small constant quantity. The place of the mean velocity in moderate velocities is about $\frac{1}{4}$th or $\frac{1}{3}$th of the depth from the bottom; in very great velocities it is higher. (See Dubuat's Table of Velocities, also Robison's *Mechanical Philosophy* and *Theory of Rivers*.) There are, however, anomalies in these principles which render their application extremely doubtful. It is unnecessary to enter into detail of Dubuat's method of rendering rivers navigable by increasing their breadth or by diminishing their inclinations, nor of the different cases of the motions of canals for irrigation or drainage, and the effect of obstruction, such as bridges, sluices, dams, &c.; they have been investigated very fully by Professor Robison and by M. Le Creulx*.

Such is a brief outline of Dubuat's work†, ingenious in many respects and abounding with new views and valuable suggestions; but whoever has had occasion to investigate the uncertain motions of rivers will find that the analogies attempted to be derived from the motions of water in pipes and artificial channels are extremely vague. His formula of the uniform motion of water, modified as it is by contraction and resistances, approximates very nearly to reality. In all cases his theory of the effects of curves is quite contrary to nature, and this he acknowledges in reference to several experiments in the Seine and Marne rivers. His application of his theorem of the expenditure, velocity, and inclination of the surface of a river being known, to determine the dimensions of the bed, is necessarily incorrect.

* *Examen Critique de l'Ouvrage de M. Dubuat sur les Principes de l'Hydraulique*: par M. Le Creulx. Paris 1809.

† *Elements of Mechanical Philosophy*, vol. ii., edited by Dr. Brewster; and *Theory of Rivers*.

Neither do the results of his experiments on the amount of expenditure correspond with those deducible from the rate of inclination of the surface.

The uniform motion which he has supposed in rivers scarcely exists in nature.

The article on rivers contained in the fourth volume of the *Architecture Hydraulique*, by Belidor, published in 1759, is compiled from the works of Guglielmini and Michelini.

The *Nouveaux Principes d' Hydraulique* of Bernard, published in the year 1787, contains much that is valuable relative to the origin, formation, and establishment of rivers.

His theory of the efflux of water from the sides of a prismatic vessel and along an inclined channel, and the pressure sustained by a diaphragm placed at one of its extremities, is founded upon the principles of Bernoulli, D'Alembert, Bossut, and Dubuat; the practical applications are derived from Guglielmini and other writers. His observations on the inclinations and velocities of several of the rivers in France, such as the Saone, the Durance, the Rhone, led him to conclude, that there existed no precise rules in these respects. In several, the mean velocity was found to be $\frac{4}{9}$ths of the depth.

According to Lalande, all rivers increase the height of their waters as they approach their embouchures; the Saone was observed to swell higher at its confluence with the Rhone, at Lyons, than a league above it.

Bernard concludes with Frisi, that the gravels found in the beds of rivers are not owing to the attrition of rocks and larger stones in the upper parts of the beds of rivers, but that they exhibit themselves accidentally, accordingly as they are traversed by the rivers. The swell and consequent action of rivers are greatest at their points of junction. Inundations are greater in the superior than in the inferior parts of rivers, on account of the pressure of the upper waters, although the velocity of the lower waters be greatest. The same had been remarked by Castelli and other writers.

The banks of the Po are 20 feet in height at 50 or 60 miles distance from the sea, whereas at 10 or 12 miles distance from the sea the banks are only 12 feet in height, whilst the breadth of the river is the same in both places.

Amongst the subsequent writers of the French, German, and Dutch schools, may be mentioned Fabre, Lecreulx, Sturm, Leupold, Meyer, and Brunnings.

All of them merit attention, from the many valuable observations with which they abound relative to the natural phenomena of rivers, but it is doubtful whether they have advanced the science.

In the years 1789 and 1790, Brunnings undertook an extensive series of experiments for the purpose of determining the relation between the superficial and mean velocities of the Rhine and Waal rivers which traverse Holland. For this purpose he constructed an ingenious tachometer upon the principle of exposing a disc of wood or metal of any given magnitude to the direct action of the current at different depths, so that by the pressure of the disc against a lever placed above, the pressure and consequent velocity was indicated very nearly.

The results are shown in the following Table:

Rivers.	Depth.	Velocity.		Experiments of Ximenes on the Arno.
		Surface.	Mean.	
	Met.			
Waal	1·57	0·670	0·627	0·934
Ditto	1·57	0·708	0·664	0·938
Lower Rhine .	1·88	0·874	0·779	0·892
Ditto	2·51	1·001	0·926	0·925
Higher Rhine .	2·51	1·097	1·058	0·965
Issel	2·82	1·283	1·218	0·965
Ditto	2·82	1·289	1·243	0·965
Lower Rhine .	2·82	1·307	1·259	0·963
Waal	3·45	1·025	0·938	0·915
Lower Rhine .	3·45	1·379	1·320	0·957
Higher Rhine .	3·76	1·307	1·220	0·936
Lower Rhine .	3·76	1·397	1·286	0·921
Ditto	3·76	1·416	1·361	0·962
Ditto	3·76	1·433	1·369	0·954
Ditto	4·08	1·484	1·341	0·934
Waal	4·39	1·184	1·068	0·902
Ditto	4·39	1·226	1·131	0·923
Higher Rhine .	4·39	1·467	1·332	0·908
Arno	4·57	1·004	0·923	0·919

Admitting, however, the accuracy of these experiments, it is difficult to come to any other conclusion than that of a gradual though feeble diminution of the velocity (about $\frac{1}{10}$th) between the superficial and mean velocity.

Woltmann regarded the diminution according to the ordinates of a parabola reversed.

Funk substituted the logarithmic scale, namely, whilst the depth increases in an arithmetical progression, the velocity decreases in a geometrical progression.

Eytelwein, finding that no constant law could be discovered by his experiments, finished by admitting, by way of approximation, a decrease of velocity in an arithmetical progression, and a

diminution of $\frac{1}{40}$th of the superficial velocity for each metre in depth, so that v being the velocity, d the required depth, the mean velocity of the particles will be $v\,(1 - 0{\cdot}0125\,d)$.

But the experiments of Brunnings do not authorize such a conclusion.

The tachometer of Woltmann, published in the year 1790, was constructed upon the principle of the common windmill, and consisted of four small vanes, attached to an axle and connected with wheel-work: the instrument, being exposed to the direct action of the current at different depths, indicated by the number of its revolutions the velocity of the stream.

The following Table gives the result of some of the experiments made by Funk (amongst others on the Elbe and Weser,) on the superficial and mean velocities of different parts of the same section of the river contrasted with the experiments of Dubuat.

Partial Sections.			Velocity of the Surface.	Mean Velocity according to		Expense according to	
Section	Depth.	Area.		Experiment.	Dubuat.	Experiment.	Dubuat.
	Metres.	Sq. Mets.	Metres.	Metres.	Metres.	Cub. Met.	Cub. Met.
A	0·55	12·31	0·403	0·403	0·315	4·96	3·87
B	0·91	16·75	0·471	0·442	0·370	7·41	6·19
C	2·70	36·05	0·431	0·267	0·337	9·62	12·15
D	6·28	40·78	0·451	0·264	0·353	10·78	14·37
E	6·71	50·12	0·412	0·248	0·322	12·44	16 13
	Total	156·01				45·21	52·71

The following experiments, by Brunnings, on the velocities of the Rhine, Elbe, and Weser, were also made by Woltmann's machine.

Rivers.	State of Water.	Mean Breadth.	Mean Depth.	Inclination.	Mean Velocity.	Volume of Water per second.
		Metres.	Metres.	Millemetres.	Metres.	Cub. Metres.
Rhine	ordinary	514	3·63	0·000115	0·91	1673
Ditto, at Nimuza .	high	521	4·93	0·000115	1·31	3395
Weser	low	105	1·98	0·000411	1·58	328
Ditto, at Ktow . .	high	144	4·12	0·000550	2·41	1428
Elbe	low	96	2·64	0·000254	1·15	294
Ditto, at Magdebourg	high	96	4·07	0·000363	1·63	639

It is somewhat remarkable, that the *Nouvelle Architecture* of M. Prony, published in 1790, contains nothing relative to rivers.

The section on Hydrodynamics is confined to the exposition of the ordinary motion of fluids, and the resolution of the problem of the efflux of a fluid from an orifice made in the side of a prismatic vessel on the principle of the parallelism of sections.

The discovery of the law of the resistances of a fluid in relation to the velocity, by Coulomb, paved the way to its successful application to the case of a fluid moving in natural or artificial channels by Girard; that distinguished mathematician and engineer was then charged with the works of the Canal de L'Ourcq. The researches made by him on that subject led to the publication of several memoirs on the theory of running water, in one of which he proposed the adoption for the value of the resistance, of the product of a constant quantity (determined from twelve experiments of Chezy and Dubuat,) by the sum of the first and second powers of the velocity, from which he obtained a formula applicable to every case; that is, supposing the mass of water to glide over a film of the same fluid adhering to the periphery of the channel, the mass is at first retarded by the viscosity of the rubbing surfaces in the proportion of the velocity, a second resistance arising from the asperities of the channel compounded of the number and force of the impulsions in a given time, and hence proportional to the square of the velocity.

The analogy supposed by M. Girard to exist between the motion of water in an inclined channel, and a perfectly flexible chain placed on a fixed or flexible surface, and his examination of the best form for the transverse section of a channel, which he finds to be the arc of a circle, are ingenious conceptions. His theory of the resistances which influence the motions of water was first published in the year 1804, and is remarkable for expressing them by a very simple function, compounded of the two first powers of the mean velocity, and with more accuracy, than the formula of Dubuat. Mons. Girard is also the author of several interesting memoirs on the river and canal of Ourcq*, the latter of which was laid out upon the funicular principle.

On the subject of locks for navigable canals, M. Girard devotes three memoirs, for the purpose of developing the advantages obtained in point of economy, by reducing the height of locks. The system is explained with that simplicity and elegance which characterize the writings of this author. The conclusions, how-

* *Mémoires sur le Canal de L'Ourcq et la Distribution de ses Eaux, le Desechement et l'Assainissement de Paris et les divers Canaux navigables qui ont été mis en Exécution ou projetés dans le Basin de la Seine pour l'extension du Commerce de la Capitale.* Tome 1. Paris 1830.

ever, have been contested by several engineers, and by M. Minard in his *Observations sur un Systeme à petites Chutes.*

A fourth memoir*, published in the year 1826, examines the question of the relative advantages and disadvantages which belong to the conjoined or separate systems of locks. In this memoir, M. Girard examines, under the different circumstances of evaporation and filtration, the quantity of water necessary to maintain a navigation of any given extent, the conditions of which cannot always be fulfilled.

M. Girard refers the failure of all the schemes, hitherto projected, for the purpose of replacing the defects of the common lock, to the impossibility of resolving the problem completely, without an unnecessary expenditure of mechanical force, and therefore reduces the maximum effect of the common lock to questions of the comparative time and economy required by boats for passing insulated or conjoint systems of locks. The expression for the latter case is singularly modified in favour of small rises of locks, when the boats pass in succession, and is in favour of the conjoint system with regard to time.

In the formula†, number 4, $T = \frac{l}{i} + \frac{2\,s\,\sqrt{a\,n}}{o\,\sqrt{2\,g}} - \frac{n\,L}{i}$, the time employed by the boat in passing through a number n, of simple or isolated locks, distributed over the total inclination o. The author does not take into consideration the stoppage of the boat, and consequent loss of time occasioned by the repeated changes in the force of trackage required by the isolated system. The value of the water lost can only be contrasted with the value of time under certain circumstances; the question had already been discussed, by Gauthey and others, with reference to the locks of the canals of Briarie and Languedoc. An abstract of M. Girard's other hydraulic researches has already been given

* *Quatrième Mémoire sur les Canaux de Navigation considérés sous le rapport de la Chute, et de la Distribution de leurs Ecluses.* Par M. Girard.

† The following are the formulæ:

1. $\frac{s\sqrt{a}}{o\sqrt{g}}\left(\frac{n-1+\sqrt{2}}{\sqrt{n}}\right)$

2. $\frac{s\sqrt{a}}{o\sqrt{g}}\left(\frac{n-2+N(1+\sqrt{2})}{\sqrt{n}}\right)$

3. $N = \frac{\sqrt{nn'}+2}{1+\sqrt{2}}$

a = the total height to overcome.
s = the surface of the gate.
o = the orifice or sluice for filling the lock.
g = gravity.
$n\,n'$ = number of locks.
N = number of boats together.

4. l = section of canal.
i = space passed over the platform in a second of time.
= length of the lock.

in the first part of this paper. As we before stated, M. Prony confined himself in his great work on Hydraulic Architecture to the consideration of the dynamics of fluids; but in the year 1801, having been called upon by the Ecole des Ponts et Chaussées to report on the produce of the streams which were required to supply the summit level of the canal that joins the rivers Somme and Scheldt, M. Prony investigated the subject with his usual sagacity, and the result was the publication of his work, in the year 1802, on the measurement of streams*. His method was to inclose a certain portion of the channel of the stream by means of dams thrown across it at certain distances from each other; and, by noting the time required to fill or empty the space so inclosed, the volume of water which passed through a given section in a given time was easily ascertained. M. Prony, however, does not deny the superiority of the system (where practicable) of ascertaining the expenditure of streams by means of recipients of any given capacity; but it is in his Physico-Mathematical researches † that he developes his general principles of fluids.

The principal results are:

1st, That a fluid, such as water, which runs through a pipe or canal of a sufficient length to establish an equilibrium, experiences resistances which are equal to the force of gravity, and produce uniformity in the motion of the stream;

2ndly, That although the experiments of Amontons and Coulomb on the friction of solids give the results in the direct ratio of the pressure, the experiments of Dubuat, Dobenheim and Benezeck on the friction of fluids show that pressure has little or no effect;

3rdly, That in every transverse section the different molecules taken perpendicularly to the section move with different velocities; but that there is a point where the velocity is a maximum, as in the centre of a pipe or at the surface of an open canal, and that from these centres there is a progressive diminution of velocity towards the periphery;

4thly, That besides the maximum velocity, there exists a minimum and mean velocity, by which the motion of the general mass is regulated;

5thly, That when the fluid runs through a pipe or channel capable of being wetted, a film or bed of fluid adheres to the interior of the pipe or channel, which is the true bed of the fluid mass in motion;

* *Jaugeage des Eaux Courantes:* par M. Prony. Paris 1802.

† *Recherches Physico-Mathématiques sur la Théorie des Eaux Courantes:* par M. Prony. Paris 1801.

6thly, That the experiments of Dubuat with pipes and channels composed of different substances are in accordance with this doctrine;

7thly, That the adhesion or cohesion of the particles of the fluid to each other, and to the surface of the pipe, require to be represented by different values, capable of being compared with each other.

The remainder of M. Prony's physico-mathematical researches is devoted to the examination and determination of the general relations which subsist between the longitudinal and transverse sections to the perimeters, and the velocity of the water under the influence of friction and viscosity; the whole is illustrated by tables and formulæ derived from numerous experiments by Couplet, Bossut, Dubuat and Chezy.

The corps of engineers of roads and bridges of France have contributed largely to our knowledge of the theory of rivers, and the numerous experiments which have been undertaken by different engineers of that body have confirmed in a great degree the theories advanced by preceding writers. The experiments which merit the most attention are those of MM. Raucourt and De Fontaine; the former on the river Neva at St. Petersburgh, and the latter on the river Rhine.

The object of M. Raucourt's experiments was to ascertain how far the law of the velocities coincided with the theory of the motions of water in pipes and open channels when the river was frozen, and when free from ice.

Accordingly, he embraced the opportunity of the Neva being frozen over in the year 1824; and having selected a place where the width of the river is 900 feet, and the greatest depth 63 feet, and the section very regular, and consequently assimilated to the case of an immense pipe, he provided an instrument, constructed on the principle of the common ship's log, and ascertained the velocities by sinking the instrument through several holes made in the ice at proper intervals: the maximum velocity was found to be a little below the centre of each vertical, and diminished as it approached either bank of the river. The same relative velocities, differing only $\frac{1}{8}$th from each other, were found to prevail after repeated trials. The results were, that the greatest velocity was found to be a little below the centre of the deepest vertical:

	ft.	ins.	
viz.	2	7	per second.
	1	11	ditto . . . . near the top.
	1	8	ditto . . . . near the bottom.

In the summer of the year 1826 M. Raucourt performed

similar experiments on the same river, both in calm and windy weather: the maximum velocity was then found to be equal to the velocity at the surface; but when the surface was affected by winds, the acceleration was greater or less.

M. Raucourt's experiments have been partially tried by Messrs. Detrem and Henry; the latter made the relation between the mean and superficial velocities in the proportion of 0·715 to 0·903, and the product of the Neva 116,000 cubic feet (English measure) per second: the maximum velocity diminished from the upper to the lower part of the river from 1·79 metres to 1·015 metres. The inclination per thousand metres was found to be 0·0267 *.

But the most important observations which have been made on rivers in modern times are those of M. De Fontaine on the river Rhine and its affluents.

Having been entrusted with the execution of certain works in the year 1820, for the purpose of restraining and regulating the course of that part of the river which adjoins the French territory, M. De Fontaine felt it his duty to investigate the phenomena exhibited by that river in different parts of its course, and the result has been the publication (in the year 1833) of his observations in detail, in a work, entitled, *Travaux du Rhine*. According to M. De Fontaine, the river Rhine derives its origin from the glaciers of St. Gothard, in Switzerland, whence the waters run by three principal affluents to Reichenau, where they unite into one great river; after being increased by numerous torrents from the Alps, it empties itself into the Lake of Constance, out of which it passes to be precipitated over the falls of Schaffhausen and Lauffen, and, after having received by means of the river Aar $\frac{4}{5}$ths of the waters of Switzerland, passes through the great valley which separates the mountains of the Vosges from those of the Black Forest; thence it passes through the narrow defiles of Bingen, thence through Holland, after which it divides itself into several branches (to one of which it gives its name), and empties itself into the German Sea above Leyden. In its course it receives many considerable affluents, such as the Elser, the Kinzig, the Ill, the Moder, and the Murg on the French boundary, and the Moselle, Mayne, Meuse, and others as it approaches the sea; it communicates with the Zuyder Zee by means of the Isel. The irregularity of its course and the

* *Journal des Voies de Communication*, 8vo, 1826, St. Petersburgh: "Sur le Jaugeages de la Neva et de ses differens bras."

† *Des Travaux du Fleuve du Rhin:* par A. J. C. De Fontaine, Ingénieur en chef de premiere Classe des Ponts et Chaussées.

ravages constantly committed on its shores, particularly those of Alsace, one of the most fertile provinces of France, rendered the construction of defensive works imperative, and it was to counteract these evils that M. De Fontaine was selected to fulfill this important task.

The length of the course of the Rhine from Reichenau to the sea is stated to be 1342 myriametres, viz.

From Reichenau to the French frontier . . .	420
Along the French shore	222
Thence to the sea	700
	1342

From careful barometrical observations, the heights of the low waters above the level of the sea at the following places are:

	Metres.
At Reichenau	1194·00
At the lake of Constance	405·00
At the bridge of Basle	252·30
At the bridge of Kehl	138·96
At the confluence of the Lauter, which is the limit of the French frontier .	107·00
At the bridge of Manheim	93·00
At the entry of the defiles of Bingen . .	67·00

The general inclination, according to the three great sections of the Rhine, is

	Falls.	
	Total Fall.	Per Metre.
	Met.	
From Reichenau to the French frontier	941·71	0·002242
Along the French territory	145·30	0·000653
Thence to the sea	40·00	0.000057
	1127·01	

The part of the Rhine to which M. De Fontaine principally directed his attention is comprised between Basle and Neubourg, that being the political limit between France and the German states. In this part the bed of the Rhine is situated in the alluvium which forms the bottom of the valley, and through this the Rhine forces its way by many channels, forming (in its passage) islands and sandbanks, which render its motions very irregular both in times of high and low water. Among the

different channels there is generally one more considerable than the others, and which forms the navigable channel, or *thalweg*, as it is there termed. These branches are annually diminished by artificial works, and it seems probable that in a few years hence, the whole of the waters of the Rhine will be forced into one channel. In general, however, the Rhine may be compared in the upper parts above Bavaria to an immense torrent.

The *inclinations* vary according to circumstances, but the greatest inclination is near Basle, at low water, on account of the rocks, which inclination decreases $\frac{4}{5}$ths in times of flood.

The inclination of the Upper Rhine, in its mean state, is 0·964024 per 1000 metres; while at the frontier below the confluence of the Lauter, after a course of 222·460 metres along the French shore, the inclination is only 0·395185 metres per 1000 metres, or a third only of the inclination of the upper part; then taking the total fall at 143·935 metres, the mean inclination would be 0·647015 per 1000 metres, which is nearly the inclination of the river at Brisack and Sponeck, that is, about a third of the total length of the river.

The velocities of the Rhine vary not only according to the differences in the inclinations, but according to the perpetual changes which the river undergoes in its motions from the irregularities in its bed. The following are the velocities:

Names of Places.	Velocities per Second.		
	Low Water.	Mean Water.	High Water.
	Met.	Met.	Met.
Basle	1·65	2·25	4·16
Huningen	1·70	2·75	—
In the angle of Krembs	1·88	2·62	—
——— Schalampe	2·67	2·79	—
Opposite Vieux Brisach	1·81	2·15	3·60
At Sponeck	1·52	2·87	—
At Artolsheim	1·97	—	—
At Rhinau	2·51	—	—
At Guerstheim	2·19	—	—
At the bridge of Kehl	1·50	2·13	2·85
At Offendorff	1·40	2·25	—
At Drusenheim	1·49	1·97	—
At Beinheim	1·24	1·73	—
Limit of the Bavarian frontier	0·97	1·56	—
At Manheim	0·70	1·20	2·30

The conclusions are, that the decrease of the velocities is irregular, and that they do not follow the law of the square roots of the inclinations, nor the cube roots of the wetted perimeters.

Maximum and Minimum Depths under the mean level of the Water.

The greatest depths of the Rhine do not exceed, on the average, four or five metres, except in particular places, such as the

Rock of Istein, where the depth is	9·70	metres;
At Rhein Weiller	7·00	—
At the Spur of Blodelsheim	12·00	—
At the foot of the volcanic rock of Sponeck .	11·50	—
At the extremity of ditto	18·00	—
At the foot of the Glasserwoerth	25·00	—
——— ——— Plitersdorff	13·60	—

From which it appears that the influence of corrosion is very great where the current is obstructed, and hence the necessity of the artificial works undertaken by M. de Fontaine.

Floods.—The floods of the Rhine occur periodically, namely, from the end of May to the middle of September, during the melting of the glaciers, after which the river returns to its ordinary flow. The greatest floods generally happen about March, after the first melting of the snows, but they occasionally occur in other months. The rise of the waters at Basle seldom commences until three days after the greatest rains and meltings of the snow; the greatest rise in 24 hours never having exceeded 2·92 metres at Basle in 22 years; and at Kehl, 1·38 metres in 27 years. The floods of the affluents (between Basle and Lauterbourg,) which descend from the Vosges and Black Forest mountains, are generally over before the arrival of the floods from Switzerland. This phænomenon arises from the great difference which prevails between the sections at Basle and Kehl: for some time the Rhinometers at the two places indicate nearly equal elevations; but as soon as the floods commence, the elevations no longer maintain the same relation to each other; on the contrary, when the Rhine has risen 0·01 metre at Kehl, it has risen 0·016 metre at Basle; and this relation occasionally varies with the changes in the two sections. The years 1801 and 1824 were remarkable for the extreme rises of the waters, not only in the Rhine but in all the rivers of France.

Tables are added in M. de Fontaine's report showing the maximum and minimum oscillations of the waters at Basle, Kehl, and Lauterbourg, for 22, 27, and 10 years respectively.

Expenditure of the Rhine.

(From a series of gauges taken at Basle, Vieux Brisach, and Kehl, according to the different states of the river at these places.)

The volume of water which passes,

At Basle,

		Cub. Met.
During great floods	is equal to	4624
During mean water	do.	865
During very low water	do.	330

At Vieux Brisach,

During great floods	is equal to	4630
During mean water	do.	885
During very low water	do.	340

At Kehl,

During great floods	is equal to	4685
During mean water	do.	956
During low water	do.	380

At Lauterbourg,

During great floods	is equal to	5010
During mean water	do.	1106
During low water	do.	465

From which it results, that the volume of water which passes per second during great floods, compared with the volume which passes during the lowest waters, varies from 10 to 1, to 14 to 1; and in comparison of the mean to the low water, from $4\frac{1}{2}$ to 1, and $5\frac{1}{3}$ to 1.

The remaining and indeed principal part of M. de Fontaine's report is devoted to an account of the artificial works which have been undertaken for the purpose of regulating the course of the Rhine, in which the various kinds of fascines, embankments, dams, jetties, counterforts, cuts, short channels, and the modes of defending the banks are all spoken of in detail.

The principles which have guided him in the execution of these works are,—

1st, The union of the waters into one channel, and the closing of the secondary branches;

2ndly, The avoidance of all rectilinear cuts, and the adoption of proper curves derived from observations on the rivers themselves;

3rdly, The formation of proper channels corresponding to the different volumes and velocities of the waters;

In the first case, the practice of the engineer must be governed by the volume and velocity of the waters and the nature of the soil:

In the second, by the resistance of the soil:

In the third, by the amount of the high, mean, and ordinary volumes of water.

The velocity of the waters of the adjoining part of the river must also be considered.

The advantage of the curvilinear directions is, that the force of the centrifugal projection of the current on the concave side of the river can be more easily counteracted.

The proper determination of the radii of curvature for the cuts must depend on the inclination and force of the current; and from careful observations of the lengths of the curves in different parts of the rivers, M. de Fontaine determined the maximum lengths of the radii of curvature at 2200 metres where the depth in the curved part of the river was 15·36 metres, and where the corrosion did not exceed 11 metres in depth in the curve; he fixed the minimum length of the radius at 1250 metres.

Declivities of the Rhine, from a series of experiments made with the *Stromm Messer* of Woltmann, on the velocities of the Rhine in different sections, according to the following Table.

Velocities at different parts of the Section.	Number turns of made by the Instrument in 30 seconds.	Space passed through in 30 seconds.	Velocity per second.	Average Velocity per second.	Surface of the Section of Motion of the different Strata.	Mean Velocity of the Section.	Mean Velocity deduced after the Formulæ of that of the Surface.
		Met.	Met.	Met.	Met.	Met.	Met.
At the surface.....	.....	.....	1·0000	0·9959			
— 0·10 met. below.....	68	29·308	0·9769	0·9769			
— 0·20 ———	$67\frac{1}{7}$	29·020	0·9673	0·9578			
— 0·30 ———	$65\frac{2}{9}$	28·111	0·9370	0·9388			
— 0·40 ———	$63\frac{4}{9}$	27·345	0·9115	0·9198			
— 0·50 ———	62	27·024	0·9008	0·9008			
— 0·60 ———	$61\frac{7}{10}$	26·291	0·8764	0·8817			
— 0·70 ———	59	25·429	0·8476	0·8627			
— 0·80 ———	58	24·998	0·8333	0·8437	1·0131117	0·84426	0·81189
0·8347 intersection of the two divisions.....	.....	.....	.....	0·8371			
— 0·90 met. below.....	$52\frac{5}{9}$	22·682	0·7563	0·7867			
— 1·00 ——— ..	$49\frac{3}{9}$	21.281	0·7094	0·7094			
— 1·10 ——— or — 1·10 met. from the bottom....	44	18·964	0·6321	0·6321			
Bottom	.....	.....	.....	0·5548			

Note.—These velocities have been taken from experiments made over an extent of 60 metres, by means of a float, so suspended that its specific weight did not exceed that of the water.

The conclusions from the preceding observations, are,

1st, That the greatest velocity is at the surface:

2ndly, That the velocity (which at first diminishes insensibly downwards,) decreases rapidly towards the bottom, in a ratio dependent on the nature of the bed:

3rdly, That supposing two right lines to pass through the extremity of four ordinates, determined by experiment, and conveniently chosen in the curve, which should pass through all the points obtained, the ordinates of these right lines, corresponding to the velocities observed in the other points, will differ little in the numerical expression of these velocities:

4thly, That the point of intersection of two right lines which each partial surface of partial motion circumscribes, has for its ordinate a numerical value which differs very little from the mean velocity expressed by the quotient of the surface of motions divided by the depth of the water:

5thly, That the mean velocities resulting from the preceding observations are greater than the mean velocities deduced from the velocity of the surface by means of the formula adopted for gauging streams:

6thly, That the position of the ordinates, which expresses the mean velocity of each surface of partial motion, is nearer the bottom than the surface, or $\frac{2}{3}$rds of the depth, reckoning from the surface, and $\frac{1}{2}$ the depth when the bottom is very regular.

Forms of the Surface of Rivers.

Opinions vary very much on this subject; some maintain that the surface is convex, others concave, and others horizontal. M. de Fontaine finds the form of the surface to vary accordingly as the river is rising, falling, or slack.

After explaining in detail the principles which have guided him in regulating the course of the Rhine between Basle and Lauterbourg, a distance of 194,490 metres, M. de Fontaine gives the following Table as the probable results of the action of the river when turned into the new course.

Table showing the probable results of the action of the river Rhine, when turned into different parts of its artificial course.

Names of the different parts of the River.	State of the River.	Greatest length.	Incline per Metre.	Surface of the Section.	Wetted Perimeter.	Mean Radius.	Product of the Inclination by Mean Radius.	Mean Velocity.	Volume of Water. According to the Gauges.	Volume of Water. According to the Sections.	Breadth of the Section at the surface.	Greatest depth of the Water.	Difference between the high and low Water.
		Met.		Met.	Met.	Met.	Met.	Met.	Met.	Met.	Met.	Met.	Met.
Part of the course of the Rhine, between Huningen and Old Brisach	low water	59·030	0 00097	160·15	110·15	1·4539	0·001410283	2·070	330	330·87	109·55	2·28	6·72
	mean do.		0 00096	314·47	121·82	2·5814	0 002478144	2 760	865	867·94	120·80	3·62	
	high do.		0·00096	1294·31	302·59	4·2774	0 004106340	3·574	4·624	4625·86	300·00	9·00	
Part of the course of the Rhine between Old Brisach and Rhinau	low water	37·960	0 00087	170·93	111·85	1·5282	0·001329540	2 004	340	342 54	111·24	2·25	6·75
	mean do.		0·00085	335·21	124·50	2·6924	0·002288540	2·650	885	888·31	123·46	3·65	
	high do.		0·00085	1421·50	352·56	4·0319	0·003427149	3·259	4·630	4632·67	350·00	9 00	
Part of the course of the Rhine between the Rhinau and Kehl Bridge	low water	35·000	0·000645	203·59	113·48	1·7940	0·001157169	1·8655	380	379·80	112·77	2·67	6·33
	mean do.		0·000635	392·26	127·41	3·0787	0·001954988	2·4450	956	959·07	126·25	3·73	
	high do.		0·000635	1721·00	452·66	3·8019	0·002155049	2·720	4·685	4681·12	450·00	9·00	
Part of the course of the Rhine between Kehl Bridge and Lauterbourg	low water	62·500	0 00049	257·43	115·06	2·2373	0·001096302	1·810	465	465·95	114·15	2·91	7·69
	mean do.		0·00048	471·97	127·08	3·7139	0·001804389	2·350	1·106	1107·24	125·55	4·70	
	high do.		0·00048	2010·80	453·32	4·4357	0 002155049	2·570	5·010	5167·76	450·00	10 60	
		Total 194·490											

Although the lock had been long known in Italy, its introduction into France did not take place until the reign of Francis the First, when a lock was erected on the river Ourcq, by Leonardo da Vinci. The improvement of the navigation of the Seine, and the junction of the Seine and Loire, were preludes to the execution of the canal of Briare under Henry the Fourth, in the year 1605, terminated in the year 1642 under Louis the Thirteenth, and which was followed by the canals of Orleans, of Lourg, of Beaucaire, and the junction of the Mediterranean and the Atlantic seas by the celebrated canal of Languedoc, executed by Riquet in the reign of Louis the Fourteenth, so well described by Lalande, Andréossi, Gauthey, &c., and since made the subject of the scientific researches of Navier, Prony, and Girard. The works of Huerne de Pommeuse, of Becquey, of Brisson, and Dutens show the advanced state of the canals of that country.

The following is a general statement of the canals of France, according to M. Dutens.

	Length.	Estimated Cost.	
	Met.	francs.	cns.
1st. Canals comprised in seven lines of junction of the two seas	3·068·876·90*	306,429,601	50
2ndly. Canals leading to Paris	1·020·022·64	143,935.916	0
Secondary or projected Canals.			
1st	1·955·200·00	114,682,870	00
2ndly	6·511·200·00	573,297,863	50
Total	12·555·299·54	1,138,346,251	0

On the subject of regurgitations, or the swelling of rivers, by obstacles placed in them, such as dams, weirs, jetties, bridges, or contractions of their channels, it only remains to notice the experiments of Eytelwein, Bidone and Funk.

An abstract of Eytelwein's experiments, by the late Dr. Young, has already been given in the former part of my Report. When the motion of a river is obstructed by a dam placed directly across it, the surface of the river rises, and the water passes over

* The author of this paper is indebted to M. Le Grand, Directeur Général des Ponts et Chaussées, &c., Conseiller d'Etat, for a detailed statement of the canals of France, from which the total length of the seven great lines of junction is 3,679,033 metres.

† *Rapport au Roi Louis XVIII. pour l'an* 1820, *sur la Navigation Intérieure de la France* : par M. Becquery, Conseiller d'Etat, Directeur Général des Ponts et Chaussés.

the dam. The height of the river above the dam will be the quantity designated by H in the formula, now adopted by Messrs. Poncelet and Lesbros, for the expenditure through notches in dams, where $a = 1{\cdot}80\ l\,\mathrm{H}\sqrt{\mathrm{H}}$, where 1·80 is the value of m in the expression of $\frac{2}{3}m\sqrt{2g} = 0{\cdot}610$; if therefore b be the height of the dam from the bottom of the river, and b' the depth of the water before the erection of the dam, the height of the swelling of the water will be $\mathrm{H} + b - b\,l$, as we before stated. The merit of having discovered the true law of the expenditure of water through notches in dams belongs to M. Bidone*, but he has carried his researches still further by his valuable experiments on the effects of dams and other obstructions in raising the surface of running waters.

The following Table gives the results:

Expenditure per Second.	Current without the Dam.		Height of the Dam.	Swelling.		Hydrostatic Amplitude.	Difference between the two amplitudes afterwards.	
	Velocity.	Depth.		Height on the Dam.	Amplitude.		Observed.	Calculated.
Met. Cub.	Met.	Met.	Met.	Met.	Met.	Met.	Met.	Met.
0·0208	1·361	0·047	0·134	0·102	4·33	6·69	2·56	2·43
			0·160	0·100	4·87	7·44		
			0·188	0·102	5·70	8·28		
			0·217	0·102	6·53	9·04		
0·0351	1·683	0·064	0·136	0·137	3·66	7·32	3·73	3·71
			0·161	0·140	4·44	8·17		
			0·189	0·143	5·22	9·01		
			0·215	0·144	5·87	9·71		
			0·242	0·143	6·69	10·29		
0·0467	1·936	0·074	0·135	0·167	3·36	7·90	4·56	4·91
			0·161	0·168	4·09	8·61		
			0·187	0·167	4·70	9·31		

Dubuat was the first who turned his attention to the forms and extent of swellings. Observing that the depth of the water towards the dam increased, whilst the velocity decreased in the same proportion, he concluded that the surface was a concave, similar to an arc of a circle, and calling H the height of the swelling, p the inclination of the current before the erection of the dam, and p' the inclination of the swelling immediately in front of the culminating point, which he makes equal to $\frac{1{\cdot}9\,\mathrm{H}}{p - p'}$, $\frac{\mathrm{H}}{p}$

* "Expériences sur la Dépense des Réservoirs," par George Bidone, 1824, tom. xxviii. des *Mémoires de l'Académie des Sciences de Turin.*

is the hydrostatic amplitude; and as p' is very small in proportion to p, the one amplitude will be double the other.

Funk, after having demonstrated that Dubuat's method gave too great an excess, admitted that the surface was a concave arc of a parabola, at a distance from the dam of twice the hydrostatic amplitude, of which the perimeter would be $\frac{1}{2}\,H'\,p$; consequently at any distance from the dam, the height of the swelling below the surface of the current is $2\,H' - p\,a - \sqrt{H'^2 - \frac{1}{2}H'\,p\,a}$. When $a = \frac{3\,H'}{2\,p}$ this height is zero, from whence the point or the surface of the swelling joins the current. The amplitude will then be $\frac{3\,H'}{2\,p}$, or $1\frac{1}{2}$ time the hydrostatic amplitude.

The following are the results:

Amplitudes.	
By Observations.	By Calculations.
Metres.	Metres.
7127	7007
5868	6984
1940	2128
785	900

This question has also been examined by Messrs. Bélanger and D'Aubuisson*.

In like manner the contractions of rivers, by natural and artificial causes, occasion a rise in the surface of the water equal to the difference between the heights of the water before and after the contraction: the same applies to the piers of bridges and to jetties; both cases have been examined by Dubuat, Eytelwein, and Funk. Funk in particular made several experiments on the swell occasioned in the river Weser by the bridge of Minden. The mean breadth of the river was 180·71 metres, the mean depth 5·37 metres; the produce of the water was 1318 cubic metres; the height of the swelling was found to be 0·383; the sum of the openings of the bridge was 96·03 metres; the velocity of the river before the swelling was 1·358 metres $\left(= \frac{1318}{180{\cdot}7 \times 5{\cdot}37}\right)$; but the velocity of the upper surface of the

* Page 162 of the *Traité d'Hydraulique à l'usage des Ingenieurs*; par J. T. D'Aubuisson de Voisins: Paris 1834. See also *Venturoli di Meccanica e d'Idraulica.* Milan 1818.

river is generally $\frac{1}{10}$th greater than the mean velocity $v = 1{\cdot}494$ met. As the current, however, was prevented from entering the arches by fenders, placed to protect the piers from the ice, it was necessary to adopt Eytelwein's coefficient of contraction, or 0·855; L, or breadth of the river, being 180·7 metres, and h, or depth of the river, being 53·7; from which we get the numerical value of x, which is equal to $\left.\frac{(1{\cdot}494)^2}{2\,g}\right\}\left.\frac{180{\cdot}7 \times 5{\cdot}37}{0{\cdot}855 \times 96\,(5{\cdot}37 + x)}{}^2 - 1.\right\}$

Neglecting $\frac{5{\cdot}37}{5{\cdot}37 + x}$,

we have the first value of x	= 0·437	metres.
second . . ditto	= 0·358	—
third . . . ditto	= 0·370	—
fourth and last, being the result of calculation,	= 0·369	—
whilst that of the experiment is	= 0·383	—

but such results must necessarily depend upon circumstances.

Progress of Hydraulic Engineering in England with reference to Rivers, Canals, and Drainage.

Though practical works in hydraulic engineering of great magnitude and extent have been carried on in England, the application of this science to rivers has made little or no progress here since its first introduction from the Continent. The demands of of commerce have made us partially acquainted with some of the common phænomena which they present; but the laws which govern their motions, under all the variable circumstances to which they are subject, are involved in mystery. The principles upon which the earliest Acts of Parliament were framed for the conservancy of our rivers consisted in deepening, straightening, and embanking them where necessary, and, by means of sluices and weirs, penning up or lowering the surface of the water for the purpose of producing *flashes* and overcoming the obstructions to navigation. Experience had, however, shown that navigations of this sort were liable to perpetual degradation, from the alterations produced in the regimen of the rivers by such artificial works, which frequently augmented instead of remedying the evil, whilst they obstructed the general drainage of the country.

The circuitous navigation and the trackage against the stream were at all times laborious and dilatory; these difficulties suggested the propriety of deserting the natural bed of the river, and led to the formation of separate cuts with the pound locks*,

* The first lock in England is supposed to have been erected in the year 1675, on the Exeter navigation.

and the various contrivances which were subsequently invented to supersede their use. Until the invention of the lock, therefore, very little could be done in the way of inland navigation, except in the fens, when connected with drainage; accordingly the most ancient attempts of this kind are to be found in the Carr and Foss Dykes * by the Romans,—the former skirting the uplands and fens from the river Nene at Peterborough to the river Witham near Lincoln, by a canal of forty miles in length, and the latter, which connects the Witham at Lincoln with the Trent above Gainsborough, by a level cut of eleven miles in length.

The works also undertaken by the Church in the great level of the fens, such as the cut from Peterborough to Guyhern by Bishop Morton in the year 1478, and afterwards perfected by Charles the First, conjointly with the Bedford Level adventurers, may also be mentioned.

Superficial Content of the Fens adjoining the Wash.

Between the high lands on the south and south-east, and the Great Ouse and Cam rivers, the superficial content is 217 square miles, or	138,880 acres
Between Great Ouse and Cam rivers and river Nene, 394 square miles, or	252,160 do.
Between river Nene and Glen river, 389 square miles, or	248,960 do.
Between Glen river and Old Witham river, 414 square miles, or	264,960 do.
Between Old Witham river and Tetney drain, 201 square miles, or	128,640 do.
Making a total of 1615 square miles,	1,033,360 acres.

The rivers that drain this immense district are,—

The Setch, or Nar,	Holbeach river,
Great Ouse and its tributaries,	Old Welland,
Little Ouse, or Brandon river,	Glen river,
The Cam,	Old Witham river,
Welney,	Old river,
Nene and its tributaries,	Louth river.

* The late Mr. Rennie, in his Report to the Commissioners of the First District of the North Level of the Fens, dated 17th June, 1809, speaking of the Caerr or Carr Dyke, says, "The Carr Dyke acts as a catch-water drain to the whole North Level; and if it were in good condition, and had a good outlet, it would intercept the water falling on 12,000 acres of high land, and would greatly relieve the whole level.

"This great Roman work extended originally from the river Nene below Peterborough to the city of Lincoln, and perhaps the river Trent at Torksey. I

The general drainage act of Elizabeth in the year 1600, and the failure of different attempts that had been made to drain the fens by different engineers, combined with political circumstances, led to the employment of Dutch engineers *, then the most celebrated hydraulicians in Europe; hence may be dated the commencement of British engineering.

The established maxims of the Dutch engineers were to embank the rivers, so as to prevent the land-floods and high tides from overflowing the lands to be drained, to leave open the rivers to the free action of the tides, to conduct the downfall and soakage waters by separate drains to the sea, and to place sluices at the outlets of the drains, which, while they prevented the ingress of the sea during its influx, let off the land-waters when the tides were sufficiently low. These operations, though open to objection, especially as regards the separation of the waters into cuts, and the consequent choking up of the natural outlets of the rivers, gave, however, an impulse to this department of hydraulics, which, until then, had been practised without principles or science.

With the reign of the Stuarts, therefore, may be said to have commenced that system of practical engineering which has flourished with such unparalleled success in this country, and in which so much sagacity has been displayed by Elstobb, Labyle, Kinderley, Dobson, Grundy, Edwards, Smeaton, Brindley, Watte, Whitworth, Page, and Golborn, and other engineers in modern times.

From the Report of the late Mr. Rennie in the year 1800, and according to the levels taken by his direction, it appears that the fens of Lincolnshire, particularly the East, West, and Wildmore fens are generally lower the more distant they are from the sea, and this, on the supposition that they have been originally covered by the sea, must always be the case; hence the great collection of waters which are found in the interior parts of these fens,—the difficulty therefore of draining them has always been great. The great bay or estuary through which the dif-

have traced its course for the greatest part of the way, and a more judicious and well-laidout work I have never seen." In concluding his Report, he says, "If the Carr Dyke be repaired and improved with a proper outfall to the river Welland, there is no doubt that the first and fifth district of the Fens, and indeed the whole level, will be greatly relieved."

* Vermuyden, Westerdyke, and Van Scotten.

Westerdyke's principles were to keep the waters in a body, and convey the land-flood by the nearest and quickest way to the sea that may be.

See the valuable works of Coles and Wells on the Bedford Level. Kinderley and Labyle found it the same, and it may be particularly noticed in the Rother Levels near Rye, in Sussex.

ferent rivers disembogue is very shallow and full of shifting sands and silt. The rivers, which are constantly loaded with silt, particularly in times of flood, are met by the tide equally charged with it; in the still water which is the result of the counteracting forces the sediment is deposited; banks are formed, which are nearer to or more remote from the rivers in proportion to the strength of the current; so that if the seasons be wet, the rivers run to seaward with greater velocity and propel the silt further out; and *vice versâ*, if the season be dry, the outward power is lessened, and the silt deposited nearer to the mouths of the rivers, where it prevents the free egress of the waters from the fens. Such being the statement of the case, the remedy is in a great measure pointed out.

The first object that merits consideration is the outfall.

The second, the discharging of the waters which fall on the surface of the fens.

The third, the intercepting and carrying off the upper or highland waters without allowing them to fall into and overflow the fens.

To effect the first object, Mr. Rennie recommended that the rivers should be conducted to the sea by as short a course as possible, and in this respect adopted the opinion of Kinderley, who was well aware that none of the rivers which pass through the fens are sufficiently powerful to force their way through the immense extent of shallow flats which are left dry at every tide; and therefore proposed the scheme of joining the Nene, the Ouse, the Welland, and Witham rivers. The above principles were afterwards partly carried into effect, and the result has been the most perfect system of drainage of all that district of country eastward of the river Witham, called the East Fen, containing upwards of 62,000 acres of valuable land *.

* The following is the abstract of the low lands paying drainage tax to the general Commissioners for drainage by the river Witham:

	Acres.	Roods.	Perches.
1st district, containing	24,544	3	20
2nd ———————	19,080	2	7
3rd ———————	4,669	3	7
4th ——————— ...	27,743	0	26
	34,483	0	0
5th ———————	4,781	2	19
6th ———————	11,565	2	5
Total,	126,768	2	4

But the total quantity of land drained by the river Witham is estimated to amount to nearly half a million of acres.

The following is an abstract of a statement by the late Mr. Bower relative to the improvements effected by Mr. Rennie's drainage of the East Fens.

To show the advantage of this drainage it may be necessary to state the

In 1810 the attempt was revived to effect a complete drainage of the Great Bedford Level, consisting of 300,000 acres. The drainage here passes off by the rivers Ouse, Nene, and their tributaries, which discharge their waters into the great bay or wash, called the Mætaris Æstuarium.

situation the fens were in every winter and spring prior to any works being executed under the direction of Mr. Rennie. In the year 1799 the whole of the three fens, which contain 40,482 acres, together with the low-lands and commons adjoining, containing about 20,000 acres, were under water, except a small part in Wildmoor and the West Fen; the whole of the East Fen, which contains 12,664 acres; the lower part of the West Fen, containing about 17,052 acres; and the lower part of Wildmoor Fen, containing about 7770 acres, making together 37,484 acres, were every winter under water. The East Fen deeps, containing about 2500 acres, and the part of Wildmoor Fen called No Man's Friend, containing about 1500 acres, were always under water during the summer; the former upon an average, in the driest time, about two feet deep. The quantity of high lands draining through these fens is not less than 12,000 acres, which, in ordinary wet seasons, send down upwards of 40,000 cubic feet of water per minute, which, added to the downfall water upon the fens and the higher lands in the East Holland towns, amounting to about 25,000 acres, soon overflowed the said fens and low lands adjoining. This great body of water had to find its way to the sea through three small gouts, or sluices, viz. Austin's Gout, which had an opening of fourteen feet; Maud Foster, an opening of thirteen feet; and Tichloft, an opening of four feet: the first were of little use, being so high up the river as to be overrode by the most trifling flood. The whole drainage, therefore, of the fens and low-lands had to depend upon the small sluice of Maud Foster, which sluice has three openings of thirteen feet four inches each.

From this it may fairly be said that what is now made of the fens since the drainage is a total gain. The average value at which the fens are now let is as follows:

	Acres.			£	s.	d.
Wildmoor Fen . .	10,773	at	42*s.* per acre . .	22,623	6	0
West Fen . . .	17,044	at	50*s.* per acre . .	42,610	0	0
East Fen . . .	12,664	at	40*s.* per acre . .	25,328	0	0
	40,481 acres.			90,561	6	0
Improved value of low lands .	20,000	at	20*s.* per acre . .	20,000	0	0
	60,481		Per annum	110,561	6	0
Money actually paid for the drainage . .	433,905 at 5*s.* per foot 21,695 5 0					
Ditto, upon the division and roads . .	146,800 at 5 per cent. 7,340 0 0					
Interest per annum.....				29,035	5	0
Increased annual income.....				81,526	1	0

The principles of drainage recommended by Mr. Rennie were,

1st, To shorten and deepen the courses of the existing rivers;

2nd, To form new cuts or drains in different directions through the fens, with inclinations in their beds of from three to five inches;

3rd, To form a catch-water drain round the bases of the hills skirting the fens, and to conduct the upland waters by an inclined bed of six inches per mile, through a separate outlet, into the head of the proposed Eau Brink Cut, into which all the drainage-waters were to be carried likewise.

The expense of completing this magnificent drainage was estimated at £1,188,189.

The Eau Brink Cut was originally projected by Mr. Nathaniel Kinderley in the year 1720: the object was to conduct the waters of the river Ouse by a direct cut across the marshes from Eau Brink to Lynn, of about two miles and half in length, instead of allowing them to flow by the old circuitous channel of upward of five miles in length.

This Cut was completed, agreeably to Captain Huddart and Mr. Mylne's award, under the direction of Mr. Rennie, in the year 1825.

In December, 1821, the tide rose on the average eleven feet ten inches on the cill of Old Denver Sluice; while at low water the average depth on the cill was 9·6 inches, and the average height of the water in the river was 11·5 inches.

Since the completion of the Eau Brink Cut, in the year 1825, the results have been,

That the low-water mark has fallen six feet lower than it formerly stood at Denver Sluice, and from eight to nine feet at Eau Brink.

That the spring tides now rise at Denver Sluice thirteen feet, and neap tides eight feet.

That the river has deepened between Denver Sluice and Eau Brink ten feet upon the average, and its general sectional area has increased from one fourth to one third.

That the low-water mark in Lynn harbour has fallen four feet, and the navigable channel in Lynn harbour has deepened seven feet; and that where there were formerly twelve feet in depth of water in the intercepted bed of the old Ouse between Eau Brink and Lynn, there is now a tract of 900 acres of land under cultivation, all of which has been effected by the process of warping.

The tide in the Eau Brink flows three hours, and rises in that time fifteen feet, thus leaving nine hours of ebb.

The next and most important improvement in the Bedford Level was the *Nene Cut* or Outfall. The river Nene, after passing through Northamptonshire, enters the Level at Peterborough, whence it proceeds in an irregular direction through Guyhern and Wisbeach to the sea near Gunthorpe Sluice, and thence loses itself amongst the irregular channels and sands of the Washway.

The defective state of this river and of the drainage have been at all times complained of; and the attempts which had been made to remedy it, by Bishop Morton in 1478, by Sir Clement Edmonds in 1618, by Kinderley in 1721, and by Smeaton in 1767, had in a great measure failed, not so much from a deficiency of skill on the part of the engineers as from other causes. The very successful drainage of the East Fens in Lincolnshire by Mr. Rennie induced the Commissioners of the North Level to apply to him in the year 1813, and the result was a very elaborate Report from that gentleman in the following year, detailing very fully the causes and effects of the evils, and the measures necessary to remedy them. The following facts are curious:

From accurate levels and sections of the river Nene, it appeared that the fall at low water from Sutton Wash to Crab Hole (below the sands of the Wash) was 12 feet in about 4 miles; from the surface of the water at Gunthorpe Sluice to Crab Hole, a distance of 5½ miles, the fall was 13 feet; and from Wisbeach Bridge to the same point, a distance of 11¼ miles, the fall was 13½ feet.

From Guyhern to Crab Hole, a distance of 17 miles, the fall was 14 feet 6 inches; and from Peterborough Bridge to the same point, a distance of 30¼ miles, the fall was only 18 feet 6 inches; whereas from Peterborough Bridge to Sutton Wash, a distance of more than 26 miles, the fall was only 6½ feet, or 3½ inches per mile; but at the intermediate distances, between Sutton Wash and South Holland and Gunthorpe Sluices, the fall was nearly double the above average.

From these facts it appeared evident that the great bar to the discharge of the waters of the Nene, and of course to the general drainage of the fens, was the high and shifting sands which lay between Gunthorpe Sluice and Crab Hole, independently of the narrow and confined state of the river above; Mr. Rennie therefore recommended the river to be carried by a new cut, of a suitable capacity, across the marshes to Crab Hole, 5½ miles in length.

The Cut has been since carried into execution under the

direction of Messrs. Telford and Rennie, and the result has exceeded the most sanguine expectations.

The lands *immediately* drained by this Cut were estimated to amount to 35,000 acres.

The improvements of the river were estimated at	£373,713.
And the internal drainage at	263,604.
Making a total of . .	637,317

According to Mr. Wing, the district drainage which would be effected by the river Nene would amount to 116,900 acres.

"The effect which the works, when completed, will have on the internal drainage of the fens connected with them may be appreciated," says Mr. Wing, "by the following facts: The windmills used in the North Level are not permitted to throw any water to the height of more than four feet above the lands in Thorney North Fen, which are about four feet three inches above the cill of Gunthorpe Sluice, making the greatest fall which can be obtained from the drains only eight feet three inches; but it seldom happens that the low-water mark is less than two feet above the cill, so that the general fall may be considered as not more than six feet three inches; whereas the low water at Crab Hole is nine feet nine inches below the cill of Gunthorpe Sluice, and consequently below the lands in Thorney Fen." Another important object was that at least 10,000 acres would be gained from the sea by the improvements, and this operation is now going on very rapidly. The expense of upwards of sixty windmills, costing on the average 4385*l.* per annum, would be saved, independently of other advantages, all of which are fully detailed in Mr. Wing's pamphlet *.

A similar plan for a Cut has since been carried into execution on a modified scale below Boston, in Lincolnshire, and with corresponding benefit both to the navigation and drainage.

Principles similar to the foregoing have been recommended by Mr. Rennie in his various Reports on the drainage of the marshes of Hatfield Chase, Congresbury, Romney, Holderness, &c.

The system of canal navigation in England has been carried on for more than half a century on a scale no less extensive than the drainage. The completion of the Sankey Canal in the year 1760, and of the Bridgewater Canal in the year 1761, opened the eyes of the nation to the vast advantages that were likely to be derived from artificial navigation, and led to the

* *Considerations on the Principles of Mr. Rennie's plan for the Drainage of the North Level of South Holland.* By Tycho Wing, Esq. Peterborough, 1820.

system of direct and indirect communication, which has united all the great rivers and ports of the kingdom.

In Scotland the progress of inland navigation, although less rapid, was proportionably successful; so early as the reign of Charles II. the idea of joining the Forth and Clyde rivers originated with the Duke of York. The subject was again resumed in the year 1722; in 1762 a survey was made by Messrs. Mackell and Watt; and in 1766 that great work was commenced by Mr. Smeaton, and finally completed in 1790. Between the above periods, also, Mr. Watt, the great improver of the steam-engine, made many reports on the improvement of the river Clyde and on the Monkland, Crinan*, and Caledonian Canals; and in the year 1802 Mr. Telford was employed to make surveys of the whole coast and interior of Scotland, with a view to improving its harbours and rivers, and which led to the execution of the great Caledonian Canal by that gentleman in the year 1821. Several other canals have been since completed in Scotland by different engineers.

The following is an approximate statement of the number of miles and the cost of river- and canal-navigation in England, Wales, and Scotland:

	Miles.	Cost.
River-navigation in England and Wales .	2036	£5,000,000
Ditto in Scotland	200	1,269,000
Total river-navigation	2236	£6,269,000
Canal-navigation in England and Wales .	2277	19,793,065
Ditto in Scotland	200	2,344,324
Grand total	4713	£28,406,389

Average cost of canal per mile:

In England	£9,000
In Wales	5,000 to 6,000
In Scotland	11,000

The first idea of improving river-navigation in Ireland is due to the enlightened administration of the unfortunate Earl of Strafford, who had witnessed the effects of inland navigation in the Low Countries. In the year 1703 the first act of parliament was passed for making the river Shannon navigable, and many improvements were projected: nothing, however, was effected, but an useless expenditure of 140,000*l.* of public money on the rivers Shannon and Boyne in the year 1758.

* Executed by Mr. Rennie.

Various sums of money were granted by Parliament, and frittered away in partial improvements of the Shannon, Boyne, Barrow, and Newry rivers, besides the Grand, Royal, Kildare, Naas, and Lough Earn navigations.

Up to the year 1790 there had been expended	£587,537
And in 1800 a further sum of	423,798
And from 1800 to 1831 an addition, for further improvements, of	800,000
Making a grand total of	1,811,335

The total number of miles of inland navigation which have been completed in Ireland amounts to 483 miles,—i.e. of canal, 312; navigable rivers, 171;—exclusive of the river Shannon, which is 234 miles from its source to the sea.

In the year 1809 an act of parliament was passed for the appointment of Commissioners to inquire and examine into the nature and extent of the bogs of Ireland, the practicability of cultivating them, and the best means for effecting the same.

The result was a very detailed report by Mr. Griffith on the origin, composition, and extent of the Bog of Allen, and of the first district, or eastern division of that bog, amounting to 36,430 English acres.

Mr. Griffith found that the average thickness of the Bog of Allen was 25 feet; that it was nowhere less than 12 feet, nor thicker than 42 feet; and that the height of the highest part of the bog above high-water mark in Dublin Bay was 296 feet. The cost of draining the whole of the eastern division was estimated by Mr. Griffith at 70,000*l.*, and to increase the annual value of the land 20*s.* per acre.

The second Report of the Commissioners laid before Parliament stated, That they had received detailed reports from their engineers on an extent of bog amounting to 197,000 acres;

That the several districts reported upon were intersected with streams, the channels of which were found to be generally in the under strata, usually consisting of gravel or clay;

That on the surfaces of these bogs there appeared to be abundant falls towards these streams to carry the surface-water into them;

That in respect to differences of opinion which seemed to prevail among the engineers, whether deep or surface drainage is best adapted to the reclamation of bogs, there were satisfactory proofs that the surface or bog might be highly improved, so as to bear crops without drawing off the water from the lower strata.

The Commissioners however conclude, that neither system should be exclusively preferred; the successful application of either must depend on circumstances;—that "wherever extensive bogs are to be drained, main and minor drains will be required for the purpose to act as receiving drains for the water, with a system of numerous small surface-drains to collect the water in considerable quantities."

Mr. Edgeworth's report on the seventh district is comprehensive and ingenious; his instrument for sounding and taking the sections of rivers was capable of giving correct results. Mr. Edgeworth advocated the system of surface-draining instead of deep cuts; he proposed portable wooden railways to be supported on the bogs by piles for the purposes of conveying manure to the surface, which he states to be similar to the plan adopted by Mr. Roscoe on Chat Moss. His experiments on the compression of bogs are, however, very contradictory*.

The third Report of the Commissioners is confined to a statement of former proceedings; and to giving the reports of their engineers on the surveys of 474,808 English acres of bog.

The fourth and last Report concludes the labours of the Commissioners, by giving the reports of the engineers, Messrs. Nimmo, Edgeworth, and Griffith, on 305,012 acres of bog, exclusive of 500,000 acres of bog in the counties of Kerry and Cork.

For the improvement of mountain bog, Mr. Nimmo recommends irrigation, the advantage of which in reclaiming bogs, he states, has been proved by experience in some few instances in Ireland, but principally in Scotland.

Mr. Nimmo observes, that wherever a stream flows through a bog it appears to prevent the growth of bog-plants, and the vegetation of wholesome grass is rapid on its banks; but as this system can only be applied to bogs in elevated situations, he recommends surface-draining for bogs in flat countries, adopting,

* The author of this paper, when making the survey of the present Liverpool and Manchester Railway in the year 1825, found that a cubic foot of moss, taken from Chat Moss, weighed 62·24 lbs; a heap of moss 4 yards by 3 yards, and 2½ yards in height, weighing about 22½ tons, sunk 18½ inches.

A quantity of moss, 12 inches long and 6 inches square, was put into a box with holes; its weight at first was 12 pounds; after being compressed some time it weighed only 3½ pounds: the moss was found to be reduced to 4¼ inches in thickness. It was further reduced by a compression of 20 tons, and an evaporation by heat to 1½ inch; so that the total loss in weight was 10 pounds of water, being in the proportion of five of water to one of vegetable matter, and the compression in bulk of eight to one; and in taking accurate levels of Chat Moss, its surface was found to rise and fall two feet above its average rise in wet weather.

however, the system of catch-water drains to intercept the waters from the higher grounds, and then a system of shallow drains to deliver the surface-water of the bog into the natural streams: these drains will of course vary in number and dimensions, but in no case ought they to exceed six feet in depth. Mr. Griffith agrees with the principles of irrigation laid down by Mr. Nimmo.

From the results of the reports it appears that the number of English acres of bog which have been surveyed in the twenty-five districts amounts to 1,013,358
And that there remain upon the three mountain districts of Wicklow, Erris, and Connemara . . 387,090
Exclusive of peat soil, which forms the general covering of these mountains 355,000
besides other lands, not examined; from all of which it is inferred that the extent of peat soil in Ireland exceeds 2,830,000 English acres,—whereof 1,576,000 are flat red bog,—which might be converted to the general purposes of agriculture.

As regards the Shannon river, which forms the most important feature in the internal navigation in Ireland, various examinations and surveys have been made, from the year 1715 down to the present time: the most detailed reports have been made by Mr. Rennie, Mr. Grantham, Captain Mudge, Mr. Rhodes, and Col. Burgoyne. The result of these reports may be stated in a few words, namely, That the Shannon may be considered as a combination of lakes and rivers, from its source in Lough Allen to the sea below Limerick:

The total distance from Limerick to Lough Allen is 144 miles:

The total height of the mean surface of the water in Lough Allen above that of the surface at Limerick is 143 feet seven inches, which gives an inclination of rather less than twelve inches in a mile:

The natural fall is, however, reduced to a series of horizontal planes of different lengths by locks:

The general direction of the river is extremely irregular, and broken by many streams, islands, and rocks:

The soundings vary in the same manner, and in some places are very deep, in others very shallow:

The river is liable to be overflowed to a great extent on both banks, and the large expanse of the lakes renders the vessels which navigate the river unfit for the lakes:

The works which have been constructed to overcome the natural difficulties of the navigation are either insufficient or in a state of decay; and it seems to be generally admitted that very little real good can be effected until the natural obstructions are

removed, the number of lakes reduced, and the channel deepened and improved in various parts, notwithstanding which it is generally believed that the navigation would only be fit for steamboats.

The difficulty experienced in preserving the channels of rivers free from the changes which take place in times of flood by the depositions of gravel and other obstacles, induced Col. Burgoyne to make the following statement in his Report: "It is a very usual opinion among engineers, that side artificial canals are eventually more judicious than the attempt to dredge channels in the beds of the streams themselves; and that for the purposes of navigation, rivers are only useful to supply canals with water.

"It may therefore be important, in estimating the propriety of excavating the required depth of this river according to the plan now proposed, to take into consideration the nature of the shoals. If they have been created by deposits collected by the action of the current, it may be inferred that the same process will continually tend to the same results, and that an effort to preserve the channel would require to be constant and laborious; but if the obstructions have been artificially made, or consist of a natural and solid substratum, it may be reasonable to presume that the openings once made will be permanent, or at all events require but little attention to maintain. Appearances would seem to indicate that the shoals in the Shannon come almost all under the two latter descriptions."

The Report of the Parliamentary Committee on this subject, in 1834, states, that "great detriment has arisen to the navigation from the land-floods, so prevalent upon the river, and over which there is no machinery for exercising any control."

The main question appears to be, whether the free and natural flow of the floods is to be arrested by locks, dams, and other works?

Although the principles which have guided the operations of our engineers have been various and contradictory, in general the practice has been to confine the freshes by artificial works, as in the Clyde, Witham, and other rivers, and to preserve the receptacles for tidal waters to their full extent. A contrary proceeding has tended to ruin many of our rivers and estuaries, whereby the drainage and navigation have been greatly impeded, and the destruction of several of our harbours, such as the Dee and Rye, occasioned.

The effects of embankments in Plymouth and Portsmouth harbours, and particularly in the estuary of the Mersey, (one third of the ancient capacity of which has been filled up by en-

croachments,) have materially diminished the depths of the sea-channels, and a consequent deterioration of the harbours has been the result.

On the Course, Dimensions, Inclinations, and Velocities of the River Thames, and the Effects which have been occasioned to the River by the removal and rebuilding of Old and New London Bridges, according to the Observations and Experiments which have been made on the River during the Years 1832, 1833, *and* 1834, *by Messrs. George and John Rennie.*

The general course of the river Thames is from west to east. Like other rivers, it forms the drainage of a very extensive district of country by means of rivulets and streams, which conduct the waters of the uplands into one great artery, or trunk, which conveys them to the sea. The total number of these affluents so circumstanced may be about twenty.

It is difficult to estimate the superficial extent of country drained by the river Thames, but it cannot be less than 5000 square miles.

The course of the river is very tortuous and winding, being double of its distance by a straight line.

The navigable distance from London to Lechlade is about 146½ miles; but from Sheerness the total distance is 204½ miles. The total fall of the river, from Lechlade to low-water mark, is 258 feet, or twenty-one inches per mile; and this fall is nearly uniform, although there are places where the fall varies from nineteen inches to thirty-two inches per mile, as shown in the following Table; but in no instance is the law of the funicular curve of M. Gerard established.

Rivers Isis and Thames.

Names of Places.	Length.		Fall.		Fall in feet per Mile.	Ratio of Inclinations.
	Miles.	Fur.	Feet.	In.		
From Lechlade at St. John's Bridge to Oxford at Folly Bridge	28	0	47	0	1·68	$\frac{1}{3143}$
From Oxford to Abingdon Bridge	9	0	13	11	1·73	$\frac{1}{3054}$
From Abingdon to Wallingford Bridge	14	0	27	4	1·95	$\frac{1}{2708}$
From Wallingford to Reading Bridge.	18	0	24	1	1·31	$\frac{1}{4030}$
From Reading to Henley Bridge	9	0	19	3	2·14	$\frac{1}{2467}$
From Henley to Marlow Bridge	9	0	12	2	1·35	$\frac{1}{3911}$
From Marlow to Maidenhead Bridge.	8	0	15	1	1·86	$\frac{1}{2839}$
From Maidenhead Bridge to Windsor Bridge	7	0	13	6	1·93	$\frac{1}{2736}$
From Windsor to Staines Bridge	8	0	15	8	1·96	$\frac{1}{2694}$
From Staines to Chertsey Bridge	4	6	6	6	1·44	$\frac{1}{3667}$
From Chertsey to Teddington Lock.	13	6	19	8	1·45	$\frac{1}{3641}$
From Teddington-Lock to London Bridge	19	0	2	9	·145	$\frac{1}{36414}$
From London to Yantlet Creek	40	0	2	1	·052	$\frac{1}{101537}$
From Lechlade to Yantlet Creek	186	4	218	0		
Deduct	40	0				
From Lechlade to London	146	4				

The velocity of the Thames might be expected to follow the law of variation of the inclinations; but the natural obstructions which exist in all parts of the river upwards, from bends, shoals, islands, weeds, &c., and the artificial obstacles from weirs, pound-locks, fishing-aytes, &c., render it impossible to ascertain the velocity correctly. Much depends also upon the volume of water passing down the river, and the use of *flashes*.

In general the velocity may be estimated at from half a mile to two miles and three quarters per hour, but the mean velocity may be reckoned at two miles per hour. In the year 1794 the late Mr. Rennie found the velocity of the Thames at Windsor two miles and half per hour.

The produce of the river varies also with the situation and the seasons.

The river when gauged in a very dry season in June, 1794,
at Windsor, produced 961 cubic feet per second;
at Laleham, ... 1153 do.
at Kingston Bridge . 1600 do.

According to Messrs. Rennie's experiments made on the 28th and 29th of May 1835, the produce was 1700 cubic feet per second; and on the 29th of May, after rain . 1800 do. do.

The surface of the river, however, stood about eighteen inches above the summer level.

According to Dr. Halley's computation, the quantity of water which passes through Kingston Bridge, upon the average, per second, amounts to 7920 cubic feet = 684,288,000 cubic feet per day, and 239,765,120,000 cubic feet per annum: he calculated the surface of country drained by the Thames and its tributary streams to be equal to an area of 5026½ square miles, or 140,129,776,600 superficial feet; and taking the average depth of rain which falls over the above surface in the course of a year to be twenty-four inches, amounting, consequently, to 280,259,555,200 cubic feet, he found this to be 40,494,435,200 cubic feet more than the quantity carried down by the river Thames to the sea; and he therefore concluded that one seventh of the whole was absorbed and evaporated.

Mr. Anderson, of the Grand Junction Water-works, stated in his evidence given before the House of Commons in 1834, that he had on the 4th of December, 1830, ascertained that the quantity of water flowing down the river Thames at Staines was 2050 cubic feet per second; but as the river was then about four feet above its summer level, not more than about one third of the above quantity would be carried down the river during the dry season.

Mr. Anderson further stated, that he had ascertained that the quantity of water flowing over the weir at Teddington Lock in the month of June, 1834, amounted to 700 cubic feet per second when there were eighteen inches of overfall, and 1260 cubic feet per second when there were two feet of overfall; the mean therefore of these three quantities being 1337 cubic feet per second, gives 115,516,800 per diem, or 42,163,632,000 cubic feet per annum; leaving, therefore, agreeably to Dr. Halley's computation of the surface of country drained by the river Thames, rather better than five sixths of the quantity of rain which falls in the course of the year to be absorbed and evaporated.

Below Teddington weir the river is under the combined influence of the freshes and tides, and the impediments which they meet with from the different bridges.

Previously to the erection of the old London Bridge, in the year 1209, there can be no doubt that the state of the river was very different from what it now is, and that many of the lowlands which are now embanked out, were formerly covered both by the floods and tides. The old bridge, although it obstructed the flow of the tides to their full height, operated reversely with the land-waters, by penning them back; and in extreme cases

the difference of level was found to be occasionally as much as fourteen inches between the high water below and above bridge, and five feet seven inches between low-water mark above and below bridge, depending of course on the state of the freshes and tides. The bridge was considered to act like a pound-lock, and, by penning up the water, to tranquillize the motion of the current, and deepen the navigation above. In consequence, however, of the danger and inconvenience arising from both the impeded navigation through the bridge and the floods, Mr. George Dance was instructed by the Corporation of London, in the year 1746, to draw up a series of queries, which were addressed to the Royal Society.

The result was a Report from the Society requesting certain information relative to the tides, which however did not elicit anything positive upon the subject until the year 1754, when the erection of Blackfriars Bridge was contemplated. The opinions of Mr. Robertson, as detailed in Dr. Hutton's Mathematical Tracts, were given on the unfounded supposition that the proposed bridge was to be built with piers and starlings like London Bridge, and to produce a similar obstruction. The enlargement of the water-way in the year 1759, by lowering the surface of the water several inches, caused a diminution both in the depth of the water, and in the power of the water-works. The area of the water-way was again contracted, and the river restored to its former state, on the supposition that the navigation would have been otherwise injured, and the low lands overflowed. And when the question of rebuilding the bridge came to be agitated, it was argued, That the old bridge acted as a bar to check the velocity of the river both ways;—that an increased velocity in the river would impede rather than accelerate the navigation, as wherries and small craft could not stem the current;—that the bed of the river would be laid dry during the ebb tide;—and, lastly, that the upper part of the river would be choked with mud, and all the low grounds on either side of the river would revert to marshes and be rendered uninhabitable.

On the other hand it was contended, That the tides would not rise more than a few inches higher than formerly, or fall lower than three feet;—that the old bridge not only acted as a dam to check the flux and reflux of the tides, but tended to pen back the land-waters, and to cause floods above; and that the proof of the bridge causing such an effect was the greater prevalence of floods before the enlargement of the waterway of the old bridge in the year 1759, than afterwards;—that the decrease in the velocity of the river tended to assist

the filling up and raising the bed by depositions of gravel and mud;—that independently of the annual loss of lives and property, occasioned by the contracted waterways of the bridge, the navigation was at times wholly impeded; whereas, by removing the dam, the great increase in the velocity of the current would clear the bed of the river, facilitate navigation, and effect a more perfect drainage of the country by the quicker passing off of the land-floods;—that the river being more perfectly emptied at each reflux, the flux would have less time to fill the increased void; and that, therefore, before it had attained its greatest surface of elevation, the tide would have begun to run down;—that although many shoals would have undoubtedly been exposed, yet the increased velocity of the current, assisted by dredging the hard places, would very soon reduce the channel to its ancient depth. The latter assertions have been verified to their full extent, as will be seen hereafter.

The phænomena of the tides in the port of London have been very ably discussed by Mr. Lubbock and by the Rev. Mr. Whewell in the *Philosophical Transactions* for the years 1831, 1833, and 1834,—the former gentleman in his papers containing numerous tables compiled from 13,073 observations made at the London Docks in a period of nineteen years, viz. from January 1st, 1808 to the 31st of December, 1826, with the corrections for the time of high water, as it is affected by the right ascensions, declinations, and parallaxes of the sun and moon; and the latter in his paper on the empirical laws of the tides in the port of London, and in his essay towards a first approximation to a map of cotidal lines.

In the case of the times of high water especially, says Mr. Whewell, "the general course of the variations of the quantities is as regular as can be expected, and as is requisite for my formulæ. The heights are much more anomalous; probably they are more affected by winds, &c. than the times are: and when we reflect that the tide at London may be affected by the operation of causes in a remote part of the ocean, propagating their effect by the progression of the tide-wave, we shall not be surprised at considerable deviations from the rule. The trade-winds and other winds of the tropical regions may be felt in our tides, and may even affect the means of long series of observations; for it is to be recollected that the averages which we obtain are not the averages of the effects of the sun and moon alone, but the averages of their effects, together with that of meteorological causes.

"It is moreover to be observed, that the peculiar circumstances of London in having a tide compounded of two tides

arriving by different roads, after journeys of different lengths, may easily be supposed to give rise to additional chances of irregularity."

In reference to the causes of inaccuracy in tidal observations, Mr. Whewell says,

"There is in fact no doubt that most or all the statements of such discrepancies are founded in a mistake, arising from the comparisons of two different phænomena, namely, *the time of high water*, and the time of the change *from the flow to the ebb current*. In some cases the one and in some the other of these times has been observed as the *time of the tide*; and in this manner have arisen such anomalies as have been mentioned.

"The time of the change of current or the *time of slack water* never coincides with the *time of high water*, except close in upon the shore, and within its influence; the interval of the two times is generally considerable. Great confusion has been produced by these two times not being properly distinguished; so great, indeed, that almost all the tide observations which we possess are of doubtful value.

"The persuasion that in waters affected by tides the water rises while it runs one way, and falls while it runs the opposite way, though wholly erroneous, is very general."

Mr. Whewell instances the case of the waters of the river Dee at Aberdeen, which have almost a constant current to seaward, notwithstanding the opposite direction of the flood-tide of the ocean. Many instances could also be adduced of similar phænomena constantly occurring in our estuaries and rivers.

In the river Thames the motion of the current continues for some time after the tide has made its mark, which is undoubtedly owing to the momentum. In general the tides of the river Thames have been found to observe considerable regularity both in their elevations and periodical times, except when influenced by winds and floods. In comparing, however, the sea- with the river-tides a considerable discrepancy is found to prevail in the elevations; in some cases on account of the convergence or swelling of the tidal wave, on the principle of the conservation of mechanical force, as in the Severn, &c., and in other cases a lowering of the surface by expansion, as in the Mersey, which is very narrow at its mouth.

In the river Thames the height of the tidal wave diminishes much less from the effect of friction and obstacles than might be expected. From reference to Mr. Lloyd's observations on

the rise of the tides at Sheerness, with the mean of Mr. Lubbocks at the London Docks, it appears that

The spring tide high water at the London Docks above the same at Sheerness, is	2·0361	
		0·2068
The mean high water ditto ditto ditto	2·2429	
		0·1050
The neap tide ditto ditto ditto	2·3579	
		0·6900
The spring tide low water ditto ditto	1·6679	
		0·3680
The mean level of the tides ditto ditto	2·0359	
Or, taking more correctly the half difference between spring high and low water at Sheerness, the mean spring level is	1·7249	

It seems from the above summary, that as the water decreases in height, so the height of the water's surface at London Docks above the same at Sheerness also decreases, with the exception of spring tides at the London Docks and the neap tide. These are the means, not of the highest tides, but of the tides at a particular time of the moon's southing: at Trinity high-water mark at London Bridge, it was found by Mr. Lloyd to be 1·9040 above mean spring tide high-water mark at Sheerness.

With respect to the influence of the winds on the tides in the river Thames, Mr. Lubbock states, on the authority of Sir John Hall, of the St. Katharine Docks, that "during strong north-westerly gales, the tide marks high water earlier than otherwise, and does not give so much water, whilst the ebb-tide runs out later and marks lower; but upon the gales abating and the weather moderating, the tides put in, and rise much higher, whilst they also run long before high water is marked, and with more velocity of current; nor do they run out so long or so low, &c. A south-westerly gale has a contrary effect generally, and an easterly one gives some water; but the tides in all these cases always improve the moment the weather moderates."

The very valuable tables of Mr. Lubbock, compiled with his corrections from upwards of ten thousand observations, have contributed very largely to our knowledge on this subject.

From a series of levels and observations made on the tides in September and October 1820, between the entrance of London Docks and Westminster Bridge, by Mr. Francis Giles, for the Select Committee of the Bridge-house lands, the following were found to be the facts:

1st, The high water of spring tides at the entrance of the

London Docks averaged a level of 1·5 inch higher, and ten minutes earlier time, than at the lower side of London Bridge;

2nd, The low water of spring tides at London Docks averaged a level of three inches lower, and nine minutes earlier time, than at London Bridge;

3rd, The high water of neap tides at London Docks averaged a level of one inch higher, and eight minutes earlier time, than at London Bridge;

4th, The low water of neap tides at the London Docks averaged a level of two inches lower, and fourteen minutes earlier time, than at London Bridge.

It was found also, That high water of the highest spring tides occurs at three or four o'clock, and high water of neap tides at eight or nine o'clock:

The flow of the spring tides is from four to five hours; and the ebb from seven to eight hours and half:

The high water of spring tides produced an average fall through London Bridge of eight inches, but the greatest fall upwards was 1·1 inch:

The low water of spring tides produced a fall of 4·4 inches through the bridge; but the greatest fall was 5·7 inches:

The high water of neap tides through London Bridge upwards produced a fall of 5 inches:

At the low water of neap tides the fall upwards was 2·1 inches; but the least fall at low water was 1·1 inch.

It appeared also, That it took forty minutes after low-water spring-tides to produce slack water under the bridge with a flood tide; two hours with a flood of neap, and with an ebb spring tide thirty minutes after high water, and fifteen minutes after an ebb of neap tides:

That the time of high water was about ten minutes earlier at London than at Westminster Bridge:

That the mean low-water line has a fall of—

4·0 in. from Westminster to Waterloo Bridge; 7 min. later at Westminster than at Waterloo Bridge.
4·3 do. from Waterloo to Blackfriars Bridge; 6 ditto.
3·2 do. from Blackfriars to Southwark Bridge; 5 ditto.
0·5 do. from Southwark to London Bridge; 4 ditto.

A Statement showing the Sectional Areas of the River Thames, taken in the Years of 1823 *and* 1831, *by* Messrs. Rennie.

	No. of Section.	By Survey in 1823.	By Survey in 1831.	Difference in 1831.	By Survey in 1823.	By Survey in 1831.	Difference in 1831.	By Survey in 1823.	By Survey in 1831.	Difference in 1831.
		Sectional Area of the Tidal Water below Trinity High-water Mark.			Sectional Area below Low-water Mark.			Total Sectional Area of the River Thames below Trinity High-water Mark.		
		sup. feet.	sup. feet.	sup. feet.	sup. feet	sup. feet	sup. feet.			sup. feet.
About 230 yards north of Westminster Bridge	4	15,409	16,559	increase 1150	3939	3487	decrease 452	19,348	20,046	increase 698
Near King's Arms Stairs, and Whitehall ditto	5	16,411	17,090	ditto 679	4757	6570	increase 1813	21,168	23,660	ditto 2492
Near Hungerford Stairs.....	6	16,083	17,902	ditto 1819	3891	3920	ditto 29	19,974	21,822	ditto 1848
Near Waterloo Bridge	8	16,818	16,958	ditto 140	3752	3947	ditto 195	20,570	20,905	ditto 335
Opposite Bouverie Street.....	11	13,959	14,310	ditto 351	4332	3900	decrease 432	18,291	18,210	decrease 81
Between Blackfriars and Southwark Bridge.....	13	12,982	13,822	ditto 840	3976	3381	ditto 595	16,958	17,203	increase 245

N.B.—Since the above Sections were taken the Areas have increased in a much greater ratio.

Gradation of the Ebbing and Flowing of the Tide at London Bridge, taken above and below, on the 29th of July 1821, *being the day of the new moon; by* Mr. GILES.

Above Bridge.

Low water at Pepper Alley 50 min. past 9 o'clock in the morning.

Flood Tide.

	ft.	in.
Depth of water when flood commenced	6	0
1st hour rise	2	11
2nd ditto	3	0
3rd ditto	2	10
4th ditto	2	8
45 minutes	1	0
4 hours and 45 minutes	18	5

High water at Pepper Alley 35 min. past 2 o'clock in the afternoon.

Ebb Tide.

	ft.	in.
1st hour, fall	2	1
2nd ditto	2	7
3rd ditto	2	0
4th ditto	1	9
5th ditto	1	5
6th ditto	1	2
7th ditto	1	0
55 minutes	0	11
Depth at low water	5	6
7 hours and 55 minutes	18	5

Below Bridge.

Low water at Coxe's Quay, 30 min. past 9 o'clock in the morning.

Flood Tide.

	ft.	in.
Depth of water when flood commenced	1	3
1st hour, rise	5	9
2nd ditto	5	4
3rd ditto	2	9
4th ditto	2	5
48 minutes	1	4
4 hours and 48 minutes	18	10

High water at Coxe's Quay, 18 min. past 2 o'clock in the afternoon.

Ebb Tide.

	ft.	in.
1st hour, fall	2	1
2nd ditto	4	4
3rd ditto	3	1
4th ditto	2	7
5th ditto	2	3
6th ditto	1	9
7th ditto	1	6
59 minutes	0	11
Depth left	0	4
7 hours and 59 minutes	18	10

Difference between the Levels of high- and low-water Spring Tides, between Rotherhithe and Battersea, in the year 1820.

	ft.	in.
Rotherhithe Old Horse Ferry	21	10
London Old Bridge	18	2
Blackfriars	14	9
Westminster	12	6
Vauxhall	12	2
Battersea	11	6

From Battersea Bridge to London Bridge . . 5 miles.
From London Bridge to Old Horse Ferry . . 1¼ ditto.
From London Bridge to the Nore . . . . . 44 ditto.

The following observations were made in March 1833:

	March 6.	March 7.	March 8.
	h. m.	h. m.	h. m.
The difference in time of high water at London Bridge after that at Sheerness	1 34	1 42	1 36
	ft. in.	ft. in.	ft. in.
The rise of tide at Sheerness	18 3	18 11	18 7
——— Fresh Wharf	.....	20 8	19 10
——— New London Bridge	18 1	18 6	18 3
The fall through the site of Old London Bridge	.....	2 2	1 7
The difference of level of High-water mark at London Bridge above that at Sheerness	2 5	2 7	2 9
The difference of level of Low-water mark at Fresh Wharf above that at Sheerness	.....	0 10	1 7

N.B.—The above levels are from the Trinity datum as transferred from London Bridge to Sheerness by Captain Lloyd.

Observations and Experiments upon the Velocity of the Tides of the River Thames.

The earliest experiments on this subject with which we are acquainted are those of Mr. H. Saumarez, inserted in the *Philosophical Transactions* for the year 1720, "On the strength and gradual increase and decrease of the Tides of flood and ebb in the river Thames, as observed in Lambeth Reach, off Manchester Stairs, and in the middle of the river, with a new instrument called the *Marine Surveyor*, on the 9th and 18th of June 1790, both with spring and neap tides."

These experiments are interesting, as showing the effect of Old London Bridge on the river previously to the alteration of the bridge and enlargement of the waterway in the year 1754.

Mr. Saumarez's tables indicate the depth and velocity of the floods and ebb of spring and neap tides for every fifteen minutes, and the following are the results:

	hrs.	min.
The time of flood spring tide was only	3	50
Ditto . . . . ebb . . . . ditto	8	40
Ditto . . . . flood neap ditto	4	50
Ditto . . . . ebb . . . . ditto	7	35

	miles per hour.
The greatest velocity in flood spring tides was	2·00
Ditto . . . . . ebb ditto	1·61
Total number of miles run by the flood spring tide	5·25
Ditto . . . . . ebb	10·50
The greatest velocity in neap tides was	1·44
Ditto, with ebb of neap tides	1·30
The total distance run with a flood of neap	4·75
Ditto, with an ebb of ditto	7·75

According to the experiments of Mr. Giles in the year 1823, the velocities of the flood tide are,—

From London Bridge to Putney Bridge, $2\frac{1}{2}$ miles per hour;
Between London, Southwark, and Westminster Bridges, 2 do.

And with an ebb tide the velocities are,—

Between Westminster and Waterloo Bridges, 2 miles per hour;
Between Waterloo and Blackfriars Bridges, $2\frac{1}{4}$ do.;
Between Blackfriars and London Bridges, $2\frac{3}{4}$ do.

Experiments on the Flood Tide *of the River Thames from London Bridge, 19th of June,* 1834.* (*Wind W.S.W. Fresh breeze and clear.*)

Name of place.	Time.		Tide Gauge at London Bridge.		Distance from London Bridge.	Time from low Water.		Velocity at each place.	Remarks.
	hrs.	min.	ft.	in.	miles.	hrs.	min.	miles per hour.	
London Bridge	8	6	17	3	0·0	0	31	0·00	Float put in at centre.
Southwark do.	8	30	14	11	0·28	0	55	0·70	Centre of centre arch.
Blackfriars do.	8	53	13	0	0·75	1	18	2·16	4th City arch.
Waterloo do.	9	14	11	5	1·34	1	39	1·68	2nd City arch.
Hungerford Market .	9	23	10	10	1·50	1	48	1·07	.....
Westminster Bridge .	9	36	9	3	2·00	2	1	2·30	6th Middlesex arch.
Horse Ferry	9	50	8	10	2·42	2	15	1·82	.....
Vauxhall Bridge ...	10	$3\frac{1}{2}$	7	11	2·95	2	$28\frac{1}{2}$	2·33	Centre arch.
Chelsea Col. (Stairs) .	10	34	5	10	4·21	2	59	2·48	
Chelsea Bridge	10	55	4	2	5·04	3	20	2·36	6th arch.
$\frac{1}{2}$ mile above do. ...	11	9	3	2	5·54	3	34	2·14	
1 mile do. do.	11	20	2	6	6·04	3	45	2·73	
$1\frac{1}{2}$ mile (Wandsworth)	11	31	2	0	6·54	3	56	2·73	
2 miles	11	42	1	7	7·04	4	7	2·73	
Putney Bridge	11	50	1	3	7·48	4	15	3·30	11th arch.
$2\frac{1}{2}$ miles	11	$53\frac{1}{2}$	1	2	7·54	4	18	1·03	
3 miles from Chelsea Bridge	12	6	0	9	8·04	4	31	2·40	
$3\frac{1}{2}$ miles	12	20	0	8	8·54	4	45	2·14	
4 miles	12	30	0	8	9·04	4	55	3·00	
Hammersmith Bridge.	12	35	0	10	9·20	5	0	2·00	$\frac{1}{3}$ from Middlesex pier.
$4\frac{1}{2}$ miles	12	45	1	2	9·54	5	10	2·00	
5 miles (Chiswick) ..	1	0	1	11	10·04	5	25	2·00	
$5\frac{1}{2}$ miles	1	15	2	11	10·54	5	40	2·00	
6 miles	1	35	4	5	11·04	6	0	1·50	
$6\frac{1}{4}$ White Hart, Barnes	1	45	5	1	11·29	6	10	1·50	Tide had fallen 4 inches.
Thirty yards above the White Hart...	1	50	5	5	11·30	6	15		

	hrs.	min.	ft.	in.	
Low water at London Bridge ...	7	35	20	3	under Trinity high-water mark.
High water at ditto. ...	12	30	0	8	

* These experiments were tried with floats immersed at different depths; also with Massey's patent log.

Experiments on the Ebb Tide *of the River Thames to London Bridge, 19th of June,* 1834. (*Wind W.S.W. Fresh breeze and clear.*)

Name of place.	Time.	Tide Gauge at London Bridge.	Distance from London Bridge.	Time from low Water.	Velocity at each place.	Remarks.
	hrs. min.	ft. in.	miles.			
Thirty yards above White Hart	1 50	5 5	11·30			
6¼ White Hart, Barnes.	2 0	6 1	11·29		0·12	
6 miles	2 11	6 9	11·04		1·36	
5½ miles	2 35	8 1	10·54		1·25	
5 miles (Chiswick)	2 54	9 1	10·04		1·58	
Water Works	3 5		9·66		2·07	
4½ miles	3 9⅓	9 10	9·54		1·60	
Hammersmith Bridge.	3 19	10 5	9·20		2·11	
4 miles	3 25	10 7	9·04		1·66	
3½ miles	3 40	11 2	8·54		2·00	
3 miles from Chelsea Bridge	3 58	12 0	8·04		1·67	
2¾ miles	4 16½	12 9	7·54		1·62	
Putney Bridge	4 21½	13 0	7·48		0·72	
2 miles	4 34	13 6	7·04		2·11	
1½ mile (Wandsworth)	4 47½	14 1	6·54		2·22	
1 mile	5 2½	14 8	6·04		2·00	
½ mile above	5 16	15 2	5·54		2·22	
Chelsea Bridge	5 31	15 8	5·04		2·00	
Chelsea Bridge (Stairs)	5 55	16 6	4·21		2·06	
Vauxhall Bridge	6 26	17 5	2·95		2·44	
Horse Ferry	6 40	17 10	2·42		2·25	
Westminster Bridge	6 50	18 1	2·00		2·55	
Hungerford Market	...	...	1·50			
Waterloo Bridge	7 7½	18 6	1·34		2·26	
Blackfriars do.	7 20	18 10	0·75		2·83	
Southwark do.	7 29	19 0	0·28		3·13	
London do.	7 34	19 2	0·00		3·37	
Custom House (centre)	7 42	19 3				
Traitor's Gate (Tower)	7 51	19 0				
St. Katherine's Dock entrance	8 0	18 6				
Miller's Wharf	8 10	18 0				

	hrs.	min.	ft.	in.	
Low water at London Bridge...	7	45	19	3	under Trinity high-water mark.
High water at ditto. ...	12	30	0	8	

Comparative Average Velocities of the River Thames, as taken upon the Flood and Ebb Tides, in 1831 *and* 1833.

Between Westminster and Waterloo Bridges.

Years.	First of Flood.		Last of Flood.		First of Ebb.		Last of Ebb.	
	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	2·507 =	1·709	2·833 =	1·931	2·840 =	1·936	3·221 =	2·196
1831.....	2·334 =	1·591	2·599 =	1·772	2·730 =	1·861	2·834 =	1·932
Increase	·173	·118	·234	·159	·11	·075	·387	·264

Between Waterloo and Blackfriars Bridges.

	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.	2·881 =	1·964	3·495 =	2·382	3·643 =	2·483	3·981 =	2·714
1831.....	2·490 =	1·697	3·080 =	2·100	3·100 =	2·113	3·271 =	2·230
Increase	·391	·267	·415	·282	·543	·370	·710	·484

Between Blackfriars and Southwark Bridges.

	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	2·903 =	1·979	4·327 =	2·950	4·629 =	3·156	4·926 =	3·358
1831.....	2·635 =	1·796	4·468 =	3·046	4·371 =	2·980	4·200 =	2·863
Increase	·268	·183	·141	·096	·258	·176	·726	·495

Between Southwark and New London Bridges.

	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	2·610 =	1·779	4·241 =	2·891	5·293 =	3·609	4·785 =	3·263
1831.....	2·844 =	1·938	4·89 =	3·334	6·050 =	4·124	5·625 =	3·835
Decrease	·234	·159	·649	·443	·757	·515	·840	·573

Greatest Velocities.

Between Westminster and Waterloo Bridges.

Years.	Quickest Flood.		Quickest Ebb.	
	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	3·505 =	2·389	3·333 =	2·272
1831.....	3·230 =	2·200	2·990 =	2·038
Increase	·275	·189	·343	·234

Between Waterloo and Blackfriars Bridges.

	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	4·279 =	2·917	4·186 =	2·854
1831.....	3·880 =	2·640	3·660 =	2·490
Increase	·399	·277	·526	·364

Between Blackfriars and Southwark Bridges.

	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	5·217 =	3·556	5·428 =	3·700
1831.....	4·990 =	3·400	4·590 =	3·129
Increase	·227	·156	·838	·571

Between Southwark and New London Bridges.

	Feet per Second.	Miles per Hour.	Feet per Second.	Miles per Hour.
1833.....	4·788 =	3·264	5·725 =	3·903
1831.....	5·540 =	3·777	8·160 =	5·560
Decrease	·752	·513	2·435	1·657

The surveys which have been made under the direction of the late Mr. Rennie, by order of the Lords Commissioners of the Admiralty and of the Corporation of the City of London, both above and below bridge, at different periods, and also by Mr. Telford previous to the building of New London Bridge, have left ample data of the course and sections of the river Thames; but no measures seem to have been adopted to ascertain the effect which the removal of the Old London Bridge was likely to occasion in the operation of the tides; and, as before stated, opinions being very contradictory, it occurred to Messrs. Rennie to institute a series of observations for that purpose.

Accordingly, the new bridge having been opened to the public in the year 1831, the demolition and total removal of the old bridge commenced on 22nd November following; and on the 25th, Mr. Combe (Messrs. Rennie's assistant) was instructed by those gentlemen to proceed up the river to collect information, and to make preparations for establishing a series of observations at Putney, Kew, and Richmond Bridges, and at Teddington Lock. Tide-gauges accurately adjusted by levelling to a tide-gauge similarly fixed at New London Bridge, and at Fresh Wharf, and a little below the bridge, were therefore fixed at these places, and experienced persons were appointed to keep a daily register of the high- and low-water marks as indicated by the gauges. Accordingly, everything being ready by the 30th of November, a simultaneous commencement was made at the different places on the 1st of December, and the observations were daily recorded in a book kept by each person and forwarded to London every ten days, until the first of June 1832; and in order to prevent any mistakes, the gauges were frequently visited and inspected, and upon every occasion Mr. Combe found them undisturbed and each person attentive to his duty. Up to the first of June, however, scarcely any part of the bridge which obstructed the waterway had been removed, with the exception of two piers which had been cleared away for the accommodation of the craft navigating upwards during the building of the new bridge, the works of which nearly compensated for the enlargement of the waterway under the old bridge. These alterations had, however, lessened the fall at low water about one foot.

The flood or low water in the early part of the year having been found to interfere so much with the free action of the tides, and as at the commencement of the year 1833 there was a considerable *fresh* in the river, it was deemed unnecessary to resume the observations until the beginning of the month of March; at which period, however, both in the year 1833 and 1834, the

gauges were again correctly fixed, and recorded as before, and the results of the three years have been separately and collectively analysed and compiled for three months in each year, as follows:

The prevailing winds in the months of March, April, and May were,

In the year 1832, northerly in excess;
——— 1833, north and south equal;
——— 1834, north 2, south 1.

The sectional areas at London Bridge:

Years.		Sup. feet.
1825,	previous to any alteration, in Old London Bridge below Trinity datum	7360
1832,	after two piers of the old bridge were removed . .	8700
1833,	when nearly the whole of the masonry and starlings of the old bridge had been removed	13800
1834,	Old London Bridge entirely removed	17600

Summary for Three Years of High- and Low-water Mark above and below Trinity datum, at Putney, Kew, and Richmond Bridges, and Teddington Lock.

AT PUTNEY BRIDGE.

High Water.

Years.	Tides.		Above T. dat. ft. in.		Below T. dat. ft. in.			ft. in.
1832.	88	stood from	1 1	to	5 9;	mean of all the tides		1 11·17
1833.	84	———	1 2	–	4 4;	———		1 5·76[a]
1834.	89	———	1 4	–	4 7;	———		1 9·61[b]

Low Water.

1832.	88	stood from	10 0	to	12 7;	mean of all the tides		11 8·08
1833.	84	———	9 9	–	12 5;	———		11 3·94[c]
1834.	89	———	11 9	–	13 0;	———		12 3·64[d]

Duration of the Day Flood Tides.

			hrs. min.		hrs. min.			hrs. min. sec.
1832.	87	flowed from	2 45	to	4 30;	mean of all the tides		3 50 13
1833.	80	———	3 0	–	4 30;	———		3 40 26[e]
1834.	89	———	3 0	–	4 30;	———		4 56 37[f]

	ft. in.	
[a] Or	0 5·41	which the high-water mark had risen.
[b] Or	0 1·56	
[c] Or	0 4·14	which the low-water mark had fallen.
[d] Or	0 7·64	
[e] Or	9 min. 47 sec.	decrease in the duration of flood tides.
[f] Or	6 min. 24 sec.	

Conclusions.

The changes at Putney Bridge, situated 7 miles 3 furlongs above London Bridge, have been as follow:

1. The high-water mark at the top of spring tides stood in 1834	3 ins. higher than in	1832.
	2 ditto . . ditto . .	1833.
2. The high-water mark at extreme of neap tides 1833	14 do. . . ditto . .	1832.
	3 ditto . . ditto . .	1833.
3. The high-water mark on average tides 1834	1·56 higher than in	1832.
	3·85 lower than in	1833.
4. The low-water mark at the top of spring tides stood in 1834	21 ins. lower than in	1832.
	24 ditto . . ditto .	1833.
5. The low-water mark at the extreme of neap tides, in 1834	5 ins. lower than in	1832.
	7 ditto . . ditto . .	1833.
6. The low-water mark upon the average stood in 1834	7·56 ins. lower than in	1832.
	11·70 ditto	1833.
7. The range of the flood tide upon the average was in . . 1834	9·12 greater than in	1832.
	7·85 ditto . ditto .	1833.
8. The range of the flood tide, however, at the top of spring tides was in 1834	15 ditto . . ditto .	1832.
	17 ditto . . ditto .	1833.
9. The range of the flood tide at the extreme of neaps was in 1834	21 ditto . . ditto .	1832.
	1 ditto less than in	1833.
	6 min. 24 sec. longer than in	1832.
10. The duration of the flood tide upon the average was in 1834	16 min. 11 sec. longer than in	1833.

AT KEW BRIDGE.

High Water.

Years.	Tides.		Above T. dat. ft.	in.	Below T. dat. ft.	in.		ft.	in.
1832.	88	stood from	1	7 to	4	8;	mean of all the tides	1	3·44
1833.	82	———	1	10 –	3	8;	———	0	9·61[a]
1834.	89	———	2	0 –	4	3;	———	1	5·37[b]

Low Water.

1832.	88	stood from	4	6 to	8	11;	mean of all the tides	8	0·39
1833.	82	———	4	10 –	8	10;	———	7	5·59[c]
1834.	89	———	8	2 –	9	11;	———	9	0·88[d]

[a] Or 0 ft. 5·83 in. which the high-water mark had risen.
[b] Or 0 1·93 which the high-water mark had fallen.
[c] Or 0 6·89 which the low-water mark had risen.
[d] Or 1 0·49 which the low-water mark had fallen.

Duration of the Day Flood Tides.

		hrs. min.	hrs. min.		hrs. min. sec.
1832.	88 stood from	2 15	to 4 0;	mean of all the tides	3 5 6
1833.	82 ———	1 45	– 3 15;	———————	2 50 54[a]
1834.	89 ———	2 30	– 3 30;	———————	3 1 47[b]

Conclusions.

The changes at Kew Bridge, situated 13 mil. 0 fur. 12 pol. above London Bridge, have been as follow:

1. The high-water mark at the top of spring tides stood in 1834	5 ins. higher than in		1832.
	2 ditto	ditto	1833.
2. The high-water mark at the extreme of neap tides stood in 1834	5 ditto	ditto	1832.
	7 ins. lower than in		1833.
3. The high-water mark upon the average stood in 1834	1·93 ditto	ditto	1832.
	7·76 ditto	ditto	1833.
4. The low-water mark at the top of spring tides stood in 1834	44 ditto	ditto	1832.
	40 ditto	ditto	1833.
5. The low-water mark at the extreme of neap tides stood in 1834	12 ditto	ditto	1832.
	13 ditto	ditto	1833.
6. The low-water mark upon the average stood in 1834	12·49 ditto	ditto	1832.
	19·29 ditto	ditto	1833.
7. The range of the tide upon the average was in 1834	10·56 greater than in		1832.
	11·53 ditto	ditto	1833.
8. The range of the flood tide however at the top of spring tides was in 1834	8 ditto	ditto	1832.
	6 ditto	ditto	1833.
9. The range of the flood tide at the extreme of neaps was in 1834	21 ditto	ditto	1832.
	4 ditto	ditto	1833.
10. The duration of the flood tide upon the average was in 1834	3 min. 19 sec. longer than in		1832.
	10 min. 53 sec. ditto		1833.
11. The average of high-water mark stood at Kew Bridge above that at Putney in 1834.....	3·49 ins. less than in		1832.
	3·94 ditto	ditto	1833.
21. The average declivity of low-water line between Kew and Putney Bridges, per mile, was in 1834.....	0·8707 ditto	ditto	1832.
	1·3405 ditto	ditto	1833.

	hrs. min. sec.	
[a]	Or 0 14 12	decrease in the duration of the flood tide.
[b]	Or 0 3 19	

AT RICHMOND BRIDGE.

High Water.

Years.	Tides.	Above Trin. dat. ft. in.	Below Trin. dat. ft. in.		ft. in.
1832.	88 stood from	1 10 to	3 9;	mean of all the tides	0 8·67
1833.	84 ———	2 2 –	3 0;	———	0 2·76[a]
1834.	89 ———	1 11 –	3 7;	———	1 1·70[b]

Low Water.

1832.	88 stood from	1 4 to	5 6;	mean of all the tides	4 4·23
1833.	84 ———	0 9 –	5 2;	———	3 7·41[c]
1834.	89 ———	4 7 –	6 4;	———	5 5·73[d]

Duration of the Day Flood Tides.

		hrs. min.	hrs. min.		hrs. min. sec.
1832.	84 flowed from	1 15 to	2 45;	mean of all the tides	1 59 3
1833.	84 ———	1 15 –	2 30;	———	1 52 37[e]
1834.	89 ———	1 15 –	2 20;	———	1 50 10[f]

Conclusions.

The changes at Richmond Bridge, situate 16 miles 0 fur. 6 pol. above London Bridge, have been as follow:

1. The high-water mark at the top of spring tides stood in 1834	1 in.	higher than in	.	1832
	3 ins.	lower than in	.	1833
2. The high-water mark at the extreme of neap tides stood in 1834	2 ins.	higher than in	.	1832
	7 ins.	lower than in	.	1833
3. The high-water mark upon the average stood in . . . 1834	5·03	ditto ditto	.	1832
	10·94	ditto ditto	.	1833
4. The low-water mark at the top of spring tides stood in . 1834	39	ditto ditto	.	1832
	46	ditto ditto	.	1833
5. The low-water mark at the extreme of neap tides stood in 1834	10	ditto ditto	.	1832
	14	ditto ditto	.	1833
6. The low-water mark upon the average stood in . . . 1834	13·50	ditto ditto	.	1832
	22·32	ditto ditto	.	1833

[a] Or 0 ft. 5·91 in. which the high-water mark had risen.
[b] Or 0 5·03 which the high-water mark had fallen.
[c] Or 0 8·82 which the low-water mark had risen.
[d] Or 1 1·50 which the low-water line had fallen.
[e] Or 6 min. 26 sec. } decrease in the duration of the flood tide.
[f] Or 8 min. 53 sec. }

7. The range of the flood tide upon the average was in . . 1834	8·47 ins. greater than in 1832	
	11·38 ditto ditto . 1833	
8. The range of the flood tide however at the top of spring tides was in 1834	14 ditto ditto . 1832	
	22 ditto ditto . 1833	
9. The range of the flood tide at the extreme of neaps was in 1834	9 ditto ditto . 1832	
	6 ditto ditto . 1833	
10. The duration of the flood tide upon the average was in 1834	8 m. 3 s. less than in . 1832	
	2 m. 27 s. less than in . 1833	
11. The average high water mark stood at Richmond Bridge above that at Kew in . 1834	3·10 ins. less than in . 1832	
	3·23 ditto ditto . 1833	
12. The average declivity of low-water line between Richmond and Kew Bridges, per mile, was in 1834	·3388 ditto ditto . 1832	
	·4776 ditto ditto . 1833	

AT TEDDINGTON LOCK.

High Water.

Years.	Tides.		Above Trin. dat. ft. in.	Below Trin. dat. ft. in.		ft. in.
1832.	76	stood from	1 8 to	0 8;	mean of all the tides	0 5·13
1833.	84	———	2 2 –	0 8;	———	0 9·51[a]
1834.	87	———	1 2 –	2 0;	———	0 3·88[b]

Low Water.

1832.	76	stood from	0 7 to	1 3;	mean of all the tides	0 7·52
1833.	84	———	1 7 –	1 6;	———	0 0·35[c]
1834.	89	———	0 9 –	2 7;	———	1 9·16[d]

Duration of the Day Flood Tides.

			hrs. min.	hrs. min.		hrs. min. sec.
1832.	55	flowed from	0 45 to	2 45;	mean of all the tides	1 32 10
1833.	72	———	0 30 –	3 15;	———	1 36 2[e]
1834.	71	———	0 30 –	2 30;	———	1 13 5[f]

[a] Or 0 ft. 4·38 in. which the high-water mark had risen.
[b] Or 0 9·01 which the high-water mark had fallen.
[c] Or 0 7·87 which the low-water line had risen.
[d] Or 1 1·64 which the low water line had fallen.
[e] Or 3 min. 52 sec. } decrease in the duration of the flood tide.
[f] Or 19 min. 5 sec. }

Conclusions.

The changes at Teddington Lock, situated $18\frac{3}{4}$ miles above London Bridge, have been as follow:

1. The high-water mark at the top of spring tide stood in 1834	6 ins.	lower than	in	1832
	12	ditto	ditto	1833
2. The high-water mark at extreme of neap tide stood in 1834	16	ditto	ditto	1832
	16	ditto	ditto	1833
3. The high-water mark upon the average stood in 1834	9·01	ditto	ditto	1832
	13·39	ditto	ditto	1833
4. The low-water mark at the top of spring tide stood in 1834	16	ditto	ditto	1832
	28	ditto	ditto	1833
5. The low-water mark at extreme of neap tide stood in 1834	16	ditto	ditto	1832
	13	ditto	ditto	1833
6. The low-water mark upon the average stood in 1834	13·64	ditto	ditto	1832
	21·51	ditto	ditto	1833
7. The range of the tide upon the average was in 1834	4·63 in.	greater than	in	1832
	8·12	ditto	ditto	1833
8. The range of the tide at the top of spring tide was in 1834	8	ditto	ditto	1832
	15	ditto	ditto	1833
9. The range of the flood tide at the extreme of neaps was in 1834	2 ins.	less than	in	1832
	2	ditto	ditto	1832
10. The duration of the flood tide upon the average was in 1834	19 min. 5 sec.	ditto		1832
	22 min. 57 sec.	ditto		1833
11. The average high-water mark stood at Teddington Lock above that at Richmond in 1834	3·98 ins.	less than	in	1832
	2·48	ditto	ditto	1832
12. The average declivity of low-water line between Teddington Lock and Richmond Bridge, per mile, was in 1834	·0512 ins.	less than	in	1832
	·5529	ditto	ditto	1833

Summary Table of the Day Tides,

Showing the greatest and the least tides in March, April, and May, 1832, 1833, and 1834, at New London Bridge.

In the first year none of the lower portions of Old London Bridge, (with the exception of two piers,) which prevented the natural flow of the tidal waters, were removed; and in the second year almost the whole of that structure was cleared away as regarded the masonry and starlings, although the section of the river was far from being completed; many portions still remaining one or two feet above low-water mark, and which were finally removed in the year 1834.

New London Bridge.	Fall through Old London Bridge.		Range of Flood Tide.		Duration of Flood Tide.		Surface of High Water above or below Trinity datum.		Surface of Low Water Mark below Trinity datum.	
	Greatest.	Least.	Greatest of Springs.	Least of Neaps.	Greatest of Springs.	Least of Neaps.	Greatest of Springs.	Least of Neaps.	Greatest of Springs.	Least of Neaps.
	ft. in.	ft. in.	ft. in.	ft. in.	h. m.	h. m.	ft. in.	ft. in.	ft. in.	ft. in.
1832.	3 6	1 10	16 9	10 8	4 15		1 4 above	5 0 below	15 5	15 8
1833.	2 1	0 0	18 9	13 8	4 34	5 20	0 10 ditto	1 9 ditto	17 11	15 5
1834.	5	3	19 9				1 9 *		20 3	

In conclusion, it may be stated,—That the drainage of the districts bordering on the river Thames has been greatly improved;—that barges, which used formerly to be towed up from Putney to Richmond by horses, are now carried by the current from London Bridge to Richmond in one tide;—That the fall of the low-water surface *below* Bridge has been so considerable as to cause ships, in many instances, to ground in their tiers at low-water;—and that from a register of the tides, kept by Capt. Maugham of the London Docks, the average depth at low-water on the cill of Shadwell Dock was 1 ft. 10 in. below the Old Trinity datum; and that where formerly there were 8 feet in depth upon the Dock cill, there are now only 6 feet 2 inches on the average: on the 5th of November, 1834, the tide fell as low as 4 feet 3 inches on the cill.

The accompanying Plate shows the section of the river Thames, from actual survey, from the mouth of the river Kennet to the Nore. The upper part could not be taken in time.

* During equinoctial gales in March, wind N.W., but the average rise barely exceeded six inches above Trinity datum.

TRANSACTIONS OF THE SECTIONS.

1. MATHEMATICS AND PHYSICS.

MATHEMATICS.

On the Application to Dynamics of a General Mathematical Method previously applied to Optics. By W. R. Hamilton, M.R.I.A., *Astronomer Royal for Ireland.*

The method is founded on a combination of the principles of variations with those of partial differentials, and may suggest to analysts a separate branch of algebra, which may be called, perhaps, the *Calculus of Principal Functions*; because, in all the chief applications of algebra to physics, and in a very extensive class of purely mathematical questions, it reduces the determination of many mutually connected functions to the search and study of one principal or central relation. In applying this method to Dynamics, (having previously applied it to Optics,) Professor Hamilton has discovered the existence of a principal function, which, if its form were fully known, would give, by its partial differential coefficients, all the intermediate and all the final integrals of the known equations of motion.

Professor Hamilton is of opinion that the mathematical explanation of all the phænomena of matter distinct from the phænomena of life, will ultimately be found to depend on the properties of systems of attracting and repelling points. And he thinks that those who do not adopt this opinion in all its extent, must yet admit the properties of such systems to be more highly important in the present state of science, than any other part of the application of mathematics to physics. He therefore accounts it the capital problem of Dynamics, "to determine the $3n$ rectangular coordinates, or other marks of position, of a free system of n attracting or repelling points, as functions of the time," involving also $6n$ initial constants, which depend on the initial circumstances of the motion, and in-

volving, besides, n other constants called the masses, which measure, for a standard distance, the attractive or repulsive energies.

Denoting these n masses by $m_1\, m_2 \ldots m_n$, and their $3\,n$ rectangular coordinates by $x_1\; y_1\; z_1 \ldots\ldots x_n\; y_n\; z_n$, and also the $3\,n$ component accelerations, or second differential coefficients of these coordinates, taken with respect to the time, by $x''_1\; y''_1\; z''_1 \ldots x''_n\; y''_n\; z''_n$, he adopts LAGRANGE's statement of this problem; namely, a formula of the following kind,

$$\Sigma\,.\,m\,(x''\,\delta\,x + y''\,\delta\,y + z''\,\delta\,z) = \delta\,\mathrm{U}, \quad . \quad . \quad (1.)$$

in which U is the sum of the products of the masses, taken two by two, and then multiplied by each other and by certain functions of their mutual distances, such that their first derived functions express the laws of their mutual repulsion, being negative in the case of attraction. Thus, for the solar system, each product of two masses is to be multiplied by the reciprocal of their distance, and the results are to be added in order to compose the function U.

Mr. Hamilton next multiplies this formula of Lagrange by the element of the time $d\,t$, and integrates from the time o to the time t, considering the time and its element as not subject at present to the variation δ. He denotes the initial values, or values at the time o, of the coordinates $x\,y\,z$, and of their first differential coefficients $x'\,y'\,z'$, by $a\,b\,c$ and $a'\,b'\,c'$; and thus he obtains, from Lagrange's formula (1.), this other important formula,

$$\Sigma\,.\,m\,(x'\,\delta\,x - a'\,\delta\,a + y'\,\delta\,y - b'\,\delta\,b + z'\,\delta\,z - c'\,\delta\,c) = \delta\,\mathrm{S}, \quad (2.)$$

S being the definite integral

$$\mathrm{S} = \int_0^t \left\{ \mathrm{U} + \Sigma.\,\frac{m}{2}\,(x'^2 + y'^2 + z'^2) \right\} d\,t. \quad . \quad (3.)$$

If the known equations of motion, of the forms

$$m_i\,x''_i = \frac{\delta\,\mathrm{U}}{\delta\,x_i}\;,\quad m_i\,y''_i = \frac{\delta\,\mathrm{U}}{\delta\,y_i}\;,\; m_i\,z''_i = \frac{\delta\,\mathrm{U}}{\delta\,z_i}. \quad . \; (4.)$$

had been completely integrated, they would give the $3\,n$ coordinates $x\,y\,z$, and therefore also S, as a function of the time t, the masses $m_1 \ldots m_n$, and the $6\,n$ initial constants $a\,b\,c\;a'\,b'\,c'$; so that, by eliminating the $3\,n$ initial components of velocities $a'\,b'\,c'$ we should in general obtain a relation between the $7\,n + 2$ quantities $\mathrm{S}, t, m, x, y, z, a, b, c$, which would give S as a function of the time, the masses, and the final and initial coordinates. We do not yet know the form of this last function,

but we know its variation (2.), taken with respect to the $6n$ coordinates; and on account of the independence of their $6n$ variations, we can resolve this expression (2.) into two groups, containing each $3n$ equations: namely,

$$\frac{S}{\delta x_i} = m_i x'_i, \frac{\delta S}{\delta y_i} = m_i y'_i, \frac{\delta S}{\delta z_i} = m_i z'_i, \quad . \quad . \quad (5.)$$

and

$$\frac{\delta S}{\delta a_i} = - m_i a'_i, \frac{\delta S}{\delta b_i} = -m_i b'_i, \frac{\delta S}{\delta c_i} = - m_i c'_i; \quad . (6.)$$

the first members being partial differential coefficients of the function S, which Mr. Hamilton calls the *Principal Function* of motion of the attracting or repelling system. He thinks that if analysts had perceived this principal function S, and these groups of equations (5.) and (6.), they must have perceived their importance. For the group (5.) expresses the $3n$ intermediate integrals of the known equations of motion (4.), under the form of $3n$ relations between the time t, the masses m, the varying coordinates x, y, z, the varying components of velocities x' y' z', and the $3n$ initial constants a b c; while the group (6.) expresses the $3n$ final integrals of the same known differential equations, as $3n$ relations, with $6n$ initial and arbitrary constants a b c a' b' c', between the time, the masses, and the $3n$ varying coordinates. These $3n$ intermediate and $3n$ final integrals, it was the problem of dynamics to discover. Mathematicians had found seven intermediate, and none of the final integrals.

Professor Hamilton's solution of this long celebrated problem contains, indeed, one unknown function, namely the *principal function* S, to the search and study of which he has reduced mathematical dynamics. This function must not be confounded with that so beautifully conceived by Lagrange, for the more simple and elegant expression of the known differential equations. Lagrange's function *states*, Mr. Hamilton's function would *solve* the problem. The one serves to form the *differential* equations of motion, the other would give their *integrals*. To assist in pursuing this new track, and in discovering the form of this new function, Mr. Hamilton remarks that it must satisfy the following partial differential equation of the first order and second degree, (the time being now made to vary,)

$$\frac{\delta S}{\delta t} + \Sigma . \frac{1}{2m} \left\{ \left(\frac{\delta S}{\delta x}\right)^2 + \left(\frac{\delta S}{\delta y}\right)^2 + \left(\frac{\delta S}{\delta z}\right)^2 \right\} = U; \quad . (7.)$$

which may rigorously be thus transformed, by the help of the equations (5.),

$$S = S_1 + \int_0^t \left(U - \frac{\delta S_1}{\delta t} - \Sigma . \frac{1}{2m} \left\{ \left(\frac{\delta S_1}{\delta x}\right)^2 + \left(\frac{\delta S_1}{\delta y}\right)^2 + \left(\frac{\delta S_1}{\delta z}\right)^2 \right\} \right) dt \quad (8.)$$

$$+ \int_0^t \Sigma . \frac{1}{2m} \left\{ \left(\frac{\delta S}{\delta x} - \frac{\delta S_1}{\delta x}\right)^2 + \left(\frac{\delta S}{\delta y} - \frac{\delta S_1}{\delta y}\right)^2 + \left(\frac{\delta S}{\delta z} - \frac{\delta S_1}{\delta z}\right)^2 \right\} dt$$

S_1 being any arbitrary function of the same quantities, t, m, x, y, z, a, b, c, supposed only to vanish (like S) at the origin of time. If this arbitrary function S_1 be so chosen as to be an approximate value of the sought function S, (and it is always easy so to choose it,) then the two definite integrals in the formula (8.) are small, but the second is in general much smaller than the first; it may, therefore, be neglected in passing to a second approximation, and in calculating the first definite integral, the following approximate forms of the equations (6.) may be used,

$$\frac{\delta S_1}{\delta a} = - m a', \frac{\delta S_1}{\delta b} = - m b', \frac{\delta S_1}{\delta c} = - m c' \quad . \quad (9.)$$

In this manner, a first approximation may be successively and indefinitely corrected. And for the practical perfection of the method, nothing further seems to be required, except to make this process of correction more easy and rapid in its applications.

Professor Hamilton has written two Essays on this new method in Dynamics, and one of them is already printed in the second part of the *Philosophical Transactions* (of London) for 1834. The method did not at first present itself to him under quite so simple a form. He used at first a *Characteristic Function* V, more closely analogous to that optical function which he had discovered, and had denoted by the same letter, in his *Theory of Systems of Rays*. In both optics and dynamics, this function was the quantity called *Action*, considered as depending (chiefly) on the final and initial coordinates. But when this *Action-Function* was employed in dynamics, it involved an auxiliary quantity H, namely the known constant part in the expression of half the living force of a system; and many troublesome eliminations were required in consequence, which are avoided by the new form of the method.

Mr. Hamilton thinks it worth while, however, to point out briefly a new property of this constant H, which suggests a new manner of expressing the differential and integral equations of motion of an attracting or repelling system. It is often

useful to express the $3n$ rectangular coordinates $x_1\ y_1\ z_1 \ldots x_n\ y_n\ z_n$, as functions of $3n$ other marks of position, which may be thus denoted, $\eta_1\ \eta_2 \ldots \eta_{3n}$; and if $3n$ other new variables, $\varpi_1\ \varpi_2 \ldots \varpi_{3n}$, be introduced, and defined as follows,

$$\varpi_i = \Sigma.\ m \left(x' \frac{\delta x}{\delta \eta_i} + y' \frac{\delta y}{\delta \eta_i} + z' \frac{\delta z}{\delta \eta_i} \right) \quad . \quad . \quad (10.)$$

it is, in general, possible to express, reciprocally, the $6n$ variables $x\ y\ z\ x'\ y'\ z'$ as functions of these $6n$ new variables $\eta\ \varpi$; it is, therefore, possible to express, as such a function, the quantity

$$\mathrm{H} = \Sigma.\ \frac{m}{2} (x'^2 + y'^2 + z'^2) - \mathrm{U}, \quad . \quad . \quad . \quad . \quad (11.)$$

under the form

$$\mathrm{H} = \mathrm{F}\,(\varpi_1, \ldots \varpi_{3n},\ \eta_1, \ldots \eta_{3n}) - \mathrm{U}\,(\eta_1, \ldots \eta_{3n}), \quad . \quad (12.)$$

in which the part F is rational, integer, and homogeneous of the second dimension with respect to the variables ϖ. Now Mr. Hamilton has found that when the quantity H is expressed in this last way, as a function of these $6n$ new variables, $\eta\ \varpi$, its variation may be put under this form,

$$\delta\,\mathrm{H} = \Sigma\,(\eta'\,\delta\,\varpi - \varpi'\,\delta\,\eta), \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

$\eta'\ \varpi'$ denoting the first differential coefficients of these new variables $\eta\ \varpi$, considered as functions of the time. The $3n$ differential equations of motion of the second order, (4.), between the rectangular coordinates and the time, for any attracting or repelling system, may therefore be generally transformed into twice that number of equations of the first order, between these $6n$ variables and the time, of the forms

$$\eta'_i = \frac{\delta\,\mathrm{H}}{\delta\,\varpi_i},\ \varpi'_i = -\frac{\delta\,\mathrm{H}}{\delta\,\eta_i} \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

To integrate this system of equations, is to assign, from them, $6n$ relations between the time t, the $6n$ variables $\eta_i\ \varpi_i$, and their $6n$ initial values which may be called $e_i\ p_i$. Mr. Hamilton resolves the problem, under this more general form, by the same *principal function* S as before, regarding it, however, as depending now on the new marks $\eta\ e$ of final and initial positions of the various points of the system. For, putting, in this new notation,

$$\mathrm{S} = \int_0^t \left(\Sigma.\,\varpi\,\frac{\delta\,\mathrm{H}}{\delta\,\varpi} - \mathrm{H}\right) d\,t, \quad . \quad . \quad . \quad . \quad (15.)$$

and considering the time as given, he finds now the formula of variation

$$\delta S = \Sigma (\varpi \delta \eta - p \delta e), \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

and therefore the $6n$ separate equations

$$\varpi_i = \frac{\delta S}{\delta \eta_i}, \quad p_i = - \frac{\delta S}{\delta e_i}, \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

which are forms for the sought relations.

Professor Hamilton thinks that these two formulæ of variation, (13.) and (16.) namely

$$\delta H = \Sigma (\eta' \delta \varpi - \varpi' \delta \eta), \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

and

$$\delta S = \Sigma (\varpi \delta \eta - p \delta e), \quad . \quad . \quad . \quad . \quad . \quad . \quad (B.)$$

are worthy of attention, as expressing, under concise and simple forms, the one the differential and the other the integral equations of motion, of an attracting or repelling system. They may be extended to other problems of dynamics, besides this capital problem. The expression H can always easily be found, and the function S can be determined with indefinite accuracy by a method of successive approximation of the kind already explained.

These properties of his *Principal Function* are treated of more fully in his "Second Essay on a General Method in Dynamics*"; in which he has introduced several forms of a certain *Function of Elements*, connected with the Principal Function, and with each other, and adapted to questions of perturbation; and has shown that for the perturbations of a ternary or multiple system with any laws of attraction or repulsion, and with one predominant mass, the differential equations of the varying elements of *all* the smaller masses may be expressed together, and as simply as in the usual way, by the coefficients of *one* disturbing function, (namely, the disturbing part of the whole expression H,) and may be integrated rigorously by a corollary of his general method.

* This Essay will be found in the *Philosophical Transactions* for 1835.

On Conjugate Functions, or Algebraic Couples, as tending to illustrate generally the Doctrine of Imaginary Quantities, and as confirming the Results of Mr. Graves respecting the Existence of Two independent Integers in the complete expression of an Imaginary Logarithm. By W. R. HAMILTON, M.R.I.A., *Astronomer Royal for Ireland.*

ADMITTING, at first, the usual things about imaginaries, let

$$u+v\sqrt{-1} = \phi.(x + y\sqrt{-1}), \quad \text{(a.)}$$

in which x, y are one pair of real quantities, and u, v are another pair, depending on the former, and therefore capable of being thus denoted, $u_{x,y}$, $v_{x,y}$. It is easy to prove that these two functions, $u_{x,y}$, $v_{x,y}$, must satisfy the two following equations between their partial differential coefficients of the first order:

$$\frac{du}{dx} = \frac{dv}{dy}, \quad \frac{du}{dy} = -\frac{dv}{dx}. \quad \text{(b.)}$$

Professor Hamilton calls these the *Equations of Conjugation*, between the functions u, v, because they are the necessary and sufficient conditions in order that the imaginary expression $u + v\sqrt{-1}$ should be a function of $x + y\sqrt{-1}$. And he thinks that without any introduction of imaginary symbols, the two real relations (b.), between two real functions, might have been suggested by analogies of algebra, as constituting between those two functions a connexion useful to study, and as leading to the same results which are usually obtained by imaginaries. Dismissing, therefore, for the present, the conception and language of imaginaries, Mr. Hamilton proposes to consider a few properties of such *Conjugate Functions*, or *Algebraic Couples*; defining two functions to be *conjugate* when they satisfy the two equations of conjugation, and calling, under the same circumstances, the pair or couple (u, v) *a function of the pair* (x, y).

An easy extension of this view leads to the consideration of relations between several pairs, and generally to reasonings and operations upon pairs analogous to reasonings and operations on single quantities. For all such reasonings it is necessary to establish definitions: the following definitions of sum and product of pairs appear to Mr. Hamilton natural:

$$(x, y) + (a, b) = (x + a, y + b), \quad \text{(c.)}$$

$$(x, y) \times (a, b) = (x\,a - y\,b, x\,b + y\,a), \quad \text{(d.)}$$

and conduct to meanings of all integer powers and other ra-

tional functions of pairs, enabling us to generalize any ordinary algebraic equation from single quantities to pairs, and so to interpret the research of all its roots, without introducing imaginaries.

Without stopping to justify these definitions of sum and product, which will probably be admitted without difficulty, Mr. Hamilton inquires what analogous meaning should be attached to an exponential pair, or to the notation $(a, b)^{(x,y)}$; or, finally, what forms ought to be assigned to the conjugate functions $u_{x,y}$, $v_{x,y}$, in the exponential equation

$$(a, b)^{(x,y)} = (u_{x,y}, v_{x,y}). \quad \text{(e.)}$$

In the theory of quantities, the most fundamental properties of the exponential function $a^x = \phi(x)$ are these:

$$\phi(x)\,\phi(\xi) = \phi(x+\xi), \text{ and } \phi(1) = a; \quad \text{(f.)}$$

Mr. Hamilton thinks it right, therefore, in the theory of pairs, to establish by definition the analogous properties,

$$(a, b)^{(x,y)}\,(a, b)^{(\xi,\eta)} = (a, b)^{(x+\xi,\, y+\eta)}, \quad \text{(g.)}$$

and

$$(a, b)^{(1,0)} = (a, b). \quad \text{(h).}$$

Combining these properties with the equation (e.) and with the definition (d.) of product, and defining an equation between pairs to involve two equations between quantities, Mr. Hamilton obtains the following pair of ordinary functional equations, or equations in differences, to be combined with the two equations of conjugation:

$$\left.\begin{aligned} u_{x,y}\, u_{\xi,\eta} - v_{x,y}\, v_{\xi,\eta} &= u_{x+\xi,\, y+\eta}, \\ u_{x,y}\, v_{\xi,\eta} + v_{x,y}\, u_{\xi,\eta} &= v_{x+\xi,\, y+\eta}, \end{aligned}\right\} \quad \text{(i.)}$$

and also the following pair of conditions,

$$u_{1,0} = a, \qquad v_{1,0} = b. \quad \text{(k.)}$$

Solving the pair of equations (i.), he finds

$$\left.\begin{aligned} u_{x,y} &= f(\alpha' y + \beta' x) \,.\, \cos(\alpha y + \beta x), \\ v_{x,y} &= f(\alpha' y + \beta' x) \,.\, \sin(\alpha y + \beta x), \end{aligned}\right\} \quad \text{(l.)}$$

$\alpha\ \beta\ \alpha'\ \beta'$ being any four constants, independent of x and y, and the function f being such that

$$f(r) = 1 + \frac{r}{1} + \frac{r^2}{1\,.\,2} + \frac{r^3}{1\,.\,2\,.\,3} + \&c.; \quad \text{(m.)}$$

and having established the following, among many other general properties of conjugate functions, that if two such functions be put under the forms

$$\left.\begin{aligned} u_{x,y} &= f(\rho_{x,y}) \,.\, \cos \theta_{x,y}, \\ v_{x,y} &= f(\rho_{x,y}) \,.\, \sin \theta_{x,y}, \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (\text{n.})$$

f still retaining its late meaning, the functions $\rho_{x,y}$ $\theta_{x,y}$ are also conjugate, he concludes that the 4 constants of (l.) are connected by these two relations,

$$\beta' = +\alpha, \quad \alpha' = -\beta, \quad \ldots\ldots\ldots \quad (\text{o.})$$

so that the general expressions for two conjugate exponential functions are:

$$\left.\begin{aligned} u_{x,y} &= f(\alpha x - \beta y) \,.\, \cos(\alpha y + \beta x), \\ v_{x,y} &= f(\alpha x - \beta y) \,.\, \sin(\alpha y + \beta x); \end{aligned}\right\} \quad \ldots \quad (\text{p.})$$

and it only remains to introduce the constants of the *base-pair* (a, b), by the conditions (k.). Those conditions give

$$a = f(\alpha) \,.\, \cos \beta, \; b = f(\alpha) \,.\, \sin \beta, \quad \ldots\ldots \quad (\text{q.})$$

and therefore, finally,

$$\left.\begin{aligned} \alpha &= \int_1^{\sqrt{a^2+b^2}} \frac{dr}{r}, \\ \beta &= \beta_0 + 2\,i\,\pi, \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (\text{r.})$$

i being an arbitrary integer, and β_0 being a quantity which may be assumed as $> -\pi$, but not $> \pi$, and may then be determined by the equations

$$\cos \beta_0 = \frac{a}{\sqrt{a^2+b^2}}, \quad \sin \beta_0 = \frac{b}{\sqrt{a^2+b^2}} \quad \ldots \quad (\text{s.})$$

The form of the direct exponential pair $(a, b)^{(x,y)}$, (or of the direct conjugate exponential functions u, v,) is now entirely determined; but the process has introduced *one* arbitrary integer i.

Another arbitrary integer is introduced by reversing the process, and seeking the *inverse exponential* or *logarithmic pair*,

$$(x, y) = \underset{(a,\,b)}{\text{Log.}}\,(u, v). \quad \ldots\ldots\ldots \quad (\text{t.})$$

Professor Hamilton finds for this inverse problem the formulæ

$$x = \frac{\alpha\rho + \beta\theta}{\alpha^2 + \beta^2}, \quad y = \frac{\alpha\theta - \beta\rho}{\alpha^2 + \beta^2}; \quad \ldots \ldots \quad \text{(u.)}$$

in which α β are the constants deduced as before by (r.) from the *base-pair* (a, b), and involving the integer i in the expression of β; while ρ and θ are deduced from u and v, with a new arbitrary integer k in θ, by expressions analogous to (r.), namely,

$$\left.\begin{aligned} \rho &= \int_1^{\sqrt{u^2+v^2}} \frac{dr}{r}, \\ \theta &= \theta_0 + 2k\pi, \end{aligned}\right\} \quad \ldots \ldots \quad \text{(v.)}$$

in which θ_0 is supposed $> -\pi$, but not $> \pi$, and

$$\cos\theta_0 = \frac{u}{\sqrt{u^2+v^2}}, \quad \sin\theta_0 = \frac{v}{\sqrt{u^2+v^2}}. \quad \ldots \quad \text{(w.)}$$

By the definition of quotient, which the definition (d.) of product suggests, the formulæ (u.) may be briefly comprised in the following expression of a logarithmic pair:

$$(x, y) = \frac{(\rho, \theta)}{(\alpha, \beta)}; \quad \ldots \ldots \quad \text{(x.)}$$

and, reciprocally, the direct exponential pair (u, v), as already determined, may be concisely expressed by this other form of the same equation,

$$(\rho, \theta) = (x, y)\,(\alpha, \beta), \quad \ldots \ldots \quad \text{(y.)}$$

if we still suppose

$$\left.\begin{aligned} (u, v) &= (f\rho \,.\, \cos\theta, f\rho \,.\, \sin\theta), \\ (a, b) &= (f\alpha \,.\, \cos\beta, f\alpha \,.\, \sin\beta). \end{aligned}\right\} \quad \ldots \ldots \quad \text{(z.)}$$

Thus all the foregoing results respecting exponential and logarithmic pairs may be comprised in the equations (y.) and (z.)

When translated into the language of imaginaries, they agree with the results respecting imaginary exponential functions, direct and inverse, which were published by Mr. Graves in the *Philosophical Transactions* for 1829, and it was in meditating on those results of Mr. Graves that Mr. Hamilton was led, several years ago, to this theory of conjugate functions*, as

* An Essay on this theory of Conjugate Functions was presented some years ago by Professor Hamilton to the Royal Irish Academy, and will be published in one of the next forthcoming volumes of its *Transactions*.

tending to illustrate and confirm them. For example, Mr. Graves had found, for the logarithm of unity to the Napierian base, the expression

$$\underset{e}{\mathrm{Log}} \,.\, 1 = \frac{2\,k\,\pi\,\sqrt{-1}}{1 + 2\,i\,\pi\,\sqrt{-1}},$$

which is more general than the usual expression. This result of Mr. Graves appeared erroneous to the author of the excellent Report on Algebra, which was lately printed for the Association; but it is confirmed by Mr. Hamilton's theory, which conducts to it under the form of a relation between real pairs, namely,

$$\underset{(e,\,0)}{\mathrm{Log}} \,.\, (1, 0) = \frac{(0,\, 2\,k\,\pi)}{(1,\, 2\,i\,\pi)}:$$

and the connexion of this result with that Report was thought to justify a greater fulness in the present communication* than would have been proper otherwise on a question so abstract and mathematical.

On the Theory of Exponential Functions. By John Thomas Graves, *of the Inner Temple, Esquire, A.M.*

In October, 1826, the author of the Memoir, of which the following pages contain an abstract, was engaged in researches, and obtained results, which were communicated to the Royal Society of London in the year 1828, and published in the *Philosophical Transactions* for 1829, under the title "An Attempt to Rectify the Inaccuracy of some Logarithmic Formulæ."

Certain theorems of Newton, Euler, and Moivre were known to establish a remarkable connexion between exponential and trigonometrical functions; and the corrections made by M. Poisson and M. Poinsot in formulæ of the latter class, induced the author to apply similar corrections to those of the former, more generally than appeared to have been previously accomplished. Accordingly, his original paper exhibited formulæ involving *arbitrary integers*, by means of which he considered that a solution was afforded for various difficulties that had formerly

* Since this communication was prepared, Professor Hamilton has learned that Professor Ohm of Berlin has been conducted by a different method to results respecting Imaginary Logarithms, which agree with those of Mr. Graves: as do also the results obtained in other ways, by Mons. Vincent and by Mr. Warren. The partial differential equations (b.) have been noticed and employed, for a different purpose, by Mr. Murphy of Cambridge.

perplexed mathematicians. In particular, he professed to elucidate the subject of the logarithms of negative and imaginary quantities, which, at different periods, had occasioned controversies between Leibnitz and Jean Bernoulli, Euler and D'Alembert.

The researches of others have since confirmed the views of the author, whose claim to independent discovery and priority of printed publication is undisputed. In a paper of subsequent date, published in the same volume of the *Phil. Trans.*, the Rev. John Warren of Cambridge, by original investigation, arrived at some of Mr. Graves's results. In June, 1832, M. Vincent published at Lille, results identical in effect with the author's principal formulæ. M. Vincent claims to have anticipated Mr. Graves in their discovery, and appeals, in corroboration of this statement, to unpublished documentary evidence in the archives of the Société Philomatique, containing the Rapport of MM. Ampère and Bourdon on a Mémoire read August 18, 1827, as appears by the procès-verbal of that day. This Mémoire is said by M. Vincent to have been *substantially* the same as that of June, 1832, and to have been communicated to M. Gergonne as early as April, 1825. Finally, Professor Hamilton, of Dublin, has deduced from his ingenious "Theory of Conjugate Functions or Algebraic Couples" a complete confirmation of the author's system.

Mr. Peacock, in his "Review of the recent progress of Analysis," (page 267 of the Transactions of the Association for 1833,) noticed the researches of Mr. Graves, but did not acquiesce in his conclusions, which he conceived to be difficult to reconcile with received opinions, and to be founded on the untenable assumption of a *periodic* logarithmic base. It was for the purpose of removing the impression which the high authority of Mr. Peacock is calculated to produce that the Author presented to the Association a second paper on the subject, in order to invite the attention of analysts to a condensed statement of his reasoning and results, exhibited in a more systematic and popular shape than in his former essay.

He is of opinion that the embarrassments and absurdities which still encumber the doctrine of exponential functions have chiefly arisen from calculating without fixed original principles; from occasionally regarding disintegrated properties, of partial and collateral application to such functions, as the foundations of essential and unlimited theorems; from incautiously employing developments in unterminated series, without reference to their complements and the limits of their accuracy; and, above all, from applying algebraic rules, that are appropriate

only to individualized values, to formulæ more or less indefinite, containing those values *among others*. This is in fact the paralogism of applying to an equivocal term used in *one* sense, a predication proved only with respect to a different sense. He adopts the position of M. Crelle, (*Journal für die reine und angewandte Mathematik*, tom. vii. cah. 3 and 4,) that no equation is admissible, of which one side may not be proved to be, by previous consistent postulates, an "identical transformation" of the other. He would not banish diverging series from analysis, but he agrees with M. Poisson and M. Cauchy in holding that the *remainder* or complement of a series, even after an infinite number of terms, ought always to be taken into consideration, since postponement, however long continued, cannot, of itself, destroy. He goes so far as to maintain that, even in converging series, this remainder, though an infinitely small quantity, may, in certain cases, produce sensible effects. Thus, in his opinion, we are not always at liberty to assume that the sum of the series obtained by differentiating an infinite number of terms of a converging development will approximate indefinitely to the differential coefficient of the function, because (as he shows by example) the differential coefficient of the infinitely small remainder may be of finite magnitude. He assumes the received symbolic rules of algebraic addition, subtraction, multiplication, and division, (which are in accordance with certain leading and elect truths of numerical science,) and he proceeds in like manner to define exponential quantities and logarithms by means of properties which he supposes that mathematicians would generally acknowledge to be characteristic and fundamental. He admits also the theorems of the integral and the differential calculus as derived from the consideration of limits. From these definitions and postulates, he contends that his conclusions not only legitimately follow, but are *consistent with received notions*, as far as the latter are consistent with themselves and with each other.

He explains a^x (where a and x may be any quantities, real or imaginary,) by means of the following functional definition, viz. "a^x comprises in succession every function (ϕx) of x, which, independently of x and x', fulfils the following conditions:

$$\left.\begin{array}{r}\phi x \ \phi x' = \phi(x + x') \\ \phi 1 = a\text{"}\end{array}\right\} \quad \ldots\ldots \quad (1.)$$

From this definition, (which Mr. Hamilton recommended him to make, in explicit terms, the basis of his former Essay,) he proceeds to evolve all the properties of a^x. It embodies the well-known characteristic which led to the extension of expo-

nential notation from integral to fractional, to incommensurable, to negative, and to imaginary quantities. He contends that there are no propositions connected with the theory more fundamental than that, first, "in any exponential system, the exponent of the product of similar exponential functions of any quantities is equal to the sum of the exponents of the factors"; and that, secondly, "an exponential function of 1 is equal to the base."

If $a^x = y$, the search of either symbol, (y the power, a the base, x the logarithm,) as a function of the other two, furnishes three principal problems.

First, To find y in terms of a and x.

The solution is $$a^x = f(x f^{-1} a) \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

In this formula the notation $f\theta$ signifies $\cos\theta + \sqrt{-1}\,\sin\theta$, $\cos\theta$ and $\sin\theta$ being functions of any real or imaginary quantity θ, which, independently of θ and θ', fulfil the following conditions:

$$\left.\begin{aligned} \cos\theta\cos\theta' - \sin\theta\sin\theta' &= \cos(\theta+\theta') \\ \sin\theta\cos\theta' + \cos\theta\sin\theta' &= \sin(\theta+\theta') \\ (\cos\theta)^2 + (\sin\theta)^2 &= 1 \end{aligned}\right\} \quad . \quad . \quad . \quad (3.)$$

Let $a = r + \sqrt{-1}\,s$, r and s being real, then the notation $f^{-1}a$ signifies

$$2i\pi + \frac{s}{\sqrt{s^2}}\cos^{-1}_{0+}\frac{r}{\sqrt{r^2+s^2}} + \sqrt{-1}\,l\frac{1}{\sqrt{r^2+s^2}} \quad . \quad . \quad (4.)$$

In this formula i denotes 0, or any integer positive or negative; $\frac{s}{\sqrt{s^2}}$ denotes 1 or -1, according as s is *not less* or less than 0. When s is positive or negative, $\sqrt{s^2}$ denotes the *positive* square root of s^2. The author makes considerable use of the class of expressions of which $\frac{s}{\sqrt{s^2}}$ is an example. They are extremely convenient in general formulæ, particularly on account of their property of obviating the necessity of separate cases. $\cos^{-1}_{0+}\theta$ represents the arc, when radius $= 1$, in the first positive semicircle (including 0 and π) whose cosine $= \theta$. In the statement of propositions having limits, he suggests the peculiar importance in these investigations of expressing clearly whether the limits, or either of them, are to be taken inclusively or exclusively. $l\theta$ denotes the ordinary real Neperian logarithm of θ.

The value of $f^{-1}a$, corresponding to a particular i in (4.), he denotes by $f_i^{-1}a$. There is a *discontinuity* in $f_i^{-1}a$ or $f_i^{-1}(r+\sqrt{-1}\,s)$, as above defined. When r is negative, $f_i^{-1}(r+\sqrt{-1}\,s)$ is suddenly diminished by (quàm proximè) 2π on the completion of the passage of s through 0 from positive to negative. For the purposes to which the author applies $f_i^{-1}a$, it is not necessary that for all nascent and transitive, as well as finite and quantitative *states* and values of the r and s and the r' and s' belonging respectively to a and a', it should be predicable absolutely that $f_i^{-1}a$, as above defined, is *the same individual function* of a and i, that $f_i^{-1}a'$ is of a' and the same i. It is sufficient for him, that, in all imaginable cases, $f_i^{-1}a$, when i is supposed to be *arbitrary*, comprises *all* the roots of the equation $f\theta = a$, and, when i is supposed to be individualized, denotes a *unique* value. These latter objects are attained by his notation, as above explained, which arbitrarily defines $\frac{s}{\sqrt{s^2}}$ to mean 1, whenever $s = 0$.

That value of a^x which is expressed by $f(xf_i^{-1}a)$, he denotes by the symbol a_i^x, and terms the ith *value* of a^x: a_i^x is an individual solution of ϕx in equation (1.); a_i^x and $a_i^{x'}$ are similar individual exponential functions of x and x', in a system where a_i^1 is equal to a, and independent of i. The theorems contained in the author's paper depend upon the original definitions and principles assumed; and if different subsequent definitions, subservient only to *notation*, were employed,—if a value of a^x different from his a_0^x were arbitrarily assumed as the primitive, the same theorems would still exist, though they might require to be differently expressed. He gives symmetrical converging developments and easily calculable formulæ for the real and imaginary parts of $(r+\sqrt{-1}\,s)_i^{x+\sqrt{-1}\,x'}$, x and x' being real as well as r and s.

Second. The second problem is to find a such that $a^x = y$, x and y being given quantities, real or imaginary.

The general solution is

$$a = f\left(\frac{1}{x}f^{-1}y\right) = y^{\frac{1}{x}}, \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

for y will certainly be found *among* the values of a^x, when a

is equal to *any* quantity furnished by formula (5.), and conversely, if any value of $a^x = y$, a cannot but be equal to *some one* of those quantities.

If, however, the problem be to find a, such that *the* i^{th} *value* of a^x may $= y$, i being given as well as x and y, it may be impossible to solve the problem by any value of a representable by admitted algebraic symbols, or reducible to the form $r + \sqrt{-1}\, s$. The general result of the author's investigations on this branch of the subject is, that being given the equation $a_i^{r+\sqrt{-1}\,s} = p + \sqrt{-1}\, q$, we shall have, when r is not $= 0$, at least n algebraically representable solutions, and may have $n + 1$ solutions, if $r^2 + s^2$ be greater than $n\sqrt{r^2}$; and that we can have at most but one such solution, and may have not even one, when $r^2 + s^2$ is not greater than $\sqrt{r^2}$. When $\sqrt{r^2}$ is equal to or greater than 1, one representable a at least may always be found to satisfy the equation. The "chance of representability" of a, when $a_i^{r+\sqrt{-1}\,s}$ is given, when i is for the first time taken at random, and r is not $= 0$, may be denoted by $\frac{r^2+s^2}{\sqrt{r^2}}$, certainty being denoted by 1. Let r be $= 0$, then the equation becomes $a_i^{\sqrt{-1}\,s} = p + \sqrt{-1}\, q$. This equation, i being given, as well as s, p, and q, will have an infinite number of roots for a, if $\frac{1}{s}\, l \frac{1}{\sqrt{p^2+q^2}}$ be greater than $(2i - 1)\pi$, and not greater than $(2i + 1)\pi$; otherwise, it will have not one representable root.

Third, The third problem is to represent all the logarithms of a given quantity in a given base.

Let $a^x = y$, then every quantity which, being substituted for x, allows any value of a^x, as explained by (1.), to be equal to y, is, according to the author's definition, a "LOGARITHM" of y in the base a.

The solution of this third problem is

$$x = \frac{f^{-1} y}{f^{-1} a} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

Any particular logarithm (x) will be of the form $\frac{f_{i'}^{-1} y}{f_i^{-1} a}$, i' and i being arbitrary independent integers.

i, in the denominator of the preceding formula, names, according to the author's nomenclature, the "ORDER" of the logarithm, and i', in the numerator, its "RANK" in that order.

If x be a logarithm of y of any rank in the ith order in the base a, we shall have $a_i^x = y$.

When individualization is required, the author proposes to denote the logarithm of y in the base a, of the ith rank in the ith order by the symbol $a\text{-}\log_i^{i} y$, since the ordinary symbol $(\log y)$ yields no information as to the base, the order, or the rank. Thus, e being the Neperian base, $e\text{-}\log_i^{i} 1 = \dfrac{2i\pi}{2i\pi - \sqrt{-1}}$.

Having solved these general problems, the author proceeds to affix limits to some commonly received equations, to explain some of the difficulties and paradoxes incident to the subject,—to account for known facts, and to deduce novel facts relative to the equation $a^x = y$,—to apply his theory to other useful formulæ connected with exponential functions, and to show how far it accords with ordinary notions in a variety of particular cases; but the limits of an abstract preclude an enumeration of his results. The following, however, may be noticed:

Let

$$p = \frac{1}{\sqrt{2}} \sqrt{r^2 + s^2 + 1 - \sqrt{(r^2 + s^2 + 1)^2 - 4r^2}},$$

then we shall have

$$\cos_{i\pm}^{-1} (r + \sqrt{-1}\, s)$$

$$= 2i\pi \pm \left\{ \cos_{0+}^{-1} \frac{rp}{\sqrt{r^2}} + \sqrt{-1}\, l \left(\frac{\sqrt{r^2}}{p} - \frac{s}{\sqrt{1-p^2}} \right) \right\} \quad . \quad . \quad (7.)$$

With reference to this formula, it is observable that $\dfrac{\sqrt{r^2}}{p} + \dfrac{s}{\sqrt{1-p^2}}$ is the reciprocal of $\dfrac{\sqrt{r^2}}{p} - \dfrac{s}{\sqrt{1-p^2}}$, and that when $s = 0$, p is equal to 1, $\sqrt{r^2}$, or either, according as $\sqrt{r^2}$ exceeds, is less than, or is equal to, 1.

By showing that the commonly received equation $(a^x)^{x'} = a^{xx'}$ requires to be thus modified $(a^x)^{x'} = 1^{x'}\, a^{xx'}$, and by determining the corresponding individual values of the modified equation, he points out the defect of the reasoning of M. Clausen, of Altona, (noticed by Mr. Peacock, page 347 of his Report for 1833,) which seems to prove that a value of $e^{-4n^2\pi^2}$ is equal to 1. He takes occasion to enforce the important distinction between the algebra of formulæ that are left more or less indefinite and of individualized values. He remarks, for in-

stance, that though $f^{-1} a + f^{-1} a' = f^{-1} (a a')$, yet, as $f^{-1} a + f^{-1} a$, *in its indefinite form*, admits the addition of any one value to any other value of $f^{-1} a$, it has twice as many values as $2 f^{-1} a$: that $f^{-1} (a^2) = 2 i \pi + 2 f^{-1} a$, and that $f^{-1} 1 + f^{-1} 1$, or generally $f^{-1} 1 + f^{-1} a$, considered as an indefinite formula, is precisely equivalent to $f^{-1} 1$ or $f^{-1} a$ respectively.

$a^x = 1^x a^x_0$ or generally $a^x = 1^x a^x_i$, *i. e.* all the values of a^x are given by multiplying any single value in succession by all the values of 1^x. Now 1^x has an infinite number of values, unless x be a "rational fraction" (*positive or negative, including integers,*) in which case the number of values is equal to the denominator of the fraction in its lowest terms.

If a^x have among its values two quantities differing only in sign, x is a rational fraction, with, in its lowest terms, an even denominator. Let a be positive and x a rational fraction, which in its lowest terms $= \frac{m}{n}$, the number of real values of a^x will be one or two, according as n is odd or even. Let $a^x_i = y$, then x, if a be negative and y positive, must be a rational fraction, with, in its lowest terms, an even numerator and odd denominator; if a be positive and y negative, an odd numerator and even denominator; if a and y be both negative, an odd numerator and odd denominator. When x is of the form $r + \sqrt{-1}\, s$, a real, and r irrational, a^x can have only one real value. When a is real, r *rational*, and s not $= 0$, $a^{r + \sqrt{-1} s}$, if it have one real value, has an infinite number. When x is of the form $\sqrt{-1}\, s$ and a real, whenever one value of a^x is real, all the other values, of which there are an infinite number, are also real.

A quantity $(p + \sqrt{-1}\, q)$ may have no real logarithm, and can have no more than one in a given base $(r + \sqrt{-1}\, s)$, unless the "moduli" of the quantity $(= \sqrt{p^2 + q^2}$, adopting the phraseology of M. Cauchy,) and of the base are both $= 1$, in which case the number of real logarithms is infinite. When one real logarithm exists, and one only, it is $= \frac{l \sqrt{p^2 + q^2}}{l \sqrt{r^2 + s^2}}$. When an exponent is real and rational, and in such case only, it will reappear at intervals with different ranks in different orders, as a logarithm of the same quantity in a given base.

In conclusion, the author states, that, as all the values of 1^x were before known (at least when x was real) to be comprised in the formula $\cos (2 i x \pi) + \sqrt{-1} \sin (2 i x \pi)$, the principal

novelty of his theory consists, 1st, in always determining (and that, in a form capable of approximate numerical computation) *some single value* of a^x (*ex. gr.* a_0^x), which appeared not to have been accomplished, for all real and imaginary values of a and x; and, 2ndly, in showing that the complete formula for the logarithms of a given quantity in a given base involves TWO *arbitrary independent integers*, or that every quantity has an infinite number of orders of logarithms in a given base, and an infinite number of logarithms in each order. He suggests the application of his results to the theory of numbers, of equations, and of factorial functions.

P.S.—Mr. Graves has learned, since his paper was presented to the Association, that Professor Ohm, in a volume, published in 1829, of his highly valuable system of Algebra, gives some formulæ for exponential functions which agree with the principles promulgated, probably about the same time, in the First Part of the *Phil. Trans.* for 1829, but are confined to cases where the given quantities are *real*. This distinguished German analyst, however, was aware that expressions of a similar kind might be obtained, which, like those of the preceding Abstract, would include powers, where the root and the exponent, and logarithms, where the number and the base, were *imaginary*.

PHYSICS.

Notice of the Reduction of an anomalous Fact in Hydrodynamics, and of a new Law of the Resistance of Fluids to the Motion of Floating Bodies. By JOHN S. RUSSELL, *M.A.*

The author has been induced to contribute this paper to the Transactions of the Association, in consequence of a statement made last year by Mr. Challis in his excellent Report on Hydrodynamics, the first part of which is contained in the last volume of the Proceedings. The paragraph containing the statement referred to is the last of the Report, and is to be found in page 150, beginning "A singular fact," &c. The author also refers to another passage in the Report of Mr. Challis, consisting of the two first sentences of the paragraph immediately before the former quotation (p. 149).

From these statements, and many others that could readily be quoted, it appears that the theory of the resistance opposed by fluids to the motion of floating bodies remains in a very imperfect state; that the resistance is generally stated to

increase with the square of the velocity; that this law is subject to a *remarkable* exception at some point where the resistance suddenly ceases to increase in the former ratio, and appears to follow a new and unknown law. To this subject the author has been recently induced to pay considerable attention, and he has enjoyed some facilities of observing these phænomena on a large scale, as well as of making experiments on a more limited one, which have induced him to take a view of the subject considerably different from any with which he has had the good fortune to meet.

In regard to the point of velocity at which the phænomenon occurs, he states that it is in the transition from 8 to 9 miles an hour; and that after passing that point, the force required to propel the boat at the *higher* velocity is *less* than at the *lower*. It is also consonant with his observations and with exact measurement, that the vessel at this point rises out of the water, so that a vessel drawing 12 inches of water when at rest, rises 2 inches out of the water when brought up to a velocity of 9 miles.

Such is the fact; and it is equally a fact, as Mr. Challis remarks, that theory *never* predicted anything of the kind. It appears to the author that the reason why theory has hitherto been so ineffectually applied to this subject is that the theory of immersed bodies has been confounded with the theory of floating bodies. The immersed and the floating body are in circumstances totally different. He has therefore considered them apart from each other, and has arrived at the following conclusions, which are entirely different from the principles hitherto received, and which perfectly coincide with the facts noticed, and readily account for them.

The following are the results of the investigation.

1. That in all cases and at all velocities the displacement of water by a floating body is diminished by communicating horizontal rectilineal motion to it: that this effect is not confined to velocities of 8 and 9 miles an hour, but extends from the bottom of the scale of velocity to the top of it.

2. That this emersion is independent of the form of the body, and will take place equally with the worst and best form of vessel, the only difference being that the other elements of resistance will render more force necessary to communicate the required velocity in the former than in the latter case.

3. That for the velocity of one mile an hour, the section of immersion when compared with that section when at rest, considered as unity, will be diminished by ·0228, or $\frac{1}{43}$ nearly; at 5 miles an hour, the emersion becomes ·114 = $\frac{1}{10}$ nearly; and

by a further increase to 9 miles, ·205 = $\frac{1}{5}$ nearly. The immersion goes on diminishing at superior velocities in a continuous ratio, the emersion becoming at 20 miles ·456 = $\frac{1}{2}$; at 30, ·684 = $\frac{3}{5}$; at 40 miles an hour, only $\frac{1}{10}$ of the whole section will be immersed; and at 43·859 miles the elevating force will exactly balance the gravitation of the vessel, and she will rise entirely on the top of the water, descending to skim its surface and again rising above it at alternate intervals of equal duration.

4. The law may be generally expressed thus: *If any floating body be put in motion with a given velocity, the pressure which it exerts downwards upon the fluid in virtue of gravity is diminished by a quantity proportional to the weight of a column of the fluid having the height due to the velocity; and the ratio of the height of such a column to the velocity it represents will express the ratio of the dynamical section of immersion to the statical one, and the resistance will be that due to this diminished section.*

5. Although the author has not verified this law experimentally to higher velocities than 20 miles an hour, yet from its perfect coincidence with observation up to that point, he has sufficient confidence in its correctness to predict that it will hold rigorously in the higher velocities; and if either this theory or a more accurate one substituted for it should be found to hold, we may yet save our science from some imputation of sluggishness.

Let S = section of statical immersion.
v = velocity of motion.
g = measure of gravity.
s' = dynamical section of immersion.
$v\,s$ = displacement of statical section.
$\frac{v^2}{2g}$ = height due to velocity.

$$v\,s' = \mathrm{S}.\left(v - \frac{v^2}{2\,g}\right),\ s' = s\left\{1 - \frac{v}{2g}\right\}$$

When $s = 0$, $v = 2\,g = 64$ feet per sec.

Table showing the relation of the dynamical section of immersion due to a given velocity, the statical section being considered as unity.

Miles per hour.		Feet per second.		Amount of Emersion.
1	.....	1·46	.....	·0228125
2	.....	2·92	.....	·0456250
3	.....	4·38	.....	·0684375
4	.....	5·84	.....	·0912500

Miles per Hour.	Feet per Second.	Amount of Emersion.
5	7·30	·1140625
6	8·76	·1368750
7	10·22	·1596875
8	11·68	·1825000
9	13·14	·2053125
10	14·60	·2281250
11	16·06	·2509375
12	17·52	·2737500
13	18·98	·2965625
14	20·44	·3193750
15	21·91	·3421875
20	29·20	·4562500
25	36·50	·5703125
30	43·80	·6843750
35	51·10	·7984375
40	58·40	·9125000
41	59·86	·9353125
42	61·32	·9581250
43	62·78	·9809375
43·859	64	1·0000000

On the Collision of imperfectly Elastic Bodies. By Eaton Hodgkinson, *Salford, Manchester.*

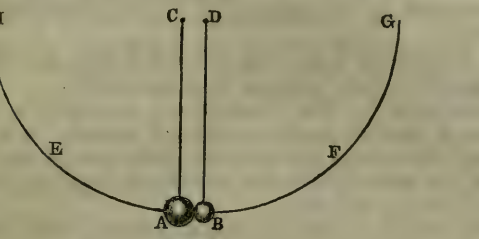
The theory of imperfect elasticity of which Newton gave the elements, from experiments alluded to in the *Principia* (scholium to the laws of motion), has not always been received with that cordiality which attaches to scientific deductions clearly proved; and among our neighbours the French, it is seldom used. This circumstance, together with the remarks in a work of great value by a member of the University of Cambridge*, and a suggestion of its distinguished author, made me desirous to repeat the experiments of Newton; and to seek for data necessary to supply, amongst other things, the laws that regulate the elasticities after collision in bodies of the same, and of different natures.

In this research I have been, as on former occasions, gratuitously supplied with every requisite, so far as I found it necessary, by Mr. Fairbairn, engineer, of Manchester.

To obtain the results, the mode I usually adopted was nearly that used by Sir Isaac Newton himself, in which two balls, A, B, were suspended from points C, D, with equal radii, so as just to be in contact when hanging vertically; and the curves AEH, BFG, were circular arcs round the centres C, D, inscribed on a wall contiguous. The arcs were divided accord-

* Mr. Whewell, in his *Mechanics*.

ing to their chords, each way, starting from the lowest point, the point of junction of the balls when still; since the velocities acquired by bodies falling down those arcs are as their



chords. In the experiments with the larger balls, two persons usually supported the balls at any points, G, H, of equal height, as directed, and let them fall at the instant that a sharp blow was given on the wall for a signal, the author and another person observing the points E, F, to which the balls returned after having impinged at the lowest point. The chords of the arc, fallen through and returned, were, as mentioned above, considered as the measures of the velocities of impact and recoil. In some of the experiments one of the balls was at rest at the lowest point, before impact. The resistance of the air, in the lighter bodies especially, was generally allowed for.

In the *tabulated results* of experiments accompanying this, each number set down for the elasticity is the greatest from about ten impacts; and in the smaller balls, especially in the greater arcs, it is often from as many as twenty, on account of the difficulty of obtaining, with large arcs, direct and central impacts.

Conclusions from the Experiments referring to the "Tabulated Results" for proofs and illustrations.

Conclusion 1. All rigid bodies are possessed of some degree of elasticity; and among bodies of the same nature, the hardest are generally the most elastic.

This conclusion obtains a good illustration from metals. Thus, the soft metal lead has an elasticity of ·20, as exhibited by its mean ratio; brass, which is harder than lead, has its elasticity ·36; bell-metal, which is harder than brass, has ·59; cast iron, still harder, has ·66; and steel, the hardest metal of all, has ·79 for its elasticity. (Expts. 13, 10, 12, 1 to 3, 31.) The same conclusions might be drawn from the elasticities of

other bodies increasing in hardness: thus, malleable clay, stone, hard-baked clay, glass, give elasticities of about ·17, ·79, ·89, ·94. (Expts. 18, 27, 29, 24.)

Conclusion 2. There are no perfectly hard inelastic bodies, as assumed by the earlier, and some modern, writers on mechanics.

If Conclusion 1. be true, this will follow as a consequence, the proofs of both being of the same nature.

Conclusion 3. The elasticity, as measured by the velocity of recoil divided by the velocity of impact, is a ratio which (though decreasing as the velocity increases,) is nearly constant, when the same rigid bodies are struck together with considerably different velocities.

The proofs of this are very numerous; they may be taken (with some anomalies,) from almost every experiment. In experiments 1 and 2, cast-iron balls striking together with velocities as 4, 6, 8, 10, 12, gave, in the one case, elasticities ·69, ·66, ·66, ·61, ·59; and in the other case ·70, ·69, ·66, ·64, ·62. In experiments 10 and 11, balls of soft brass struck together with velocities varying from 4 to 20, gave for their elasticities ·38, ·37, ·36, ·30, ·33; and even lead, which permanently changes its figure at every impact, preserves considerable approximation to equality in its elasticities, as may be seen from Experiments 13 and 14. The same may be said of other bodies besides metals, as will be evident by inspection of the tables of results; the irregularity and decrease of elasticity being greater in those bodies that least recover their forms after impact. It is probable, too, that the decrease of elasticity, in some bodies, from the larger impacts, is somewhat less than as indicated in the table, on account of the great difficulty then of obtaining perfectly central impacts.

Conclusion 4. The elasticity, as defined in Conclusion 3, is the same whether the impinging bodies be great or small.

This fact is proved by Experiments 1, 2, and 20, in which the elasticities of cast iron are ·64, ·66, and ·73; differing in the first and second experiments only $\frac{1}{35}$, though the weights of the equal balls in experiment 1 are more than five times the weight of those in experiment 2. In the 1st and 20th experiment, the difference of elasticity is but $\frac{1}{8}$th, though the balls vary in weight as 74 to 1.

Conclusion 5. The elasticity is the same, whatever be the relative weights of the impinging bodies.

This will be shown by comparing the results of experiments 5 and 58, in which the same stone ball was struck against two balls of cast iron, one 33 times as heavy as the other; the elas-

ticity in the two cases being ·71 and ·76, or nearly equal. In experiments 6 and 60, balls of brass, varying in weight as 30 to 1, were struck against the same stone ball, and their elasticities varied only from ·62 to ·68.

Various other proofs, both of this and the preceding "Conclusion," may be obtained from the tabulated results; and therefore the elasticities given in the tables will apply, whatever be the relative or absolute weights of the impinging bodies.

Conclusion 6. In impacts between bodies differing very much in hardness, the elasticity with which they separate is nearly that of the softer body.

This may be shown by many examples: thus, lead, the elasticity of which is ·20 (Exp. 13.), is much harder than cork, whose elasticity is ·65 (Exp. 25.); but the elasticity of lead struck against cork is ·57, differing only $\frac{1}{8}$ from that of cork (Exp. 44.). The elasticities of steel, cast iron, stone, and glass, are ·67, ·73, ·79, ·94, (Expts. 30, 20, 27, 24); and these bodies are very hard, compared with lead, whose elasticity is ·20; but if they be successively struck against lead, the resulting elasticities will be ·19, ·17, ·28, ·25 (Expts. 50, 49, 56, 32); differing not widely from that of lead. There is frequently, however, a considerable loss of elasticity in impacts between bodies differing much in hardness, arising from the softer body being crushed with the blow, in the manner that a soft body would be by a hammer.

Conclusion 7. In impacts between bodies whose hardness differs in any degree, the resulting elasticity is made up of the elasticities of both; each body contributing a part of its own elasticity in proportion to its relative softness or compressibility.

From Conclusion 6 we see that if any body, as lead, be struck successively by two other bodies, as cork and steel, one very soft and the other very hard compared with itself, the lead in the first case will contribute scarcely any of its elasticity, the cork giving nearly the whole of its: and in the second case the lead would contribute nearly all its elasticity, and the steel scarcely any. (Expts. 13, 25, 30, 43, 50.) Hence we may conclude that if the lead had been struck against another body of equal softness or compressibility with itself, the lead would have contributed half of its own elasticity, and the other body half of its own, to form the resulting elasticity.

This reasoning seems to be borne out by experiment, as will be seen further on. Admitting it therefore to be generally correct, we see that in the two extreme cases of collisions, between bodies of *equal* hardness and of *very different* hardness,

each body contributes a portion of its own proper elasticity in proportion to its relative compressibility. Hence in collisions between bodies whose hardness differs in any other degree, it seems natural to conclude that the same law is preserved.

To exhibit this in a form capable of submitting it to the test of experiment: Let a and b represent the relative hardness of two bodies, a' and b' their respective elasticities, to find the elasticity resulting from their collision.

Since in bodies considered as springs the compression of each is inversely as the hardness, or resistance to compression, calling $\frac{1}{a}$ = the compression of the first body, we have

$\frac{1}{b}$ = the compression of the second.

Whence $\frac{1}{a} + \frac{1}{b}$ = the compression from the two.

$$\therefore \frac{\frac{1}{a}}{\frac{1}{a} + \frac{1}{b}} = \frac{b}{a+b} = \text{the compression from the first}$$

body in terms of the whole compression.

$$\frac{\frac{1}{b}}{\frac{1}{a} + \frac{1}{b}} = \frac{a}{a+b} = \text{the compression from the se-}$$

cond, in terms of the whole.

But by the 7th Conclusion,

$\frac{b}{a+b} \times a'$ = the elasticity contributed by the first body;

$\frac{a}{a+b} \times b'$ = the elasticity from the second.

Whence their sum $\frac{a\,b' + b\,a'}{a+b}$ = the required elasticity from both.

The modulus of elasticity seems to afford the best means of judging of the relative powers of bodies to resist incipient compression. I have therefore selected that datum from Tredgold's *Essay on Cast Iron*, in the few cases that answered my purpose,

and supplied it in some others from my own experiments; reckoning the modulus in lbs., and for a base of an inch square.

Cast iron	18,400,000 lbs.	Tredgold.
White marble	2,520,000	
Elm	1,340,000	
Lead, cast	720,000	
Best double shear steel*, not hardened	31,165,000	
Bell metal†, same as in our experiments	11,380,000	
Soft brass‡, same as in our experiments	10,440,000	
Glass, from window-glass	8,580,000	
Ivory	1,630,000	
Cork	2,369	

Suppose it were required to find the elasticity of glass struck against brass.

The modulus of a glass being 8,580,000, and that of brass 10,440,000, their relative hardness is as 86 to 105 nearly; and the elasticity of glass is ·94 and of brass ·41 (Expts. *24*, *22*.): hence in the formula, for the elasticity above, we have $a = 86$, $a' = \cdot 94$, $b = 105$, $b' = \cdot 41$,

$$\therefore \frac{a\,b' + b\,a'}{a + b} = \frac{86 \times \cdot 41 + 105 \times \cdot 94}{86 + 105} = \cdot 70,$$ the elasticity required (being $\frac{1}{10}$th less than that given by Exp. 34.).

In impacts between other bodies we have as follows:

Names of Bodies, with their Elasticities.	Computed Elasticities.	Real Elasticities.	Errors.
Glass (·94) against lead (·20)	·257	·25	$\frac{1}{36}$
Glass (·94) against bell-metal (·67)	·82	·87	$-\frac{1}{17}$
Ivory (·81) against cork (·65)	·64	·60	$\frac{1}{15}$

* A bar of best double shear steel, not hardened, ·99 inch deep, ·96 inch broad, and 6 feet 8 inches long, weighing 22½ lbs. was laid on props 6 feet asunder, and 196 lbs. suspended from the middle bent it ·63 inch without injuring its elasticity: other weights, as 252, 308, 364, 420, bent it ·81, 1·00, 1·17, 1·35. The experiment was made with great care, and a long wedge of wood was employed to measure the deflections.

† A bar 2 feet between the supports, ·51 inch deep and 1·03 inch broad, bent ·27 inch with 121½ lbs. without injuring its elasticity: 304 lbs. bent it ·62, and 318 broke it.

‡ A bar 2 feet between the supports, ·52 inch deep, and 1·04 inch broad, bent ·15 inch by 66¼ lbs. without injuring the elasticity: other weights, as 94¼, 150¼, 171¼, 219¼, bent it ·22, ·48, ·70, 2·87, showing its great softness and flexibility. Its modulus calculated for double the weight necessary to destroy its elasticity was only 5,270,000 lbs., half that given above.

The modulus for glass was obtained from the mean between three experiments made by bending slips of window-glass, giving 9,600,000 lbs., 8,505,000 lbs., and 7,634,000 lbs.

The modulus for ivory was obtained by bending two slips of ivory; and for cork by compressing a rectangular piece 16 inches long and 2·05 inches section; the decrement with 127½ lbs. being ·42 inch.

Names of Bodies, with their Elasticities.	Computed Elasticities.	Real Elasticities.	Errors.
Ivory (·81) against lead (·20)	·39	·44	$-\frac{1}{9}$
Ivory (·81) against brass (·41).....	·76	·78	$-\frac{1}{39}$
Ivory (·81) against bell-metal (·67)...	·79	·77	$\frac{1}{38}$
Brass (·41) against bell-metal (·67)...	·53	·55	$-\frac{1}{27}$
Brass (·41) against cast iron (·73) ...	·52	·50	$\frac{1}{25}$
Brass (·41) against steel (·67).....	·47	·47	—
Brass (·41) against limestone* (·79) .	·71	·73	$-\frac{1}{37}$
Lead (·20) against limestone (·79) ...	·32	·28	$\frac{1}{7}$
Lead (·20) against elm † (·60).....	·37	·41	$-\frac{1}{10}$
Brass (·41) against elm (·60)	·58	·52	$\frac{1}{9}$

Other instances might be adduced, but the above may be sufficient to show the consistency of the formula, and of the 7th Conclusion, from which it is deduced.

* I have supposed the modulus of limestone to be 2,520,000 lbs., the same which Mr. Tredgold found for white marble. The balls we used were somewhat softer than it; but Mr. Tredgold's results being obtained from the flexure at the time of fracture, must be too low, as he himself has observed.

† I have assumed the modulus of elm, struck across the fibres, to be 1,000,000 lbs.; its value in the direction of the fibres being 1,340,000 lbs., as before given.

Tabulated Results of Experiments on the Elasticities of Bodies subjected to various degrees of impulsive force.

Experiment.	Description of Balls.	Weight of each Ball in lbs. avoirdupois.	Approximate Ratio of weights of Balls.	Velocities of impact, as measured by chords of arcs passed through before the blow.	Elasticities, as measured by the ratios of the velocities of recoil to those of impact (unity representing perfect elasticity.)	Mean Value of Elasticity.	Remarks.
				(Radius 10 feet.)			
1.	Two cast iron balls.....	20 lbs. each.	1 to 1.	4, 6, 8, 10, 12 feet.	·69 ·66 ·66 ·61 ·59	·64 } mean ·66	
2.	Ditto	3 lbs. 13 oz. each.	1 to 1.	4, 6, 8, 10, 12	·70 ·69 ·66 ·64 ·62	·66 }	
3.	Ditto	20 lbs. and 3 lbs. 13 oz.	5·24 to 1.	2, 4	·69 ·68	·68 }	
4.	Cast iron ball, Leaden ball.....	20 lbs. and 8 lbs. 14 oz.	2·25 to 1.	3, 9	·15 ·11	·13	*Experiment* 4. In this case and some others, where one body is much harder than the other, the softer body is crushed, and its elasticity decreased.
5.	Cast iron ball, Boulder-stone ball	8 lbs. 14 oz. and 1 lb. 12 oz.	5 to 1.	3, 6, 9	·74 ·68 ·69	·71	
6.	Boulder-stone ball, Soft brass ball	1 lb. 12 oz. and 8 lbs. 14 oz.	1 to 5.	3, 6	·63 ·61	·62	
7.	Boulder-stone ball, Leaden ball	1 lb. 12 oz. and 8 lbs. 14 oz.	1 to 5.	3, 6, 9	·21 ·16 ·13	·17	
8.	Boulder-stone ball, Elm ball (struck across the fibres)	1 lb. 12 oz. and 4 lbs. 11 oz.	1 to 2·5.	3, 6, 9	·60 ·56 ·51	·56	
9.	Two balls of elm (struck across the fibres)	4 lbs. 11 oz. each.	1 to 1.	4, 8, 12	·64 ·61 ·55	·60	
10.	Two balls of soft brass (16 parts copper, and 1 part tin)	8 lbs. 14 oz. each.	1 to 1.	8, 16, 20	·36 ·30 ·33	·33 } mean ·36	*Expts.* 10 *to* 12. The brass balls were much bruised by the impacts. The bell-metal balls were bruised too, but in a less degree.
11.	Ditto ditto	4 lbs. 7 oz. each.	1 to 1.	4, 8	·38 ·37	·38 }	
12.	Two balls of bell-metal (16 parts copper, & 4 parts tin	8 lbs. 14 oz. each.	1 to 1.	6, 12, 18	·63 ·59 ·53	·59	
13.	Two balls of lead.....	5 lbs. 1 oz. and 4 lbs. 12 oz.	1 to 1.	8, 12, 16, 20	·21 ·17 ·17 ·18	·18 } mean ·20	*Expts.* 13. *and* 14. In lead, a permanent change of figure usually takes place at every impact.
14.	Ditto ditto	8 lbs. 14 oz. and 4 lbs. 12 oz.	2 to 1.	2, 4, 6	·22 ·22 ·20	·21 }	
15.	Leaden ball, Elm ball.....	5 lbs. 1 oz. and 4 lbs. 11 oz.	1 to 1.	8, 16, 24	·43 ·42 ·37	} ·41	
16.	Ditto ditto	Experiment varied and	repeated.	4, 8, 12	·46 ·37 ·42		
17.	Elm ball, Soft brass ball.....	4 lbs. 11 oz. and 4 lbs. 7 oz.	1 to 1.	4, 8, 12	·57 ·52 ·47	·52	*Experiment* 18. The results from clay were very irregular; those set down are the means from two experiments.
18.	Two balls of clay, just malleable with the hand	8 lbs. 14 oz. each.	1 to 1.	4, 8, 10, 16	·23 ·15 ·15 ·13	·17	
19.	Clay ball, Soft brass ball.....	8 lbs. 14 oz. each.	1 to 1.	3, 4, 6, 8, 10	·21 ·17 ·17 ·13 ·13	·16	

Experiment.	Description of Balls.	Weight of each Ball, in lbs. avoirdupois.	Approximate ratio of weights of Balls.	Velocities of impact, as measured by chords of arcs passed through before the blow.	Elasticities, as measured by the ratios of the velocities of recoil to those of impact (unity representing perfect elasticity).	Mean Value of Elasticity.	Remarks.
				(Radius 3 feet.)			
20.	Two balls of cast iron	4 oz. 5 drs. each.	1 to 1.	1·5, 2, 3, 4	·75 ·73 ·73 ·69	·73	
21.	Two balls of bell-metal (same mixture as in Exp. 12)	9 oz. 7 drs. each.	1 to 1.	2, 4, 6	·76 ·65 ·60	·67	
22.	Two balls of soft brass (same as in Exp. 10.)	9 oz. 7 drs. each.	1 to 1.	2, 4, 6	·43 ·42 ·38	·41	
23.	Two ditto (same mixture)	4 lbs. 7 oz. & 4 oz. 12 drs.	15 to 1.	1, 2, 3	·45 ·45 ·43	·44	
24.	Two balls of glass	3 oz. 11 drs. and 3 oz. 8 drs.	1 to 1.	1·33	·94	·94	
25.	Two balls of cork	5 oz. 2 drs. each.	1 to 1.	1, 3, 4	·71 ·67 ·61	·65	
26.	Ditto			6 (3 each way)	·62		
27.	Two limestone balls	1 oz. 9 drs. each.	1 to 1.	2, 3	·80 ·78	·79	
28.	Two ivory balls	1 oz. 13 drs. each.	1 to 1.	2, 4	·84 ·77	·81	
29.	Two balls of hard-baked clay	1 oz. 10½ drs. & 1 oz. 12½ drs.	1 to 1.	2, 4	·89 ·88	·89	
30.	Two balls of best double shear steel, (not hardened)	18 oz. 14 drs. each.	1 to 1.	2, 4, 6	·73 ·66 ·61	·67	
31.	The same balls, hardened		1 to 1.	2, 4, 6	·80 ·78 ·73	·79	*Experiment* 31. The mean is taken from the two first experiments. The balls were a little cracked with the hardening; but that has not, I think, affected any result, except, perhaps, the last.
32.	Leaden ball, Glass ball	4 lbs. 12 oz. and 3 oz. 8 drs.	21·7 to 1.	1, 2, 3	·33 ·25 ·21	·26 } mean ·25	
33.	Leaden ball, Glass ball	5 oz. 4 drs. and 3 oz. 8 drs.	1·5 to 1.	2	·23	·23	
34.	Soft brass ball, Glass ball	9 oz. 7 drs. and 3 oz. 8 drs.	2·7 to 1.	1, 2	·80 ·75	·78	
35.	Bell-metal ball, Glass ball	9 oz. 7 drs. and 3 oz. 8 drs.	2·7 to 1.	1, 2	·89 ·85	·87	
36.	Cast iron ball, Glass ball	5 oz. 4 drs. and 3 oz. 8 drs.	1·5 to 1.	1	·91	·91	
37.	Leaden ball, Ivory ball	5 oz. 4 drs. and 1 oz. 13 drs.	2·9 to 1.	1, 2	·46 ·38	·42 } mean ·44	
38.	Leaden ball, Ivory ball	4 lbs. 12 oz. & 1 oz. 13 drs.	42 to 1.	1, 2, 3	·50 ·42 ·42	·45	
39.	Soft brass ball, Ivory ball	9 oz. 7 drs. and 1 oz. 13 drs.	5·2 to 1.	1, 2, 3	·79 ·78 ·77	·78	
40.	Bell-metal ball, Ivory ball	9 oz. 7 drs. and 1 oz. 13 drs.	5·2 to 1.	1, 2	·78 ·76	·77	
41.	Cork ball, Ivory ball	5 oz. 4 drs. and 1 oz. 13 drs.	2·9 to 1.	1, 2, 3	·64 ·59 ·57	·60	

42.	Cast iron ball, Cork ball.....	5 oz. 4 drs. each.	1 to 1.	1, 2	·58	·58		·58	*Expts.* 42, 43, 44. It is worthy of remark, that the elasticities of cast iron, steel, and lead, struck against cork, are all the same quantity, and nearly the same as that of cork, but rather below it.
43.	Shear steel ball (not hardened), Cork ball.....	9 oz. 7 drs. and 5 oz. 4 drs.	1·8 to 1.	1, 2	·58	·58		·58	
44.	Leaden ball, Cork ball	5 oz. 4 drs. each.	1 to 1.	1, 2	·58	·55		·57	
45.	Bell-metal ball, Soft brass ball..	4lbs. 7 oz. and 4 oz. 12 drs.	15 to 1.	1, 2, 3	·63	·59	·56	·59 } mean ·55	
46.	Bell-metal ball, Soft brass ball..	9 oz. 7 drs. each.	1 to 1.	2, 4, 6	·60	·51	·43	·51 }	
47.	Cast iron ball, Soft brass ball...	4 oz. 5 drs. each, nearly.	1 to 1.	2, 4	·56	·45		·50	
48.	Cast iron ball, Bell-metal ball...	4 oz. 5 drs. each, nearly.	1 to 1.	2, 4	·75	·69		·72	
49.	Cast iron ball, Leaden ball.....	5 oz. 2 drs. each.	1 to 1.	2, 4	·18	·16		·17	
50.	Shear steel (not hardened), Leaden ball.....	9 oz. 7 drs. and 5 oz. 4 drs.	1·8 to 1.		·19			·19	
51.	Shear steel (ditto), Soft brass ball	18 oz. 14 drs. and 9 oz. 7 drs.	2 to 1.	2, 3, 4	·51	·50	·40	·47	
52.	Soft brass ball, Limestone ball	9 oz. 7 drs. and 2 oz. 3 drs.	4·3 to 1.	1, 2, 3	·79	·77	·78	·78 } mean ·73	
53.	Ditto ditto	4oz. 12 drs. and 2 oz. 3 drs.	2·2 to 1.	2, 3, 4	·71	·68	·65	·68 }	
54.	Cast iron ball, Limestone ball .	5 oz. 2 drs. and 2 oz. 3 drs.	2·3 to 1.	1, 2, 3	·77	·76	·74	·76	
55.	Bell-metal ball, Limestone ball	4oz. 14 drs. and 2 oz. 3 drs.	2·2 to 1.	1, 2, 3	·79	·79	·75	·78	
56.	Leaden ball, Limestone ball ...	4 lbs. 12 oz. and 2 oz. 3 drs	35 to 1.	1, 2, 3	·33	·27	·25	·28	
57.	Boulder-stone ball, (very hard), Limestone ball ...	1 lb. 12 oz. and 2 oz. 3 drs.	12·8 to 1.	1, 2, 4	·75	·72	·72	·73	
58.	Boulder-stone ball, Cast iron ball	1 lb. 12 oz. and 4 oz. 5 drs.	6·4 to 1.	1, 2, 3	·77	·76	·76	·76	
59.	Boulder-stone ball, Bell-metal ball	1lb. 12 oz. and 4 oz. 14 drs.	5·7 to 1.	1, 2, 3	·79	·77	·76	·77	
60.	Boulder-stone ball, Soft brass ball	1 lb. 12 oz. and 4 oz. 12 drs.	5·9 to 1.	1, 2, 3	·69	·67	·68	·68	

Theoretical Explanations of some Facts relating to the Composition of the Colours of the Spectrum. By the Rev. JAMES CHALLIS.

A ray of homogeneous light, in the undulatory hypothesis, consists of the isochronous undulations of an elastic medium; and the velocity at a given instant, of the undulating particles situated on a straight line drawn in the direction in which the light travels, is expressed by the function $m \sin\left(\frac{\pi x}{\lambda} + c\right)$, x being the distance of any point on the line from a fixed point. The condition of homogeneity is, that m and λ be constant: the colour depends on λ.

If two rays be combined, for which m is the same and λ different, the compound ray, by the principle of the coexistence of small vibrations, is expressed by

$$m \sin\left(\frac{\pi x}{\lambda} + c\right) + m \sin\left(\frac{\pi x}{\lambda'} + c'\right);$$

or if $\frac{1}{L} = \frac{1}{2}\left(\frac{1}{\lambda} + \frac{1}{\lambda'}\right)$ and $\frac{1}{2} = \frac{1}{2}\left(\frac{1}{\lambda} - \frac{1}{\lambda'}\right)$, by

$$2m \cos\left(\frac{\pi x}{l} + c_1\right) \sin\left(\frac{\pi x}{L} + c_2\right).$$

In the spectrum the greatest and least values of λ are to each other nearly in the ratio of 3 to 2, so that $\frac{1}{2}\left(\frac{1}{\lambda} - \frac{1}{\lambda'}\right)$ is at least equal to $\frac{1}{6\lambda}$, and l at least equal to 6λ. Hence in the periodic function $\cos\left(\frac{\pi x}{l} + c_1\right)$ the periods recur much less frequently than those of the other factor, $\sin\left(\frac{\pi x}{L} + c_2\right)$, in which L is an harmonic mean between λ and λ'. It does not appear that the eye can appreciate periods of slower recurrence than those corresponding to the rays of the spectrum. We may, therefore, conclude that the periodicity of $\cos\left(\frac{\pi x}{l} + c_1\right)$ would not be perceived, and that the eye would be sensible only of that of the other factor. The compound ray will therefore be of an intermediate colour.

Newton asserts, in a letter to Oldenburg, that "if any two colours be mixed, which in the series of those generated by the prism are not too far distant from one another, they, by their mutual allay, compound that colour which in the said series appeareth in the mid-way between them*."

Dr. Young states that "perfect sensations of yellow and of blue are produced respectively by mixtures of red and green, and of green and violet light†."

According to the theory here proposed, the compound colour is independent of the origins of the component rays, for c and c' may be any arbitrary quantities; and this agrees with experience.

It follows, too, from this theory that the red and violet cannot be produced by mixing two colours of the spectrum, but every other prismatic colour may. Dr. Young takes *red*, green, and *violet* as fundamental colours in his theory of composition. In Mayer's theory, *red*, yellow, and blue are the fundamental colours, and violet is supposed to be a compound only because it produces, without being mixed with any other colour, a sensible impression of redness. See Herschel's *Treatise on Light*, Art. 515.

The difference between two rays expressed respectively by the functions $m \sin\left(\frac{\pi x}{L} + c\right)$ and $2\ m \cos\left(\frac{\pi x}{l} + c_1\right) \sin\left(\frac{\pi x}{L} + c\right)$, is exhibited in figures 1 and 2. Since it is known by experience that the eye is not sensible to a momentary interruption of a ray (as exhibited in fig. 3.), there seems to be no reason to expect that it would perceive any difference between the rays of figures 1 and 2. Nothing in light corresponds to *discords* in sound.

Fig. 1. 

Fig. 2.

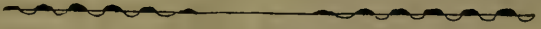
Fig. 3. 

Fig. 5.

Sir John Herschel is of opinion that the fact of the complete imitation of the prismatic green by a mixture of adjacent colours

* Horsley's Edition of *Newton's Works*, vol. iv. p. 303.

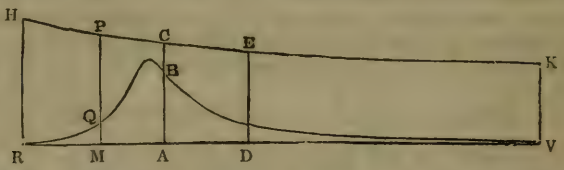
† *Lectures on Natural Philosophy*, vol. i. p. 439.

favours the idea of the possibility of an analysis of white light distinct from that afforded by the prism. (*Treatise on Light*, Art. 516.) The tendency of the preceding theory is to show, that the possibility of decomposing one ray is no ground for thinking that another exactly the same in appearance is also decomposable.

If m were not the same in the two component rays, the compound ray would not be so like a homogeneous ray, since the intervals between the points of no velocity would not all be equal. In mixing two simple rays there must consequently be an *adjustment* of the quantity of light in each to bring out the purest compound.

Composition of all the Colours of the Spectrum.—Let RQBV (fig. 4.) be the curve line (as determined by the experiments of Fraunhofer,) whose ordinates represent the intensity of light from R, the red end, to V, the violet end of the spectrum.

Fig. 4.



Draw AB, dividing the area into two equal parts, RAB, VAB. Divide each of the parts into the same number of small equal portions (m). Let RH and VK be taken in the proportion of the values of λ for the extreme red and violet rays, and let the curve line HPK be such that an ordinate PM is as the value of λ corresponding to the intensity QM. Also let DE be an harmonic mean between RH and VK. Then, compounding the small portions two and two, taking one in each of the areas RAB, VAB, beginning with the extreme portions at R and V, and proceeding with the others in succession to B, there will be as many compound rays as there are portions, and each will be expressed by such a function as

$$2m \cos\left(\frac{\pi x}{l} + c_1\right) \sin\left(\frac{\pi x}{\mathrm{L}} + c_2\right),$$

in which l is greater in proportion as the combined portions are nearer each other, and L is always intermediate to the values of λ corresponding to AC and DE. The sum of all these functions is the function expressing the result of compounding the whole spectrum. Nothing can be anticipated respecting the nature of the final expression, except that it in-

dicates that the points of no velocity are at irregular intervals from each other, as exhibited in fig. 5. This would appear by combining two or three of the functions by mechanical construction. It seems probable, then, that this condition is necessary to produce *white* light, and that the whiteness is more perfect in proportion as the intervals are more irregular. The colours which are neither white nor those of the spectrum, may be conceived to correspond to undulations in which there is an approach to regularity by the preponderance of two or more sets of equal intervals.

Newton asserts that the sun's light is not perfectly white, but has a tincture of yellow. If there be a preponderance of any colour, the preceding theory would lead us to expect it would be of that which corresponds to the ordinate A C, which, as may be judged from Fraunhofer's curve, is situated in the yellow part of the spectrum. (See the figures to Articles 419 and 496 of Herschel's *Treatise*.) For the greater number of the component portions have l very large, and L very nearly equal to the value of λ, corresponding to the ordinate A C*.

If a part of the spectrum towards the violet end be intercepted, and the rest compounded as before, A C will be shifted a little towards the red end, but D E considerably more so. Thus D E and A C will be brought nearer each other, and the compound, if yellow before, will now be more decidedly yellow. By stopping a still greater portion, these ordinates will approximate still more, till they coincide, and at length D E passes to the other side of A C. In the mean while the resulting colour will pass through orange till it becomes red.

If the spectrum he progressively stopped, beginning at the other end, the resulting colours will be approximations to those that lie towards the violet end. The ordinates A C, D E will never in this case coincide, since the greater portion of the light of the spectrum lies towards the red end.

If the middle part of the spectrum were stopped, the colour which results by compounding the remainder may not be any in the spectrum, though the two parts of which it is composed, taken separately, give nearly spectrum colours; for by the union of these two parts, the intervals between the points of no velocity become more irregular than in either of them, the effect of combination being in general to increase the irregularity.

All this agrees very well with what is said in Art. 409 of Herschel's *Treatise*. "If the violet light be intercepted, the

* The residuum colour would be different for a different form of the curve. May not the colours of the fixed stars be owing to a difference of this kind?

white will acquire a tinge of yellow; if the blue and green be successively stopped, this yellow will grow more and more ruddy, and pass through orange to scarlet and blood red. If, on the other hand, the red end of the spectrum be stopped, and more and more of the less refrangible portion thus successively abstracted from the beam, the white will pass first into pale, and then to vivid green, blue-green, blue, and finally into violet. If the middle portion of the spectrum be intercepted, the remaining rays, concentrated, produce various shades of purple, crimson, or plum-colour."

The subject of this paper admits of more lengthened and accurate treatment than is given to it here. The object of this communication is merely to call attention to a circumstance which appears to have been overlooked in the undulatory theory of light, viz. *an analogy existing between the composition of colours and the composition of small vibrations.*

On the Achromatism of the Eye; in continuation of a Paper in the last Volume of the British Association Reports. By the Rev. BADEN POWELL, *M.A., F.R.S., Sav. Prof. of Geometry, Oxford.*

In the paper referred to the author inadvertently introduced a formula which he did not observe was incorrect till the sheet had been printed. The correct expression will be found by taking the general formula for the principal focal length (F) after refraction through two surfaces, at which the relative indices (taking the sines in the order of transmission) are $\mu\ \mu$, and the radii $r\ r$ (remaining to be affected by their proper signs), which is (see the author's *Optics*, p. 23,)

$$\frac{1}{F} = \frac{1}{r_{\prime}} \frac{\mu_{\prime} - 1}{\mu_{\prime}} + \frac{1}{r} \frac{\mu - 1}{\mu_{\prime}\,\mu}.$$

Adapting this to a double convex lens (when r becomes negative), and equating similar expressions for the red and violet rays, the condition of achromatism will be found to be

$$\frac{\mu_{\prime r}}{\mu_{\prime v}} = \frac{[(r + r_{\prime})\,\mu_r - r_{\prime}]\,\mu_v}{[(r + r_{\prime})\,\mu_v - r_{\prime}]\,\mu_r}.$$

When this is fulfilled, achromatism may be produced by the nature of the medium in which the focus is formed. This practically differs little from what was given in the former paper.

On the Theory of the Dispersion of Light by the Hypothesis of Undulations. By the Rev. BADEN POWELL, *M.A., F.R.S., Sav. Prof. of Geometry, Oxford.*

The object of this communication was principally to give a brief view of the nature of the explanation afforded by M. Cauchy's analysis of the dispersion, which has hitherto presented so formidable a difficulty, *whether to the undulatory or to any other theory*, and of some important suggestions which have been made with respect to it. An attentive examination of the quantities entering the analysis points out a limitation, or condition, which must be annexed to M. Cauchy's conclusion. It is the object of his analysis to show that there exists generally *a relation* between the *length* of a wave and the *time* of its propagation. It appears from the nature of the expressions employed, that, in order that this should hold good universally, we must add to his original hypothesis as to the constitution of the ætherial medium this condition, that "*the distance between two molecules must not be very small compared with the length of an undulation.*

On the Repulsion excited between Surfaces at minute Distances by the Action of Heat. By the Rev. BADEN POWELL, *M.A., F.R.S., Sav. Prof. of Geometry, Oxford.*

That bodies at very small but finite distances repel each other when heated, seems probable from the analogy of expansion by heat;—was supposed to be proved from some very delicate, but perhaps doubtful, experiments of Libri, Sargey, and Fresnel;—and has been assumed by Professor Forbes as affording an explanation of the vibrations of heated metallic bars, first observed by Mr. Trevelyan.

A simple mode of trying the experiment occurred to the author of this communication. Two lenses of very small curvature are laid upon one another, without pressure, and form the coloured rings. These afford an accurate test of the interval between the glasses. Heat being applied, the rings always *contract*, and the colours always *descend* on the scale; or the glasses are *separated*, and consequently repel each other.

The curvature, or warping, of the glass, owing to the heat, will, upon consideration, be found to be such as ought, in the first instance, to diminish the angle of contact, and consequently to make the rings *enlarge*.

It should also be observed that the curvature must be suffi-

ciently small to produce rings of considerable diameter, otherwise the surfaces in contact will not be sufficiently large to allow sufficient effect to overcome the weight or inertia of the glasses.

The author has made various other researches on the subject, which have been recently communicated to the Royal Society, and which will appear in the forthcoming volume of its *Transactions*. In particular he has attempted to compare the times of communication of heat through two glasses in several degrees of contact (as estimated by the tints), and has found it rather more rapid with the higher degrees of contact. But when the central black is produced, it requires a considerable heat to overcome the powerful attraction which subsists at that minute distance. Some singular illustrations of the intensity of this force have been observed. It seems not improbable that at this distance there may be a *limit* where attraction becomes predominant. The contact between glass and liquids is probably within this limit, since no heat seems capable of overcoming the attraction. Again, the repulsive power seems capable of being excited by heat only within a certain limit the other way, and between surfaces regularly opposed to each other. An iron at a bright red heat could not be made to repel a delicately suspended gilt card disc, though brought to about one tenth of an inch distance.

Suggestions respecting Sir John Herschel's Remarks on the Theory of the Absorption of Light by coloured Media. By the Rev. WILLIAM WHEWELL, *F.R.S. F.G.S.*

At the meeting of the British Association last year, Sir John Herschel made some remarks*, the object of which was, I conceive, to show, that though it may not be easy to determine at present in what way the dark lines of the spectrum and other phænomena of absorption *are* produced by the undulations of the luminiferous æther, it is not difficult to show that there are ways in which those undulations *may* produce phænomena of such a kind. I would beg leave to add one or two considerations, which appear to me to bear upon what he then stated. He observes that if undulations have to traverse canals which ramify and meet again, there may be certain relations among the lengths or other conditions of these canals which may produce a destruction of undulations for one particular rate of vibration, and thus produce a dark line for one particular colour in the luminiferous vibrations. To this view might be objected,

* Since published in the *Philosophical Magazine*, December, 1833.

(1.) the great number of the dark lines in many kinds of light, which would appear to make a very complex structure of media necessary, (2.) the difficulty of conceiving that, on such an hypothesis, the absorptive properties of media should be the same in all directions. Both these objections seem to be much diluted by the following considerations:

1. A combination of channels of vibration which would destroy any rate of vibration, would also destroy all the *harmonics* of that rate, if the vibrating body were a line. If the fundamental rate were much slower than those which were noticed by the senses, this consideration would give many more vibrating rates near each other. Thus, if the fundamental rate were a million undulations a second, we should have a dark line for every multiple of this; and, therefore, since red light makes 458,000,000,000,000 vibrations, and violet light 727,000,000,000,000 vibrations, per second, we should have 270,000,000 lines in the spectrum on this account only.

But it is to be observed that the vibrating masses of the æther are not lines. The experiments on vibrating plates have shown that the harmonics of plates are more numerous and varied than those of lines, as theory also shows. But the vibrating masses of æther are solid spaces, and the way in which they may be divided by nodal surfaces into portions vibrating isochronously will be still more various; so that in this way the rates of vibration for which the vibration is extinguished may become as numerous as any observations can require.

2. If we conceive with Sir John Herschel a medium which will not transmit vibrations except through certain canals, these canals must have a determinate direction; and therefore such a constitution of diaphanous bodies would give different proportions in different directions. But let a medium consist of certain particles regularly distributed, the intervening space being filled by a medium capable of vibration. Let it be supposed, also, that each vibration, on reaching a medium so disposed, proceeds in part directly, and in part by the indirect routes which go round some of the particles and rejoin the direct course. We have thus combinations of ramifying and reuniting paths, which, though very complex in each direction, are the same in different directions, in consequence of the regular distribution of the particles. If the distribution, though regular, have a reference to certain axes, as in many crystals, the phænomena of absorption may be different in different directions with regard to these axes.

In this way the theory of ramifying canals comes to coincide with the theory of vibrations, of which parts are differently re-

tarded, and thus interfere with each other; a theory which has been suggested by other authors.

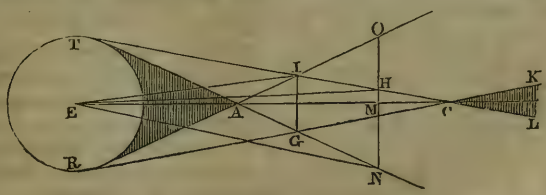
It is acknowledged that the above hypotheses are arbitrary. The object is to show that there is no incongruity between the undulatory theory and the phænomena of absorption. For it may be observed, the above hypotheses do not at all interfere with the laws hitherto assumed in the calculations of the undulatory theory. When the laws of the absorption here spoken of are known, the undulatory theorist will have before him the task of pointing out what *is* the constitution of transparent media. The object of the present remarks is to show that the existence of a constitution which shall embrace the facts as far as we know them, is not at all at variance with the undulatory theory.

On the Visibility of the Moon in Total Eclipses. By the Rev. T. R. Robinson, *D.D., &c.*

Some years since, the late Sir John Leslie brought forward an hypothesis, that the planets, and in particular the moon, shine, not, as is commonly supposed, by reflected light, but by a kind of phosphorescence; either absorbing solar light and emitting it with some modification, or becoming luminous in consequence of its action. He supported this opinion with his usual ingenuity, and in particular availed himself of the arguments afforded by the moon's secondary light, and the red appearance of her disc in total eclipses. The first of these he thought far too intense to be the result of "earthshine," and the second still more disproportioned to that which is commonly reputed its cause, the refraction of rays transmitted through the earth's atmosphere. His reasoning on these facts is, however, vitiated by defective data, for he certainly underrates the reflective force of unpolished surfaces, and exaggerates the moon's light; and the author would not have reverted to it but for an appearance that presented itself during the eclipse of December 26th, 1833. While the moon was entering the shadow, the eclipsed portion exhibited the usual yellowish red glare, which in this case, from the great illuminating power of the telescope, was very striking, giving the idea of an immense globe of brass faintly ignited. This was *least bright* but *most coloured* at the eastern limb, and the division between it and the portion still enlightened by the sun was made by a zone of *bluish grey* light about 30″ or 40″ broad, which was seen by several persons with this telescope. This disappeared when the moon was totally immersed. At the middle of the

eclipse her surface was all red, but towards its close a kind of twilight spread over that part towards the place of emersion, which became at length so bright as to make the moment of its occurrence inappreciable. As soon as a portion of illuminated surface was decidedly seen, the above zone could again be traced, and was seen till about one third of the disk had reappeared. This zone the author supposes to be the effect of those rays that pass through the atmosphere, their illumination being greatest near the edge of the shadow and gradually decreasing towards its axis, which, as he presently shows, it is improbable that any of them ever reach except at a distance far beyond the moon. The reddish light must proceed from some other cause, which the author does not attempt to conjecture; it is commonly ascribed to the absorption of the more refrangible rays in their transmission, and illustrated by reference to the setting sun and the clouds of evening. But if this absorption be effected by the vapours diffused in the atmosphere rather than by the air itself, we are forced to conclude that no light can pass through the region where they occur bright enough to be sensible when reflected from the moon.

The deviation of a ray of light passing through our atmosphere will be twice the *horizontal refraction* of that stratum of air which is at the vertex of its path. Considering this stratum as an elementary zone of a refracting sphere, it will form an image of the sun at the distance $\frac{r}{\sin 2\,H}$ when r = zone's radius and H the horizontal refraction. In this image all the rays transmitted by the zone must be found; and the inverse proportion of their areas would give the ratio of direct sunshine to that of the refracted light were air perfectly transparent. This ratio can easily be transferred from the image to the lunar section; and summing the effect of any number of these zones, we obtain the lunar illumination derived from the source.



Let, for instance, T I and T G be refracted rays coming from the lower and upper points of the sun's disc, and also R I and R G. I G will be the image; after which the rays diverge and

all that have passed through the element T will be found in the line N H at the distance of the moon; and going round the zone, all its light in the circles described with N M, H M as radii.

The cones T A R and K L C are dark, but in I A C and I C G the intensity is doubled by the section overlapping. As the refracting zone is taken higher in the atmosphere H diminishes, and the point A approaches M till they coincide, and the point M, or that point of the moon which is central in the shadow, receives no light from the higher zones.

Putting S = sun's semidiameter,
and p = moon's hor. parallax,

we have

$$\mathrm{I\,A\,M} = \mathrm{A\,E\,I} + \mathrm{A\,I\,E} = \mathrm{S} + 2\mathrm{H};$$

but when A coincides with M, $\mathrm{I\,A\,M} = p$, and at the limit $\mathrm{H} = \frac{p - \mathrm{S}}{2}$, or at their mean values (Baily's formulæ)

$$\mathrm{H} = 20'\,29''\cdot 7,$$

scarcely less than that observed by the academicians at Quito.

The angle

$$\mathrm{N\,E\,M} = \mathrm{S} + \overline{2\mathrm{H} - p}$$

$$\mathrm{M\,E\,H} = \mathrm{S} - \overline{2\mathrm{H} - p}.$$

Hence, calling I the intensity of light in the pencil transmitted through the element T, we obtain for the zone's addition to the density of illumination in O H or N M, omitting the higher powers of h, the height above the surface,

$$d\,\mathrm{D} = \frac{p^2 \times \mathrm{I} \times d\,h}{r\,\{\mathrm{S}^2 + (2\,\mathrm{H} - p)^2\}};$$

integrating which between the limits (H) = 35′ 6″ and H = 20′ 29″·7, we obtain half the illumination of the point M. If we take $\mathrm{H} = p - \mathrm{S} - n$ for the latter limit, we obtain the mean illumination of a space, whose diameter is $2\,n$. For this we require to know the relation between S and H, but it depends on the law of density, of which we know little more than this, that it must be between the decreasing geometrical and arithmetical progressions when h increases in the latter. For the present purpose it is sufficient to assume it such as will represent the actual condition of the atmosphere between (H) and H. This is afforded by the observations which Gay Lussac made in his celebrated aëronautic expedition. The heights as given in the account of it depend on Laplace's hypothesis of the decrease of temperature, *being each derived from comparison with the barometer of the observatory*; but we avoid this para-

logism, if we break the total elevation into a sum of smaller ones, as through a few hundred feet the average temperature can differ but little from the half-sum of the extremes. Deriving the densities from the barometer heights and temperatures, using the expansion of *dry* air, we find very nearly

$$h = \frac{a\,x}{1 - x^3},$$

when $a = 39534$ feet and x is the decrease of density divided by that at the surface.

As the horizontal refraction is proportional to the density of air at the vertex of the ray's trajectory,

$$x = \frac{(\mathrm{H}) - \mathrm{H}}{(\mathrm{H})}.$$

The quantity I is given with sufficient accuracy for this inquiry (see *Méc. Cél.*, iv. 283.) by the equation

$$\mathrm{I} = e^{-2\varepsilon \mathrm{H}},$$

e being the base of Napier's logarithms, and ε a coefficient derived from the observed extinction of light in traversing the atmosphere, which for $\mathrm{H} = (\mathrm{H})$ is supposed to give

$$\mathrm{I} = \frac{1}{3746}.$$

Substituting these expressions and putting

$$2\varepsilon(\mathrm{H}) = \phi,\ \frac{2(\mathrm{H}) - p}{2(\mathrm{H})} = \varkappa,\ \frac{\mathrm{S}}{2(\mathrm{H})} = \sigma,\ \frac{2\,\mathrm{H} - p}{\mathrm{S}} = y,$$

and developing $d\,h$, we obtain

$$d\,\mathrm{D} = \frac{-p^2\,a\,\sigma}{r\,\mathrm{S}^2 \times \varepsilon^{\phi(1-\varkappa)}} \times \frac{e^{-\phi\sigma y}}{1 + y^2}\{\mathrm{A} - \mathrm{B}\sigma y + \mathrm{C}\sigma y^2 -, \&c.\}$$

in which $\mathrm{A} = \frac{1 + 2\,x^3}{1 - x^3}$, and the others B, C, &c., its successive differential coefficients divided by 1, 1·2, &c., which are easily derived independently. This series is converging, though but slowly, and 27 terms of it give for the integral between the limits $y = \pm \frac{2(\mathrm{H}) - p}{\mathrm{S}}$.

$$\mathrm{D} = \frac{10^{1.93368}}{10^{10}} \left\{5{\cdot}34377 - 0{\cdot}02635\right\}.$$

Thus we find that for a circle 7 minutes radius round the axis of the shadow, the illumination derived from this source must

be the hundred and ten millionth of direct sunshine, which assuredly would illude all vision; and as the moon's latitude at the middle of the eclipse was only four minutes, a considerable part of its disc ought to have been dark.

The only part of this reasoning which may be questioned is the coefficient ε. It was derived by Laplace from Bouguer's experiments, and we cannot but feel that we have a right to demand something better from the improved powers of modern research. The actinometer, described at the last Meeting of the Association, (see Reports of the Cambridge Meeting, p. 379,) promises such a result, which, if possible, should be obtained at different elevations, and accompanied with determinations of the density and hygrometric state of the air. In the ascent of Gay Lussac he did not pass the region of clouds; but even at heights much inferior, it is possible the quantity of absorption may be very different from what prevails at the surface. We also want a comparison of moonlight with sunshine. The author states that these remarks are submitted to the Section in hopes that some of its members may be induced to turn their attention to supply these desiderata. It is also probable that, if carefully examined, traces of polarization would be found in that blueish grey illumination which has been noticed at the same time. If they were wanting in the red part of the lunar disc, this would decide the difference of their causes. This, however, the author could not try, as, independent of other reasons, the first were too faint to be visible through a plate of tourmaline, the only analyser which he possessed.

Account of some Observations made for the purpose of determining the Positions of the Axes of Optical Elasticity in oblique prismatical Crystals. By Professor MILLER.

In crystals belonging to this system it is well known that one of the axes of optical elasticity coincides with that crystallographic axis which is perpendicular to the other two axes. The object of the observations was to determine the position of the remaining two axes of optical elasticity, and, if possible, to discover some general law by which their position could be made to depend upon the form of the crystal. After having found the positions of the axes of elasticity in a variety of oblique crystals, the hypothesis suggested by Neumann's observations on gypsum, viz., that the faces of the crystal could be referred to the axes of elasticity as crystallographic axes, was tried, but, except in one instance, did not succeed. In many crystals no other relation between the form and axes of elasti-

city could be traced. But in felspar, epidote, and pyroxene, according to the author's own observations, and in chromate of lead according to the observations of Professor Nörrenberg, one of the axes of elasticity in question coincides very accurately with the axis of one of the principal zones of the crystal.

An Account of a new Phænomenon of sonorous Interference. By R. ADDAMS, *Lecturer on Chemistry and Natural Philosophy.*

Two systems of sound waves, simultaneously generated by a *tuning-fork*, in tubes, interfere and neutralize each other when the axes, or lines of direction in which the two systems are propagated, are at right angles to each other.

The apparatus which the author of this communication employs to demonstrate the foregoing case of interference, consists of two glass tubes, one inch in diameter, each furnished with a piston, in order to adjust the length of the included column of air, so as to make it unisonant with a tuning-fork (according to the method first devised by Mr. Wheatstone). These tubes are placed rectangularly, one vertically, the other horizontally, and with their mouths in contact, edge to edge.

When a tuning-fork is vibrated, and held so that the medial line between its branches coincides with the intersecting point of the axes of the tubes, there will be no sound heard; but upon covering the mouth of either tube with a card, an audible sound is reciprocated by the air in the open one.

Variations in the intensity of the sound occur by altering the angle of position of the tubes.

Account of Magnetical Observations in Ireland, and of a New Method of observing the Dip and the Force with the same Instrument. By the Rev. Professor LLOYD, *F.T.C.D.*

In the last Report of the Transactions of the British Association for the Advancement of Science it was recommended, "that a series of observations upon the intensity of terrestrial magnetism be executed in various parts of the kingdom, similar to those which have been carried on in Scotland by Mr. Dunlop; and that observations should be made in various places with the dipping-needle, in order to reduce the horizontal to the true magnetic intensity." After alluding to the time occupied in the preliminary observations required in such a task,—such as those made to ascertain the magnetic condition of the needles used for the force, and the changes of this condition

dependent on temperature,—Professor Lloyd proceeded to give an account of a series of observations which had been commenced in Ireland by Captain Sabine and himself, and of the steps which had been taken to carry into effect the recommendation of the Association in that country.

The first object of the observers was to compare, with accuracy, the direction and intensity of the magnetic force at Dublin and Limerick, the two stations from which it was proposed to set out. This was accomplished with much care, the magnetic intensity at the two stations having been compared by a repeated interchange of needles; and in this manner a close approximation was made to the direction of the magnetic lines in Ireland, and thus the most favourable points of observation ascertained. The latter of these two stations has recently been compared with London, by an interchange of needles between Captain Sabine and Commander Ross; and a similar comparison of the total intensity in London and Dublin has been made by Professor Lloyd, so that the series is thus connected with observations taken elsewhere. The series itself will, it is hoped, be shortly completed, and a connected view of the results laid before the Association at its next meeting.

Besides the usual method of determining the terrestrial magnetic intensity suggested by Hansteen, Professor Lloyd adopted another, in which the dip and the intensity are ascertained by the same observation, and with one instrument. This method consists in observing the direction assumed by an ordinary dipping-needle under the combined influence of magnetism and gravity. If two small weights be successively attached to the southern arm of the needle, and if ζ and θ denote the inclinations of the needle in the two cases, these angles will be connected with the dip and the force by the equations

$$\mu \cos \zeta = \phi \sigma \sin (\delta - \zeta)$$

$$\nu \cos \theta = \phi \sigma \sin (\delta - \theta),$$

in which μ and ν denote the *moments* of the added weights, δ the dip, ϕ the force, and σ a constant depending on the distribution of magnetism in the needle. Hence, if the two moments, μ and ν, be known, and if the angles, ζ and θ, be observed, the two unknown quantities, δ and ϕ, will be completely determined.

The friction of the axle, however, which is the main source of error in the dipping-needle, will affect these quantities differently in different positions. Professor Lloyd has found from theory that the limit of error in the determination of the *dip* arising from this cause will be least when the position of the

needle coincides with the line of the dip, while that of the *force* is least when the needle is at right angles to that line. These, then, are the most advantageous positions for the determination of the two elements; and accordingly the best mode of applying the preceding method consists in observing, 1st, the position of the needle when *unloaded*; and, 2ndly, when *loaded* with a weight sufficient to render it nearly perpendicular to the line of the dip. As the inclination of the needle in the first position is nearly equal to the dip, and would be accurately so if the centre of gravity of the needle perfectly coincided with the axle, it is convenient to consider this first angle as the approximate value of the dip, and to seek the *correction* required in order to reduce it to its true value. If ε denote this correction, it can be readily shown from the formulæ already given, that

$$\delta = \zeta + \varepsilon, \quad \sin \varepsilon = \rho \frac{\cos \zeta}{\cos \theta} \sin (\zeta - \theta),$$

ρ denoting the ratio of the *moment of the needle* itself to that of the weight afterwards added. When the needle is well constructed, this ratio is very small, and the correction itself may be disregarded. The force is deduced from the second position of the needle when loaded, and is given by the formula

$$\phi = \frac{\beta \cos \theta}{\sin (\delta - \theta)},$$

β being a constant, which is determined from the values of δ and θ at the place where the force is taken as unit.

In the usual method the horizontal force is determined by the rate of vibration of a horizontal needle, and the actual force deduced by multiplying it by the secant of the dip. The instrumental errors, therefore, are of two kinds, and as these may have the same sign, the limit of error is thereby increased. But even supposing the determination of the horizontal force to be *perfect*, the limit of error in the actual force, arising from the error of the dipping-needle, is to that in the method now proposed, in the ratio of the tangent of the dip to unity; so that the latter method is more accurate whenever the dip exceeds 45°, and in our latitudes its accuracy is nearly three times greater than that of the received method. This result has been verified by observation, and it has been found that, with a small circle $4\frac{1}{2}$ inches in diameter, the value of the force deduced from the mean of two or three observations may be depended on with certainty, to the third place of decimals inclusive.

On an apparent Anomaly in the Measure of Rain. By Sir THOMAS M. BRISBANE, *President of the Association.*

Sir Thomas M. Brisbane has for some time observed a curious fact with respect to the rain collected in his gauge, the receiver of which is 7 feet from the ground, and about 210 feet above the level of the sea. The rain always stands in the gauge some hundredths of an inch higher *an hour or two* after it has fallen than it does *four or five hours* after; and the author suggests that the phænomenon may be owing to atmospheric air absorbed by the drops in their descent, and afterwards slowly escaping from the gauge.

Second Report of the Result of Twelve Months' Experiments on the Quantities of Rain falling at different Elevations above the Surface of the Ground at York, undertaken at the request of the Association by WILLIAM GRAY, Jun., *and Professor* PHILLIPS, *F.R.S. F.G.S., Secretaries of the Yorkshire Philosophical Society; with Remarks on the Results of these Experiments, by Professor* PHILLIPS.

I. *Report of the Experiments.*—The report presented to the Cambridge Meeting of the Association* contained the register for twelve months of the quantities of rain collected above the top of York Minster, on the top of the Yorkshire Museum, and on the ground adjacent; the elevations of the upper stations being 212 feet 10½ inches, and 43 feet 8 inches.

The present series of twelve months is continuous with the former, and commences on February 1, 1833. The same gauges were used with the same precautions as in the previous year; but for particular objects the intervals of measuring the contents of the gauges were purposely and considerably varied. It will be recollected that the discharge-pipe of the gauges was stated to have been kept stopped with a cork, and during the whole of the first twelve months this was always observed; but for more than three months of the present series the cork of one of the gauges (the middle one) was left out. Comparative experiments were made to determine the probable increased loss from evaporation arising from this cause and a compensation calculated. The corrected numbers are placed in the column, and the original observations in smaller figures on the side.

* See *Reports of the British Association*, vol. ii. p. 401.

1833-1834.	Minster.	Museum.	Ground.	Remarks.
	Inches.	Inches.	Inches.	
Feb. 1 to Feb. 28.	1·509	2·108	2·834	
March . . 13.	·327	·560 (·539)	·791	Chiefly snow and hail and cold rain drifting from N.N.W., &c.
28.	·546	·687 (·663)	1·018	
April . . 21.	·570	·745 (·709)	1·030	
May . . . 20.	·686	·787 (·735)	1·015	Rain fell on the 2nd, 3rd, 4th, and 20th of May.
30.	0·	0·	0·	
June . . . 17.	1·525	1·942(1·902)	2·386	
July . . . 1.	·559	·649	·791	
August . . 3.	·810	1·030	1·246	Small Hymenoptera in the Minster gauge.
19.	·391	·484	·575	With Mr. Lubbock. Small Hymenoptera in the Minster gauge.
September . 16.	2·175	3·000	3·835	
October . . 8.	·386	·473!	·460	This is the only observation in which the middle station had most water. The nights were excessively dewy.
14.	·050	·083	·106	With Major Emmett, just after the fall of perpendicular rain.
17.	·263	·373	·406	
November . 12.	1·216	1·574	1·894	High winds.
December . 31.	1·811	2·558	3·641	Most violent gales N.W. Snow on the 11th of December.
February . 1.	2·139	2·798	3·678	
Total of 12 months	14·963	19·852	25·706	

Rain almost daily.

II. *Remarks on the Results of the Experiments.*—The quantities of rain collected in other gauges at York and in the neighbourhood agree nearly with those recorded for the ground gauge in these experiments. For 1833, from February 1 to December 31, we have,

At York { In the garden gauge . . . . . 22·028
In Mr. J. Gray's self-registering gauge 22·205

In Dr. Wasse's gauge, Moat Hall, on higher ground, 12 miles N.W. . . . . 23·488

Mr. Cholmeley's, at Brandsby; level with top of Minster, 12 miles N. . . . . 24· +

The quantities and ratios at the several stations for particular periods of the year are as under:

Periods.	Mean Temp.	On Minster. In. of Rain.	On Museum. In. of Rain.	On Ground. In. of Rain.	Ratios.		
Whole year	48°·2	14·963	19·850	25·706	58·20	77·21	to 100
7 colder months .	40·8	8·817	11·959	15·858	55·6	75·4	
7 warmer months	55·5	7·415	9·606	11·850	62·6	81·0	
5 colder months .	39·3	7·548	10·285	13·856	54·6	74·2	
5 warmer months	58·5	6·146	7·932	9·848	62·4	80·5	
Winter quarter .	36·3	5·459	7·464	10·153	53·8	73·3	
Spring	47·6	2·129	2·779	3·854	55·2	72·1	
Summer	60·8	3·285	4·105	4·998	65·7	82·1	
Autumn	48·3	4·090	5·503	6·701	61·0	82·1	

On comparing these ratios with those obtained in 1832, it is impossible not to be struck with their *wintry character*, which agrees with the fact of the months of February, March, April, October, November, and December, being almost diurnally rainy. In 1832–3 the greatest part of the rain fell in the warm months; but in 1833–4 the cold months were most rainy. In 1832–3 the mean diminution of rain was

on the Minster (d) . . . . . 33·9 per cent.
and on the Museum (d') . . . 14·7

but in 1833–4 these numbers were,

41·8
22·8

In 1832–3 the mean annual value of the function of the height was $h^{\cdot 50}$,—in 1833–4 it was $h^{\cdot 43}$. By uniting the observations of thirty months, (to August 30, 1834,) $h^{\cdot 45}$.

It is hence very apparent that a few years' observations will give this mean value very accurately, determine the limits of its variation, and the dependence of this upon the monthly temperature and other causes. I shall, however, purposely abstain from discussing the subject any further at present, because the experiments will be continued under the present arrangement six months longer, and thus any conclusions which may be offered rendered more trustworthy. Besides, I do not wholly despair of being furnished with some aid from other observers in different regions and under various circumstances, for I am far from thinking that all the conditions of this curious problem can be determined at one locality, however favourably situated*.

* My friend Mr. W. D. Littledale has established three gauges at Bolton-Hall, in Craven, and Mr. A. Halliday has obliged me by undertaking a similar labour at Manchester.

The following table shows the sum of the diminutions per cent. of quantity of rain at the two upper stations for two years, with a column of numbers inversely proportional to the temperature during the several parts of the year.

	In 1832-3.	In 1833-4.	Mean.	$\frac{\Delta t}{t'}$	Difference.
Whole year	48·6	64·6	56·6	58·7	2·1+
7 cold months	61·9	79·0	70·4	69·4	1·0—
7 warm months	41·7	56·4	49·0	51·0	2·0+
5 cold months	74·8	71·2	73·0	72·0	1·0—
5 warm months	47·1	57·1	52·1	48·4	3·7—
Winter	82·2	73·4	77·8	78·0	0·2+
Spring	60·2	72·7	66·4	59·5	6·9—
Summer	30·4	52·2	41·3	46·6	5·3+
Autumn	57·5	56·9	57·2	58·6	1·4+
Mean values of $d+d'$, whole year			56·60		11·0+
7 cold and 7 warm months		59·75			12·6—
5 cold and 5 warm months		62·55			
3 cold and 3 warm months		59·55	60·91		
Spring and Autumn		61·80			
			117·51		
General mean (Δ)			58·75		

By comparing the last column with the mean values of $\overline{d+d'}$, their almost exact coincidence will be immediately evident; and therefore it appears that the conclusion advanced in the last Report, p. 408, as to this value being an inverse function of the temperature, is now strongly supported by additional observations, the whole nearly agreeing with the simple formula

$$\frac{\Delta t}{t'} = d + d'.$$

On the Difference of the Quantity of Rain at different Heights above the Surface of the neighbouring Ground. By LUKE HOWARD, *F.R.S., &c.*

The author, referring to the experiments on this subject, printed in the second volume of the Reports of the British Association, pp. 401, &c., proposes a different opinion as to the cause of the augmentation of the quantity of rain at the lower stations.

He allows that this effect of the coldness of the superior rain enters for something *into the aggregate of the causes of increase by descent*; but that it is considerable or appreciable he does not admit.

He observes (and refers for further particulars to his work on the Climate of London), that rain falls principally in two ways: 1. By the condensation or collapsing of the mass of an elevated cloud, (effected by the subtraction of the electrical atmosphere of the cloud, or by the extension of the atmospheres of the *spherules* of the cloud, through their mutual attraction, the electrical charge now taking its seat upon the smaller surface of a congeries of larger *drops*, as is manifest by the charge these give to the insulated rod,) or, 2. *By loss of heat in the whole mass of air* from which it is about to rain (this lowered temperature being the actual cause of the rain), in which case the separation of the water is effected precisely in the manner of the precipitation of solids from a menstruum in which they were held in solution. The nascent drops exist in every part of the raining space, and find their way to the earth subject to the small augmentation, by virtue of the lower temperature found at greater heights above mentioned.

In the first case *the rain at the top of a building and the rain at the ground are equal in amount*; in the second, there is an augmentation in the lower strata, which so overbalances the former case, both in frequency and amount, as to give *the averages* the character they exhibit.

An attempt to connect some of the best-known Phænomena of Meteorology with established Physical Principles. By Professor STEVELLY, *A.M., of Belfast.*

The author examines, in this point of view, the following four points:

1. The nature and origin of clouds;
2. The production of rain, and some of its consequences;
3. The origin of wind from clouds or rain;
4. The formation of hail.

1. *Nature and Origin of Clouds.*—The author adopts the opinion, that the constituent particles of clouds are minute spherules, but not vesicles; and refers the suspension of clouds to two causes: the extreme slowness of descent through the air of such exceedingly minute particles, and the repulsive action of the electrical atmospheres of these particles upon the ambient air.

Clouds are stated to owe their origin to the excess of vapour

which at any time happens to exist in the air, collected into drops by the capillary attraction of the elementary particles, which now appear as spherules of water; the spaces around them becoming drier than before, and the whole space occupied by the cloud having its elastic tension reduced in proportion to the quantity of vapour converted into water. In consequence of this circumstance, a diminution of sensible temperature will be occasioned, and a secondary formation of clouds may take place, notwithstanding some addition, on the other hand, from the development of the heat latent in the vapour.

The atmospheric equilibrium being thus disturbed, wind will blow from all points toward the cloud, and if this was previously in motion, there will be a comparative calm before the cloud, and a strong wind following it. Hence the appearance of the edges of driving clouds varies; small portions detach themselves from the ragged posterior part, and float away, while the anterior part is of smoother outline, and suffers little change.

Another consequence of the conversion of the vapour into drops of water is an increase of electrical intensity in the cloudy space.

Clouds frequently divide into portions which have opposite electrical states, when they come into contact with a hill, in consequence of the effect of the ordinary laws of induction.

2. *Origin of Rain.*—When two oppositely electrified clouds rush together, and the spherules unite into drops, these descend lower in the atmosphere, or fall in the form of rain, which is more or less heavy, according to the densities of the original clouds or the degree of their electrical intensity. On the principles of electrical induction may also be explained the cessations and renewals of rain, and the intermitting peals of thunder.

As the rain descends, a void space is occasioned in the place lately occupied by the clouds, and a depression of temperature in the superior regions, by the *expansion* of the air. An increase of temperature, to a smaller extent, happens below, from the *condensation* of the air.

3. *Origin of Wind.*—Breezes and gales are produced by the secondary formation of clouds, particularly when the clouds are formed from a mass that has, in appearance, attached itself to a hill.

Squalls are gusts of wind caused by heavy showers passing over the country in vast and distinct patches. In the front of the shower the wind is driven out by the rain most violently in the direction in which the general current was previously moving. Towards the close of the shower, however, the wind becomes moderate, or even reversed, the chief rush of the air

from behind being directed upwards, to supply the spaces above. The author states that the phænomena of tropical and other extraordinary rains and winds agree with the results which may be deduced from the foregoing facts and considerations.

4. *Origin of hail.*—Referring to Sir J. Leslie's experiments, and to the well-known effects of the compressed air in the engine at Chemnitz, the author explains the formation of hail by stating, that when very sudden and violent falls of rain take place, especially in summer, the air, expanding into the void space left aloft, robs the succeeding rain so effectually of its caloric as to freeze the drops. The author proposes to publish his views in an enlarged form, with adequate illustration by statements of observed facts.

Extract of a Letter to Professor Forbes *from Professor* Christie.

The writer observed a very peculiar and well-defined light proceeding, in the form of a ray, from the sun as it was setting, having the sun for its base, and retaining the same position for about half an hour. The ray was absolutely vertical, gradually decreasing in splendour, until it was lost, at about the height of 20° or 25° above the horizon, expanding but slightly from its base to this point, and it was unaccompanied by any lateral rays. Its expansion in breadth did not in any part exceed a degree and a half on each side. These circumstances attracted his attention on the occasion of his first witnessing the appearance; and on a more particular examination he was persuaded that it could not be of the ordinary description of rays, proceeding from an opening in a mass of cloud. Independently of its permanence in a very peculiar position, there were appearances in the ray itself which precluded such an opinion. The 30th of June, the day on which he first observed the phænomenon, had been clear and hot. At the time of the observation, above the sun there were faint streaks of haze, scarcely to be denominated cloud, increasing in density towards the horizon, and on these the ray was exhibited; but he does not remember to have noticed any well-defined clouds, even in the horizon. At sunset on the 17th of July he again witnessed this phænomenon, but the ray was stronger and better defined than on the former occasion, and although of much greater extent, reminded him of the form and appearance of the tail of the comet of 1819. Its position was, as before, absolutely vertical, and it continued visible for about half an hour. The sky had

been cloudless all day, and the sun intensely hot; a decided change of temperature took place about sunset, at which time a fine breeze from E.S.E. sprung up, gradually increasing, rendering the evening and night cold. At sunset the sky was clear, except towards the north and west, where dense masses of cloud rose a few degrees above the horizon; and also in the intermediate part above the sun, where streaks of thin haze were rendered visible by its light: upon these a narrow but slightly expanding vertical band of well-defined light, having the sun for its base, was again exhibited.

It had occurred to Professor Christie, after he had first observed this light on the 30th June, that it might be due to a succession of images of the sun imperfectly reflected by strata of thin vapour, and all the appearances which he observed on this second occasion tended to impress this notion more strongly on his mind; but he thinks repeated observation of the phænomenon will be required before it can be decided whether this be the correct explanation. On both occasions, the position in which Mr. Christie saw the light, was looking almost immediately across some extent of sea, the sun setting behind the Hants and Dorsetshire hills. The light was most brilliant very shortly after sunset, and gradually declined in brightness till it wholly disappeared, about half an hour afterwards; but its direction was invariable, and its general character the same during the whole time of its continuance. Early on the morning of the 18th July there was thunder and lightning, with rain, which continued more or less until the middle of the day.

Notes on mean Temperatures in India. By Lieut. Col. SYKES, *F.R.S.*

The author states the results of several observations of mean temperature in India, at different elevations above the sea, between 10° and 25° N. lat., and 70° and 82° E. long., for comparison with the formulæ of Meyer, and the generalizations of Humboldt and others.

Mhon, in Malwa, in lat. 22° 23′ is 2000 ft. above the sea; the mean temperature observed by Dr. Craw	74°·00
Calculated by Meyer's formula, and adopting the correction of 1° Fahr. for 300 ft. ascent	69·86
Calcutta is in nearly the same lat. (22° 35′) and its mean temp. as determined in 1781, and again after a lapse of 52 years	78·13

There is therefore a difference of 4° 13′ between the mean

temperature of Calcutta and that of Mhon, giving 480 ft. nearly to each 1° of temperature.

Ahmednuggur, in the Deccan, in lat. 19° 6′, and 1900 feet above the sea, has a mean temperature, as determined by Dr. Walker, of 78° Fahr., while the calculated mean temperature at the level of the sea is 78°·6, giving a difference of 0°·6 of a degree, for a difference of level of 1900 ft. The mean temperature at Col. Sykes's residence in Poona, lat. 18° 30′, elevation above the sea 1823 ft., was 77°·7. The calculated mean temperature at the level of the sea is 78°·94, giving 1° for 1471 feet.

The mean temperature of a spring in the excavated caves in the Hill-fort of Hurreechundurghur, lat. 19° 23′, at 3900 ft. above the sea, was 69°·5 Fahr. The calculated mean temperature at the level of the sea, 78°·45; giving 1° Fahr. for 435 ft.

The mean temperature of Seringapatam, at 2230 ft. above the sea, is 77°·06, corresponding nearly to the mean of Poona and Ahmednuggur, although the latter places are between 6° and 7° of lat. further north, and their levels nearly the same. The calculated mean temperature of Seringapatam, at the level of the sea, is, by Meyer's formula, 81°·77; by Brewster's amended formula, 79°·9, and by his general formula, including the consideration of two poles of maximum cold, (see *Trans. of the Roy. Soc. Edinburgh*, vol. ix.) 76°·55. In the first case the difference of temperature corresponding to 2230 ft.=4°·71, (or 1° for 473 ft.); in the second, 2°·84 (or 1° for 785 ft.); in the third the result is negative.

In an observation at a height of 8500 ft., the value due to each degree of alteration of temperature corresponds very closely with the general result (332 ft.) adopted by Professor Forbes in his Report on Meteorology.

The mean temperature at the equator has been stated at 81½°: the result of 21 years' observations at the Observatory of Madras, more than 10° from the equator, situated on an open beach, has determined the mean temperature of that place to be 81°·69; and in general, Col. Sykes concludes that the observed mean temperatures in India everywhere exceed those given by calculation. By the result of several years' observations, he has found that the mean temperature of the hour of the maximum diurnal atmospheric tide (between 9 and 10 A.M.) is equal to the mean temperature of the year in India. The heights of places mentioned in this communication were determined barometrically.

On a peculiar Oscillation of the Barometer. By the Rev. J. HAILSTONE.

The author presented a table of observations on the height of the barometer, at short intervals of time, between November 28, 1833, and January 10, 1834. The circumstance in these observations, which the author especially desires to point out for the attention of meteorologists, is, a small and short oscillation of the mercury, sending it up seldom above a few thousandths of an inch, after which deviation it resumes its usual march again.

On the use of Leslie's Hygrometer with a new Scale. By H. H. WATSON.

In this communication, the author states the reasoning and experiments by which he was induced to apply to Leslie's hygrometer a scale of equal parts, such that the cold produced by evaporation of water, being measured upon this scale, and the parts considered to represent degrees of Fahrenheit's thermometer, the dew-point, or constituent temperature of the vapour, should be immediately known. The author states, as the consistent result of many experiments, that the difference in degrees between the temperature of the air and the dew-point, is to the degrees of cold produced in Leslie's hygrometer, by evaporation of water, as 20 to 13: this ratio is consequently employed for the divisions of the new scale of the hygrometer, which thus is supposed to give results sufficiently in accordance with the direct experiment on the dew-point, to justify its use in cases where rigorous accuracy is not demanded.

Account of Experiments on the Expansion of Stone by the application of Heat. By ALEXANDER J. ADIE, *Civil Engineer.*

The author laid this communication before the Association principally to give the expansion of sandstone, taken from what is called the Liver Rock of Craigleith Quarry. The subject under experiment with the pyrometer, is placed in the interior of a double metallic case, which is heated by means of a current of steam, a method which is very convenient for keeping up a steady temperature for any length of time, and affords great facility in preserving the substance in the same hygrometric

state. The length of the rods, of which the expansion was determined, was 23 inches, and they varied in the cross section from half an inch to three quarters of an inch square: the length is laid off on the heads of small silver studs, sunk into the stone. The micrometer microscope of the pyrometer measures the thirty-thousandth part of an inch; and as a test to satisfy himself of the correctness of the instrument, and of the uniformity of its results, the author determined the expansion of cast zinc, selected, as a simple metal, having the greatest range, and his determination of it agreed very nearly with that given by Smeaton. He then procured a rod from a very equally-grained block of sandstone, from what is called the Liver Rock of Craigleith Quarry, from the same part of the bed from which the large stone in the pillar of the mural circle in the Edinburgh Observatory was cut; it possessed a considerable degree of flexibility when wet, but gradually stiffened as it became dry. The expansion of this sandstone, as determined from a length of twenty-three inches, with a range of temperature of 145° Fahr., gives ·0270446 of an inch for 180° Fahr., or ·0011758 in decimals of its length, which is the same as the expansion of glass given by Berthoud, and very nearly the same as the expansion of cast iron as found by Lavoisier. Black marble, from Galway, in Ireland, has the least degree of expansibility of any substance which Mr. Adie has tried, with the exception of wood. He states it to be ·00043855 of its length for 180° F., which is rather more than one third part of the expansion assigned by Lavoisier to steel, and nearly half that of platinum and glass for the same number of degrees. He also measured the expansion of Carrara marble; but as the specimen used was only one foot long, he does not state the result numerically: it was less, however, than that assigned to it by M. Destigny. A rod of oak, split from the tree to insure the straightness of the fibre, expanded ·000062007 in decimals of its length, for 180° Fahr., which is just one fifteenth part of the expansion of platinum for the same number of degrees; an insensibility to the change of temperature which arises very much from the wood being very dry when the experiment was made. The number here given is a mean of three trials, but the same rod of wood gave a very different result when a very small quantity of steam was allowed to blow into the case which contained it. It is Mr. Adie's intention to repeat these and other experiments during the winter, when he hopes to be able to command a greater range of temperature in his instrument. He will then give a full account of them, and the manner in which they have been

performed, together with a drawing of the pyrometer, in order that it may be the more easy to judge of what reliance may be put in the accuracy of the results.

II. CHEMISTRY.—MINERALOGY.

Table of the Proportions of anhydrous acid in acetic acid of every degree of concentration between pure water and the hydrated acetic acid, compared with the specific gravities, water at 59° Fahr. being taken at unity. Founded on Experiments, by ADAM VAN DER TOORN.

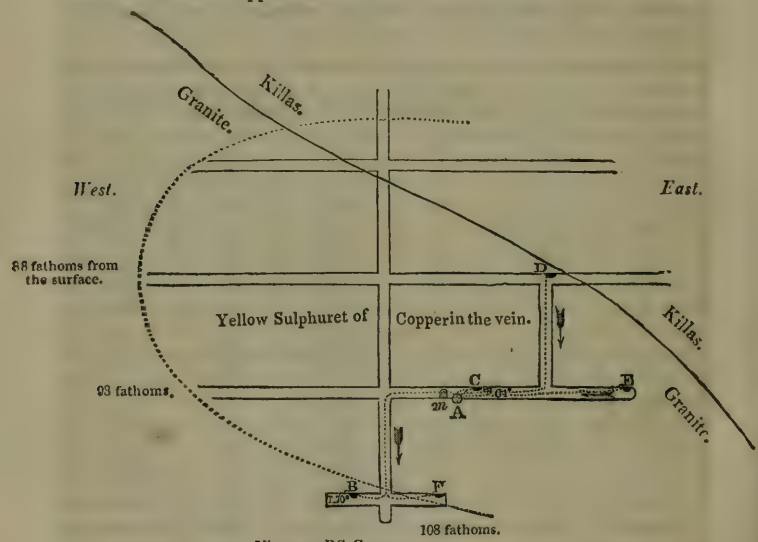
Anhydrous Acid in 100 parts by weight.	Density at 59°.	Anhydrous Acid in 100 parts by weight.	Density at 59°.	Anhydrous Acid in 100 parts by weight.	Density at 59°.
0	1·0000	29	1·0472	58	1·0740
1	1·0019	30	1·0485	59	1·0745
2	1·0037	31	1·0498	60	1·0749
3	1·0055	32	1·0510	61	1·0753
4	1·0072	33	1·0522	62	1·0756
5	1·0089	34	1·0534	63	1·0759
6	1·0107	35	1·0546	64	1·0762
7	1·0124	36	1·0558	65	1·0764
8	1·0141	37	1·0569	66	1·0765
9	1·0159	38	1·0580	67	1·0766
10	1·0177	39	1·0591	68*	1·0766
11	1·0194	40	1·0601	69	1·0766
12	1·0211	41	1·0611	70	1·0765
13	1·0228	42	1·0621	71	1·0763
14	1·0245	43	1·0631	72	1·0759
15	1·0261	44	1·0640	73	1·0754
16	1·0277	45	1·0649	74	1·0748
17	1·0293	46	1·0658	75	1·0741
18	1·0310	47	1·0667	76	1·0732
19	1·0326	48	1·0675	77	1·0722
20	1·0342	49	1·0683	78	1·0710
21	1·0358	50	1·0691	79	1·0696
22	1·0373	51	1·0698	80	1·0681
23	1·0389	52	1·0705	81	1·0664
24	1·0404	53	1·0711	82	1·0646
25	1·0419	54	1·0717	83	1·0626
26	1·0433	55	1·0723	84	1·0603
27	1·0447	56	1·0729	85	1·0574
28	1·0460	57	1·0735	85·11	1·0570

Account of some Experiments on the Electricity of the Copper Vein in Huel Jewel Mine. By Robert W. Fox.

The author transmitted a section of the deepest part of the mine, with a description of the manner in which he conducted the experiments on its electricity.

Huel Jewel. Deepest workings on South or Main Lode, which underlies about 23° towards the North.

Black and vitreous Copper in the Vein, with some yellow Sulphuret intermixed.



Direction of Electricity from the Vein.

Positive.		Negative.	Distance.
C	to	B	19 fathoms.
D	...	B	37 ditto.
E	...	B	34 ditto.
C	...	F	23 ditto.

Scale of fathoms.

10 20

The vein consists of black and vitreous copper above, and below, the yellow sulphuret of copper. Their boundaries are shown by the dotted lines.

A represents the observed station in a level 98 fathoms under the surface.

B, C, D, E, and F, show the points of contact where the metallic plates or copper wires were pressed against the vein, mostly by means of a wooden prop; and the dotted lines represent the copper wires employed.

Copper and *zinc* plates were alternately, or rather successively, used at each of the points of contact with the vein, except at D; but these changes of metal did not affect the character or direction of the electricity, nor did the contact of *points only* with the ore do so. But in all cases the *easterly* wires were positive with respect to the *westerly* ones. These experiments were made in order to prove that the electrical action is derived from the vein, and that it is not in any degree excited by the mere contact of the metal with the ore, as some have surmised.

In order to obtain some idea of the electric energy of the vein, the author placed a galvanic trough as in the circuit, at *m*, and caused it to act *with* the electricity of the vein, and also *against* it. In the former case, the deflections of the needle were considerably increased; and in the latter, when the electricity produced by the galvanic apparatus was opposed to that of the vein, the positive electricity from C was *reversed*, the galvanometer giving evidence of a slight negative action in that direction. The electricity from D, however, was only just neutralized, and that from E was merely diminished in intensity, the deflection of the needle being in the same direction, and equal to about 40°, from the magnetic meridian, instead of 100° at least, produced by the vein alone.

The galvanic apparatus consisted of a plate of copper, and another of zinc, plunged into strong brine, to which some sulphuric acid was added, and each plate exposed about 180 square inches to the action of the liquid. The voltaic activity was much diminished before the completion of the experiments; but even at the last, when the wires of the apparatus were applied to the galvanometer without the intervention of the vein, and the extensive circuit and comparatively imperfect contacts which it involved, a violent agitation and rapid rotation of the needle were produced.

These experiments afford strong evidence of the energy of the electricity of the vein; and this method may become useful to the practical miner, in helping him to appreciate the value of his discoveries, and enabling him to ascertain whether the ores in distant parts of a vein are connected or insulated, or whether what appear to be parallel veins are really so, or ramifications

of the same vein. Galena, and copper, and iron pyrites are the only substances usually met with in the Cornish mines which are capable of conducting voltaic electricity; and as iron pyrites is much more generally found in insulated masses than the other two, the test here suggested may be employed with a considerable degree of confidence on many occasions.

It was in Huel Jewel, and more than four years ago, that the author first obtained electro-magnetic results. The workings have been so much extended since, that the last experiments were made 60 fathoms deeper, and at least 80 fathoms further towards the east, than the first; and it is satisfactory to find that the direction of the electricity remains unchanged, viz. *positive* from the east. The temperature at the bottom level of the mine, 38 fathoms under the surface, was then 59°, and it is now, at the depth of 108 fathoms, 70°. The author has observed that when the sulphuret of copper or of lead is heated, or even slightly warmed, it becomes positively electrical, and yet the deepest parts of the veins of those ores, although warmer than nearer the surface, appear generally to be negative.

Notice respecting a remarkable Specimen of Amber. By Sir David Brewster, *F.R.S.*

This specimen of amber was brought from India by Mr. Swinton, and was found in the kingdom of Ava. Its size is nearly equal to that of a child's head, and its general aspect and physical properties, seem to differ considerably from the ordinary specimens of amber. The remarkable fact, however, which distinguishes it from all specimens of amber which the author has seen or read of, is that it is intersected in various directions by thin veins of a crystallized mineral substance. These veins, which cross one another, are sometimes as thin as a sheet of paper, and in other places about the twentieth of an inch thick. In order to determine the nature of the mineral, he extracted a portion of the thickest vein; and having obtained, by cleavage, a small rhomb, succeeded in measuring the inclination of its planes, and found it to be a *carbonate of lime*. The specimen, however, did not enable him to ascertain whether the angle was that of the pure carbonate of lime, or that of carbonate of lime and magnesia.

At the next meeting of the Association, the author hoped to be able to bring forward a detailed account of this curious specimen, and to exhibit it to the Section; but he considered the single fact which he had now mentioned as calculated to throw so much light on the origin of amber, that he trusted it

would induce those who are in possession of specimens to examine them with attention, and especially in reference to empty or filled cavities, and to veins or portions of foreign matter which may exist in the mass.

Remarks on the value of Optical Characters in the discrimination of Mineral Species. By Sir DAVID BREWSTER.

If minerals were all formed from solutions containing the same ingredients, having the same temperature, and crystallizing in perfect tranquillity, the differences recognised by the chemist, the crystallographer, and the optical observer would have no existence; but as this hypothetical state of the mineral, when in a state of fluidity or solution, is inadmissible, we must consider minerals as having been formed under the influence of many disturbing causes. In order to illustrate this remark, the author takes the case of *chabasie*, which he regards as a congeries of several substances, formed in succession round a central rhomb of the same mineral in a perfect state. The central rhomb has a certain degree of *double* refraction, which is equal in all parallel directions; but there is another rhomb formed round it which has a less double refraction, and each successive rhomb has its double refraction successively diminishing till it disappears altogether, at which period the form of the crystal would be a cube. Beyond this neutral line an opposite kind of double refraction appears, corresponding to a new series of rhombs, deviating more and more from the cubical structure.

Now it is very obvious that these changes may have, or rather must have, taken place, either from some agitation in the fluid which prevented its particles from assuming the perfect type of the mineral, or from the addition or abstraction of some of the ingredients of which the central rhomb was composed.

If a crystallographer, therefore, were to examine such a mineral, he would report to us only the condition of the outer rhomb, while the chemist would detail to us the elements which form the whole compound mass. The optical observer, however, is alone admitted into the secret, and his results are infallible. The changes which take place in the optical characters of minerals by heat, do not in the least affect their value, any more than similar changes affect the ordinary characters which are employed by mineralogists. The specific gravity of bodies varies also with heat, and probably the hardness also of the softer minerals; and it is well known that changes of temperature not very great may drive off the more valuable ingredients of minerals, and thus prevent the chemist from obtaining their actual composition.

Experiments on the effects of long-continued Heat on Mineral and Organic Substances. By the Rev. WILLIAM VERNON HARCOURT, *F.R.S.*

Mr. Harcourt gave an account of the experiments on this subject, which the kindness of the proprietors of the Low Moor and Elsecar iron works in Yorkshire, had enabled him to institute.

The blast furnaces at Low Moor are sometimes regularly worked for twelve years or more; but the average time for which they continue in action, is from six to seven years. The furnace at Elsecar is usually *blown out* at the end of three years. During these periods of time the fusion of the ironstone never ceases in the hearths; the *bottom stone* of the furnace may be considered as constantly kept at the temperature of melting iron, and the hearth-walls in some parts at a still higher heat. When the furnace is *blown out*, the cooling of so great a mass of masonry is extremely slow.

The *bottom stone*, which is about 16 inches thick, is worn away and excavated by degrees, sometimes to more than half its depth, by the action of the iron, so that a pool of metal lies in the hollow, below the level at which the iron is from time to time run off; this stone is cracked in various directions by the heat to which it is subjected, and the cracks are filled with veins of melted metal, which occasionally penetrate into the sand on which the stone is laid, and fuse it.

It is in the metal thus detained within the bottom stone that the segregation of metallic titanium takes place, disseminated in general irregularly through the mass of iron, but where accidental vacuities have admitted of its crystallization, forming clusters of cubes.

On examining with attention the bottom stones of furnaces which had been worked out, Mr. Harcourt observed in them several other species of crystals, some of which appeared to be owing simply to the mutual reaction of the ingredients of the stone itself. The stone is a felspathic grit, and if this material alone is capable, under these circumstances, of supplying instances of chemical and crystalline rearrangement, it seemed not unreasonable to expect, that by multiplying the means of such rearrangements scope might be afforded for the appearance of numerous interesting phænomena of a similar description.

For this purpose, and to forward an undertaking sanctioned by the Association, the Yorkshire Philosophical Society, with great liberality, furnished a supply of materials from its museum, and

with the assistance of Professor Phillips, Mr. Harcourt selected a variety of specimens of rocks and minerals from its collection, which were arranged, some in mass and some in powder, in a strong deal box, the capacity of which was five cubic feet. Some synthetical compositions of minerals were added, and opportunities contrived for the formation of others, by placing different substances in contact, and making provision for the passage of volatile bodies through materials with which they enter into union. Metallic substances were introduced at different points, and among different materials, both to serve as measures of heat, and to furnish illustrations of the phænomena of veins. A second box of equal dimensions was chiefly devoted to the purpose of placing organized substances, recent and fossil, animal and vegetable, under a variety of conditions with respect to the materials in which they were imbedded.

The boxes were conveyed to the foundries of Messrs. Hird and Dawson, at Low Moor, on the 17th of July 1833. They were placed immediately under the bottom stone of the furnace in the sand which supports it, and built round with fire-brick; larger pieces of various rocks, metals, &c. were placed by their side, and similarly inclosed by walls of fire-brick.

The defect most to be apprehended in this arrangement is, that the heat in the position above described may not suffice to produce the fusion or semi-fusion of the rocks and minerals; it is presumed, however, that the cracks of the hearth-stone and the shrinking of the materials will admit such an influx among them of the melted metal as will secure this object; but lest such should not be the case, Mr. Harcourt was anxious to effect a repetition of the experiments in a position nearer to the source of heat. An opportunity of accomplishing this was afforded by Earl Fitzwilliam at his foundry at Elsecar, near Wentworth House. Here, under the direction of the Superintendant, Mr. Hartop, holes were worked *in the bottom stone itself, and in the back wall of the hearth*, to contain the subjects of experiment; the number of holes was twenty-three, those in the bottom stone being about a foot in diameter and in depth, whilst those in the back wall were two feet in depth and six inches in diameter, worked obliquely downwards. In some of these were placed crucibles of six inches in diameter, and eight inches in height; in others similar cylinders of granite and limestone, hollowed out, and containing various materials, the spaces round, and the interstices within, being filled with powders of different kinds of rocks: in the rest the minerals and organized substances were imbedded in powders of the same kind, without any other receptacle than the stone itself.

The orifices of the holes were filled to the depth of three or four inches by stoppers made of the gritstone of which the furnace was built.

The danger in this disposition of the materials is, that a portion of them may be obliterated by the intensity of the heat and the wear of the furnace; but there is reason to expect that enough will remain to show what light is likely to be derived from such experiments, and in what manner they may hereafter be most advantageously conducted.

The experiments themselves are nearly similar to those which have been before described, the chief difference consisting in a more liberal introduction of crystals, especially of that class which includes water as a constituent part. As examples of the intention with which these were added, it may suffice to notice the selection of *natrolite*, a mineral which, if the water it contains were expelled, might be expected to pass into sommite; and of *apophyllite*, which in the same case might perhaps resolve itself into tabular spar and quartz.

The time at which the Elsecar furnace commenced working, was in October 1833; it is probable, therefore, that an examination of the hearth may become practicable before the end of 1836.

Dr. Clark gave *an account and an explanation of the successful application of the Hot Blast to the production of Cast Iron.*

In the Clyde iron-works, near Glasgow, during the first six months of the year 1829, every ton of cast iron required for its production 8 tons $1\frac{1}{4}$ cwt. of splint coal, reduced to coke, at a loss of 55 per cent.

During the first six months of the year 1830, after the application of Mr. Neilson's invention, when the air had been heated to about 300° Fahr., every ton of cast iron required 5 tons $3\frac{1}{4}$ cwt. of splint coal, converted into coke. Adding 8 cwt. of coal consumed in heating the air, the saving effected was $2\frac{1}{2}$ tons of splint coal on every ton of cast iron produced. And the same blast was found to be capable of making much more iron, the diminished requisite of air being pretty nearly proportioned to the diminished fuel required.

But during the first six months of the year 1833, when the temperature of the blast had been raised to above 600°, and when the process of coking the coal had been discovered to be superfluous, and was accordingly omitted, a single ton of cast iron was produced by only 2 tons $5\frac{1}{4}$ cwt. of splint coal. Even when we add 8 cwt. of coal to heat the air, the quantity of

splint coal required in 1833, to make a ton of cast iron, was only one third of what was used in 1829. The blast machinery continued the same, but the same blast made twice as much iron as in 1829.

The same coal produced thrice as much cast iron; the same blast twice as much.

The iron-furnaces alluded to are worked 23 hours out of the 24; a half-hour every evening, and another every morning, being occupied with letting off the iron produced.

During every working-hour, the solid materials which feed the furnace at the top amount to 2 tons almost exactly, while the air forced in at the bottom, in the same time, amounts to the surprising quantity of 6 tons.

Since a smelting-furnace must have a certain elevated temperature, in order to work it favourably, when we consider the cooling effect of 6 tons of air an hour,—2 cwt. a minute, —supplied at the bottom of the furnace, and entering near the hottest part, it is easy to account for the increased energy of the furnace when this prodigious refrigeratory is removed, by heating the air before it passes into the furnace.

On hydrated Salts and metallic Peroxides; with Observations on the doctrine of Isomerism. By Professor Graham.

Various classes of salts, besides the arseniates and phosphates, contain water, which is essential to their constitution: of this the sulphates of magnesia, and the protoxides of zinc, manganese, iron, nickel, copper, and cobalt, are examples.

These salts crystallize from their aqueous solutions, either with seven or five atoms of water, one of which is in a state of much more intimate union than the other six or four. Thus, crystallized sulphate of zinc loses six atoms of water, at a temperature not exceeding 65°, when placed over sulphuric acid in vacuo, but retains one atom of water at 410° and all inferior temperatures. This salt may be viewed as a sulphate of oxide of zinc and water, with six atoms of water of crystallization; a constitution which may be expressed as follows, $\dot{H}\dot{Z}n\dddot{S}+6\dot{H}$. This sulphate may be made anhydrous, but when moistened always regains one atom of water, slaking with the evolution of heat. This last atom of water appears to discharge a basic function in the constitution of the salt, and affords a clue to the disposition of this sulphate to form double sulphates. Sulphate of zinc combines with sulphate of potash, and forms a well-known double salt, in which the basic water of the sulphate of

zinc, is replaced by sulphate of potash, without any further change. The formula of the double sulphate is $(\dot{K}\,\dddot{S})\,\dot{Z}n\,\dddot{S}\,6+\dot{H}$. In the double salt, the whole six atoms of water are retained with somewhat greater force than in the simple sulphate; but even the double sulphate becomes anhydrous below 212° in vacuo.

The sulphates of the other metallic oxides mentioned are quite analogous to sulphate of zinc in their habitudes with water, although the particular temperature at which they part with their water of crystallization is different in each. The analogy holds also in the double sulphates of those oxides.

Of hydrous sulphate of lime, or gypsum, the two atoms of water which it contains appear to be essential, and are retained at 212°. At a temperature not exceeding 270°, this salt becomes anhydrous, but retains the power of recombining with two atoms of water, or setting. The salt is then in a peculiar condition. It is the debris of the hydrate, and not a neat chemical compound. Heated above 300° the salt becomes properly sulphate of lime, and has lost the disposition to combine with water.

The protochlorides, and corresponding cyanides of zinc, manganese, iron, &c., are disposed to combine with two atoms of water. Hence the cyanide of iron combines with *two* atoms of cyanide of potassium, to form the double cyanide of iron and potassium, commonly called the ferroprussiate of potash.

Berzelius found the peroxide of tin formed by the action of nitric acid on metallic tin, to differ in certain properties from the same compound precipitated from a persalt of tin by an alkali, and distinguished the first under the name of the nitric acid peroxide of tin. Both peroxides combine with muriatic acid, but the muriate of the nitric acid peroxide is peculiar in being insoluble in water strongly acidulated with muriatic acid. But the precipitated peroxide of tin assumes, I find, all the properties of the other modification, when kept for some time exposed to the heat of boiling water, or even when strongly dried over sulphuric acid in vacuo, at the ordinary temperature of the atmosphere. The two modifications are merely different hydrates of the peroxide of tin, but it is difficult to ascertain what proportion of water is essential to each. The hydrates, combine with acids, and form two sets of compounds; but absolute peroxide of tin itself (which is obtained by heating the hydrated peroxide to redness,) has no disposition to combine with acids. The same is true of many other metallic peroxides; they combine as hydrates only with acids. There

are at least two hydrates of peroxide of iron: the muriate of that which contains least water is red in solution, and the muriate of the other, yellow; but these muriates pass readily into each other. Mr. R. Phillips observed of the red muriate, that it is precipitated by an access of acid, which, it may be remarked, establishes an analogy between it and the muriate of the nitric acid peroxide of tin, which possesses the same property.

Metallic peroxides can in general be obtained by the application of a moderate heat to their hydrates, in a state in which they are the debris of hydrates, and not neat chemical compounds. Upon heating peroxides in this condition to redness, they generally glow or become spontaneously incandescent at a particular temperature, (a phænomenon to which the attention of chemists has been particularly directed by Berzelius,) and lose their solubility in acids at the same time. Till they have undergone this change, they are not absolute or proper peroxides. Various salts, such as phosphates, antimoniates, &c., exhibit the same phænomenon when heated; but they all had possessed water, which is essential to their first constitution, but not to their second.

The doctrine of isomerism, or that two bodies may exist of the same composition, but differing in properties, has been proposed by Berzelius as a sequence from such facts as the preceding. But the propriety of the inference may be doubted. Most, if not all cases of apparent isomerism may be explained by reference to one or other of the following facts:

1. Water is essential to the constitution of many bodies. Thus, what have been called metaphosphoric acid, pyrophosphoric acid, and common phosphoric acid, are three different phosphates of water, or compounds of one absolute phosphoric acid with three different proportions of water.

2. A particular condition of bodies must be recognised, in which they are the debris of some compound, and not proper chemical compounds of their constituents. Thus, on heating a certain borate of water and magnesia to redness, water only is expelled; but what remains is not a simple borate of magnesia, but a mixture of boracic acid and magnesia, from which the former may be dissolved out by water. Stucco in a state for setting is in this particular condition. But this is a department of corpuscular philosophy which stands much in want of further development.

3. The proximate constitution of many bodies may be widely different, of which the ultimate composition is the same. Thus the cyanic acid of Wöhler is undoubtedly an oxide of cyanogen, but we have no evidence that cyanogen exists in fulminic acid,

which consists of the same proportions of carbon, nitrogen, and oxygen as cyanic acid. It is wrong, therefore, to speak of the fulminic as a second cyanic acid, and useless to couple them together as isomeric bodies. Tartaric and racemic acids are of the same ultimate composition, but they certainly contain different radicals, and probably have as little natural relation to each other as any two vegetable acids which could be named. Why, then, associate them as isomeric bodies, and call them the tartaric and paratartaric acids?

4. A minute trace of adventitious matter may sometimes affect the properties of a chemical body to a surprising degree.

Professor Rose, of Berlin, has shown that the two kinds of phosphuretted hydrogen, one of which is spontaneously inflammable in air, and the other not so, are of the same composition and specific gravity. To account for their possessing different properties, recourse is had to the doctrine of isomerism. But my observations indicate the existence of a *peculiar principle* in the spontaneously inflammable species, which principle may be withdrawn, and leaves the gas not spontaneously inflammable. Phosphuretted hydrogen gas, which is not spontaneously inflammable in air, may be made so, by the addition to it of one ten-thousandth part of its volume of nitrous acid vapour. There are grounds for supposing that the peculiar principle of the ordinary gas is a volatile oxide of phosphorus analogous to nitrous acid, and that it is present in a minute, almost infinitesimal, proportion. Subsequently to the meeting of the Association, an account of the author's researches on phosphuretted hydrogen has been published in the number for Dec. 1834, of the *London and Edinburgh Journal of Science.*

On some new Chemical products obtained in the Gas-works of the Metropolis. By George Lowe, *F.G.S., M.R.I., M. Art. Soc., Engineer to the Chartered Gas Company.*

Mr. Lowe stated that in consequence of the recommendations adopted at the last meeting of the Association, he was induced to lay before the Section some specimens of the products of heat, obtained at the Metropolitan Gas-works. He exhibited a fine specimen of artificial pyrites, containing cubical and octahedral crystals.

These are produced by a long-continued action of fire, at a dull red heat, and are deposited on the aluminous interior coating of the cast iron pots, in which muriate of ammonia is sublimed into the sal ammoniac cakes of commerce.

The rough muriate contains also some sulphate of ammonia, and the clay soon becomes saturated with muriate of iron.

Mr. Lowe conceived that this artificial mode of producing the bisulphuret of iron, in crystals, would be an interesting fact to the geologist, as affording some confirmation of the igneous origin of *trap rocks*, in reference especially to the observation made by Professor Sedgwick and Mr. Murchison, that rocks of an aluminous nature are often found at the point of contact with basaltic matter, to be not only indurated, but to contain crystals of pyrites.

He also showed upon a portion of a worn-out cast iron retort numerous octahedral crystals of protoxide of iron, the effect of long-continued heat. Good specimens of these crystals are very rare, now that only the best iron, and that of the second melting, is used in the gas-works to which Mr. Lowe is attached. A wrought iron bolt, which had been for many hours acted upon by steam, at a bright red heat, presented a crystallized surface.

Mr. Lowe likewise laid before the Section specimens of pure Prussian blue, and of blue and green pigment, obtained from the refuse lime-water of gas-works.

This refuse was for many years allowed to run to waste into the river Thames; of late it has been evaporated under the bars of the furnaces, and passed, partly decomposed, up the chimney. It may now be rendered available for a more useful purpose.

On the quantity of Carbonic Acid in the Atmosphere. By Henry Hough Watson. *Communicated by Dr.* Dalton.

At the commencement of his undertaking, the author confined his experiments principally to the quantity of carbonic acid in the atmosphere of the town of Bolton; and then, to arrive at the difference in quality between an atmosphere in its natural purity and one like that of Bolton, which we know to be artificially impregnated, he fixed upon Horrocks Moor, a situation three miles north-west of the town of Bolton, and elevated, as he had found by barometrical observation, about 584 feet above it; and made the remainder of his experiments upon air received at this place, except that thrice during the course of his investigation he operated upon air received on the top of Winter Hill.

Winter Hill is situate from five to six miles north-west of Bolton, and about a mile north-east of the well-known Rivington Pike; its height above Bolton is about 1211 feet.

The author gives his first experiment as an example of his method of analysis. A bottle capable of holding 188·400 grains of

water was filled with air, by repeated blasts of a pair of hand bellows, and into it were poured 480 gr. measures of lime-water such as requires 460 gr. measures of test sulphuric acid, for neutralization, the test acid being such that sulphuric acid of specific gravity 1·135 constitutes $\frac{1}{100}$th part of it: 520 gr. measures of pure water were added. The mouth of the bottle was secured; and the liquor, after being frequently and well agitated, (which was done in most instances daily for a week or more,) was passed through a paper filter with the washings of the bottle; it was then found to be neutralized with only 270 gr. measures of the $\frac{1}{100}$ test acid; this being 190 gr. measures less than it would have required previously to being put into the bottle.

Now if 100 gr. measures of sulphuric acid, sp. gr. 1·135, is equal to 17½ grains by weight of real dry sulphuric acid, 190 gr. measures of the $\frac{1}{100}$ test acid is equal to 0·3325 of a grain by weight of real sulphuric acid. And taking the atomic weight of sulphuric acid at 35, and that of carbonic acid at 19·4, the 0·3325 of a grain of sulphuric acid is equal to 0·1843 of a grain by weight of carbonic acid, or 0·3921 of a cubic inch.

And deducting 1000, the bulk of the liquor put into the bottle, from 188400, the total capacity of the bottle, we have 187400, the number of water grain measures of air operated upon, =742·3 cubic inches.

Then 742·3 : 0·3921 : : 10,000 : 5·282.

Therefore, in this instance, 10,000 volumes of air contained 5·282 volumes of carbonic acid.

The following is a list of the Experiments made on Air from the town of Bolton.

Time when the air was received.	Whether a fair or rainy day on which the air was received.	Number of fair or wet days previous to that on which the air was received.	Temperature when the air was received.	Number of volumes of carbonic acid in 10,000 volumes of the air.
1832. Sept. 28th, at ½ past 5 P.M.	fair.	8 fair days.		5·282
Oct. 5th, at 1 P.M.	rainy.	1 fair day.	53°	5·282
Oct. 11th, at 10 A.M. to 3 P.M.	rainy.	4 wet days.	60	5·282
Oct. 13th, at noon.	rainy.	6 wet days.	57	4·448
Dec. 31st, at noon.	rainy.	8 wet days.	38	5·282
1833. Feb. 22nd, at 10 at night.	a little rain at night.	1 fair day.	36	5·559
March 9th, at ½ past 3 P.M.	snow.	2 fair days.	*	5·000
April 30, at ½ past 6 in the morning.	fair A.M.	4 wet days.	38	8·620
May 7th, at noon.	fair.	3 fair days.	75	5·000
May 9th, at 2 P.M.	fair till after the air was received.	5 fair days.	74	4·196
May 11th, at 12 at night.	fair.	2 wet days.	54	5·559
May 15th, at ¼ past 6 in the morning.	rainy.	1 fair day.	60	4·739
May 18th, at 12 at night.	fair.	1 wet day.	51	6·393
May 29th, at noon.	fair.	9 fair days.	60	5·838
June 4th, at 9 at night.	rainy.	2 wet days.	56	4·196
June 11th, at 10 at night.	rainy.	1 fair day.	51	4·196
July 23rd, at ½ past 1 P.M.	rainy.	5 wet days.	57	5·000
Sept. 24th, at ½ past 8 at night.	rainy.	1 fair day.	58	6·393
Dec. 14th, at noon.	rainy.	1 wet day.	46	4·448
		Mean of the above 19 experiments.....		5·300

* In this and the first experiment the temperature was not observed.

The following is a list of the Experiments made on Air from the Country.

Place where the air was received.	Time when the air was received.	Whether a fair or rainy day on which the air was received.	Number of fair or wet days previous to that on which the air was received.	Temperature when the air was received.	Direction from which the wind blew when the air was received.	Number of volumes of carbonic acid in 10000 volumes of the air.
Horrock's Moor.	1833. Aug. 1st, at ½ past 7 at night.	fair.	2 fair days.	61°	north.	3·890
Winter Hill.	Aug. 8th, at 3 P.M.	fair.	2 fair days.	55	west.	4·448
Horrock's Moor.	Sept. 6th, at ½ past 2 P.M.	fair.	1 fair day.	65	east.	4·739
Ditto.	Sept. 14th, at 3 P.M.	rainy.	3 wet days.	62	north.	3·890
Ditto.	Oct. 2nd, at 3 P.M.	fair.	2 fair days.	60	north-north-west.	4·196
Ditto.	Oct. 24th, at 3 P.M.	rainy.	6 wet days.	52	south-south-west.	4·448
Ditto.	Nov. 6th, at 2 P.M.	rainy.	4 wet days.	46	west, and very strong.	3·614
Ditto.	Dec. 6th, at ½ past 2 P.M.	rainy.	9 wet days.	40	west, and very strong.	3·614
Winter Hill.	Dec. 25th, at 1 P.M.	fair.	12 wet days.	34	north-north-west.	4·196
Horrock's Moor.	1834. Jan. 2nd, at ¼ past 2 P.M.	fair.	7 wet days.	34	north.	4·196
Ditto.	Jan. 30th, at ¼ past 2 P.M.	rainy.	1 fair day.	41	south-east.	4·196*
Winter Hill.	Feb. 11th, at ¾ past 2 P.M.	fair.	2 wet days.	40	west.	4·196
					Mean of the above 12 experiments.....	4·135

When the wind blew from the west and north-west, it was from the seaward.

The author's experiments on country air, from which only can uniform results be expected, differ very little from those of De Saussure, who found the mean of 100 experiments to be 4·15. He does not, however, agree with De Saussure in attributing the variations observable in his experiments to the variations of the seasons, and the dryness or wetness of the weather, (with which the Table shows that they do not accord,) but rather to the unavoidable errors of experiment.

* This air was received during a dense fog.

On the Chemical Composition of the crystallized Oxichloride of Antimony. By J. F. W. JOHNSTON, *F.R.S.E. F.G.S., Reader in Chemistry and Mineralogy in the University of Durham.*

When a solution of oxide of antimony in muriatic acid is diluted with water, a white powder is precipitated, which has been long known under the name of the powder of algaroth. If the diluted solution be set aside, the precipitate assumes the crystalline form, presenting the appearance either of a fine sand with little lustre, of long transparent slightly yellowish needles radiating from a centre, or of a congeries of microscopic right oblique prisms having the acute terminal angle about 84° 40′. These crystals are slightly yellowish, transparent, having occasionally a high degree of lustre, give off no water when heated, but at an elevated temperature decrepitate and emit fumes of chloride of antimony. Heated with dry, or boiled with a solution of, carbonate of soda, they are decomposed, and oxide of antimony remains. Nitric acid also decomposes them by the aid of heat, leaving antimonic acid.

Several analyses of this substance have been published, but in none of them, the author believes, was the compound employed in a crystallized state; and as it is partially decomposed by washing with water, it is obvious, that unless in this state the true constitution of the compound cannot be obtained by analysis. In four experiments, crystals prepared at different times gave the author 11·32, 11·26, 11·22, 11·215 per cent. of chlorine respectively. Of these the highest is preferred, for the reason above stated. In six experiments, by three different methods, Mr. Johnston obtained: 1st, 76·82; 2nd, 75·93, 76·506, 75·98; 3rd, 76·6, per cent. of metallic antimony. Of these he prefers the last. The compound, therefore, consists of

	Chlorine,	11·32 =	2·55 atoms.
	Antimony,	76·6 =	9·498
Loss,	Oxygen,	12·08 =	12·08
			Atoms.

or the (Cl + O) : Sb : : 14·247 : 9·498 : : 3 : 2 nearly. It consists, therefore, of oxide combined with chloride of antimony, and they are in the proportion of one atom of chloride to 4½ of oxide, or of 2 : 9. This gives the formula 2 (3 Cl + 2 Sl) + 9 $\dddot{\text{Sl}}$. The results of calculation compared with experiment are as follow:

				Calculation.	Experiment.
	1	⅔	Cl 6·63	11·49	11·32
			Sl 8·06	76·72	76·6
36·29	4·5		Sl 36·2		
		⅔	O 6·75	11·79	12·08
			57·74	100·	100·

The chlorine and antimony found by experiment are, as was to be expected, a little less than is indicated by theory, causing the amount of oxygen to appear greater than it ought to be.*

On the phænomena and products of a low form of Combustion. *By* CHARLES J. B. WILLIAMS, *M.D.*

It must have been often observed, that after a candle is extinguished in a dark room, if no spark be left on the surface, the wick continues to be, for a few instants, faintly luminous. This phænomenon attracted the author's attention many years ago, and on investigating the matter further, he found that wax, tallow, oil, resin, sealing-wax, and many other compound inflammables, are luminous in the dark, when heated to a point considerably below redness. A bar or mass of iron, heated to incandescence, and then allowed to cool till it ceases to give out light in a dark place, affords the most convenient means of exposing substances to the degree of heat required for this phænomenon. If small portions of wax or tallow be thrown on this iron, they give out a pale bluish light, which, if the heat approaches to incandescence, assumes the form of a lambent flame. Various animal and vegetable oils, resins, lac, caoutchouc, cotton, hemp, linen, paper, flour, starch, gum, silk, cloth, leather, hair, feathers, and almost all compound combustibles, exhibit, in various degrees, the same phænomenon. Sugar does so very slightly. Camphor and other volatile matters, and olefiant gas, may be made to show the light by bringing the vapour or gas into contact with a hot iron held over them. A short statement of the most material of these facts was published in the *Annals of Philosophy* for July 1823. The author has lately found that some of them were noticed by Mr. T. Wedgwood, in the *Phil. Transactions* for 1798, and were by him suspected to be "some kind of inflammation." The luminous appearance has generally, however, been considered to be of the nature of simple phosphorescence, like that of fluor spar and other minerals when heated. These substances give out light independently of access of air, and under water or oil; and the cause of this singular property, to which the term phosphorescence has been applied, is wholly unknown. On comparing this phænomenon, however, with that of heated inflammables, the author saw enough difference to induce the belief that they are not similar, but that the latter is owing to a kind of low combustion.

* For a fuller account of these experiments see *Edinburgh Journal of Science*, January 1835.

To bring this matter to the test of experiment, he tried whether the light would continue to appear when the bodies in question are heated without the contact of air. Wax, tallow, and other inflammable matters were heated in different close tubes in the dark; they were observed to give out no light until they were opened, when it appeared as usual. On closing them again, if the heat was kept up, the light gradually disappeared, but was restored on again opening the tubes to the air. A roll of paper, heated in a close tube till part was charred, gave out no light, but a piece of paper applied externally to the heated tube became immediately luminous. Some tallow heated in a ladle till it became luminous, lost its light on being plunged into carbonic acid gas.

It having thus been proved that the absence of oxygen prevented the appearance of the light, it was natural to expect that a free supply of this element would increase it. Some wax was heated in a ladle till it became luminous in the dark, and on being plunged into oxygen gas it became brighter, and if the heating had been considerable, although there was no spark, it burst out into an open flame. Wax, lac, cocoa oil, tallow, sperm oil, sulphur, and some other things could be kindled into open combustion in this way; but with paper, most vegetable oils, silk, &c. the pale light was only brightened by contact of oxygen.

The author considers it, therefore, proved that the light observed was not phosphorescence, but depended on chemical action between the air and the subject of the experiment; that it was, in short, a form of combustion. The bodies which give out most light are wax, animal oils, hair, silk, wool, fine white paper, cotton fabrics, æther vapour, olefiant gas, and sulphur. Some of these, as paper, tallow, and cocoa oil, begin to give out light in a dark room below 300°. Wax requires a temperature of at least 400°; and this, the author remarks, is the reason why wax candles burn with little or no smell, whilst, in those of tallow, portions near the wick are heated sufficiently to undergo the imperfect combustion, which causes the odour so disagreeable in an imperfectly extinguished candle. The degree of heat necessary for low combustion may be estimated by the fact, that as soon as oils or other compound inflammables begin to give out vapours, they will be found to be luminous in the dark. When, therefore, tallow or oil is heated to ebullition in contact with the air, the surface is actually undergoing combustion. If the heat be further increased, the pale luminosity elevates itself into a lambent flame, which, under circumstances favourable for the accumulation of heat, will

burst out into open ignition. It is to be remarked, however, that this low combustion differs from ordinary inflammation in its products, and that the transition from one to the other is not gradual, but abrupt, and attended with a slight explosion.

Several of the metals exhibit the phænomenon of low combustion. The action is limited in most cases by the speedy formation of a coat of oxide on the surface of the metal; but in the case of arsenic, whose oxide is volatile, a pale flame surrounds it at any temperature capable of raising it into vapour, and continues until the metal is consumed. The fresh filings of zinc, iron, cobalt, antimony, tungsten, and copper, become momentarily luminous when thrown on an iron heated below redness. Potassium presents the phænomena of low combustion at ordinary temperatures; in fact, the rapid tarnishing of its surface, after it is cut or rubbed, is accompanied by the evolution of a faint light, which becomes brighter if the temperature is raised.

The light of this low combustion is worthy of notice, which varies somewhat in different shades of pallid or bluish white. It becomes a question, what constitutes this light? That of ordinary flame is supposed to consist of minute particles of the combustible, or of its product, in a state of incandescence. Sir H. Davy extended this supposition even to the low combustion of phosphorus, attributing its feebleness of heat to the extreme paucity and tenuity of the particles of phosphoric acid thus raised to a white heat. The author conceives that if this were the cause, there would be a red tinge occasionally present, as the result of the cooling of these particles to the red degree of heat. In most ordinary flames such a red tinge occurs, and is particularly apparent in daylight, but the author has never seen it in low combustion. The lowest luminous degree of heat has commonly been stated to be red, that called by artisans *cherry red* being the first visible degree. If, however, we examine the phænomena of incandescence in a room otherwise perfectly dark, by watching a large piece of iron cool from a red heat, we shall find that, before it ceases to be luminous, *it loses wholly its red light, and appears of a pale or milky white.* This, although fainter, is precisely the colour of the lights of low combustion.

The author then drew a brief comparison between the products of low combustion, and those of fermentation and putrifaction, in which he noticed a new process for the expeditious manufacture of vinegar. In this case an infusion of malt, or sweet-wort, is made to drop through a room full of faggots of

twigs, so as to be exposed freely to the air in their interstices; and what goes in at the top as *cold-wort*, comes out at the bottom, in the course of an hour, *hot vinegar*.

Abstract of the Discoveries made by Dr. REICHENBACH, *in his examination of the products of destructive Distillation. By* WILLIAM GREGORY, *M.D., F.R.S.E.*

Dr. Reichenbach, in the course of a series of experiments, of great extent and accuracy, on this subject, has shown that the products of destructive distillation are of a very complex nature, and contain, besides a variety of principles previously known, not less than six new principles, all of which are susceptible of some practical application. These new principles are:

1. Paraffin.—This is a solid body, white, without taste or smell, soluble in hot alcohol and æther, which deposit the greater part on cooling, insoluble in water, fusible at 100° Fahr., boiling at a very high temperature, and distilling unchanged. It is not acted on by the strongest reagents, and from its permanence is susceptible of many useful applications. It burns with a bright light and without smoke. Sp. gr. 0·870.

2. Eupion.—This is a very mobile and volatile liquid, boiling at about 112° Fahr., and distilling unchanged. It is equally permanent with paraffin, and, like it, burns brilliantly without smoke. It has an extremely fragrant smell. It is more expansible by heat than any known liquid, and is the lightest known liquid under ordinary pressure, having a sp. gr. of 0·655. Its expansibility recommends it for thermometers, and it seems well adapted for burning, from the brightness and purity of the light it produces.

3. Kreosote.—This is a liquid, sp. gr. 1·037, transparent and colourless, said to combine a low refractive with a high dispersive power. It boils at 400° Fahr., and distils unchanged. It possesses a strong smell of smoke, and is the antiseptic ingredient of tar, smoke, and pyroligneous acid. It is sparingly soluble in water, abundantly in alcohol and acetic acid. It coagulates strongly the albumen of animal substances. It has been applied with success to the cure of toothache, acts as a powerful styptic, and is the active ingredient of tar, tar-water, aqua binelli, and Dippel's oil. It may be usefully employed in the art of curing hams and other smoked meats.

4. Pittakall.—This is a solid body, resembling indigo, of a splendid blue colour, passing on the polished surface into the

aspect of gold. It is not volatile when pure. It is easily fixed on cloth, and forms a permanent dye of remarkable beauty.

5. Picamar.—This is the bitter principle produced in destructive distillation. It is an oily liquid, heavier than water, boiling at a temperature above 500° Fahr. It is very permanent. It has an intensely bitter taste. From its permanence and fixity, it is well adapted for greasing machinery.

6. Kapnomor.—This is a liquid, sp. gr. 0·977, boiling at 365° Fahr. Its most important property is its power of dissolving caoutchouc. It forms the chief part of the coal naphtha employed in the arts.

Besides these new substances, Dr. Reichenbach has recognised acetic acid, pyroligneous and pyroacetic spirits, in the products of destructive distillation. He considers, and apparently with good reason, the pyroligneous spirit as a mixture of alcohol with pyroacetic spirit. The alcohol is formed by the fermentation of sugar in the sap of the wood, and distils over when heat is applied to the wood. Naphthaline is not, according to Dr. Reichenbach, a product of destructive distillation, properly so called, but is always formed when any of the products above mentioned are exposed, in the state of vapour, to a red heat.

Dr. Reichenbach has also shown that the naphtha distilled from the Italian and Persian petroleum, is not produced by destructive distillation, but is oil of turpentine unaltered, the origin of which he attributes to the pine-forests of which most coal beds are composed. Some very fine naphtha, sent by Mr. Swinton from the East Indies, was found by Dr. Christison to have all the characters of oil of turpentine. Dr. Christison supposed that this oil had been fraudulently substituted for the naphtha, but Dr. Reichenbach has succeeded in obtaining a similar oil from several species of coal, by distilling along with water, in which case no destructive distillation could occur.

One naphtha, however, from Rangoon, appears to be a product of destructive distillation. Dr. Christison discovered in it paraffin, which he called petroline, Dr. Reichenbach's experiments not being at that time known in this country; and Dr. Gregory has lately proved in it the presence both of eupion and kapnomor. There is reason to think, therefore, that this naphtha, and perhaps some others, have been produced at a high temperature.

III. MATHEMATICAL INSTRUMENTS AND MECHANICAL ARTS.

On a new Sympiesometer. By Professor Forbes, *F.R.S.*

A barometer acting by measuring the volume of a confined portion of air, first recommended by Dr. Hooke, has been recently constructed in a convenient form, under the name of the Sympiesometer, by Mr. Adie of Edinburgh. The chief difficulty found in operating with this instrument consists in ascertaining the precise temperature of the inclosed air. This is proposed to be accomplished by placing both the gaseous ball and that of the attached thermometer in one common chamber, surrounded with mercury; whilst the difference of temperature which may exist between that mercury and the external air is determined by means of a small differential thermometer. The scale of the mercurial thermometer is read downwards, and the volume of gas is indicated by a thermometric scale of its expansions under a constant pressure of 30 inches.

On the construction of Achromatic Object-Glasses. By David Dick, *Architect and Engineer, Edinburgh.*

Having several years ago attempted the construction of a triple object-glass of 4 inches diameter, of which the interior surfaces were cemented together, as recommended by the late Professor Robison, Mr. Dick found that, when the surfaces were found to coincide, it was rather difficult to separate them without scratching, and therefore preferred to proportion the radii of curvature so as to leave a small interval of the meniscus form, which was filled up with the cementing substance. This mode of construction suggested to him the possibility of employing a cementing substance having such an action on the green light, in relation to that of the two sorts of glass, that the colours of the secondary spectrum might be diminished, if not entirely removed. By referring to the discoveries of Sir David Brewster regarding the action of the different refracting media on green light, it was found that Canada balsam, oil of turpentine, and in a very high degree the oil of cassia, were all possessed of the quality sought for, and the author has in fact succeeded in the construction of object-glasses of considerable size, which produce images almost, or

perhaps quite, as free from colour as the images produced from reflection.

To prove the durability of a glass thus constructed, the author mentions the fact of a four-inch object-glass, which was put together three years ago with Canada balsam, and has been exposed to heat, cold, and solar light, without injury. In order to remove a doubt recently started on this subject, the glass has been subjected to a heat of 140° for more than half an hour, and immediately afterwards tried on the moon, when it appeared to have suffered no injury.

Considerable care must be observed in putting in the cement. It should be poured upon the centre of the concave lens, over which the centre of the convex lens being let down should be brought into contact with the cement, so as to prevent the introduction of air bubbles; the superfluous cement is then to be gently pressed out, the pressure being applied at the edges of the lenses. When this has been done, should the lenses subsequently be shifted much, or turned round their centres on each other, the distinctness of vision would almost invariably be destroyed, and is not afterwards recovered.

On a new Klinometer and portable Surveying Instrument. By John Dunn, *Optician, Edinburgh.*

[With a Plate.]

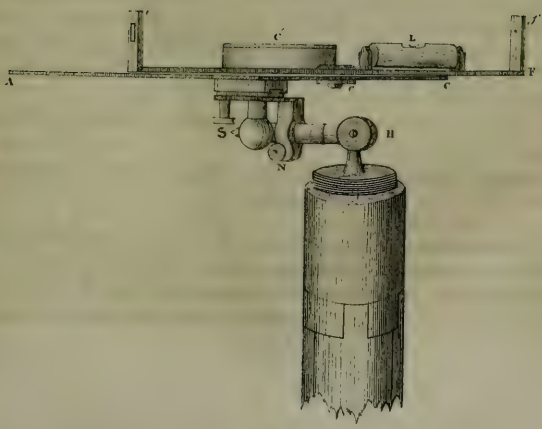
Fig. 1. represents this instrument drawn to half its real size. On the brass plate A B C D, there is traced a semicircle, divided into half-degrees, and within it a series of rectangular coordinates, commencing at the centre. Round the centre of the semicircle, an arm, E F, moves, carrying the sights *e f*, and a spirit-level L, turning on two pivots; and at one corner of the plate is placed a small compass-box, *c'*, removeable at pleasure. The plate is attached to the tripod T, fig. 2, by a universal joint, H S; and the clamping-screws, S and N, enable the observer to secure it in any required position.

To use this instrument as a klinometer, the edge A D is laid on the dip of the stratum, and the arm E F is made horizontal, by means of the level L, when the angle of the dip is indicated on the semicircle by the edge I K, and the ratios of the base to the altitude and slope of the inclined plane by the rectangular coordinates, and the divisions on the straight edge I K. For the more accurate purposes of the mining-engineer, and in cases where the dip is to be determined over a considerable extent of surface, the instrument is placed in a vertical posi-

DUNN'S NEW KLINOMETER,
and
PORTABLE THEODOLITE.

FIG. I.

FIG. II.



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tion on its stand, and the sights *e f* directed to the top of a post of equal height with the instrument, at the other extremity of the slope.

The instrument, when placed in a horizontal position, serves as a plane table; in this case the divisions on I K, and the rectangular coordinates, offer peculiar facilities for the execution of a rough survey. The distance between two stations being found on the scale I K, the readings on the rectangular lines will at once give the easting and northing of the undetermined station; these can at once be transferred to a skeleton, prepared by tracing squares on a piece of paper, and each successive step of the survey is thus completely indicated on the map. The bearings of the different lines must, of course, be noted, in order that, by setting back upon them, the observations may be rendered independent of any changes in the magnetic direction.

On a Chronometer with a Glass Balance-spring. By E. J. Dent.

Mr. Dent presented an account of the rate of this instrument, kept at the Royal Observatory, Greenwich, since the last meeting of the Association. He shewed a chronometer in motion, with a pure palladium balance-spring; and produced a table of the variations of gold, steel, palladium, and glass, from 32° to 100° Fahr.; and another table of the quantities respectively due to *direct expansion,* and to *loss of elasticity,* in steel and palladium.

On the Polyzonal Lens. By Mr. Gordon.

Mr. Alexander Gordon exhibited Moritz's modification of Fresnel's polyzonal lens, which (with a common Argand flame) is proposed as an œconomical light for ports and harbours, and to be adopted (when a more intense flame is used) for coast lighthouses, in situations where the use of parabolic reflectors is not absolutely necessary.

On an Instrument for taking up Water at great depths. By Mr. Rennie.

Mr. G. Rennie described the principle of construction, and the practical method of employing this instrument, which has been tried by him at the estuary of the Tamar, near Plymouth, and found to succeed completely.

On the application of a Vernier to a Scale, not of equal, but of variable parts, and particularly to Wollaston's Scale of Chemical Equivalents. By Professor STEVELLY.

The form of this instrument is that of a pair of *lazy tongs*, consisting of a series of parallelograms, placed in a line, angle to angle, whose diagonals, when the series is drawn closer or pushed further apart, diminish or increase, according to the same law that connects the divisions of the portion of the scale which is to be read off. An adjusting-screw passes along the whole length of the vernier; ten divisions of which being made by the adjusting-screw to equal eleven on the scale, the vernier is set for marking decimally: the marking-points are formed by thin metallic blades coinciding with the cross diagonals of each parallelogram.

IV. NATURAL HISTORY, ANATOMY, AND PHYSIOLOGY.

BOTANY.

On the plurality and development of Embryos in the Seeds of Coniferæ. By ROBERT BROWN, *LL.D. &c.*

THE earliest observations of the author on this subject were made in the summer of 1826, soon after the publication of his remarks on the female flower of *Cycadeæ* and *Coniferæ*. He then found that in several *Coniferæ*, namely, *Pinus Strobus*, *Abies excelsa*, and the common larch, the plurality of embryos in the impregnated ovulum was equally constant, and their arrangement in the albumen as regular as in *Cycadeæ*; and similar observations made during the present summer on several other species, especially *Pinus sylvestris* and *P. pinaster*, render it highly probable that the same structure exists in the whole family.

The first change which takes place in the impregnated ovulum of the *Coniferæ* examined, is the production or separation of a solid body within the original nucleus.

In this inner body, or albumen, several subcylindrical corpuscula, of a somewhat different colour and consistence from the mass of the albumen, seated near its apex and arranged in a circular series, soon become visible.

In each of these corpuscula, which are from three to six in number, a single thread or funiculus, consisting of several,

generally of four, elongated cells or vessels, with or without transverse septa, originates. The funiculi are not unfrequently ramified, each branch or division terminating in a minute rudiment of an embryo. But as the lateral branches of the funiculi usually consist of a single elongated cell or vessel, while the principal or terminal branch is generally formed of more than one, embryos in *Coniferæ* may originate either in one or in several cells, even in the same funiculus.

A similar ramification in the funiculi of the *Cycas circinalis* has been observed by the author.

Instances of the occasional introduction of more than one embryo in the seeds of the several plants belonging to other families have long been known, but their constant plurality and regular arrangement have hitherto only been observed in *Cycadeæ* and *Coniferæ*.

On the Cocculus Indicus *of Commerce. By* G. A. W. Arnott, *M.D.*

In Wight and Arnott's *Prodromus Floræ Peninsulæ Indiæ Orientalis*, vol. i. p. 446, the *Menispermum Cocculus* of Linnæus, the *Cocculus tuberosus* of De Candolle, or the *Cocculus indicus* of commerce, is removed from the genus *Cocculus* as constituted by De Candolle, although De Candolle considered it the type of that genus, and placed in the *Anamirta* of Colebrooke. (*Linn. Soc. Trans.* xiii. pp. 52 and 66.) No reasons for this change are there given, and it is the object of this paper to state them. The proof depends, 1st, On the general accuracy of the figure of the fruit given by Gærtner (*De Fruct. et Sem.* i. *t.* 70. *f.* 7.), which is presumed to have been taken from a berry obtained from a shop; 2nd, On the correspondence of that figure with berries of the officinal plant procured from the museum of the materia medica class in the University of Edinburgh; 3rd, On the correspondence of the above-mentioned figure and berries with fruit in Arnott's herbarium, and which fruit still remains attached to a branch with its leaves; 4th, On a specimen of the male inflorescence, which comes from the same district as that in fruit, and exactly resembles it in every point, except having male flowers instead of berries; 5th, On a comparison of flowering male specimens from the botanic garden of Calcutta, in the herbarium of the Linnæan Society of London, and which specimens were derived from berries planted in that garden by Dr. Roxburgh, and transmitted to him by Heyne, from Malabar, as the plant of commerce; 6th, On the total dissimilarity of the male flowers from those of the genus

Cocculus as characterized by De Candolle, and their exact coincidence with those described by Roxburgh under his *Menispermum heteroclitum* (*Fl. Ind.* iii. p. 817.), and figured by him among his drawings in the East India Company's museum (*n.* 130.) under the name of *Men. monadelphum*, and of which Colebrooke has constituted his genus *Anamirta*. Mr. Colebrooke has named this species *An. paniculata*, but Mr. Arnott considers it better to resume the Linnæan appellation, and call it *An. Cocculus*.

Mr. Arnott also remarked, in the course of the paper, that although the order *Menispermaceæ* has been described by De Candolle, Ach. Richard, Lindley, Hooker, and by himself, as well as by most other writers on the subject, as having either (and usually) no albumen, or in small quantity, it is in reality almost always present, and of considerable thickness; and, indeed, in an examination of many species of the order, he has only yet discovered one in which it does not exist.

On Excretions from the Roots of Vegetables. By C. Daubeny, *M.D., Professor of Botany, Oxford.*

Dr. Daubeny described the experiments which he is now carrying on, in compliance with the recommendation of the Botanical Committee of last year, (*see Report of the Third Meeting*, p. 484.) with the view of ascertaining in what manner and to what extent particular plants deteriorate the soils on which they grow. The results of the experiments will be laid before a future meeting of the Association.

On the Distribution of the Phænogamous Plants of the Faroe Islands. By W. C. Trevelyan, *F.R.S.E. &c.*

The author states that the number of species is 271, of which 84 are monocotyledonous, and 187 dicotyledonous. (See the *Edinburgh Journal of Science.*)

ZOOLOGY.

On the Propagation of certain Scottish Zoophytes. By John Graham Dalyell.

The author commenced his illustrations of this subject by a few preliminary observations on the *Actiniæ* and the *Hydræ*, animals whose structure exhibits many analogies, though standing far apart in the artificial *Systema Naturæ*.

1. The *Actinia equina* is nearly cylindrical, the upper margin begirt by a triple row of 70 or 80 tentacula, with 30 or 40 purple tubercles at their root. A purple ring encircles the base, and there are two purple patches on the mouth. All the remainder is brown, speckled with green. Food is seized and conveyed to the mouth by the tentacula; smaller portions are absorbed into the system without any visible residue: the tubercles open to discharge purple flakes, after moderate supplies; but larger quantities are rejected in the form of a ball, digestion having probably operated on the surface only.

This animal is viviparous, though the fact is to be very rarely witnessed. The embryos, one or more, appear first in the tentacula, from whence they can be withdrawn, and transmitted to others by the parent, and are at last produced by the mouth. In the course of six years, a specimen, preserved by the author, produced above 276 young; some pale, and like mere specks, with only eight tentacula, others florid, and with twenty. They are frequently disgorged along with the half-digested food, 38 appearing thus at a single litter. An embryo extracted artificially from the amputated tip of a tentaculum, began to breed in fourteen months, and survived nearly five years. Monstrosities by excess are not uncommon among the young: one produced naturally, consisting of two perfect bodies, and their parts sustained by a single base, exhibited embryos in the tentacula at ten months, bred in twelve, and lived above five years. While one body was gorged with food, the other continued ravenous.

2. *Hydra tuba*, the *trumpet polypus*, thus denominated from its form, inhabits the Frith of Forth, near Edinburgh, where its natural abode seems the internal concavity of the upper oyster-shell. Removed to an artificial site, it suspends itself by its narrow base, while the long slender tentacula, above thirty in number, descend two inches, to wave as a beautiful white silken pencil in the water. Thus it is by much the largest of the *Hydræ* proper.

This animal is alike voracious as the former. Its colour, naturally a dingy white, is affected by the quality of the food, and the fertility of both species is dependent on the quantity of nutriment. The flesh of muscles seems that which is most acceptable to many of the small aquatic animals.

The embryo originates in a rude organic mass, as an external bud, near the base of the parent. Prominences above soon indicate incipient tentacula surrounding the mouth, while the lower part remains united by a ligament, which gradually decreases until it is ruptured, as the embryo withdraws to esta-

blish itself independently. It never drops from the parent, but often, when yet immature, buds are germinating from its sides, while the opposite side of the parent enlarges also; the whole presenting a shapeless and distorted congeries, which is refined by time into separate, distinct, and perfect animals. A single specimen had eighty-three descendants in thirteen months, nor were its prolific powers then exhausted.

Some authors have maintained that the *Hydræ* of fresh water propagate by buds at one season, and by *ova* at another. Nothing of this alternation occurred in the course of the author's observations, protracted during five years, on an original group, and the posterity of these marine *Polypi*.

The locomotive faculties of the *Actiniæ* and the *Hydræ* are exercised very seldom, and on the most limited scale.

3. *Tubularia indivisa.*—A splendid stem rooted below, and rising thirteen inches high, is crowned by a scarlet head, bearing some correspondence with the structure of the former animals. The mouth is situated in the centre, amidst forty or fifty less active filaments, and the margin is surrounded by thirty or thirty-five tentacula, expanding fourteen or fifteen lines between their opposite tips. A tenacious yellowish matter fills the tubular stem, which is frequently discharged in considerable quantities if the root be ruptured.

Splendid groups are formed by fifty or even a hundred specimens of this zoophyte, in immediate approximation.

An ample ovarium, resembling clusters of grapes, is borne externally on the head, and weighing it down by its exuberance. On approaching maturity the ovum drops from its cluster for evolution below, where slight prominences soon denote incipient tentacula, as in the *Hydræ*, while incorporated with the parent. But, as they extend, a knot enlarges their extremities, contrary to their ultimate acute formation in adults. Next, the nascent animal, reversing itself, enjoys the faculty of progression by means of the inverted tentacula, as on so many feet, apparently to select a site; when, again resuming the natural direction with the extremities upwards, the lower surface fixes itself below, and roots there for ever: meantime elongation of the stalk raises the head amidst its watery element.

It is evident, therefore, that the *Tubularia indivisa*, though subsequently rooted as a plant, is originally of animal nature exclusively.

The head is deciduous, generally falling from the stem a short time after removal from the sea; but regeneration ensues at intervals of from several days to several weeks, though the successive heads are never equally luxuriant, nor bear a prolific

ovarium like the first. The number of tentacula decreases progressively. Another smaller species, here denominated, provisionally, *Tubularia polyceps*, sometimes occurring in groups of five hundred heads, propagates after the same manner. The head is regenerated also, and under a similar deficiency. A specimen had originally twenty-one tentacula, but only sixteen were renovated with the second head; and with the seventh they had diminished to six. The head evidently rises as a bud within the tube from below, and its origin seems dependent on the subsistence of the internal tenacious matter.

Regeneration may be effected artificially, and even to redundance, beyond the apparent provision of Nature. Thus, twenty-two heads were obtained in five hundred and fifty days, from three sections of a single specimen. An equal number was never reproduced by any specimen preserved entire.

3. *Sertularia.*—The most luxuriant of this diversified genus may be compared to leafless shrubs in miniature, composed of stem, boughs, branches, and twigs, all clothed with variously shaped prominent cells, the habitation of so many *Hydræ* or *Polypi*, capable of protruding from them. These are generally white, or of a light grey or green colour; some more sluggish; others very vivacious, having from eight to twenty tentacula in a single row, with the mouth dilating, as a cup, in the centre, to absorb the prey. None exceed a line in diameter. The whole product is tubular, and occupied by an internal pith or animal substance, with which the polypi are connected, and on the presence of which in their vicinity their survivance depends. The largest hitherto obtained by the author from the Scottish seas, is here denominated, provisionally, *Sertularia Uber*; it rises nearly three feet high, by the slenderest stem, thus exceeding greatly the dimensions usually ascribed to these zoophytes.

Besides a profusion of cells, many specimens, of various species, bear an indefinite number of vascular substances, three or four times their size, or even larger, and of greatly diversified configuration and arrangement. They are spherical, ovoidal, formed as a vase or as a Florence flask, indented, irregular, with an orifice at the summit or in the side. It does not appear that all specimens of the same species bear uniformly the same kind of vesicle. They abound at every season, probably from the nearly equable temperature of the sea, subsisting long if undisturbed; but their origin is never to be witnessed.

Each vesicle contains from one to thirty white, grey, reddish, green, or yellow corpuscula, the number, and perhaps the size, depending partly on the species of *Sertularia*. However,

white and yellow respectively occur on different specimens of the same species, and they are the most common of all.

Preceding naturalists have maintained that the *vesicles* are the *ovaria*, and the corpuscula the *ova*, whereby *Sertulariæ* are propagated. But the author's experiments and observations greatly multiplied during many years, on many specimens of various species, have not led the author directly to the same conclusion.

The contents of the vesicle are not distinguished originally by any definite form. At a certain stage they are recognised as spherical or ovoidal corpuscula, the former being their earliest sensible shape. In advancing somewhat further, they resolve into spherical triangular prisms, betraying evident animation by extension and contraction; and motion commencing, places are interchanged among them. At length, attaining maturity, they issue from the orifice of the vesicle, not as young *Sertulariæ*, but as a race of perfect animals, bearing many features of the *Planaria*, and which may constitute a new genus, to be denominated *Planula*.

The vesicle now remains empty and transparent, nor is it known to be replenished by another brood; but occasionally a small portion appears in the centre, as of a slender twig having penetrated upwards.

These creatures are produced in extraordinary profusion. Above 1200 have issued from the vesicles of a portion of the *Sertularia Uber*; and multitudes, in still greater proportion, from those of others. But only one was contained in the vesicles of the *Sertularia abietina* examined, and in those of several species which did not attain maturity.

Ordinarily the *Planulæ* are white or yellow, opake, smooth, and flattened, somewhat triangular, tapering from the head which is always obtuse, downwards to the lower extremity, and extending from inferior dimensions to rather above a line. Those of certain species are pyriform, and of others linear, with obtuse extremities. Neither eyes nor any external organs have been discovered.

The motion of the *Planulæ* is smooth and gliding; they crawl actively over the bottom of the containing vessel or up its sides; they suspend themselves in the water by an invisible thread, as the *Planariæ*, and like them swim supine.

But in a few days their motion relaxes; they become sluggish and stationary; their figure alters, and they die, yet without that speedy decomposition incident to *Planariæ*.

Very soon afterwards a circular spot or low spherical segment of the same colour, white or yellow, is observed in just about

the identical place where the *Planula* perished. A short spinous prolongation rising from the centre, becomes a stalk, with an enlarging summit, which, forming into a cell, in a few days more bursts, to display a living polypus in full activity.

While scarcely mature, other buds are germinating along with further extension of the parts, and quickly perfected as so many more cells with their polypi; and thus by progressive multiplication is the entire specimen produced under its proper aspect.

Meantime the circular spot below, invariably white or yellow, according to the *Planula*, is losing its opacity; it breaks into divisions resembling radicles, still confined within its margin, and at last disappears in tenuity.

Thus, with the precaution of selecting specimens bearing prolific vesicles,—those exhibiting corpuscula to the eye,—a whole forest of nascent zoophytes may be easily obtained.

The author cannot affirm that amidst a multitude of observations he has ever witnessed their origin, under other conditions than the presence of the *Planulæ*, and these have been afforded by eight or ten species of *Sertulariæ*, vegetating as parasites, or, independently, from solid foundations.

Nevertheless, as truth is the sole purpose of scientific research, several difficult questions must be offered for solution. 1. When does the vesicle originate? 2. Is it deciduous and regenerated? 3. Does it include some invisible pericarp or true ovarium, containing the elements of the progeny of the *Sertularia*? 4. Is their maturity indicated by the presence of the *Planula*? 5. Does its escape by the orifice of the vesicle promote their discharge? 6. Are these elements absorbed by the *Planula* while in the vesicle, and their evolution afterwards promoted by its death?

4. *Flustra carbasea.*—The genus *Flustra* is of more simple structure, and consists of fewer parts than *Sertularia*.

The *Flustra carbasea* rises from the root by a short flattened stem, with a stout yellow margin, simply as a leaf consisting of foliaceous subdivisions, free at the margin, as they are susceptible of enlargement. One surface is covered by cells of a shuttle, or rather a slipper shape, the edges of the whole forming that surface, level, not prominent. Each cell is composed of a broad flattened top connected with the bottom by sides like those connecting the back and belly of the violin, and there is an aperture above towards one extremity for protrusion of the polypus, which, affixed by the posterior part, reposes within, folded as the letter S, and when active extends to display about twenty-two tentacula. The leaf rises vertically, and the

protrusion of the polypus is horizontal, or at right angles to its surface.

Cells are occasionally occupied by a large, irregularly round, solid, yellow, ciliated animalculum, afterwards quitting them to swim heavily below. Its motion relaxes, it becomes stationary, and dies, like the *Planulæ*, without speedy decomposition.

In just about the same spot also where the animalculum became quiescent, a yellow nucleus is soon discovered, with a pale diffusing margin. This enlarges as the nucleus declines; it gradually approximates the shuttle or slipper form of a cell, and, converted to such, it gives birth in nine or eleven days to a polypus. The adult *Flustra* was vertical, but the new cell is horizontal. One extremity, however, is already rising vertically, which, extending after a similar fashion, proves the *nidus* of a second polypus in nine or eleven days more. The protrusion of the two animals now shows them at right angles to each other. But as if the existence of the first were only a sole or foundation for securing the superstructure in its growth, it perishes as a third cell with its polypus forms above the second by enlargement of the leaf.

Thus there seems to be some relation between the spherules occupying the cells and the originating *Flustra*; but equal difficulties require solution here as with the preceding race.

5. *Cristatella mirabilis.*—Naturalists, attracted by the singular diversity of structure in the genus *Sertularia*, and too readily satisfied with mere external aspect, have devoted infinitely more attention to the simple skeleton or tube than to the animated parts. In as far as the author is aware, the tentacula of all their polypi, together with those of the *Flustra*, *Tubularia*, the *Alcyonium*, and *Pennatula*, are disposed in circular arrangement, the mouth being in the centre. Several zoophytes of very different conformation inhabit the ponds, the lakes, and the streams of Scotland; among these the propagation of the *Cristatella mirabilis* is chiefly considered by the author, as the product itself seems to have eluded the research of previous observers.

Perfect specimens occur from six lines to twenty-four in length, by two or three in breadth, of a flattened figure, fine translucent green colour, and fleshy consistence. Some of the shorter, tending to an elliptical form, may be compared to the external section of an ellipsoid; but those of the largest dimensions are linear, that is, with parallel sides and curved extremities.

The middle of the upper and the whole of the under surface

are smooth; the former somewhat convex, occasioned by a border of 70 or 80, or even of 350 individual polypi, disposed in a triple row. Their number depends entirely on the size of the specimen,—increasing as long as it grows.

This product is endowed with the faculty of locomotion, either extremity indifferently being in advance; but its progression, uncommonly slow, seldom exceeds an inch in twelve or twenty-four hours.

Each of the numerous polypi, though an integral portion of the common mass, is a distinct animal, endowed with separate action and sensation. The body, rising about a line by a tubular fleshy stem, is crowned by a head, which may be circumscribed by a circle as much in diameter, formed as a horseshoe, and bordered by a hundred tentacula. Towards one side the mouth, of singular mechanism, seems to have projecting lips and to open as a valve; folds up within, conveying the particles which are absorbed to the wide orifice of an intestinal organ, which descends perhaps in a convolution, below, and returns again to terminate in an excretory canal under the site of the tentacula. Probably the whole race of *Cristatellæ* is distinguished by a similar conformation.

The polypus is a very vivacious animal, quickly retreating for security when alarmed, and rising to expand in activity. Though each be endowed with independent life, sensation, and all the motions that can be exercised without actual transition, the whole are subjected to the volition of the sluggish mass in respect to progression:—They are borne along with it.

A specimen having been cut transversely asunder, each portion seemed to recede by common consent; but both survived, as if sustaining no injury. Neither is any polypus affected by the violence offered in its vicinity.

Twenty, thirty, or more lenticular substances, of considerable size and in the most irregular arrangement, imbedded in the flesh, are exposed through the translucent green of the animal. Its death and decomposition towards the end of autumn liberate them to float in the water. Subjected to the microscope, or, indeed, to the naked eye, their convex surfaces prove brown, the circumference yellow, and begirt with a row of spines, terminating in double hooks. Each is an ovum of the *Cristatella*, with a hard shell, and occupied by yellowish fluid contents.

In five or six months the ovum gapes at one side to allow the protrusion of an originating polypus, which by a remarkable provision of Nature now floats reversed, with the head downwards, to ensure absorption of the liquid element below. On

quitting the ovum it attaches itself to some solid substance by the base, then disproportionately large, from which a second polypus quickly rises, then a third, and a fourth; and thus with others. In earlier stages the *Cristatella mirabilis* seems to be of a circular figure, and in its most mature state there is a margin projecting beyond the root of the polypi.

6. *Cristatella paludosa.*—The indistinct descriptions of authors embarrass naturalists excessively in their endeavours to recognise the lower animals; and this will be found one principal source of multiplied synonyms, and of the errors sometimes unjustly charged on the framers of each *systema naturæ.*

That which is here designated the *Cristatella paludosa* appears generally as a grey gelatinous mass, overspreading the surface of fresh or faded leaves in a single stratum, and possibly thickening into a blackish spongy substance with age. During earlier stages, while merely superficial, it invests the under surface of the growing leaf in an irregular stellate figure with diverging points. When larger it extends into an area equal to two or three square inches over one or both sides, especially of the leaves which have fallen, and, unlike the *Cristatella mirabilis*, it is affixed in firm and permanent adhesion.

The whole is studded with white specks, proving under the microscope to be as many polypi, intimately resembling the former in their general structure and nature; but they are infinitely smaller: more than one is incorporated with a common portion; nor do the tentacula bordering the crescent exceed forty-four or forty-eight. The arrangement of the intestinal parts and their functions seem the same.

Numerous minute ova, resembling the former, but not exceeding a tenth part of their size, and destitute of circumferential spines, are dispersed throughout the greyish mass, being more accumulated towards the white bases of the polypi. They seem to escape from the recent product by transmission through the tubular body of the polypus, passing between the side and the intestinal organ, to be discharged somewhere above. The ova escape from the recent *Cristatella mirabilis* also, but in what manner has not been ascertained.

Multitudes, liberated as the *Cristatella paludosa* breaks up in decay, are usually attracted to the side of the vessel by the curve which is formed there by the water, besides some remaining at the surface. The ovum gapes as before, and the two halves sunder to give birth to a single polypus, often in a few days after discharge or liberation. The nascent animals are affixed permanently to the first spot they reach, and in the course of increment their bodies seem incorporated together.

7. It would require a separate dissertation to illustrate the mode of increment peculiar to zoophytes in detail, and to describe their extraordinary reproductive properties.

The stem of the *Tubularia indivisa* elongates only during the subsistence of the head. This having fallen, it remains stationary. The elements of this important organ, the receptacle of food and the source of the progeny, seem dispersed throughout the stem, and it is regenerated from the residue left by sections very near the root. The original cell of the nascent *Sertularia polyzonias* is accompanied by a diminutive twin invested by a common membrane; and one is always forking off in future increment as the other gains maturity. Sometimes polypi are regenerated in the vacant cells of *Sertulariæ*, provided the pith be entire; but the reproductive powers succeeded by violence are not displayed as in the *Tubulariæ*.

Wounds and lacerations, inevitably destructive to the larger animals, are suffered with impunity by those allied to the *Hydra*, and in promoting the evolution of dormant parts denote that the principle is there. In others of the lower tribes, such as those now denominated *Annulosa*, it is doubtful whether the elements of the entire animal do not even reside in every segment. The *Amphitrite ventilabrum*, which attains twelve or fifteen inches in length in the Scottish seas, regenerates either the higher or lower extremity indifferently when mutilated. The author has found also that very small intermediate sections near the extremity of this and other species regenerated the beautiful complicated anterior plume of branchiæ, and the posterior glandular parts, perhaps aiding the construction of the tube. While the body remains a fragment, the former is disproportionately large, nor can its singular *mechanical* properties be exercised when the redintegrated animal is dislodged from its original dwelling.

However luxuriant a zoophyte may appear in ultimate maturity, though consisting of hundreds of naked animals as in the *Cristatella* and *Alcyonium*, or of a thousand cells with their polypi as in the *Sertularia* and *Flustra*, the origin of all is one only. Perhaps the formation of the cell and the other inanimate parts undergoes some modification with the age of the product: the animal with which it originates is equally large, if not larger than any of its successors.

All the products described in this memoir, except the *Cristatellæ*, dwell in the sea, from whence their recovery is often as much the consequence of accident as design. Most of the preceding results have been verified only with years of observation.

On the Transformations of the Crustacea. *By* J. O. WESTWOOD, *F.L.S. &c.*

The object of this communication having reference to one of the three queries relative to the annulose animals, proposed by the Association, was to endeavour to prove the correctness of the views of Rathke, and the consequent want of foundation of those of Thompson:

1st, By a summary of the recent authorities particularly bearing upon the question; and 2ndly, by a statement of some facts which had come under the notice of the author himself.

In the former branch of the subject were mentioned the dissertation of Rathke upon the development of the ova of *Asellus aquaticus*; the memoir of Dr. Zencker upon the *Gammarus Pulex*; and the more generalized memoir of M. H. Milne Edwards, of which a report by M. St. Hilaire has been published in the *Annales des Sciences Naturelles*, in which, however, the nature of the transformations of the genera *Cymothoa, Cyamus*, and *Phronima* is particularly noticed.

From these works, as well as from Mr. Thompson's own memoirs on *Mysis* and *Artemia*, and Mr. Coldstream's paper upon *Limnoria terebrans*, (*Jameson's Edinburgh Journal*, April 1834,) it is evident that although the more typical *Crustacea* (*Malacostraca*) undergo a series of moultings, whereby an increase of size, and sometimes a slight increase in the number of locomotive organs are obtained, yet there is no violent change of form similar to the metamorphoses of insects; such, in fact, as it is asserted by Mr. Thompson that even the more typical *Crustacea* undergo.

With reference to the second branch of his notice, the author stated that although the land crab of the West Indies was that particular species upon whose habits Mr. Thompson more especially dwelt, as indicating the necessity of metamorphosis in *Crustacea*, he had obtained from the collection of the Rev. L. Guilding specimens of the ova and young, just hatched, of that species, and which he had himself extracted from beneath the abdomen of a female, where many hundred others were deposited, the young having all the appearance of the perfect animal, and not a single *zoe* being present. He had also obtained from the same collection *zoes* nearly an inch long, rather too large to admit of the supposition that they would subsequently be transformed into crabs, and dwindle into the size of the young ones just noticed.

Thus types of all the great divisions of the *Malacostraca* have been ascertained to undergo no metamorphosis;

The *Brachyura* being represented by the Land Crab,
The *Macrura* ———————— Cray-fish,
The *Schizopoda* ———————— *Mysis*,
The *Amphipoda* ———————— { *Gammarus* and *Phronima*,
The *Lœmodipoda* ———————— *Cyamus*,
The *Isopoda* ———————— { *Asellus*, *Cymothoa*, and *Limnoria*.

The author, in conclusion, suggested that there might possibly be some parasitic connexion between *Zoë* and the *Crabs* whereby Mr. Thompson's statements might be accounted for, adding that precisely analogous case exists in the young of the Coleopterous genus *Meloë* and the *Pediculus Melittæ*.

Observations on the Orbital Glands in certain tribes of Birds. By P. J. Selby, *F.R.S. &c.*

In this paper, after adverting to the little attention hitherto paid by naturalists to these glandular bodies, or their supposed use in the œconomy of the birds in which they are found, the author proceeds to point out their situation, &c., and to show that they secrete an oily fluid of a peculiar quality, which fluid is distributed, by appropriate ducts, over the eyes, and serves to defend them from the action of the water, in which the birds possessing the glands usually reside, or at least are in the frequent habit of procuring their food; that all birds belonging to the order *Natatores*, hitherto examined, possess the glands, developed to a greater or inferior extent as their habits are more or less aquatic; that they are largest in the habitual *Divers*, and in such as feed with the head submerged; that they also exist in many species of the order *Grallatores*, but only in such as are well known to submerge the head in search of food, by probing the sand &c. beneath the surface of the water. After instancing several examples belonging to both orders, and contrasting the size of the organ with the known habits of the birds, he further suggests that this oily fluid may be more especially secreted to protect the eye from the effects of saline or sea water, as the development of the gland appears in a great degree to be regulated by the marine habits of the birds, and that its mode of action is that of a thin and transparent varnish spread over the globe of the eye. The structure and form of the gland are then described, and the course of the excretory ducts pointed out.

Notice of Birds observed in Sutherlandshire, June 1834. *By* P. J. Selby, *F.R.S. &c.*

Ordo Raptores.

Fam. Falconidæ.

1. *Aquila Chrysaëtos.*
2. *Haliaëtus albicillus.* } Common in the North and West of Sutherland; very destructive to sheep and lambs. Premiums paid for their destruction: 171 full-grown birds killed within the last three years.
3. *Pandion Haliaëtus.* Common on the north-west coast.
4. *Falco peregrinus.*
5. *Falc. Tinnunculus.* Abundant.
6. *Milvus vulgaris.*
7. *Buteo vulgaris.*
8. *Circus cyaneus.* Only one individual seen.

(*No Strigidæ seen.*)

Ordo Insessores.

Tribus Fissirostres. Fam. Hirundinidæ.

9. *Hirundo rustica.*
10. *Hir. urbica.* Smoo cave, and limestone rocks, Inch-na-damff.
11. *Hir. riparia.*
12. *Cypselus murarius.* Smoo cave.

Fam. Todidæ.

13. *Muscicapa Grisola.* Rose Bank, south of Sutherland.

Tribus Dentirostres. Fam. Merulidæ.

14. *Merula musica.* Abundant more to the northern extremity of Sutherland, wherever birch coppice abounds. All of the common species, and no appearance of a smaller kind, called by Mr. Laidler the *little brown thrush.*
15. *Mer. vulgaris.* Rare.
16. *Mer. torquata.* Abundant in all the mountainous parts.
17. *Cinclus aquaticus.* Now becoming rare, as it is destroyed by every device, from an idea that it feeds upon the salmon spawn. This is not established.

Fam. Sylviadæ.

18. *Sylvia phragmites.* Met with throughout the greater part of Sutherland.
19. *Sylv. Trochilus.* Ditto, wherever birch abounds.
20. *Curruca cinerea.* In the South of Sutherland.
21. *Erythræa Rubecula.*
22. *Phœnicura Rutacilla.*
23. *Saxicola Œnanthe.* Very abundant throughout the county.
24. *Sax. Rubetra.*
25. *Sax. Rubicola.*

26. *Motacilla alba.* Common.
27. *Mot. Boarula.* In various parts.
28. *Anthus pratensis.* Very abundant.
29. *Accentor communis.*
30. *Parus ater.*
31. *Par. cœruleus.*

Tribus CONIROSTRES. Fam. FRINGILLIDÆ.

32. *Alauda arvensis.* Abundant.
33. *Emberiza miliaria.*
34. *Emb. citrinella.*
35. *Emb. Schœniculus.*
36. *Fringilla cœlebs.*
37. *Passer communis.*
38. *Linaria cannabina.* Rare.
39. *Lin. montium.* Very common.
40. *Lin. minor.* In birch woods.

Fam. CORVIDÆ.

41. *Corvus Cornix.* Common.
42. *Cor. frugilegus.* On the southern border only.

Fam. STURNIDÆ.

43. *Sturnus vulgaris.* At Smoo Cave and Scourie.

Tribus SCANSORES. Fam. CERTHIADÆ.

44. *Troglodytes europæus.*

Fam. CUCULIDÆ.

45. *Cuculus canorus.* Very abundant.

Ordo RASORES.

Fam. COLUMBIDÆ.

46. *Columba Livia.* Common; caves upon the coast.
47. *Col. Palumbus.* Rare.

Fam. TETRAONIDÆ.

48. *Tetrao Tetrix.*
49. *Lagopus scoticus.*
50. *Lag. mutus.*
51. *Lag. rupestris.* Killed upon the Benmore (Assynt) range.
52. *Perdix cinerea.*

Ordo GRALLATORES.

Fam. CHARADRIADÆ.

53. *Charadrius pluvialis.* Very abundant. Breeding upon the heather.
54. *Char. Hiaticula.* Ditto.
55. *Vanellus cristatus.* Ditto.
56. *Hæmatopus Ostralegus.*

Fam. Scolopacidæ.

57. *Scolopax Gallinago.* Abundant. *Scolopax Gallinula* is also said to breed near Tongue, but we did not meet with it.
58. *Totanus Calidris.*
59. *Tot. Glottis.* Breeds in Sutherlandshire. Breeding-station previously not known. Young procured.
60. *Tringa variabilis.* Abundant.
61. *Numenius arquatus.* Ditto in particular districts.
62. *Num. Phæopus.* Rare.

Fam. Rallidæ.

63. *Fulica atra.* Rare.

Ordo Natatores.

Fam. Anatidæ.

64. *Anser Segetum.* Discovered breeding on many of the lochs, viz. Lochs Shin, Laighal, Barncuh Naver, &c. Young in the downy state procured.
65. *Anas Boschas.*
66. *Mareca Penelope.* On many lochs. Breeding. Nest and eggs found for the first time in Britain.
67. *Fuligula marila.* Found breeding for the first time near to Enboll, in a small freshwater loch.
68. *Mergus Serrator.* Common upon all the lochs.
69. *Merg. Merganser.* Rare.

Fam. Colymbidæ.

70. *Colymbus glacialis.* Pair in summer plumage: seen in Balnachiel Bay.
71. *Col. arcticus.* This beautiful species was discovered for the first time in Britain. Breeding in most of the freshwater lochs. The eggs and young were procured and two old birds killed. Plumage of both *sexes* alike. Eggs deep oil green with darker blotches.
72. *Col. septentrionalis.* Seen in different lochs, but no nest found.
73. *Podiceps minor.* Rare.

Fam. Alcadæ.

74. *Uria Troile.*
75. *Ur. Grylle.* } Upon the southern coast.

Fam. Pelecanidæ.

76. *Phalacrocorax Carbo.*
77. *Phal. cristatus.*
78. *Sula Bassana.* Northern coast.

Fam. Laridæ.

79. *Larus marinus.*

80. *Larus argentatus.*
81. *Lar. fuscus.* }
82. *Lar. canus.* } Breeding in various places.
83. *Lar. ridibundus.* }
84. *Sterna arctica.*
85. *St. cantiaca.*

Observations on the Salmonidæ *which were met with during an excursion to the North-west of Sutherlandshire in June* 1834. *By Sir* WILLIAM JARDINE, *Bart.**

A short excursion was undertaken to Sutherlandshire in June last for the purpose of examining the natural productions of the county, for which object, including the examination of the fisheries, every facility, by permission, and direction to the tacksmen, was afforded by the Duchess of Sutherland.

The county of Sutherland having a large range of sea-coast indented with innumerable bays, its shores were a favourite resort of the Salmon, and the fisheries were valuable and carried on extensively. On account of the deterioration of them of late years, the Duke of Sutherland took them entirely under his own direction two years since. The *close* time was regulated according to the season of *running* in the different rivers. The fish were strictly preserved, and in several rivers the Gilse were all permitted to *run*. This year (the second of the improved management) the produce was in many streams doubled. Experiments were also instituted (principally in the Laxford by Mr. Baigrie) to ascertain whether the Gilse returned to the river the same year in which they were spawned; and the fact that they did so was satisfactorily established. The general weight of those that returned first was from three to four pounds. It may be here remarked that the salmon is often taken on the Sutherland shores, at the Haddock lines, baited with sand-eels, and in the Durness Firth with lines set on purpose with the same bait; thereby disproving Dr. Knox's hypothesis that their only food *in the sea* consists of *Entomostraca* and the ova of star-fish.

Of the *Migratory Salmonidæ*, that of next importance to the Salmon is what in all the North Highlands is called the *Sea Trout*, distinguished by the tacksmen as the larger and smaller kinds, the first entering the rivers about the commencement of June, the second about the middle of July. The first or largest fish was thought to be identical with the Tweed *Whitling*; 300

* The gentlemen composing the expedition were Mr. Selby, Mr. James Wilson, Dr. Greville, Sir William Jardine, and Mr. John Jardine.

were sometimes taken at a sweep of the common salmon draught net from the weight of one pound to about three. The second or smaller fish upon comparison was found to be identical with the *Herling* of the Solway Firth, the *Salmo albus* of Fleming's *British Zoology*. It occurs in numbers in proportion to the first of about ten to one.

Non-migratory Salmonidæ.—The North-west of Sutherlandshire is studded with an immense multitude of lochs, in which Trout are almost the peculiar fish; they differ from each other so much in the various districts as to warrant the suspicion that more than one species is included under the common name of *Trout*. The characters were constant in particular districts, and four very marked varieties were exhibited, differing chiefly in the general form, proportion of the fins, and form of the scales and of the intestines. By many ichthyologists the different appearances of trout are all referred to *S. Fario*, with a most extensive range of variation; but the subject appears yet to require investigation. Many of the trout in these lochs are of very fine quality.

In most of the larger lochs, particularly in the district of Assynt, the *Greater Grey* or *Lake Trout*, *Salmo ferox*, Jard., was found. This fish is noticed by several of the British writers, but only as a variety of the common trout. It is distinct, and possesses good specific characters. It reaches the weight of twenty-five pounds. In Scotland it has been taken in Lochs Awe, Shin, Loyal, Assynt, &c.; in the latter fourteen specimens were procured. Its food is almost exclusively fish. The flesh is very coarse, and of a yellowish pink colour.

The *Char*, *Salmo alpinus* (*S. Umbla*, Agass.), is found in most of the lochs; but, from the difficulty of tempting them with any bait, few were procured. They are only seen and taken in numbers when approaching the mouths of the small rivulets to spawn, and at that time are deteriorating in condition. They appear in best condition in June and July, and might then be taken in numbers with nets stretched across or into the lochs. They feed on aquatic insects, but seem active chiefly during the night.

The *Parr*, *Salmo Salmulus*, Penn., was found in many rivers sparingly, nowhere abundant, and apparently decreasing in number towards the north.

No other *Salmonidæ* were met with during the excursion; but after the above remarks upon those mentioned, specimens were exhibited of the *Gillaroo Trout* from Ireland apparently only a variety of *Salmo Fario*. The food found in the stomach consists exclusively of different species of freshwater shells,

but the coats and sides of the stomach are not more muscular than in the common trout.

The *Whitling* and *Bull Trout* of the Tweed are the young and adult states of the same fish, which is the *Salmo Eriox* of some authors, and reaches a large size.

The *Lochmaben Vendace, Coregonus marænula* (?).—The lochs in the neighbourhood of Lochmaben are the only known habitat in Scotland for this fish, and the author is not sure that there is any authentic station for it in England or Wales. The stomachs were entirely filled with minute *Entomostraca*, which certainly at times constitute the greater part of the nourishment of this fish.

Note.—All these fish were shown to M. Agassiz. All the trout he considered as varieties of *Salmo Fario*; *S. ferox*, an addition to the *Salmonidæ* of Europe, and new to him; the Whitling and Bull Trout also new to him, and differing from any of the fish he was acquainted with in the Continental rivers; Parr, the young of *S. Fario*; and the *Lochmaben Vendace* distinct from the *Coregonus marænula* of continental ichthyologists.

Notice regarding the Coleopterous Insects collected during a Tour in Sutherland. By James Wilson, *F.R.S.E. &c.*

[The following extract from this memoir will show the views of the author, who is engaged in preparing a report on the geographical distribution of insects for the next Meeting of the Association.]

In the total absence of any information regarding the entomological productions of the North of Scotland, the following catalogue of species was drawn up, as a commencement, however defective, of those local lists, which, in a completed state, will tend to illustrate an important department of natural history. The value of such lists is in a great measure independent of their presenting the names of new or rare species: it consists in their exhibiting a true picture of the prevailing entomological character of countries. From an assemblage of such pictures the general distribution of species and the laws by which it is regulated and maintained are eventually to be deduced. No apology need, therefore, be offered for the want of novelty in the following catalogue:

COLEOPTERA.

Cicindela campestris.
Cychrus rostratus.
Carabus catenulatus.
Carabus arvensis.
cancellatus.
violaceus.

Carabus hortensis.
glabratus.
clathratus.
Helobia brevicollis.
Gyllenhalii.
Leistus rufescens.
Lamprias chlorocephalus.
Clivina fossor.
Dyschirius gibbus.
Broscus cephalotes.
Feronia nigrita.
melanaria.
nigra.
orinomum.
Abax striola.
Pœcilus cupreus.
Argutor erythropus.
pullus.
Harpalus ruficornis.
limbatus.
æneus.
Tarus basalis.
Curtonotus aulicus.
Bradytus apricarius.
Amara eurynota.
communis.
vulgaris.
familiaris.
similata.
Patrobus rufipes.
Calathus piceus.
cisteloïdes.
melanocephalus.
mollis.
Clisthopus rotundatus.
Agonum mœstum.
viduum.
parumpunctatum.
Anchomenus albipes.
Loricera pilicornis.
Badister bipustulatus.
Trechus minutus.
Blemus paludosus.
Peryphus littoralis.
Notiophilus aquaticus.
biguttatus.
Elaphrus cupreus.
Blethisa multipunctata.
Dyticus marginalis.
Hydroporus trivialis.
Colymbetes bipustulatus.
agilis.
uliginosus.
Gyrinus marinus.
natator.
Helophorus aquaticus.
griseus.
granularis.
Hydrobius melanocephalus.
fuscipes.
orbicularis.
Sphæridium 4-maculatum.
Necrophorus vespillo.
Oiceoptoma rugosa.
thoracica.
Silpha obscura, *var.*
Phosphuga atrata; and *var.*
Meligethes viridescens.
Byrrhus pilula.
fasciatus.
æneus.
varius.
Hister carbonarius.
Onthophilus striatus.
Geotrupes stercorarius.
sylvaticus.
vernalis.
lævis.
Aphodius rufipes.
fimetarius.
terrestris.
fossor.
rufescens.
Phyllopertha horticola.
Trichius fasciatus.
Ctenicerus tessellatus.
pectinicornis.
cupreus.
Elater minutus.
Anathrotus ruficaudis.
niger.
Campylis linearis.
Sclasatomus æneus.
Hypnoidus riparius.
Cataphagus obscurus.
marginatus.

Atopa cervina.
Malthinus biguttatus.
Cyphon melanurus.
Telephorus bicolor.
rusticus.
dispar.
nigricans.
testaceus.
pallidus.
Anobium castaneum.
Hypera arator.
Hylobius abietis.
Barynotus mercurialis.
Merionus obscurus.
Otiorhynchus tenebricosus.
lævigatus.
atro-apterus.
ovalis.
Hylacites gemmatus.
Strophonomus coryli.
Sciaphilus muricatus.
Sitona lineata.
Phyllobius argentatus.
mali.
reniformis.
parvulus ?
mali, *var.* ?
Rhagium bifasciatum.
Donacia sericea.
cincta.
Donacia simplex.
Galeruca tanaceti.
capreæ.
Chrysomela staphylæa.
fastuosa.
Phaedon vitellina.
Raphani.
Coccinella tredecimpunctata.
Helops caraboïdes.
Goerius olens.
Creophilus maxillosus.
Staphylinus murinus.
castanopterus.
stercorarius.
æneocephalus.
Ocypus similis.
Quedius tristis.
picipennis.
Philonthus splendens.
politus.
varians.
Othius fulgidus.
Gyrohypnus longiceps.
linearis.
Lathrobium lineare.
Stenus ——— ?
Tachinus rufipes.
marginellus.
Tachyporus chrysomelinus.
Aleochara concolor ?.

Remarks on the different Species of the Genus Salmo *which frequent the various Rivers and Lakes of Europe. By* M. Agassiz.

The genus *Salmo*, as it has been established by Linnæus and Artedi, or rather by Rondeletius, has supplied Cuvier with the type of a peculiar family, in which he has retained the generic characters of Linnæus, viz. one dorsal fin with soft rays, and a second one, which is rudimental and only adipose. Cuvier places this family in his order *Malacopterygii Abdominales*, between the *Siluridæ* and the *Clupeæ*; and he subdivides it, on just grounds, into a great number of generic sections, which comprehend a vast variety of exotic species. In his work on the fishes of Brazil the author added several new kinds to those which Cuvier established; and is of opinion that, in the natural classification, it is now absolutely necessary to unite the family

of the *Clupeæ* to that of the *Salmonidæ*, since the only difference between them consists in the presence or absence of an adipose fin, an organ assuredly too insignificant to constitute the distinctive character betwixt two families, and the less so as there are some genera of the family which possess it, whilst in others it is completely wanting, as, for example, in the *Siluridæ*. We may with equal truth affirm, that all the real *Salmones* of Cuvier have not this adipose fin, for in many species of the genera *Sarrasalmus*, *Myletes*, &c., it is composed of rays which are truly osseous.

Restricted to the limits which Cuvier has assigned to it, the genus *Salmo* comprehends all the species of which the body is somewhat lengthened, the mouth large, and supplied with teeth, which are conical, pointed, and formidable, implanted into all the bones of the mouth, that is to say, into the interior maxillary bones, both superior and inferior, into the vomer and palate bones, into the tongue itself, and into the branchial arches. The margin of the upper jaw is formed by the interior and superior maxillary bones, and constitutes only a single continuous arch, as in the higher classes of animals; a conformation which in the class of fishes is found only in the *Clupeæ*.

It is also singular that the number of branchial rays is seldom exactly the same on the opposite sides of the head, the number varying from ten to twelve. The pectoral and the ventral fins are of a middling size; the latter placed about the middle of the belly, opposite to the dorsal, at their base, and along their insertion there is a fleshy fringe, somewhat similar to the long scales which are found on the greater number of the *Clupeæ*. The caudal fin is attached to a very fleshy root, and is moved by very powerful muscles.

This elastic tial is to these fishes a most powerful lever: when wishing to leap to a great height, they strike the surface of the water with a kind of double stroke. By this means they overcome obstacles which appear insurmountable, and leap over nets which are intended to confine them: the most formidable waterfalls can scarcely arrest them. The several species of this genus are found in the northern and temperate regions of Europe, Asia, and America.

The fishes of this family are very ravenous, and feed principally upon the larvæ of aquatic and other insects and of the small crustacea; they also devour fishes of a smaller size. Their alimentary canal is short, but the stomach is proportionally long and strait. At its pyloric extremity may be observed a great number of appendices, which are connected with the pancreas, and to which is generally, but erroneously, applied

the name of cæcum. The swimming-bladder of all of them is very large, and opens into the œsophagus near the bottom of the gullet. Though unable here to enter into the subject very fully, the author states his persuasion that this organ ought to be regarded as the lungs of fishes; that the circulation of the blood in these animals has been inaccurately interpreted when it is supposed that in their heart there may be traced a pulmonary course; also, when their branchiæ have been identified with the lungs of other animals; and, finally, when their great dorsal artery has been considered as analogous to the aorta of the mammalia.

Most of the varieties of salmon reside in fresh waters; in summer they pay a visit to the sea, and do not mount up again to the rivers, except for the purpose of there depositing their spawn. It is sufficiently remarkable that most of our species deposit their ova in November and December, and that the young fry of course come into existence in the coldest season of the year. From this circumstance we may suppose that it is owing to this habit of enduring intense cold in the first days of their existence, that they can subsequently support all that variety of temperature to which they are soon to be exposed.

In proportion as the genus *Salmo* is now circumscribed within its natural limits, so much the more is it difficult to characterize the various species; and M. Agassiz affirms without hesitation, that since no one has devoted himself to their history, so no one has yet succeeded in determining, with any degree of precision, their distinctive characters. The greatest obstacle to the solution of this problem arises from our ignorance of the accuracy of the characters hitherto employed to distinguish the several species the one from the other.

Naturalists have especially attached themselves to the form of the head and to the arrangement of the colours; but these two particulars are much too variable to supply precise characters. As to the variation in the colour, we may say it is infinite. There are, however, two circumstances which especially modify the tints of the salmon tribes, namely, their age and the season of the year. The younger fish are in general much more spotted than the older ones, whose tints become more and more uniform. The *Salmo Hucho*, for example, with violet spots more or less distinct, has, when young, large black transverse bands upon the back down to the middle of its sides. In the second and third years of its existence these bands break up into black spots, less deep in colour, and they disappear more and more, till in its latter years the fish acquires a colour which is almost uniform. The *Salmo lacustris* of Linnæus, when young, has

large black and ocellated spots upon all the superior parts of its body; but from the third year they diminish, and ere long they entirely disappear.

The *Salmo Umbla*, so long as it is young, is of a greenish yellow colour, with the abdomen white; and at a later period of life these tints assume a deeper hue of a more lively green, and finally pass into a blackish green. The abdomen soon becomes silvery white, afterwards yellow and orange coloured, and then of a golden lustre. Its flanks are very soon adorned with ocellated yellow spots, more or less distinct; but ere long there are no spots at all. In the *Salmo Fario* the spots vary even more. In the young they are found yellow, green, brown, and even black and violet, also black and red; but at length they all entirely disappear.

The author has also noticed that the seasons have an influence on the colours of the different kinds of *Salmo*.

It is during the autumn, and at the time of the greatest cold, that is to say in October, November, December, and January, that their tints are most brilliant, and the colours become more vivid by the accumulation of a great quantity of coloured pigments. We might almost say that these fishes bedeck themselves in a nuptial garb as birds do. The colour of their flesh varies according to the nature of their aliment. This family of fishes feeds, as we have said above, especially upon the larvæ of aquatic insects and of small crustacea. It is in the waters which contain the most of these last that the most beautiful salmon-trout are found. Direct experiments which were made in lakes have proved, to the author's satisfaction, that the intensity of the colour of the flesh arises from the greater or smaller quantity of *Gammarinæ* which they have devoured.

As to the structure of the head, it offers, in the opercular bones, in the surface of the cranium, and in its proportions relative to the whole body, very excellent characters: but those, on the other hand, which are taken from the proportional length and size of the jaw-bones are of no value at all; the lower jaw is longer or shorter than the upper according as the fish opens or shuts its mouth; and this consideration introduced into the characteristics of the family has very considerably contributed to multiply the institution of species. The hook which forms the jaw of the *Salmo Salar* is not even a peculiar characteristic of this species, since the full-grown males of all the species of the genus present a crooked prolongation of their lower jaw to a greater or less extent.

Possessed of these facts, which had been collected with the most minute and jealous precautions, M. Agassiz tried to deter-

mine the various species which are found in the fresh waters of the Continent, grounding his examination upon the study of the interior organization and upon the particulars already determined which the integuments present concerning the structure of the scales. He has also introduced the shape of the body and the proportional size of its internal parts as important accessories to the description of the species. Of these investigations he proposes to give an account in his treatise upon the fishes of the fresh waters of Central Europe, confining himself here to a short statement of the results which he has obtained.

It is a very singular fact that those fishes which are the most widely distributed, and those which are most highly prized, are precisely those whose natural history is the most perplexed. The opinions, too, which are most general concerning their geographical distribution are not at all in unison with the real state of things. There scarcely exists a country to which some peculiar species of salmon has not been assigned; and the author adds that even in the *Règne Animal* of Cuvier are many nominal species, which are not even local varieties, as he purposes ere long to demonstrate.

The cupidity of the fisherman, the rivalry of epicures, and the fastidiousness of the palate of salmon-eaters, have, without doubt, contributed to spread these opinions upon the narrow limit assigned to the haunts of the species of the salmon. There is especially a famous variety in the annals of epicurism, over which the greatest possible obscurity has been cast, it is the *Ombre Chevalier*, the Char, or Alpine Trout.

After having attentively examined the Continental varieties, M. Agassiz with eagerness availed himself of the opportunity lately afforded him of examining near their native haunts several species of this genus which are found in England. Through the kindness of Sir William Jardine and of Mr. Selby, he has also had an opportunity of examining all those which they have collected from the Scottish lakes; and the result has been that he has succeeded in determining the perfect identity of many of them with the species found in other countries in Europe, while, on the other hand, he is convinced by the observations of these naturalists that there are species peculiar to Scotland. Nevertheless it is true that systematic authors, from having allowed themselves to fall into error through the prevailing opinions circulated concerning the vast multitude of species of this genus, have been investigating the characters of a great number of merely imaginary species. But to the philosophical naturalist the distinctions upon which they support themselves

in establishing the differences of species are quite insufficient, and the comparative examination of these pseudo-species admits of very different results.

M. Agassiz is convinced that all the fish belonging to this family on the Continent may be reduced to the six following species:

1. *Salmo Umbla*, Linn.; the *Char* of England; the *Ombre Chevalier* of the Lake of Geneva; the *Rötheli* of Swiss Germany; and the *Schwarz Reutel* of Saltzburg.

   Synonyms: *Salmo Salvelinus*, Linn.; *Salmo alpinus*, Linn.; *Salmo salmarinus*, Linn., (but not the *Salmo alpinus* of Bloch).

This fish is found in England and Ireland, in Sweden and Switzerland, and in all the southern parts of Germany.

2. The *Salmo Fario*, Linn.; the *Trout* of brooks; *Common-Trout*, *Gillaroo-Trout*, and *Parr*.

   Synonyms: *Salmo sylvaticus*, Schrank; *Salmo alpinus*, Bloch; *Salmo punctatus*, Cuvier; *Salmo marmoratus*, Cuvier; *Salmo Erythrinus*, Linn.

It is found as extensively as the first species.

3. *Salmo Trutta*, Linn.; *Sea-trout*, *Salmon-trout*. It is the same as the *Salmo Lemanus* of Cuvier, and the *Salmo albus* of Rondeletius.

4. *Salmo lacustris*, Linn. The same as the *Salmo Illanca* and the *Salmo Schiffermulleri* of Bloch.

Found in the lakes of Lower Austria, and in the Rhine above Constance.

5. *Salmo Salar*, Linn.; the *True Salmon*. The *Salmo hamatus* of Cuvier is the old fish, and the *Salmo Gadeni* of Bloch the young fish.

Found in the northern seas, whence it ascends the rivers even as far as the Swiss lakes.

6. *Salmo Hucho*, Linn.

Peculiar to the waters of the Danube.

It results, then, from these observations, that the different species of the Salmon family, far from being confined within the narrow limits of some small bodies of fresh water, are, on the contrary, very widely distributed. They also thrive in all climates, at least in all elevations above the surface of the ocean, whether in fresh water or in salt. Nevertheless they prefer those situations where the water is limpid.

The author concludes by stating that it is not upon vague data that he has drawn these several conclusions, but upon the

actual examination of living specimens of all the species that have been named, and that he has himself studied them in the localities where they were caught.

Dr. Allen Thomson exhibited some specimens of the following reptiles:

Amphiuma means (*didactylus* of Cuvier), *Menopoma* (of Harlan), *Menobranchus lateralis*, and *Proteus anguinus*; and made some remarks upon the place which these animals and the *Cœcilia* hold among the other Batrachian reptiles.

Dr. Thomson then exhibited a few specimens and drawings of the young of the common Thornback at the period when the external branchial filaments exist. He described the connexion of these filaments with the internal gills, and the circulation of the blood in the single vessel running through each of the fifteen filaments that project from the side of the neck, which he had observed in the animal, kept alive for some days.

On the Laryngeal Sac of the Reindeer. By J. S. Traill, *M.D., F.R.S.E. &c.*

The curious pouch connected with the larynx of the reindeer was detected by Camper; but his figure does not convey any correct idea of the form and position of that membranous sac. Dr. Traill minutely described this sac, and exhibited drawings of it when inflated *in situ*, from which it appears to have an elongated form, with a blunt, bifid extremity towards the angle of the jaw, and to taper to a point at the opposite end, which reached to within 6 or 8 inches of the anterior part of the sternum. Its length equals 18 inches; its greatest diameter about 5½ inches. Its blunt extremity is covered by a delicate expansion of a pair of muscles, that derive their origin from the transverse processes of the cervical vertebræ, and from the horns of the os hyoides. These muscles appear to act as *compressors* of the sac when the animal inclines to expel the air. The only aperture of the sac communicates with the superior angle of the thyroid cartilage by an orifice capable of easily admitting the fore finger. The animal from which these drawings were taken was a male, from Norwegian Lapland, dissected by Dr. Traill in 1822.

On the Ancient Inhabitants of the Andes. By J. B. Pentland.

The author having offered some observations on the physical configuration of the Andes of Peru and Bolivia, and on the distribution of organic life at different elevations on the decli-

vity of these gigantic chains, stated the reasons which have led him to conclude that there existed there at a comparatively recent period a race of men very different from any of those now inhabiting our globe, characterized principally by the anomalous form of the cranium, in which two thirds of the entire weight of the cerebral mass is placed behind the occipital foramen, and in which the bones of the face are very much elongated. Mr. Pentland entered into details to prove that this extraordinary form cannot be attributed to pressure or any external force similar to that still employed by many American tribes, and adduced in confirmation of this view the opinion of Cuvier, of Gall, and of many other celebrated naturalists and anatomists.

The remains of this race are found in ancient tombs among the mountains of Peru and Bolivia, and principally in the great inter-alpine valley of Titicaca, and on the borders of the lake of the same name. These tombs present very remarkable architectural beauty, and appear not to date beyond seven or eight centuries before the present period.

The race of men to which these extraordinary remains belong, appears to Mr. Pentland to have constituted the inhabitants of the elevated regions situated between the 14th and 19th degrees of south latitude before the arrival of the present Indian population, which, in its physical characters, its customs, &c., offers many analogies with the Asiatic races of the Old World.

GEOLOGY.

On the Geology of Berwickshire. By David Milne, *Advocate, A.M., F.R.S.E. F.G.S.*

Mr. Milne commenced his paper by describing the boundaries of the district he had examined, and for the better illustration of which he exhibited a coloured map and sections. The district in question comprehends the Lammermuir hills on the north, the valley of the Tweed on the south, and a line drawn north and south through Melrose on the west. He mentioned, that there are at least four different formations or groups of rocks to be found in this district. First, the *grauwacké* rocks, composing the greater part of the Lammermuir hills. Second, *the old red sandstone*, which ranges along the base of these hills, and is found filling up their valleys and burn-courses. Third, *the coal-measures*, which, to a certain extent, are distinctly developed, resting on the old red sandstone, and forming the lower parts of Berwickshire; and fourthly, *the trap*, which forms the

greater number of the isolated hills, that are outliers from the mountain chain of the Lammermuirs.

Mr. Milne then described the external appearance or configuration of the district as that of an oval-shaped basin, cut across at the east end by the German Ocean, and the northern edges of which are the grauwacké hills, some of which rise 1800 feet above the level of the sea. The country then slopes down to the valley of the Tweed, and is diversified by a number of tributary streams, which easily cut and form deep ravines in the soft clay strata, of which the lower parts of the country are chiefly composed. Next to the grauwacké formation, in point of level, is the old red sandstone group, which ranges along the base of the Lammermuir, Galawater, and Cheviot hills, and occupies perhaps one fifth of the intervening space, between the hills and the Tweed, but is never visible at a lower level than 200 or 300 feet above the sea, or higher than 900 or 1000 feet above the sea. The coal-measures and marl strata occupy the west and lowest parts of the surface of the basin, being cut through by the Tweed, in its course from Kelso to the sea; the higher parts of the river, above Kelso to Jedburgh, displaying sections of *the old red sandstone.*

After this general sketch of the three several deposits of stratified rocks in this district, Mr. Milne proceeded to notice the situation of the *trap-rocks*, the exact boundaries of which, he said, it was more difficult to describe; though it may be remarked that they occur most abundantly in the grauwacké and the old red sandstone series. The traps in these two different groups also possess very distinct characters, the grauwacké trap being remarkable for its compactness, and the old red sandstone trap being of a looser and more friable texture. Almost all the isolated hills, which diversify the appearance of the upper parts of Berwickshire, as Cowdenknowes, the Dirringtons, Cockburn Law, Lamberton, Home Castle, Kyles's Hill, and others of inferior note, consist of this less compact trap, and all occur within the limits of the old red sandstone.

Mr. Milne then entered into a more detailed account of these different formations, pointing out some circumstances characterizing each of them.

I. *The Grauwacké Hills.*—These have been usually described as running from St. Abb's Head across the country to the Irish Channel. Though this is true as a general remark, yet, on an examination of these hills in detail, it is found that a considerable portion of them, perhaps $\frac{1}{4}$th or $\frac{1}{5}$th, consists of trap-rocks intermixed. One half of the promontory of St. Abb's Head consists of trap; and there are few sections in the

ravines of the mountain torrents of these hills wherein masses of trap may not be perceived insinuating themselves, even among the grauwacké strata, and deriving from them a stratified appearance. The grauwacké strata, from this cause, have been dislocated and contorted in a thousand different ways, and therefore exhibit no uniformity in their dip and direction. But there is still on the whole, and more particularly in those parts which have not been disturbed by immediate contiguity to trap, a tendency to a particular direction or run, viz. from east to west. The texture of the rock is finely granular, and is generally of a greenish, or sometimes of a yellowish brown colour. Occasionally it passes into a slate which is quarried for various purposes.

No fossils have been found in the grauwacké rocks, nor any mineral except copper. There are in several parts of the Lammermuir range, veins of this metal, some of which have been worked, as at Elmfond, Fasney, and Norton, and run in a direction very nearly east and west.

II. The next series of rocks, in descending from the hills, is *the old red sandstone formation*, which rests on the flanks of the Lammermuirs. They consist of a coarse conglomerate at their basis, of a slaty sandstone in their central parts, and of soft beds of unconsolidated sand or clay in their upper parts.

This formation not only flanks the base of the grauwacké range, but is found filling all the ravines and valleys of these hills up to a certain level. The series is one apparently of inconsiderable thickness at the sides of the hills where it rests on them; but towards the plains, and at a distance from the hills, it is found to be of great depth. In the upper parts of Lammermuir the conglomerate appears to have a thickness of no more than 10 or 20 feet, whilst on the banks of the Tweed, between Kelso and Melrose, there are cliffs of conglomerate 80 or 100 feet high. The same remark applies to the sandstones, which have been deposited over the conglomerate, deep sections of them being visible on the Tweed, whilst in the upper parts of Lauderdale they are much more shallow. This fact, Mr. Milne observed, could be at once accounted for on the supposition that these old red sandstone rocks had been deposited in an ocean or sea which washed the sides of the Lammermuir hills, and increased in depth at a distance from them. The grauwacké strata, on which the conglomerate of this formation has been deposited, must have formed the bed of that ancient ocean; and accordingly, though the conglomerate presents great unevenness and irregularities in its level, the upper part of the red sandstone series very nearly occupies one level throughout the whole

district, but slopes gradually from the hills. Mr. Milne observed that the conglomerate of the old red sandstone is composed of fragments, varying in size from small gravel to boulders of a foot or two in thickness; they consist of the same rocks of which the neighbouring hills are composed, being either grauwacké or trap, though the grauwacké fragments greatly predominate. All the fragments have been completely rounded, as if they had been worn down by the action of water; not that they seem to have been transported from a great distance, for the fragments are now generally either at the very base of the parent rocks or are in the immediate vicinity of them; but that they seem to have been acted on like shingle, or a bank of gravel at the foot of a sea-cliff, the pebbles of which have been worn and smoothed by the incessant motion of the waves.

The fragments are agglutinated together by a cement of small gravel or sand, hardened by oxide of iron, which gives a red tinge to the mass; and wherever the fragments are oblong or flat, their flat sides are almost always parallel to the line of stratification.

That these conglomerate rocks were deposited on the grauwacké, and from the debris which must have been collected at the foot of them, is not only the only possible way of explaining their present situation and appearance, but is proved by sections at various points where the junction is seen. Mr. Milne then referred to several drawings of these points of junction.

The conglomerate is overlaid by a deposit of sandstone, which, as already observed, is thinner near the edge of the deposit than at greater distances from the hills. There is one character in the mineralogical appearance of the rock, besides its red colour and slaty structure, by which it is everywhere marked, viz. the occurrence of white or greenish white spots or patches upon its longitudinal fracture: these white spots do not generally exceed two inches in diameter, being sometimes oval, but generally very nearly circular.

The upper part of the old red sandstone formation consists of beds of red sand and red clay, which are so little consolidated, that in the part of the country where they are best seen, (viz. between Whiteburn, Greenlaw, and East Gondon,) numbers of hillocks and rounded knolls have been formed by the effect of the rains, and the rivulets which now encircle them. In many places where the formation is less ferruginous, these upper beds are worked for the sand they yield.

No fossil remains of any kind have been found in this formation.

Since the deposition of these rocks they have been subjected

to the most violent disturbance and dislocation. Through a great many different parts of the red sandstone girdle, flanking the hills, the trap is now seen protruding, and bearing, far above the rest of the surface of the surrounding country, the red sandstone strata on its top or sides.

At Home Castle, which is built upon basalt, a large quantity of the sandstone is seen enveloped in the trap. The whole mass of trap here is very considerable, and may be perhaps altogether two miles in circumference. Home Castle is about 200 or 250 feet above the red sandstone plains surrounding it; and very near its walls the red sandstone above referred to may be seen very highly inclined, leaning upon the basalt. There are various other hills of trap, which occur among the old red sandstone strata, such as the Dirringtons, Lamberton Hill, Kyles Hill, Eildon Hill, &c. The protrusion of these immense masses of trap (some of them forming hills 1000 or 1200 feet above the level of the sea, and 300 or 400 feet above the surrounding country,) could not have failed to elevate the district immediately in contact with them, and the effect of this elevation must necessarily have been to produce great rents or fissures across the strata so elevated and disturbed. Suppose that by the elevation of Home Castle rock, for example, the red sandstones, which were originally horizontal, were pushed upwards so as to raise one part several hundred feet above the surrounding country; the width of the cracks or rents caused by this elevation, and their extent through the country, would, of course, depend upon the height to which the strata were raised, and the distance to which the disturbing force operated. But one thing is evident, that these rents or fissures would generally run from the point of highest elevation or greatest disturbance as a centre; and whilst *there* the rents would be of considerable width, they would gradually diminish in width in proportion to their distance from that centre. This observation is well illustrated by what actually occurs in the neighbourhood of Home Castle; for two or three trap-dykes (to be afterwards more particularly described) are found to run across the country for several miles from that point as a nucleus, this nucleus having served as the source or fountain-head to supply the different currents of trap which now form the dykes that have filled up these extensive rents.

These trap-rocks seem to be generally confined to the old red sandstone group, and occur more frequently next the edge of the group contiguous to the hills than to the one more distant from them.

There are several instances of the lower conglomerate having been cut through and hardened by veins of trap; indeed, in one

locality, a mass of conglomerate, about 30 feet thick and 100 yards long, (the breadth unknown,) may be seen resting on the top of a trap-hill, which has risen up between the grauwacké and the old red sandstone. This is near the sea-coast, at a small village called Burnmouth.

III. Mr. Milne then proceeded to describe the lowest parts of the basin, viz. those occupied by rocks decidedly members of the coal-deposit, from which extensive supplies are obtained along the south bank of the Tweed, and also by those other rocks, of more doubtful character, which some geologists have considered as new red sandstones.

Mr. Milne here observed, that in speaking of the Berwick coal-fields, or coal formation which occurs along the south bank of the Tweed, he only meant to state the fact, that strata are developed there, having one and all of the distinctive features of the coal-measures, derived from the mineralogical characters, as well as the organic remains found in them. These strata have, however, been described as subordinate members of the mountain limestone group, and to this opinion he cordially acceded. But his object was merely to state the fact of extensive deposits of coal, and its usual concomitant rocks, being in that neighbourhood, when he spoke of them under the convenient appellation of coal-measures.

There are on the south bank of the Tweed altogether eight workable seams of coal, and the collieries extend from near the shore at Berwick to the river Till, which joins the Tweed about 20 miles from the sea. Those strata, with the rest of the coal-measures, rise at Berwick, about north-north-west; but further inland they rise more and more decidedly to the westward; and near the Till, where they are not far from the Cheviot hills, they rise nearly due west. In short, they appear to lie conformably to the belt of old red sandstone, which winds along the foot of the Lammermuir and Cheviot ranges, and rise always to the hills nearest to them.

These coal-seams vary in thickness from $2\frac{1}{2}$ feet to $5\frac{1}{2}$ feet, and are worked so extensively as to supply with fuel not only the district of Northumberland and Durham wherein they occur, but also the greater part of Berwickshire and Roxburghshire.

It is from the same coal-deposit that all the lime used for agricultural purposes is procured.

It is hardly necessary to add that the sandstones, limestones, and shales, accompanying the coal which is worked south of the Tweed, contain all the fossil remains usually characteristic of a coal-deposit. *Crinoidea* with the *Producta*, *Spirifera*, *Modiola*, and other marine shells are abundant, whilst the *Equiseta-*

ceæ, *Filices*, and similar plants, are easily distinguishable in the impressions visible on the sandstones and shales. It is by these limestones that the remarkable foldings are exhibited, which do not occur in the strata of shale lying above and below them. These foldings are seen at Berwick and at Scremerston on the shore.

These coal-measures cross the Tweed, and are observable in the lower parts of Berwickshire. But the only members of them there indubitably belonging to the formation are the sandstones and a few shales. The rest of the formation, of more doubtful character, consists of thick beds of argillaceous blue clay, and strata of marl and sandstone, slightly impregnated with calcareous matter.

The thick beds of sandstone of decidedly carboniferous character are dark red, white, and yellowish, as usually occurs in coal-fields, and the same beds or strata may be traced running through the country for many miles. All the freestone quarries in Berwickshire are worked in these carboniferous strata, which are sometimes 50 or 80 feet in thickness. These sandstones are filled with all the impressions of vegetable remains usual in coal-fields, and no difference of any sort can be observed between them and the sandstones of the Mid Lothian deposits.

On some of the beds of shale found on the banks of the Tweed, not far below Coldstream, impressions of marine shells are abundant, which seem to be of the genus *Modiola*.

Mr. Milne here also mentioned that on the north side of the Tweed, along the sea-coast, these coal-measures are accompanied not merely by the characteristic limestone, but also by three seams of workable coal. These coal-seams may be traced along the coast from Scremerston and Berwick, and are undoubtedly a continuation of the seams which occur there. But they form a narrow belt along the coast, and at length disappear under the German Ocean, at a point where the trap of Lamberton Hill projects into the sea, and throws up the coal-measures, not only on their edges, but so as to form an obtuse angle with the horizontal basis of the hill. About thirty years ago these three seams of coal were worked on several parts of the Berwickshire coast, and the proprietor has lately again advertized them to be let.

Mr. Milne then came to describe those other deposits of doubtful character, which some have considered as of more recent origin, and belonging to the new red sandstone series. Mr. Milne described them as consisting generally of blue clay beds, and their marl strata, the latter being generally of a lightish brown, sometimes a yellowish colour. The ordinary

dip and deviation of the strata are like those of all the other strata towards the hills; and though, in particular localities, they do not lie altogether conformably to the coal-measures, yet, on the *whole*, they may be said to be conformable; and in some places, as will immediately be seen, they are actually *overlaid* by the coal-measures. In these beds of soft blue clay numerous strata of sandstone are seen, but not of any great thickness or running to any extent. They are commonly wedge-shaped, and thin away to nothing. These imbedded masses of sandstone very commonly contain, nay, sometimes are entirely composed of, accumulations of small conglomerate, containing numbers of pebbles, vegetable impressions, and even fossil remains, in curious and interesting confusion. This conglomerate not unfrequently is highly ferruginous. It was in the latter kind that Lord Greenock discovered an entire tooth and the remains of others. This tooth has been described in the *Edinburgh Philosophical Journal*. It was sent to London, and submitted to the inspection of Mr. Clift; but Dr. Grant has since more minutely examined it, and particularly its internal parts, which were not seen by Mr. Clift, and he is decidedly of opinion that it is a tooth of the *Lophius piscatorius*, or sea devil, and further, to use his own words, that it "has been preserved to us precisely as it fell from the jaw upon the loose sand."

Besides these imbedded sandstones there is, in this marl formation, a yellowish calcareous and cellular rock, which has all the appearance and many of the properties of magnesian limestone. This rock is seen on the banks of the Tweed, principally near Coldstream: the strata are thin, none of them exceeding a foot in thickness. It is not, however, only on the banks of the Tweed that this mineral has been found; it is associated in a beautifully crystallized state with the Scremerston seam of coal worked near Berwick, and even in some parts is blended with the coal so as to render the latter impure, and in a great measure unfit for sale. This limestone has been analysed, and, out of 100 parts, found to contain 50 of carbonate of lime, 44 of magnesia, 4 of silica, and 1·2 of peroxide of iron. The specimen analysed was from Birgham Haugh. In beds of dark blue clay or shale, immediately in the vicinity of these strata of magnesian limestone, nodules of iron ore occur, though far less pure and genuine than generally occurs in the coal-fields.

Another mineral of occasional occurrence in the marl-formation is gypsum. There are three kinds, red and white gypsum in veins intersecting the clay beds of blue marl; and *selenite*, which fills up the cracks and interstices of the marl beds, where they are exposed to the air.

The red gypsum occurs in irregular masses, from the size of a walnut to 3 or 4 feet in diameter. The white is in thin veins, not always, but generally in the same beds with the red gypsum; and whenever they come in contact, the thin white vein is invariably cut off and intercepted by nodules of the red, which has therefore been the more recently formed. Although gypsum occurs in abundance in this district, no *rock-salt* in a mineral state has been found: but several springs are known in it which contain a considerable quantity of salt; for example, on an analysis of well-water at the Manse of Eccles, out of 87 parts, 57 were found to be sulphate of lime, and 30 of common salt; and in the mineral water of Dunse Spa (also within the limits of the marl group), as analysed a number of years ago by Dr. F. Home, a large proportion of common salt was found.

Vegetable fossils have been found among the marl-beds forming very extensive deposits. At three or four several localities large trees have been discovered, in beds of blue clay, in a petrified state. The trunks vary in size from a few inches to several feet in diameter; but none have yet been discovered of any length: indeed, none exceed 3 or 4 feet, and they have generally the appearance of having been transported from some distance, being rounded at the ends. These trees have been converted into a hard calcareous rock, which does not always assume the shape and size of the tree enveloped in it, but is generally a little larger, and on being broken presents an accumulation of small twigs and branches of trees, which are found to be of the same species as the imbedded trunks. These fossils have been all ascertained to belong to the genus *Coniferæ*.

These fossil trees are always covered or skinned over by a coaly matter, which seems to have been the original bark, and which has been occasionally found nearly one inch thick.

The internal parts of those fossils have not been so entirely displaced by the intrusion of calcareous matter as to have lost all their woody structure. On the contrary, specimens are constantly met with in which the branch or trunk displays all the concentric rings formed by the annual growths. The original resinous matter of the tree has been seen oozing or exuding from its interior fibres.

Many of the trees have been flattened, and flattened so entirely as to show that the whole of the interior parts have been, as it were, squeezed out, whilst the bark above has been preserved, of course in a state of coal, and now appearing as thin seams of lignite in the beds of clay.

Besides these deposits of trees in the beds of clay, there are numberless impressions of vegetables in the marl-strata very

similar to those found in coal-fields. The plants are entirely flattened, some of the impressions being those of small branches, and of very delicate structure. It is manifest that if these plants have not actually grown in the places where they are now found, they could not have been transported far, from the small degree of injury which they appear to have sustained. In some cases impressions of leaves have been found.

The animal remains found in a fossil state are very few. In addition to the fish's tooth already noticed as having been found in the sandstone conglomerates of Tweed banks, there are a few shells of a minute character which appear to be the *Teredo*, the *Serpula*, and *Modiola*, and which occur not only in these conglomerates but also in marl-strata, clearly contemporaneous with it.

As to the position of these marl-strata, in respect to their dipping under or overlying the coal-measures, Mr. Milne stated that there are two or three localities where these are distinctly seen to be covered by the coal-measures. In particular, one locality on the sea-coast was mentioned where these marl-beds and the coal-measures are found in contact, and where the genuine character of these respective strata is placed beyond all doubt by the occurrence of gypsum in the one and of seams of coal in the other. A section is there well exposed, showing the contact of the coal-measures and marl-strata, the latter manifestly lying beneath the coal-measures.

Mr. Milne alluded to the opinions of several distinguished geologists, that the marl-rocks which he had just been describing belonged to an epoch more recent than the mountain limestones or carboniferous group; and there was no doubt that they have many of the characters of the true marls or new red sandstone formation. But the nature of the fossils found in it, as well as the fact of its being seen dipping under the coal-measures, Mr. Milne stated, had led him to consider the formations as subordinate to them, and deposited nearly under the same circumstances. These circumstances were, the prevalence of the same sea and a similar climate, as proved by the occurrence of the same marine shells in both kinds of strata. One distinction between them might be the unconsolidated condition of the calcareous deposits on the north of the Tweed, as compared with the compact limestones on the south of the Tweed; and also the absence of the larger marine shells and corallines from these marl-beds, and the occurrence in them of deposits of trunks of fossil trees and branches, which have not been often found in the same uncompressed state in the coal-measures. Whether or not these data would justify the impression that the sea,

at the bottom of which these marl-strata were formed, was not of the same depth as that part of the ocean where the thick beds of limestone south of the Tweed have been deposited, he did not venture to say. But Mr. Milne remarked that it was a confirmation of this view, that the same fossil trees which are found in the marl-beds do not occur further south, as they would not probably be drifted very far from the shores whereon they grew. Besides, it is well known that currents and eddies at the bottom of the sea are more frequent along the coast and the mouths of large rivers than at a distance from land; so that the same cause might serve to explain the formation of those wedge-shaped sandstone strata in the thick beds of clay and marl frequent on the banks of the Tweed, as well as the gravelly conglomerates, where are seen mixed up together not only fragments of various rocks, but vegetables, small shells, and fishes' teeth.

Another deposit derived from the marl strata just described consists of lacustrine deposits of shell-marl. There are several of these worked on account of the calcareous matter which they afford, to be spread over the land for agricultural purposes. On the estate of Kimmergham near Dunse, (the property of James Bonar, Esq.,) there is a mass of this nature about seven acres in extent. There is at the surface a covering of peat, which, in some places, is ten feet deep. Below this there are two beds of white calcareous marl filled with minute shells, the beds being separated by a stratum of blue clay. Each of the beds of marl is about six feet in thickness. The shells found in them seem to be of exactly the same genera as those found by Mr. Lyell in the lacustrine deposits of Forfarshire, the *Planorbis*, *Lymnæa*, &c. In addition to these shells, remains of the beaver, and of a large species of deer, were some years ago discovered in this bog. The remains of the beaver, it is believed, are now in the museum. A specimen of the horns found in the moss was exhibited, together with portions of the marl, containing multitudes of minute shells.

In the parish of Merton, where a shell-marl moss of 100 acres occurs, horns of the same species of deer were found, as well as the remains of beavers. These horns were pronounced by Sir Humphry Davy to belong to an extinct species.

IV. The only remaining formation in the district is the trap, which in Berwickshire, as in most other districts, may be divided into three kinds, according to the epochs at which it was successively ejected.

1. The older trap occurs, as has been already mentioned, not only in large amorphous masses among the grauwacké strata, but also occasionally alternating with these rocks, and assuming

their regular stratified appearance. An example of the stratified trap may be seen at Fassney Water, (a locality described by Professor Playfair,) and on the north face of Soutra, about 200 yards east from the London road. In these places it has all the appearance of sienite, both from its hardness and the intermixture of red felspar and hornblende. It is hardly necessary to add that these sienitic and other trap strata, which appear in this stratified form, have acquired that condition from the grauwacké strata, between which they have been pushed up in a manner similar to what occurs in Salisbury Crags; and the like effect as is there seen has been produced upon the grauwacké rocks, which are greatly hardened, and even made to assume so crystalline an appearance as to render it difficult to find the exact line of division.

2. The trap of St. Abb's Head belongs to a more recent period. It may be traced, except for a very short interval, occupied by grauwacké, southwards along the coast, to a point where it is found enveloping the conglomerate of the old red sandstone. On this part of the coast the conglomerate may be seen in vertical beds, and at another point, viz. at Eyemouth Harbour, in immense horizontal masses, resting on the trap, and dipping, at a small angle, into the sea. Here copper is found in the trap in great abundance, not in the form of veins, but in small nodules, which, by oxidizing on exposure to the air, give a curious appearance to the surface of the rock, which is in consequence speckled over with green patches.

To the same epoch may be referred the eruption of most of the trap-hills of Berwickshire; those at least which have protruded through the old red sandstones, some of which, as, for example, the Eildon Hills, are about 1300 feet above the level of the sea.

There are trap-dykes which traverse the red clay beds and sandstones of this formation, some of which run from Home Castle, and in which numerous red crystals occur. Some of these crystals are of that red colour and jasper appearance as to lead to the opinion that the trap had occasionally taken up some particles of the adjacent red strata and jaspidified them. These dykes abound also with large crystals of glassy green felspar.

This old red sandstone trap is of various textures, from the crystalline basalt to the friable and almost vesicular tufa which is seen on the outskirts of the trap-hills. It sometimes also occurs as a soft breccia or conglomerate, the imbedded portions being manifestly derived from the rocks or soil among which it had flowed. In one locality the conglomerate consists of very

small pebbles or gravel, which are agglutinated together by a tufaceous paste or mud, having exactly the appearance of a stream of hardened lava. This occurs in the middle of the old red sandstone formation, on the banks of the Whitadder, north-west of Dunse.

3. There are a few examples of trap ejected after the deposition of the coal-measures, which in consequence are greatly disturbed in its neighbourhood. The whole of Lamberton Hill (near the sea coast) is an example of this, the coal-measures which run along its base for about four miles on the shore being now seen not only vertical, but even inverted to a considerable extent. The trap here has risen up, and is so extensive as to have upraised not merely the coal-measures on the one side, but the grauwacké on the other, and completely obliterated the old red sandstone group at this point, the only trace of it left being a patch of conglomerate on the top of the hill.

A few miles to the south of Berwick there is another mass of trap, which forms the Kyloe hills, and from which a dyke runs fifteen miles in a straight line towards Home Castle rock. In the Tweed below Coldstream it thins out to nothing. The dyke is a light-coloured greenstone. It varies in width, though generally speaking it is broader near the Kyloe hills than at its further extremity.

The usual effects of trap in hardening the strata with which it is in contact, are observable in this dyke and in those previously described.

In some places there has been a slight overflow of the trap-dyke into the softer strata in contact with it, as, for example, the shales and coal, which could less easily resist the lateral pressure of the confined current.

There do not, however, appear, at any of the localities where the dyke and the sedimentary rocks are seen in contact, to have been any other changes effected on them. They are in no case turned up on their edges, or altered in their general bearings. But the case is widely different with the *trap-hills*, all of which have, wherever they are in contact with the trap, upraised the adjoining rocks. This difference between the effect of trap-hills and the effect of dykes may be explained by supposing that they were merely currents of trap, which flowed into fractures or rents previously existing across the country, caused, perhaps, by the elevation of particular points by masses of trap which have been pressed up from below. Such a rent was very likely to be produced by the elevation of the Kyloe hills, and the direction it took would naturally be towards some other point where a similar disturbing and rending force existed.

Kyloe dyke was traced by Mr. Milne for about fifteen miles in a direct line, towards the trap-hills of Home Castle. May not the consideration just stated account for the direction of this dyke?

Another circumstance was noticed by Mr. Milne as a probable effect of the trap upon the incumbent strata, viz., the occurrence of indurated clay-beds, and even of *chert*, in the immediate vicinity of it. At Carham there are thick beds of a coarse gritty limestone, which contain abundance of quartz, of a dusky brown and red colour. These beds of limestone are themselves of a whitish cream colour; and much indurated clay, of the same colour, occasionally a little tinged with green or red, accompanies them. These strata rest upon a porphyry, which is in some places amygdaloidal, containing small grains of quartz tinged with green earth.

Near Dunse the same chert is again seen, but in strata of calcareous sandstone, which are of about the same thickness as the limestone beds at Carham. They are here also immediately incumbent on trap. At Newton Dony, at Marchmont, at Preston, and at Berwick the same indurated marls have been found, which are sometimes so compact as to have been mistaken and burned for limestone; but which proved to be only marls hardened by their contact with, or vicinity to trap.

The only other subject to which Mr. Milne adverted was the changes which appear to have been produced on the surface of the district, and on its elevation above the level of the sea, at successive periods.

Mr. Milne described at least four apparent elevations of the land at successive periods: 1st, The elevation by which the grauwacké strata were upraised; 2ndly, The elevation by which the old red sandstones were made to emerge from the waters wherein they were deposited; 3dly, The elevation which converted the marine strata of the coal-measures, or mountain limestone of Northumberland, into dry land; and, 4thly, a still more recent elevation, the precise epoch of which has not yet been exactly determined.

It may, perhaps, throw light on the causes of these successive elevations to remember, that at the time when these formations were respectively disturbed and elevated, trap-rocks appear to have risen up, which at each successive outburst most probably acted, not merely upon the particular group of rocks among which they now protrude, but on the whole district of country including the grauwacké range. These outbursts of ancient lava would most probably, like the cones on the sides of a volcanic mountain, take place laterally, where the resistance would be less than directly among and through the

grauwacké hills; and thus it is that after the ejection of the old red sandstone trap, along the sides and base of the grauwacké range, the more recent eruptions are more distant from the hills, and among the more modern deposits of coal-measures. But still, these succesive upheavings of trap, though they have found an outlet among the softer rocks, may have increased the elevation of the grauwacké at different periods, without there being on these occasions any visible eruption of trap among these hills. It is perhaps a confirmation of this remark, that the old red sandstone conglomerate, which was of course originally at the same general level along the base of the grauwacké range, is now 800 and 900 feet higher in the western parts of it, than at the sea-coast, and the rise is most remarkably uniform and regular on proceeding inland from the coast. At the sea-shore, as already stated, the conglomerate is lifted upon the top of the trap, and dipping into the sea. About two miles inland (at Foulden) it is about 150 feet above the sea;—at old Melrose, in the valley of the Tweed, it is 300 feet above the sea;—at Greenlaw, nearer the hills, it is 480 feet;—at Dod's Mill, near Spottiswoode, 500 feet above the sea;—at Norton, in Lauderdale, 540 feet;—at Carfrae Mill, still nearer the central range, 640 feet;—and at the foot of Soutra Hill, on both sides of the ridge, (which is probably about 28 miles from the sea,) between 820 and 890 feet above its level.

Since, however, the elevation of the country at these successive periods, corresponding to the three kinds of trap now visible in the district, there seems to have been a fourth, though it is admitted that this fact is more problematical, and is supported by indications of a less decisive character. The vertical coal-measures at the foot of Lamberton Hill, along the sea-coast, have seen described. Immediately south of Burnmouth there is a tract of table-land, now about 100 feet above the level of the sea, which extends between the beach and the base of the hill. It is in shape a triangle, the base of which runs along the foot of Lamberton Hill for about $1\frac{1}{2}$ mile, and the two sides form the present sea-cliffs for about $3\frac{1}{2}$ miles in extent. This table-land consists of the vertical strata, which run parallel with the base, and are seen at the two sides of the triangle, at the sea-shore, running right across the table-land. It is not a little curious that these vertical strata should all have had their edges worn down to a horizontal and level plain, just as would have been the case if the rocks had been exposed to the action of marine currents incessantly sweeping over their edges. When the tide is far out, exactly the same appearance is presented by the vertical rocks, which form the bottom of the shore,

for a considerable distance out from the existing cliffs; and were there to be an elevation of the coast, we should have another table-land, formed of vertical strata, with their edges worn down to a nearly horizontal level, like the table-land, at present about 100 feet above the level of the sea.

Perhaps, connected with this very recent elevation of the coast, may be some extensive rents and fissures in the land visible near St. Abb's Head, and particularly on the north side of it about Dunglass.

One of the most perceptible of these fissures runs for about $1\frac{1}{2}$ mile from the Siccar Point past the ruins of a church called St. Helen's, and towards the valley of the Pease bridge, where the rent is nearly 150 feet deep. In the part of its course first described, the valley is perfectly dry, and there are no symptoms of any rivulet having ever run in it. The strata of grauwacké are here and there nearly vertical, and form a smooth unbroken wall for several hundred yards, on both sides of the valley, which has been formed by the sundering, or separating, or slipping of the strata from off each other.

Similar rents are seen at Cockburnspath and at St. Abb's Head, some of which are about 180 feet deep, and have small rivulets running at the bottom of them (which are too insignificant to have cut through these hard strata to such a depth); but some of them are so shaped that they never could have had rivulets running in them at all.

This district bears upon its front the well-marked symptoms of diluvial action. Large boulders of mica-slate, and every variety of trap are found buried in the alluvial strata on the banks of the Tweed, as well as at the foot of the hills; and the hills are most generally devoid of vegetation, and bared to the rock upon their south-western flanks. This is particularly the case with Home Castle rock, Cowdenknowes, Stitchel, Bemerside, and others of less note.

A good deal of red soil is found scattered over localities, and even among the grauwacké hills, where alone it could have been brought and deposited by a flood, which swept the red sandstones of Roxburghshire, and, as it were, painted the south front of the Lammermuirs with a vermilion edge, to mark the force and direction of its waters.

On the Coal-fields of Scotland. By Major-General Lord Greenock, *F.R.S.E. F.G.S.*

[With a Plate.]

It is more than forty years since Dr. Ure published his *History of the Parishes of Rutherglen and Kilbride*, in which

he noticed the discovery of organic remains either of some species of large fishes or of Saurian reptiles in the coal-fields of the West of Scotland: since that period new facts of a similar nature have been brought to light in the coal-districts of Clackmannanshire, Fifeshire, and the Lothians, as well as near Glasgow, showing that these remains are not confined to particular localities, but that they are very generally distributed throughout the whole extent of the coal-formation in the great valley of the Scottish lowlands.

The specimens that accompany this paper were found in the bituminous shale or blaes which lies immediately above, and in contact with, what is called the *Jewel* coal, in Sir John Hope's coal-works at Stoney Hill near Musselburgh. These organic remains appear to abound in all the pits where the *flat* seams are worked in the Mid Lothian coal-field; they have also been observed in the *edge* seams, at the Edmonstone Colliery, in the same coal-field, and at Dguart in Fifeshire.

The *Jewel* coal is the lowest of what are usually termed the flat seams, and that of Edmonstone the highest of the edge seams; but whether these remains may be most abundant in that part of the series, or whether they are equally distributed through the whole, is a question that must be determined by further investigation. The observations which the author has hitherto had opportunities of making lead him at present to believe that this will be found to become more rare, if they do not entirely disappear, as they descend in the series, and approach the limestone containing marine shells and *Encrinites*, although their reappearance in such vast abundance in an inferior portion at Burdie House is a circumstance not easily to be accounted for.

It may be necessary here to explain that the flat seams are merely the upper beds, five in number, which, being nearer to the surface, are comparatively more level than the edge seams, or those which, occupying an inferior position in the series, dip down to a greater depth in the basin, and are consequently seen, at the places where they are worked, to stand at a much higher angle; but it has now been ascertained that the flat seams, where they have been met with in this coal-field, are in every respect conformable to the edge seams.

The flat seams contain the most valuable coals in the district; but they occur only partially in the Mid Lothian coal-field, as they are not to be found to the southward and westward of the road from Edinburgh to Dalkeith, having, it is said, been thrown off by a dyke near Sherriff Hall, beyond which some of the edge seams appear to have been brought up and flattened. These are worked as flat coals at the Dalhauria, Polton, and Eldon collieries.

Mr. Bald, in giving a section of the edge seams, estimates the total depth of the coal strata in the basin to be at least 500 fathoms, and that the aggregate amount of the thickness of the whole of the seams of coal, twenty-six in number, is 109 feet 6 inches.

Although the Firth of Forth is generally considered to be the northern termination of the Edinburgh coal-fields, there appear to be sufficient reasons to warrant the supposition that the coal-district on the opposite coast of Fifeshire was originally a part of the same deposit. That the coal strata do extend across the water is evidently shown both by the circumstance of their being worked near Wemyss Castle, 300 yards beneath the bed of the river, and their outcrop being seen on both sides of the Forth beyond the low-water mark, as well as at Inchkeith, which is situated in the middle of the channel; at the same time it must be confessed, that with the knowledge we possess respecting them it would be very difficult to prove their exact correspondence, either by their lines of bearing or by the quality of the coals: but when all the disturbances by which they are known to have been affected on both sides of the Forth are taken into consideration, it will not appear improbable that the same causes may have operated, even in a still greater degree, to produce similar derangements and dislocations in those parts that are now concealed beneath the water, which might sufficiently account for any alteration that may be observed in their appearance when they emerge from it on either shore.

The upburst of the trap hills that surround Edinburgh, which, from the occurrence of glance coal, and other appearances observable in them, we may with great probability suppose to have taken place after the deposition and consolidation of the coal series, may very possibly have obliterated many beds of coal that might have previously existed where they now stand, and have variously affected all the others within reach of their influence.

On the other side of the water we see, by the plan accompanying Mr. Landale's reports on that district, that the coal strata meet with so much interruption from trap dykes, that instead of proceeding to any distance on their regular lines of bearing, the greater part of them have been deflected to the eastward, and take directions nearly parallel to the line of the coast.

It appears also from the same authority that the number of coal beds, and the total thickness of the coal in them, in the Fife district, is very nearly the same as in the Edinburgh coal district, according to the accounts given of it by Mr. Eald, viz.

Fifeshire District.	Edinburgh District.
29 beds of coal.	26 beds.
119 feet 6 inches.	109 feet.

The two seams of coal, the workings of which have lately been resumed on the estate of Captain Boswell at Wardie, have apparently been thrown out of their natural position by some disturbance: they rise from the beach near that place in a saddle form, having on the east side an inclination of one in seven, and dipping to the west at an angle of one in fourteen. The coal is said to be of caking quality, which is rarely met with in these coal-fields: one of the Dunnshire seams, marked No. 25. in Mr. Landale's plan, appears to be the only coal of this description in the Fifeshire district. It is a smith's coal, and of the same thickness as the upper bed at Wardie; but it would not be easy to trace any other connexion between them, although the coal of Wardie evidently extends across the firth to the opposite coast.

The nodules of ironstone, of which there is a great abundance in the bituminous shale of Wardie, are very remarkable; for scarcely one is to be found that does not contain an organic nucleus, either a coprolite or some portion of a fossil fish. Similar nodules, containing the same remains, have been also observed on the opposite shore and at Inchkeith.

The specimens of coprolites and fossil fishes which were exhibited by Mr. Trevelyan at the Cambridge Meeting, were from this locality, and additional specimens were now produced.

On the Ossiferous Beds contained in the Basins of the Forth, the Clyde, and the Tay. By Dr. Hibbert.

The author pointed out, in a general manner, the order of succession observed by the beds which were deposited later than the primary and transition schists. These were the peculiar grey micaceous sandstone, principally to be found on the north of the Tay, known by the name of the Arbroath pavement; the red sandstone, into which the Arbroath pavement passes; and the stupendous masses of conglomerate materials, formed by rolled fragments of primary and transition rocks, which repose at the foot of the Grampians. It was incidently stated that, near Cratown, the conglomerate strata were traversed by a trap rock, containing large crystals of glassy felspar, which gave to it the exact character of one of the modern trachytes of the Siebengebirge. The conglomerate rocks were supposed to have been formed at two distinct epochs. The author expressed a sus-

picion that certain patches of sandstone, occurring both on the east and west coast of Scotland, might be considered as new red sandstone.

That the grauwacké schist and its associate beds of limestone contain organic remains, has not yet been shown. The author exhibited a specimen of the Arbroath pavement containing vegetables, and he stated that Mr. Lindsay Carnegie of Kimblethmont in Angus had presented to the College Museum some striking specimens of remains inclosed in the Arbroath pavement, one of which appeared to belong to a crustaceous animal.

But it was shown that organic remains had been most abundantly found in the carboniferous group, characteristic of the basins of the Forth and the Clyde, which the author had previously described at the meetings of the Royal Society of Edinburgh. Certain limestones for instance, namely those of Burdiehouse, East Calder, Burntisland, &c., which he conceived to be of freshwater origin, and belonging to the lower members of the carboniferous group, severally contain both vegetable and animal remains.

The limestone of Kirkton, near Bathgate, is remarkable for its mammillated and ribboned structure; which last peculiarity is produced by thin layers of pure flinty matter alternating with other distinct layers, which are severally calcareous, argillacous, or bituminous. This rock has a striking resemblance to the tertiary limestones of Auvergne, which exhibit a similar character where they come in contact with volcanic eruptions; and hence, as the limestone of Kirkton alternates with tufa, and is in the immediate neighbourhood of trap-rocks, it probably owes its peculiar geological character to similar circumstances. This limestone contains numerous plants, as well as the remains of a most remarkable crustaceous animal, a nearly complete specimen of which the author was enabled to exhibit to the Meeting, through the kindness of Dr. Simpson of Bathgate, into whose possession the relic had fallen. The author remarked, that a larger head of the same animal had been described by Dr. Scholer; but as this naturalist had unfortunately not seen the extremity of the animal, the description was of necessity imperfect*.

* Incidental to this notice, Mr. Smith of Jordan-hill, near Glasgow, exhibited to the Society the more perfect head of the animal described by Dr. Scholer. And Mr. Jameson Torrie placed in Dr. Hibbert's hands a memoir just published by Dr. Harlan of America, in which fossil remains are figured of a similar character, but of the diminutive size of five inches only. The generic name of *Eurypterus* has been given to the American specimen. Dr. Hibbert announced that drawings, accompanied by a description of this singular animal, would be shortly published.

The limestone quarry of Burdiehouse was very briefly described, as many details regarding it have already been published by the author. This limestone is a very deep-seated bed in the carboniferous series. Above it are alternating beds of sandstone, shale, and thin seams of coal. A limestone containing marine shells and corallines follows, while the whole is surmounted by the coal-measures of Loanhead. The Burdiehouse limestone incloses a variety of plants, minute *Entomostraca* (among which there appears to be a *Cypris*), various undescribed fish, the bones of gigantic animals, large scales, and coprolites. Among the bones are pointed teeth of the extraordinary length of three inches and three quarters, and of the width of one inch and a half at their base, which resemble those of Saurian reptiles. These teeth are adorned with a most beautiful brown enamel, as well as the large scales which are so plentifully found in the quarry. There were also exhibited some bony rays of the extraordinary length of fifteen inches, which must have belonged to an immense fish.

The author announced that all the relics of fish hitherto discovered at Burdiehouse would be submitted to the inspection of M. Agassiz, who, in the invaluable work on fossil ichthyology which he was publishing, promised to fill up, with the success of a Cuvier, this great blank in natural history*.

On the Geological Structure of the Orkney Islands. By J. S. Traill, *M.D.*

The geological character of these islands is very simple; the whole group, with the exception of a small granitic district near Stromness, consisting of rocks belonging to the old red sandstone formation. The prevailing rock is a species of sandstone flag, much charged with argillaceous matter. It occurs in distinct strata, usually slightly inclined, which form hills of but small elevation, but often present very magnificent cliffs around the coasts. It has a colour varying from pale greenish to blackish grey. It has a slaty structure, and readily splits into layers, the thickest of which form a very durable building-stone, as the remains of very ancient Scandinavian edificies attest; while the thinnest form excellent flags, or even a tolerable roofing-slate. It is in this slaty rock that the fossil fishes are found. It occasionally contains bitumen, so as in a few places, especially in the islet of Rushholm, to approach to bituminous shale.

* Dr. Hibbert likewise displayed the teeth and other relics of a large fish, which he had recently discovered in the black limestone of Ashford, in Derbyshire.

Connected with the sandstone flag we find thick beds of common sandstone, of a yellowish or tile-red colour. It forms the chief part of the mountains of Hoy, the highest point in Orkney; and also several headlands in Pomona and in Edey. In the vicinity of the red sandstone we occasionally find the stratified flag assuming a higher inclination.

Last year Dr. Traill discovered a thicker bed of basalt in the sandstone of Hoy; and there are many veins of basalt and greenstone traversing the slaty rocks, particularly in Shapinshey, and in that part of Pomona where the fossil fishes are found. It may not be unworthy of notice, that the general direction of these last trap veins is towards that part of Hoy in which the bed of basalt occurs.

Granite exists in Orkney only in one district. It constitutes a chain of moderate hills, running from the southern boundary of the township of Yesnaby, in a south-east direction, to Stromness; occupying a length of about six miles, with a breadth varying from one to half a mile. The granite again appears on the north side of the small island of Græmsey; but the slaty rock is interposed between it and the mountains of Hoy. This granite is close-grained, contains much felspar, and often approaches to gneiss in structure.

The granite is everywhere in immediate contact with a coarse conglomerate, consisting of nodules of quartz, and fragments of granite and sandstone, imbedded in an arenaceous base. The junction of these rocks is well seen at the western end of Græmsey, on the shore at Stromness, and in the burn of Cairston. The conglomerate is of small extent, almost immediately passing into sandstone flag. Both the granite and the conglomerate bear a striking resemblance to the prevailing rocks on the eastern side of Sutherland and the south of Caithness; and the sandstone flag of Orkney is so exactly similar to the slaty rock of the latter county, which also contains fossil fishes, that it is impossible to resist the conclusion, that these rocks belong to the same geological epoch. The researches of Messrs. Sedgwick and Murchison have proved that the Caithness flag is a member of the old red sandstone; repeated observations, and an examination of most of the Orkney Islands, have convinced Dr. Traill that the sandstone and sandstone flag of that group ought to be referred to the same formation. In no part of these islands did he discover any traces of a coal formation, unless, with some geologists, we are to consider the slaty rock charged with bitumen as the lowest bed of that deposit; for certainly no vestige of its other more important members exists in Orkney.

Fossil Fishes.—Dr. Traill exhibited many specimens of the fossil fishes discovered in the slaty flag of Orkney. They are reported to occur in several parts of that group of islands; but Dr. Traill only saw them near Smaill in Pomona, about two miles from the northern extremity of the granitic chain. They occur in a quarry about 100 feet above the level of the sea. The quarry is covered by 3 feet of soil and debris; then we find from 9 to 11 feet of solid strata of flag: but no fish appear until we reach the two lowest beds, which are together about 2 feet in thickness. The uppermost chiefly contains fishes, of a flattened form, with a granular skin: which appear to belong to the family *Raja*. One of these measured 15 inches in length, of which the tail was 6, and the greatest breadth of the body 6 inches. Unfortunately the specimens of these, which Dr. Traill had collected, never reached Edinburgh. The lowest bed of the quarry abounds most with fishes, and from it almost all the specimens exhibited were extracted. These fishes, in a high state of preservation, were carefully examined by the distinguished naturalist M. Agassiz, who detected among them eight distinct species, five of which were quite new to him, and even belonged to three new genera. M. Agassiz considers the species of the fish to indicate that the rock in which they occur is of an era prior to the coal formation. The only trace of vegetable remains observed in that quarry was a single leaf of some monocotyledonous plant, resembling that of a reed or a *Canna*. Below the fish slate a shining rock occurs, which contains no organic remains.

Professor Jameson exhibited a *fossil fish*, the *Cephalaspis* of Agassiz, which he had found in the old red sandstone (Forfarshire) several years ago, long after he had determined that the sandstone of Caithness, Orkney, Shetland, and of the whole tracts of country on the east and west of Scotland were of the same geognostical age.—Mr. Blackadder exhibited a fossil fish from Glammis millstone quarry in the same district.

On the Fossil Fishes of Scotland. By M. Agassiz.

The high geological antiquity of the greater part of the stratified mountains of Scotland gives a peculiar interest to the investigation of their organic remains; as they lead us to the knowledge of the condition of our planet at a period in regard to which we possess only a few insulated fragments of information. The mollusca, zoophytes, &c. of these formations have been examined by many, but the remains of vertebrate animals

have been but little investigated; and of fishes, we are acquainted with those only which have been described and figured by Messrs. Sedgwick and Murchison, and which have also been noticed by Cuvier and Pentland. The occurrence of a large number of these was known, but no particular information as to their nature was communicated. For a long period M. Agassiz has been anxious to have an opportunity of examining these interesting fossils, and this has been afforded him by the meeting of the British Association at Edinburgh.

The collections which have afforded him the most important materials are the following: That of the Royal Society, which, through the unwearied exertions of the Secretary, Mr. Robinson, contains many remarkable remains from Burdiehouse; Dr. Traill's collection, containing many interesting fishes from Orkney; Lord Greenock's extensive series of ichthyolites from the coal formation, and especially from Newhaven. In Professor Jameson's possession is a large head of a fish from the old red sandstone of Forfarshire, of which Messrs. Murchison and Sedgwick have shown M. Agassiz a less perfect specimen, but one which exhibits the other parts of the body. Mr. Torrie submitted to his examination an extensive collection of fossil fishes from Caithness, similar to those described by Messrs. Sedgwick and Murchison; and also some fishes from Gamrie, first noticed by Mr. Murchison, who also described their geological position.

Of the fossil fishes not from Scotland which he has seen on the present occasion, he will take another opportunity to speak.

As to the determination of the Scottish fishes he remarks generally, that they all belong to two orders of the class, viz. some to the order of Placoidian Fishes, Agass. (*Cartilagineæ*, Cuv.); but the larger number to the division Ganoidian Fishes, Agass., and two to the section *Heterocerci*, in which the upper lobe in the caudal fin is longer than the lower.

In the old red sandstone there are two species from Glammis, Forfarshire, viz. one species of the genus *Cephalaspis* (Ganoidian), which has hitherto been found in this formation only. The most remarkable characters of this genus are the shield-like covering of the head, which is prolonged backwards in the form of two horns as in the Trilobites, and the manner in which the eyes are placed near each other on the head. The other species belongs probably to the genus *Hybodus* (Placoidian), but of this only an ichthyodorulite has been seen.

The fishes from Caithness and Orkney approach one another most nearly; though amongst the latter there are several new genera, and in all eight species. Those from Caithness seem to

belong to two species only. Amongst the Orkney fishes there are two very remarkable genera, resembling the *Acanthodes* of the coal formation, also having very small scales; but the new *Cheiracanthus* is furnished with a spine in the pectoral fin only, and the other, the *Cheirolepis*, instead of having the spine, is provided with a row of small scales. M. Agassiz has been convinced by the examination of many specimens that the genus *Dipterus* has two dorsal fins and two anal fins, which sometimes are opposite one another and sometimes alternate; and these are types of two genera, the *Diplopterus* and the *Pleiopterus*.

The fishes from Burdiehouse are also very numerous; in their characters they agree with those of the coal formation, but are more removed from those of Saarbrück than are the remains found at Newhaven.

The most remarkable amongst them is an animal which, from the structure of its teeth, might be considered as a reptile, and which must have been of very considerable dimensions; but which, from its skeleton and its scales, is decidedly a fish. This animal forms a new genus under the name *Megalichthys*, and confirms the opinion formerly expressed, that we observe in older deposits organic remains which, with the usual characters of their family, unite the characters of the types which have made their appearance at a more recent period. Unfortunately, no perfect specimen of the *Megalichthys* has been found, and it has not been possible to bring together all the different parts of the skeleton. Another new genus, related to the *Amblypterus*, has a long dorsal fin extending beyond the ventral fin and the anal fin, and may be named the *Euronotus*. The other species belong to the genera *Pygopterus* and *Amblypterus*. Very large ichthyodorulites occur not unfrequently, and seem to belong to the genus *Hybodus*.*

At Newhaven eight species occur, of which some bear a considerable resemblance to the fossil fishes of Saarbrück, though still distinguished from them by some characters. They belong to the genera *Pygopterus*, *Amblypterus*, and *Palæoniscus*; and there is one species which will in all probability form a new genus, as it differs considerably from the genus *Acrolepis*. Placoidian fishes are also found, but only in fragments, so that their specific characters have not been determined; and there

* M. Agassiz was led by the specimens which he subsequently examined at Leeds, to consider the *larger* relics of Burdiehouse as belonging to two large animals instead of one. The large scales and the long bones are referred by him to *Plammolipis*, while the large teeth and *round* scales are supposed to belong to the animal he has named *Megalichthys Hibberti*.

are two other species, of which small traces only have been obtained.

In the coal formation of Fifeshire a new specimen of *Palæoniscus* has been found.

M. Agassiz remarks that it may appear strange that he should consider the Gamrie fossil fishes as belonging to the coal formation, but they seem to be so nearly related to that deposit that he cannot regard them as of much more recent origin. There are three species, namely, one *Cheiracanthus*, one *Palæoniscus*, and a third, of which perfect specimens have not yet been obtained.

From this short notice it must be evident how important the study of the fossil fishes of Scotland is for advancing our knowledge of the beings which existed before the oolitic period, and how much we may yet expect from future careful investigations.

On the Geology of the Pentland Hills. By C. Maclaren.

These hills are about fifteen miles in length and from three to six in breadth. The fundamental rock is transition slate, accompanied by grauwacké in vertical strata, which are covered unconformably by conglomerate and various felspar and claystone porphyries, in beds dipping to the south-east, at angles varying from 10° to 35°. Beds of conglomerate, alternating with grauwacké, abound in the western part; in the eastern, the grauwacké is accompanied chiefly by felspar, claystone porphyries, and amygdaloids. A vast mass of sandstone forms the termination of the chain on the west, and rises to the height of nearly 1800 feet in the two Cairn Hills. The age of the hills, or the period of their elevation, is indicated by the position of the secondary rocks on their flanks. The sandstone of the Cairn Hills inclines against the transition rocks, at a considerable angle, on the north side, and at Craigintarrie appears in beds almost vertical. On the south side the older strata of the coal formation are found at various places, in a position highly inclined or vertical, while a newer portion of the same series is found in horizontal beds, or dipping in towards the hills at a low angle, and in juxtaposition with the former. It follows that the elevation of the transition rocks took place at a period subsequent to the deposition of the older, but previous to the deposition of the newer, part of the coal formation.

Account of the central Portion of the great Mountain Range of the South of Scotland, in which arise the Sources of the Tweed. By W. MACGILLIVRAY.

The mountains forming the most elevated part of this range are situated in the parishes of Tweedsmuir, Megget, and Mannor, which form the southern and south-eastern parts of the inland county of Peebles, and are continuous with the high land forming the celebrated pastoral districts of Yarrow and Ettrick in Selkirkshire, and with the higher parts of the parish of Moffat in Dumfries. The region is composed of uniform, smooth, rounded hills of grauwacké, scarcely ever precipitous or even abrupt, clothed to the summits with *Junceæ*, *Cyperaceæ*, grasses, heath, and pasture plants, and separated into groups or ridges by long, narrow, straight valleys, which, although generally green, seldom present any natural wood, even along the clear streams that flow into the valley of the Tweed. Whitecoon, Hartfell, and other mountains, were described, and the alpine plants observed on them enumerated, with the view of contrasting this region with the Grampian range. An account was also given of the vegetation of the clenchs or ravines. The Tweed was then followed from its sources to Peebles, and finally to the mouths of the Gala and Ettrick, in the whole of which space the rocks are composed of grauwacké, grauwacké-slate, clay-slate, slate-clay, and occasional small beds of limestone, none of which, however, are wrought. The districts of Yarrow and Ettrick, which are of precisely similar geological structure, were then described, with reference to their scenery, vegetation, and animal productions.

Perhaps few districts in Scotland, of equal extent, present less varied geological phænomena than that which contains the sources of the Tweed. The general direction of the strata is from south-west to north-east; they are usually highly inclined, but present every degree of inclination: the general dip is to the north-west. The composition of the grauwacké exhibits considerable variety.

In form, the hills approximate in a considerable degree to many of the granite masses of Aberdeenshire, but they never present the precipices and corries, which characterize the more elevated of the latter. The whole district, with its rounded, smooth, sloped mountains, connected in elongated heaps, its long, narrow, straight, or slightly tortuous valleys, its argillaceous and pebbly soil, its clear and rapid streams, and its grassy vegetation, with the absence of natural and the scarceness of

planted wood, forms a strong contrast with the mountainous districts of the middle and northern divisions of Scotland, in which peaked and serrated and ridgy mountains, with precipices and corries, rugged and winding valleys, slopes covered with debris, and patched with heath and bracken, brown or limpid streams fringed with birch and alder, rivers and lakes with cataracts and islands, dark forests of pines and thickets of briars, still give interest to the ancient land of the Gael.

The object of this paper was principally to show the propriety of taking the geology, botany, and zoology of a district in connexion with each other.

Notice of the Limestone of Closeburn, in reply to a Query of the Geological Committee. By C. G. S. MENTEATH.

The limestone quarry at Closeburn is situated in a small valley, surrounded by hills of transition rocks and under thick beds of the old sandstone formation. There are two beds of limestone in this deposit, separated by beds of stone eighteen feet thick, chiefly argillaceous, with some calcareous matter in their composition. The upper bed of limestone is 14 feet thick, and from the analysis made of it by the late Dr. Murray, contains 40 per cent. of magnesia, 57 of carbonate of lime, and 3 of iron. The lower bed of limestone is 18 feet thick, and contains 88 per cent. of carbonate of lime, the remainder being clay and sand, with a few grains of iron. The upper part of this bed consists of hard compact limestone, of a reddish colour, 9 feet thick; then 4 feet of thin beds not more than 6 inches thick, between which are interspersed thin layers of a kind of stone marl, containing 10 per cent. of carbonate of lime, with impressions of shells; the remaining 5 feet consist of laminæ of a more red colour, and contain organic remains of *Orthoceras*, *Ammonites*, and shells of the *Producta* of a larger size than are found in any other quarry in Scotland.

The limestone deposit at Closeburn is situated in one of the small valleys of the mountain chain running across the island from the German Ocean to the Irish Sea; no similar deposit of limestone has as yet been recognised elsewhere in this chain of hills.

Notice of the Flints of Aberdeenshire. By Dr. KNIGHT.

Dr. Knight read a notice on the flints found in various parts of Aberdeenshire, and more especially in the vicinity of Peterhead. He particularized the fossils found in them, and exhibited an interesting series of specimens.

On the Old Red Sandstone and the Formations beneath it. By R. I. MURCHISON, *F.R.S. &c.*

Mr. Murchison presented a tabular view of the order of succession of various undescribed formations of great thickness, and distinct from each other in their organic remains and mineralogical characters, which rise from beneath the old red sandstone of England and Wales. He then dwelt upon certain remains of fishes which he had traced through the central division of the old red sandstone of England and Wales, over an area exceeding 3000 square miles. The most striking of these fishes (*Cephalaspis*, Agassiz,) it now appears is common to the central portion of the old red sandstone of England and strata occupying the same geological position in Forfarshire and other counties in Scotland. Mr. Murchison further expressed his opinion that the Arbroath pavement is the equivalent of the tilestones or lower member of the old red sandstone of England. Mr. Murchison stated that Dr. Lloyd of Ludlow was the person who first called his attention to the fishes of the old red sandstone of Salop.

On the Change of Level of the Land and Sea in Scandinavia. By C. LYELL, *F.R.S.*

Mr. Lyell prefaced his statement with a brief sketch of the state of the controversy touching the gradual rise of Scandinavia, at the time of his visiting that country. It was more than a hundred years since the Swedish naturalist Celsius had declared his opinion that the level of the waters, both of the Baltic and the ocean, was suffering a gradual depression.

In confirmation of this phænomenon, Celsius had appealed to several distinct classes of proofs: 1st, The testimony of the inhabitants on the northern shores of the Gulf of Bothnia, that towns formerly sea-ports were then far inland, and that the sea was still constantly leaving dry new tracts of land along its borders; 2ndly, The testimony of the same inhabitants, that various insulated rocks in the Gulf of Bothnia, and on some parts of the eastern shores of Sweden, then rose higher above the level of the sea than they remembered them to have done in their youth; 3rdly, That marks had been cut on the fixed rocks on the shore some thirty years or more before, to point out the level at which the waters of the Baltic formerly stood when not raised by the winds to an unusual height, and that these marks already indicated a sinking of the waters. On the whole, Celsius concluded

that the rate of depression amounted to three or four feet in a hundred years. To this conclusion it was objected that there were many parts of the Baltic where the level of the sea had not fallen, as could be proved by ancient pines and castles standing close to the water's edge, and other natural and artificial monuments. It was remarked that the new accessions of land were chiefly where rivers entered the sea, and where new sedimentary deposits were forming; and that the marks were not to be depended upon, because the level of the sea fluctuated in consequence of the action of the wind.

Von Buch, in the course of his tour in Sweden and Norway, about twenty-five years ago, found at several places on the western shores of Scandinavia deposits of sand and mud containing numerous shells referable to species now living in the neighbouring ocean. From this circumstance, and from accounts which he received from inhabitants of the coasts of the Bothnian Gulf, he inferred that Celsius was correct in regard to a gradual change of relative level. As the sea cannot sink in one place without falling everywhere, Von Buch concluded that certain parts of Sweden and Finland were slowly and insensibly rising. Mr. Lyell, together with Von Hoff and others, still continued to entertain doubts with regard to the reality of this phænomenon, partly on grounds stated by former writers and above enumerated, partly because Sweden and Norway have been, within the times of history, very free from violent earthquakes, and because the elevation was said to take place not suddenly and by starts, according to the analogy of the intermittent action of earthquakes and volcanos, but slowly, constantly, and insensibly.

Mr. Lyell visited some part of the shores of the Bothnian Gulf between Stockholm and Gefle, and of the western coast of Sweden between Uddevala and Gothenburg, districts particularly alluded to by Celsius. He examined several of the marks cut by the Swedish pilots under the direction of the Swedish Academy of Sciences in 1820, and found the level of the Baltic in calm weather several inches below the marks. He also found the level of the waters several feet below marks made seventy or a hundred years before. He obtained similar results on the side of the ocean; and found in both districts that the testimony of the inhabitants agreed exactly with that of their ancestors recorded by Celsius. After confirming the accounts given by Von Buch of the occurrence on this side of the ocean, of elevated beds of *recent* shells at various heights, from 10 to 200 feet, Mr. Lyell added that he had also discovered deposits on the side of the Bothnian Gulf, between Stockholm and Gefle, con-

taining fossil shells of the same species which now characterize the brackish waters of that sea. These occur at various elevations of from 1 to 100 feet, and sometimes reach 50 miles inland. The shells are partly marine and partly fluviatile: the marine species are identical with those now living in the ocean, but are dwarfish in size, and never attain the average dimensions of those which live in waters sufficiently salt to enable them to reach their full development. Mr. Lyell concluded by declaring his belief that certain parts of Sweden are undergoing a gradual rise to the amount of two or three feet in a century, while other parts visited by him, further to the south, appear to experience no movement.

On Marine Shells of recent Species, at considerable elevations, near Preston. By W. GILBERTSON.

The situation of these fossils is in the county of Lancaster, betwixt the Lune and the Mersey: the greatest elevation at which they have yet been found is 350 feet above the sea, in the excavations made by the Preston Water Company at the foot of Longridge Fell.

The shells are interesting, as being of the same species as those now found on our shores; and showing, therefore, that this elevation has taken place since the creation of existing species: and the stations and roads of the Romans being upon this deposit, prove that there has been no change in the form of existing species during the period that has elapsed since they occupied this country.

Notices in reply to a Question proposed by the Geological Committee at Cambridge, as to the Relations of Mineral Veins and the Non-metalliferous Joints in Rocks. By JOHN PHILLIPS, *F.R.S. G.S., Professor of Geology in King's College, London.*

The author states that his attention was first drawn to the special investigation of the direction and other characters of the joints of rocks, during an examination of the upper slate system of Westmoreland and Yorkshire in 1823*, and again excited by observations on the magnesian limestone of the North of England in 1828†. Since that period he has at different times gathered additional facts on the subject by investigations among

* *Transactions of the Geological Society*, vol. iii. p. 1.
† *Phil. Mag. and Annals*, vol. iv. (1828.) p. 401.

the oolitic, coal, and mountain limestone strata, in the latter of which he has endeavoured to ascertain what relation of direction, intersection, width, and other characters there may be between joints and mineral veins.

Geologists have in general given little attention to the joints in stratified and especially secondary rocks, though the structure which they impart to these rocks is often quite as remarkable and characteristic as that derived from the stratification. In most instances they cause in rocks of a given mineralogical character a definite figure of the separable blocks; as the prisms of basalt, the tables of slate, the rhomboidal masses of shale, the cuboidal blocks of limestone: they are often arranged with so much regularity as to assume some of the leading features of symmetrical crystallization. There are several kinds of joints.

1. *Cracks*, which are usually confined within the substance of one bed of stone, and appear to have been caused internally by the process of condensation of the mass. Some of these, called by the workmen *dry cracks*, though scarcely visible to the eye, open with slight blows, and often display on their faces dendritical oxide of iron, or oxide of manganese. The width of the space left between the opposite faces of a crack varies in some ratio to the nature of the rock; thus in some septariate ironstone beds and nodules they are very wide, in certain limestones almost evanescent. Some of them are empty, others filled with carbonate of lime, quartz, and other substances, and in particular places with sulphuret of lead, carbonate of copper, &c.

That the cracks have been produced since the deposition of the rock is easily proved, for they divide imbedded shells, plants, fishes, &c.; in conglomerate rocks also they are found to divide the rolled masses, as at Oban and in the Righi, where veins of quartz filling cracks traverse many different sorts of pebbles.

2. *Joints* which go through one bed of stone, or even through several of the conformable beds of the same quality, in several directions, dividing them into blocks of characteristic forms.

3. *Fissures* traverse a great variety of strata, though of very different *quality*, as limestone, shale, sandstone, and coal; these are often termed *backs*, or *slines*; they, as well as the other joints, are of great importance to quarrymen and miners; they materially influence the lines of the escarpments of rocks along hill-sides and valleys, the direction of streams, &c.

In some rocks, *e.g.* magnesian limestone, they are open for great lengths and depths, or filled with clay and pebbles, introduced from the surface: in the oolite and mountain limestone they exhibit every gradation of sparry repletion from a few cry-

stals on the walls to a solid mass of fibrous or lamellar carbonate of lime occupying the whole cavity, and occasionally interspersed with metallic substances.

In passing through different sorts of rocks, the fissures preserve *nearly* the same inclination from the vertical, but their breadth and regularity are very unequal. The same fissure which in limestone is open, or filled with spar to some inches' width, may be reduced to a mere divisional plane in the alternating shales: the fissures in coarse sandstone are irregular, those in fine shale possessed of almost crystalline symmetry.

Viewed upon a horizontal plane, the fissures and joints almost always, except in greatly deranged masses of rocks, appear to follow definite directions; so as by their mutual intersections to dissect the area into compartments, of which the figure varies according to the nature of the rock. This variation appears principally due to the joints, for the great lines of fissure hold nearly the same courses throughout. The joints usually terminate in the fissures; these last are of unequal extent, some being more persistent than others, so as to deserve the title of *master fissures*.

In slate rocks of the North of England there are often several sets of joints, besides the cleavage, which are more or less distinct and continuous; some of them vertical, others inclined at considerable angles, and passing in different directions, reminding us of the various intersections of the Cornish veins. In the Craven slates the most constant of all the divisional planes is that of cleavage which ranges W.N.W. and E.S.E., having a variable dip to the S.S.W. or N.N.E.; it is crossed by vertical joints ranging nearly N. and S.

In the mountain limestone tracts also, the great fissures are in general parallel to one or other of two lines: they either range nearly E. and W., or W. of N. and E. of S.; of these directions the latter is perhaps the most predominant. Fissures in other directions are indeed often noticed, but they are of less importance.

In the magnesian limestone of Yorkshire the directions of the principal and most continuous fissures are from E. to W. and from N. to S.: the same is the case with some parts of the oolitic ranges on the north of the Vale of Pickering.

The author concludes, from actual observation, that throughout the greatest part of Yorkshire north of the Aire and Wharfe, the most continuous fissures are nearly vertical, and range either E. and W. or nearly so, or a little W. of N. and E. of S. The tract of country thus defined connects itself on the north with the mountain limestone districts of Durham, Cumber-

land, and Northumberland, as far as the valley of the South Tyne; so as to constitute one great elevated geological area, bounded on the north by a dislocation, varying in amount from 600 to 1200 and 2000 feet, which ranges in general E. and W.; on the west by the great Cross Fell fault, of at least 3000 or 4000 feet, ranging S. by E., from Brampton to near Kirby Lonsdale; and on the south by the double Craven fault, of 1000 to 3000 feet, which goes to the E.S.E.

Within this area numerous mineral veins are found, principally running E. and W., or parallel to the northern boundary, comparatively few productive veins being found passing N. and S., though dislocations and rock dykes of importance, and a great proportion of the leading fissures, take the direction N.N.W., which is nearly parallel to the great Cross Fell fault.

This east and west tendency of the veins is conspicuous in all the northern mining tracts of the Tyne, Wear, Tees, and Swale; it is less predominant in the southern mining fields in Wharfdale and Niddersdale, which are near to the E.S.E. fault in Craven. That fault is remarkable for its double line of dislocation; and for the coincidence of its direction with the most continuous divisional planes of the slate, with one parallel dyke of greenstone, and one or more bands (veins) of pyritous slate.

There is therefore in this great tract of country a clear general analogy in direction between the lines of convulsive movements, mineral veins, and open or sparry fissures. The fissures, joints, and cracks often display in their contents, and circumstances of arrangement, a close affinity to metalliferous veins; and the whole investigation appears to indicate among them all a common and fundamental relation. Professor Phillips is of opinion that the definite direction of the joints and fissures is one of the facts to be most attended to in a theoretical point of view, since it appears extremely probable that this character of joints is the primary phænomenon which, by presenting *lines of least resistance* to the disturbing forces concerned, has permitted the dislocations to follow certain principal parallels. The agencies whereby not only the slip veins, but often the undisturbed fissures, joints, cracks, and hollows in rocks, and insulated cavities in shells, have received their metallic or sparry contents, constitute a really distinct branch of inquiry, though it is already evident that the solution of either problem cannot be effected without unfolding common principles of symmetrical aggregation, and chemical and electrical action not yet familiar to geologists.

On some Caverns containing Bones, near the Giant's Causeway. By JAMES BRYCE, *Jun., M.A., F.G.S. &c.*

The author was made acquainted with the existence of these caves by Dr. M'Donnell of Belfast, in the beginning of August last. As he was ignorant of comparative anatomy, he requested Dr. Scouler, Professor of Mineralogy and Geology in the Royal Dublin Society, to join him in an examination of the caves. The following facts are the result of the joint observation of Dr. Scouler and the author.

Only three caves on the north-east of Ireland have been yet found to contain bones; they are situated in the immediate vicinity of Ballintoy, about 5 miles from the Giant's Causeway; one of them a few perches west of a small bay or creek called the Port of Ballintoy, another about 50 perches further east, and the third in Carrickarede Island. At the first-mentioned place a trap dyke traverses a chalk cliff of about 40 feet in height; the entrance to the cave is in this dyke at the base of the cliff: the direction of the dyke is soon crossed by the cave, which is afterwards entirely in the chalk. The mouth of the cave is about 10 feet above high water, and 4 perches distant from the line to which it generally advances. A ridge of columnar basalt which runs along the coast protects this bay from the fury of the waves, and it seems impossible in the present state of the surface that the highest tides could flow into it. Persons living in the neighbourhood assert that the tide has not risen so high within their memory. The cave consists of two chambers, separated from one another and from the entrance by low and narrow passages, which admit the body of a man with difficulty. Beyond the inner and larger chamber, which is 7 feet high and 6 wide, one of these passages extends to an unknown distance. The floor of the cave, particularly in the two chambers, is covered with loose masses of chalk, chalk flints, and trap, of various sizes, intermixed with sand and white gravel, the latter consisting of chalk and flint. The sides and roof and many parts of the floor are covered with incrustations of stalactite and stalagmite, which in some cases meet and form columns. The bones occur loosely among these stones, or imbedded in the sand and gravel at the depth of 3 or 4 feet; they are often cemented together by stalagmite, or along with other substances form a conglomerate with a stalagmitic base. There were found bones of the horse, ox, deer, sheep, and perhaps goat; otter, water-rat, cod-fish, and of several birds. The bones were always detached, and never lying together so as to form an entire skeleton of an individual.

The second cave is in a chalk cliff terminating a small bay called Pait-Dhu, whose sides are so steep in all parts as to prevent the descent of most quadrupeds. At the entrance the cave is about 12 feet high and 9 wide, the floor being on the level of the water. The tide has thrown up a shingle bank, which nearly blocks up the mouth, and prevents the entrance of the sea at present, though there are indications that it has not been long excluded: there was no termination found to the cave, but after 120 feet it is low and narrow. The bones met with belonged to the horse, ox, sheep and goat, deer, dog, badger, pigeon, cormorant, and gull; they were dispersed, as in the former cave, over the floor, which was of similar composition. A very interesting conglomerate, of recent origin, was found here in loose masses; it consisted of rolled pieces of chalk, flint, trap, and bits of opal, and sometimes included bones.

The cave that remains to be noticed occurs on the north-west side of Carrickarede Island, which consists entirely of amygdaloid and greenstone. The high cliffs which form the shore near the cave being constantly washed by the sea, the cave is quite inaccessible from the land: from the sea it is only to be entered at low water in calm weather. At other times a heavy surf breaks into it, so that a visit to it is attended with some danger, and the risk of being detained several days. The floor ascends a little from the entrance, so that in ordinary tides the water does not rise far into it. For nearly fifty yards from the mouth it preserves a width of 70 feet, and a height of 40 or 50: beyond this its dimensions are very much contracted. It probably goes nearly quite through the island. The floor is of the same structure as before, except that chalk, flint, and stalagmitic incrustations are less frequent than in the other caves. A black mould, apparently of vegetable origin, is in some places interstratified with the sand. Bones of the following animals were found: horse, ox, deer, sheep, dog, cod-fish, skate, wolf-fish. A white greasy substance, like decomposed bone, was found in some places among the sand.

It is difficult to account for the formation of these bone deposits: few of the bones bear the marks of attrition, and none of them marks of gnawing. All the caves are inaccessible to most of the animals named; it is therefore most probable that the bones of the tame animals were introduced by man, who may have made these caverns a place of refuge, while some of the wild animals may have lived and died where we now find their remains: in confirmation of which it will be recollected that Carrickarede cave afforded no bones of wild animals. Some of the bones may doubtless have been washed in by the waves.

It is worthy of remark that no bones of the hog were found; which seems to show that the animal did not exist in the country when the remains were introduced into the caves.

On some Caves in the Island of Rathlin and the adjoining Coast of the County of Antrim. By THOMAS ANDREWS, *of Trinity College, Dublin.*

Of these caves six were examined by the author, viz. four in Rathlin, one in the rock called Carrick-a-rede, and one in the mainland near Ballintoy. In a cave in Rathlin, a thick layer of sea sand containing marine shells was found beneath the stalagmite, near the termination of the cave; and in another cave a different variety of water-worn sandstones was discovered in a similar situation, but no trace of shells could be seen. In another large cave at Rathlin, a rude piece of antiquity formed of iron and resembling the handle of a sword, was found quite close to the skeleton of a sheep. The length of the Rathlin caves varied from 150 to 250 feet; the dimensions in other respects were very different. It seems obvious from these circumstances, from the position of the entrances to some of the caves and the narrowness of those of others, 1st, that many of the animals could not have entered them in their present position and state; 2nd, that the sea must have formerly entered them at a much higher relative elevation than its present level.

On the Anatomical Structure of recent and fossil Woods. By W. NICOL.

With a view to acquire some precise information on this subject, the author cut transverse sections of various trees, and soon perceived that the reticulated texture of the recent *Coniferæ* was essentially the same as that of the fossil specimens. As soon as he was thoroughly convinced of the similitude, he mentioned the circumstance to many of his friends, and showed them both the recent and fossil sections in the microscope. The similitude was so striking as to be admitted by all; but as the fossil sections presented no appearance of regular annual layers, some were led to entertain doubts as to the actual identity. On examining soon after some fossils which he had found in the vicinity of Whitby, and one in particular which he had found among the debris of the porphyritic pitchstone in the island of Eigg, he observed the same reticulated structure associated with the most regular annual layers. These left no doubt on his mind as to the Coniferous origin of the whole; but it was

not until last year that he was enabled satisfactorily to remove the doubts that some still entertained, by showing that the *Araucaria Cunninghami*, and a species of *Callitris* from Moreton Bay in New Holland, were equally destitute of annual layers.

Since the author first began to direct his attention to the structure of fossil woods, he has examined some hundreds of specimens from various countries widely separated from each other. From the tertiary formations he has obtained monocotyledons and dicotyledons, but, with one exception, no *Coniferæ*. The exception is in the island of Sheppy, from whence, in the Edinburgh College museum, there are two specimens the coniferous origin of which cannot be doubted. The tertiary formation of the island of Antigua is well known to furnish many specimens of fossil wood; and out of a hundred and fifty specimens from that quarter there was not a single coniferous plant, the greater part being dicotyledons, the rest monocotyledons. From a tertiary formation in the island of Java, the author has lately examined several specimens, but found no *Coniferæ* among them. Yet although coniferous fossils would seem to occur sparingly, at least in some tertiary formations, they are evidently widely distributed among the different rocks of the carboniferous deposits. The author has examined many specimens of fossil wood not only from the coal districts of this country, but also from those of New Holland, and Nova Scotia in America, and found them all to be *Coniferæ*. Most of them are siliceous, but some are calcareous, and others partly siliceous and partly converted into the bituminous state of jet. Of the last, Mr. Nicol has in his possession a very illustrative specimen which he found in the lias formation in the vicinity of Whitby. The siliceous portions of this specimen, which have been twisted round in different directions, display distinctly the coniferous reticulated structure, and the larger portions of the jet have the same blackish zic-zac lines which occur in every transverse section of that bitumen, but show no trace of the original structure. In another specimen, however, consisting partly of an earthy matter and portions of jet, which he also found in the same locality, some of the portions of jet retain, though much obscured, the true coniferous structure, and this is the only example of the kind he has ever observed.

Jet, as generally found in the vicinity of Whitby, consists of detached masses resembling the trunks or branches of trees very much compressed. When cut into thin slices it is perfectly transparent, and of a deep red or pale yellow colour, according to the thickness; but although the three principal sections present a peculiar and constant structure, the author has never

observed, except in the above specimen, the true coniferous structure. The coniferous origin of jet will, in all probability, be generally admitted; and the analogy of the naphtha of coal to the turpentine of the *Coniferæ*, as lately indicated by Dr. Reichenbach, gives room to surmise that coal in general has resulted from the bituminization of coniferous plants.

The *transverse* section, though always sufficient to distinguish the *Coniferæ* from the monocotyledons and dicotyledons, does not furnish any criterion by which to distinguish with certainty one species from another. The longitudinal section, however, parallel to the partitions extending from the centre to the surface, enables us to divide the whole family of *Coniferæ*, whether recent or fossil, into two distinct divisions. The discs or areolæ to be seen in that section are so different in form and arrangement in the two divisions, that there is no risk of mistaking any species in the one for any species in the other.

The first divison, which may be called the *Pine* division, includes the true pines, the cypresses, the junipers, the thujas, the callitrises, and *Salisburia adiantifolia*; the second divison, which may be called the *Araucarian*, includes the araucarias and dammaras. The author first took notice of the discs in the recent pine division, and then pointed out examples of similar discs occurring in fossils of the same division.

The discs or areolæ to be seen in all the species do not occur in every part of a section. They are generally distributed in groups more or less extensive. Their form is for the most part circular; but they are sometimes slightly elliptical, and when that is the case the transverse diameters are perpendicular to the longitudinal partitions. They are arranged in a vertical direction, most frequently in single rows. Double rows, however, are often to be seen; but more than that number the author has never observed, although he has cut and examined many hundreds of sections. The discs in a row are sometimes in contact with one another, but often detached at very different distances; and wherever they occur in double rows, they are always placed side by side in a horizontal direction. In many of the true pines the discs have the largest dimensions towards the inner side of each annual layer, and they are larger and better defined than those in most of the other tribes of this division. When a section of any of the larger true pines is properly cut, the discs often present an apparently flat surface, consisting of a number of distinct concentric rings, especially when illumined with artificial light, as that of a candle, and viewed with a garnet lens of the fortieth or fiftieth of an inch radius. It does not always happen that the discs are wholy composed of concentric circles. In many

of them four or five circles towards the circumference, and one or two towards the centre, are all that is to be seen; and it often happens that the discs are cut so thin that scarcely a trace of the circles remains.

Among the fossil *Coniferæ* a form and arrangement of discs perfectly similar to what occurs in the recent true pines is often to be seen. In a specimen, for example, from the coal formation of Nova Scotia in America, belonging to Professor Jamieson, the discs are numerous and well defined. They are arranged both in single and double rows. When in double rows, they are placed side by side in a horizontal direction. Their size is equal to that of the discs occurring in many of the larger true pines, and some of them display distinct concentric circles. A similar form and arrangement of discs the author has often seen in other specimens, particularly in one from Australia, and in another from the vicinity of Whitby. In the transverse section these three fossils have regular annual layers, and present with great distinctness the coniferous reticulated structure. Hence it appears that in several widely distant regions fossil *Coniferæ* occur perfectly resembling in anatomical structure the recent true pines.

With regard to the other division of the *Coniferæ*, namely, that which includes the Araucarias and Dammaras, we find in the longitudinal section, parallel to the partitions extending from the centre to the surface, groups of discs differing widely from those occurring in the Pine division, not only in size, but also in form and arrangement. In this division, which we have distinguished by the name of Araucarian, the discs occur in single, double, and triple rows. They are in general arranged in groups, but sometimes a row may be seen quite detached from all others. The number of discs in a row varies from two or three to sixty or seventy, and in any one row they are generally equidistant and near one another. In the double and triple rows the discs are never placed side by side in a horizontal direction, but always alternate with each other; and this is a character by which an Araucarian species may at once be distinguished from any one species of the pine division. When the discs are placed at a certain distance from each other, they are circular; when in contact, the approximating sides are compressed; and when at an intermediate distance, the approximating sides become rectilinear. When the discs in the double rows are at a particular distance from each other, they are partly polygonal and partly circular. The contiguous boundary of each consists of four straight lines, and that part of the periphery next the partition is a segment of a circle. When the discs are arranged

in triple rows, those constituting the middle row are the most perfect equilateral hexagons; and those constituting the side rows are partly polygonal and partly circular, similar in every respect to those constituting the double rows. The corresponding sides of the polygonal discs are generally connected by two fine lines or fibres placed near the ends of each; and when the discs are well defined, they generally display concentric circles in the interior, and two parallel straight lines corresponding with the rectilinear boundaries of the polygonal kind. The largest Araucarian discs are scarcely a quarter of the size of the largest of those occurring in the true Pines. They are so minute that to see them with advantage they should be magnified four or five hundred times, and illumined by the light of a snow-white cloud.

On cutting longitudinal sections of various fossils parallel to the partitions extending from the centre to the surface, discs are often to be seen, resembling in every respect those occurring in the Araucarian division of recent *Coniferæ*. They occur in single, double, triple, and sometimes even in quadruple rows. They are sometimes circular, sometimes polygonal, at least in the double and triple series, but in general the polygonal form predominates. When the arrangement is in the triple or quadruple series, the discs in the middle row or rows are in general hexagons, more or less regular; but in most specimens they are very much obscured, and in many parts even completely obliterated. In respect of size they are much smaller than those of the pine division of coniferous fossils; but they seldom or never display more than the external bounding line, whether that line be circular or polygonal.

Many examples of fossil *Coniferæ* possessing discs similar to those of the recent *Araucariæ* might here be adduced, but at present it may suffice to notice three. Of these the fossil tribe at present laid bare in Cragleith quarry first claims attention. In that fossil the discs are in some parts entirely obliterated, but in other parts, though much obscured, they are sufficiently obvious. They are arranged in single, double, triple, and quadruple rows. For the most part they are of a hexagonal form, and distinctly alternate with each other. They merely retain the external bounding line, and the vessels containing them are often very much distorted. Their size is smaller than that of those of the fossils belonging to the Pine division, but it is difficult to say whether the double, triple, or quadruple series predominates. One of the fossil *Coniferæ* from New Holland, contains discs similar to those in the Cragleith tree, but much bolder, and better defined. They are chiefly arranged in single and double

rows; and in the double rows those in one row alternate with those in the other row, and the contiguous boundaries of each are polygonal. In another from the vicinity of Whitby, the discs are distinctly seen in several places. They occur in single, double, and triple rows, and are in some parts circular, and in other parts polygonal. Their form, however, is often rather indefinite on account of their boundaries being very much obscured.

After having examined the structure of many fossil *Coniferæ*, Mr. Nicol has not met with even one possessing characters essentially different from those to be seen in one or other of the recent tribes. The transverse section is analogous in both; the discs, wherever they are well defined, agree with those in one or other of the divisions above mentioned. In some instances, it is true, even where the reticulated texture in the transverse section is tolerably perfect, the discs in the longitudinal section are often very much obscured, and even totally obliterated; but this is no proof that they did not exist in the wood before the commencement of the petrifactive process. In some recent woods the discs are very obscure, even nearly as much so as in some of the fossil kind; and were such to become petrified, it is highly probable they would entirely disappear. The author has seen fossil sections which on a cursory view seemed to have no discs, but which on careful inspection showed traces of them in several parts.

The recent attempts to establish new fossil genera would seem to have arisen from considering a single section of one or two true pines as containing the characters of all the *Coniferæ*; and yet the discs in the Araucarias and Dammaras are so strikingly different from those in the true Pines, that it is impossible to mistake the former for the latter. Had even a single longitudinal section of an Araucaria been examined, it would have been seen that multiplicate rows of discs of a polygonal form could not be admitted as a foundation for a new fossil genus. Had a few sections of some of the common Pines been examined, it would have been seen that double rows of discs existed in recent as well as in fossil *Coniferæ*, and therefore could not be adopted as the foundation for a new fossil genus. Had even a limited number of transverse sections of recent *Coniferæ* been examined, such a diversity in the size of the pith would have been seen as to preclude the idea of erecting into a new genus a single fossil, the distorted pith of which had a mean diameter of about four tenths of an inch. The author has in his possession a portion of the stem of an *Araucaria Brasiliensis* the pith of which is upwards of three tenths of an inch in diameter. Had

a sufficient number of fossil sections been examined, it would have been seen that in some of them the whole structure in the longitudinal direction had been so much obscured that scarcely a trace even of the longitudinal partitions remained, and that therefore the absence of discs could not be admitted as the foundation of a new fossil genus. The presence or absence of discs must often depend on the thickness of a section. In proof of this Mr. Nicol has prepared a section of the present Cragleith tree, which when of the proper thickness showed the discs very distinctly, but which now shows not a trace of them, in consequence of the thickness being a little diminished. A source of deception too often arises from the water absorbed in the process of grinding. In some fossil woods, particularly those of a whitish colour, the translucency is such, when the substance is penetrated with water, that discs may be seen; but when the water is evaporated, a degree of opacity ensues which renders them invisible.

Mr. W. C. Trevelyan exhibited slices of fossil wood, from a specimen which he had brought from Färoe, with drawings by Mr. MacGillivray, who considers it an undescribed species, and proposes naming it *Penuce Ferroensis.*

It occurs in the island of Suderoe, in the bed of clay associated with coal, (all the other strata in that island belonging to the trap family,) of which Mr. Trevelyan has given a short account in the *Transactions of the Royal Society of Edinburgh* vol. ix. p. 461.

Captain MacConnochie, Secretary to the Royal Geographical Society, gave an account of the origin and progress of that Institution. He communicated some details relative to the late expedition to the Niger, and to the expeditions which are about to be sent out to the interior of Africa and to British Guiana.

Mr. Hall's model of a part of Derbyshire was exhibited.

Mr. Saul exhibited drawings of the incisors and canine teeth of the fossil Hippopotamus, from a gravel-pit near Huntingdon.

Dr. Buckland laid before the Section a drawing, by Mrs. Turner of Liverpool, of a large fossil marine plant, found in the new red sandstone of that neighbourhood in 1829.

IV. ANATOMY AND PHYSIOLOGY.

Observations on the proper method of studying the Nervous System. By Sir Charles Bell, *F.R.S. K.H., &c.*

Sir Charles Bell commenced by stating the remarkable similarity in the ideas entertained on the nervous system from the time of Galen to that of Monro and Baillie, which he attributed to the anatomists and teachers rigidly following the same mode of investigation, and having the subject presented ever in the same aspect. After illustrating by different examples how men placed in exactly similar circumstances have the same conceptions elicited, he proceeded to show how inconsistent the minute anatomy of the nervous system in the human body was with the prevailing doctrine of one source of energy, and the notion of the brain being the *officina spirituum*, and to prove that to explain the meaning of the seeming intricacies of the nervous system, it was necessary to consider the nerves as possessed of different endowments. In prosecuting the inquiry there were three distinct considerations to be attended to—

1. The minute distribution of the nerves,
2. The functions of the parts to which they go,
3. Their roots, or the distinctions in their origins.

With regard to the first, he observed that during the period of his early teaching, and some time previously to that, the anatomists of Europe had brought the knowledge of the branching and distribution of the nerves to great perfection, but with no commensurate improvement in the knowledge of their functions.

As to the second head, he said it had been most negligently considered; it was the investigation into the functions of the part to which the nerves went which must be the ground of all rational theory.

His first illustration was taken from the eye. Six nerves crowd towards this small organ; what purpose could this serve? But when we consider not only the capacity of vision, but the exquisite and peculiar sensibilities of the surfaces of the eye; when we consider the sensibilities as putting in action all the guardian motions of the eye; when we consider the globe of the eye moved by four muscles subject to volition, and others whose motions are instinctive; the motions of the eyelids; the motions of the iris; and the motions of the eyeball; the conviction arises that there is a relation between the many

nerves going to the organ and the various functions it performs. He then gave a view of the many actions performed by the features, and especially by the mouth and lips, contending that these different motions could not be performed by the operation of one uniform source of energy in the brain, and one mode of communication between the brain and exterior organs, and that these considerations laid open to us the reason why different nerves came to the same part, and formed connexions which, without seeing the necessity for such combination, would appear to us matter of accident.

Sir Charles Bell proceeded to show how the investigation of the roots of nerves threw further light upon this interesting subject. He spoke of the columns of the spinal marrow, the double roots of the spinal nerves, the ganglions on the posterior root, and the resemblance of the fifth nerve of the head to the spinal nerves. Taking the great work of Monro upon the nervous system, he presented in succession the plates of the roots of the spinal nerves, that of the ganglion of the fifth, and that of two nerves going to one muscle. He called upon those gentlemen who were of his own standing to remember the zeal with which their old professor treated of these subjects, and asked them if they thought his gratitude was due to any other authority. "Often," said he, "have I hung over these plates and repeated all the dissections." These are the points of anatomy that have suggested the experiments to ascertain whether nerves were common nerves, or whether each was endowed with powers differing from those of others, and resulting from the column or part from which it took origin. A short history of his experiments on the spinal nerves and on the fifth, terminated this discourse.

In continuation of the preceding remarks, Sir Charles Bell reminded his audience of the extraordinary complication of nerves presented in the human body after minute dissection. He laid before the section the plates of the nerves given by some of our best authors, and asked if there could be found any clue to this remarkable intricacy. He then proceeded to show that there was a method in addition to those he had pointed out before, a method of inquiry which enlarged the field of our observations, and vastly increased the interest of the subject. This was comparative anatomy; the investigations of which, still following the functions of the parts, shewed the nerves increasing in number and in complication, in proportion as additional actions were required, in the parts constituting the system of the animal body.

He presumed it would be granted that Nature wrought with

great uniformity, and that if it were proved in any one instance that sensibility and motion resulted from nervous matter, it must be admitted that whenever motion and sensation were observable in a creature, there there must be nervous matter. As in some of the lowest animals we perceive motion to result from the influence of heat and light, where yet no nerves were visible, it leads to the inquiry, what is the function of a nerve? is a nerve of itself a source of energy, or is it only a track of nervous matter wrapped up in membrane for the purpose of conveying an influence?

He proceeded to observe that in the lowest links of the chain of animals there was ever attached to its nearer or central extremity a little mass of nervous matter, or ganglion; and that this central mass, it was reasonable to suppose, was the real organ, whilst the nerves were the appendages, the *interruncii*, between the central organ and the external organ of sense, or between that central organ and the moving instrument of the animal. He proceeded to describe the ganglionic cord of the Annelides, to show that the system in these lower animals was essentially the same with that of man, although the extraordinary accumulation of the central masses in the brain and spinal marrow of the latter obscured the resemblance. He here introduced the name of Mr. Newport with high approbation of his talents: he said, having observed the happy methods that gentleman employed in investigating the nervous system of the invertebral animals, he persuaded him to investigate their medullary cord, and to ascertain whether or not there was a distinction of an anterior and posterior portion of that cord; and in a very few days afterwards that gentleman brought him a preparation of the nervous system of the lobster, in which it was shown that the anterior or lower portion of the cord passed over the ganglion, and that the posterior portion merged in the ganglion. Here was a remarkable confirmation of the strict resemblance between the spinal marrow of the higher animals and the medullary cord of the *Invertebrata*.

Such then, Sir Charles Bell contended, were the modes of investigating the nervous system: 1st, By minute dissection of the nerves of the human body; 2nd, By the study of functions, which requires both the finest hand and the highest capacity for observation; 3rd, The observation of the roots of the nerves and the different sources from which they proceed; 4th, Experiments upon the living animal by observing what functions are cut off by the division of certain nerves, a mode of proceeding which for many reasons ought not to be lightly undertaken, and which could be successfully prosecuted only under the guidance

of knowledge obtained by the former methods; 5th, By comparative anatomy, the most satisfactory of all the modes by which the apparent confusion of the nerves of the human body were to be unravelled and systematized. Sir Charles Bell then gave a short account of his paper about to be published by the Royal Society, in which he has followed out the relations between the cerebrum and the sensitive and motor nerves; and where he has distinguished two portions of the crus cerebri, one descending anteriorly to the transverse septum of the *pons*, the other posteriorly to that *septum*; the anterior relating to the nerves of motion, the posterior to the nerves of sensation: and he proceeded in some detail to show that any attempt to explain the most familiar symptoms of disease in the brain must be imperfect without the knowledge of these facts.

On the interest and importance to the Medical Profession of the study of Mental Philosophy. By Dr. ABERCROMBIE.

The remarks on this subject were delivered in a closing address to the Medical Section. Dr. Abercrombie said he was aware of the objections which had been brought against admitting the philosophy of mind as one of the regular sections of the Association; and to a considerable extent he admitted their truth, as it might be difficult to preserve such discussions from those hypothetical speculations by which this important science had been so much obscured and retarded in its progress. But by treating it as a branch of physiology, he trusted this might be avoided, by rigidly restricting the investigation to a careful observation of facts, and the purposes of high practical utility to which they might be applied. Keeping in view the importance of these rules, he earnestly recommended the subject to medical inquirers, as capable of being cultivated on strict philosophical principles as a science of observation, and as likely to yield laws, principles, or universal facts, which might be ascertained with the same precision as the laws of physical science. For this purpose, however, inquirers must abstain from all vain speculations respecting the nature and essence of mind, or the mode of its communication with external things, and must confine themselves to a simple and careful study of its operations.

Respecting the means of cultivating the philosophy of mind as a science of rigid observation, Dr. Abercrombie alluded to the study of mental phænomena and mental habits in ourselves and in other men; the whole phænomena of dreaming, insanity, and delirium; and the mental conditions which occur in connexion with diseases and injuries of the brain. The sub-

jects of dreaming and insanity, which have hitherto been little cultivated with this view, he considered as capable of being prosecuted on sound philosophical principles, and as likely to yield curious and important results respecting the laws of association and various other processes of the mind.

The practical purposes to which mental science may be applied, Dr. Abercrombie considered briefly under the following heads: 1. The education of the young, and the cultivation of a sound mental discipline at any period of life. In all other departments we distinctly recognise the truth, that every art must be founded upon science, or upon a correct knowledge of the uniform relations and sequences of the essences to which the art refers; and it cannot be supposed that the only exception to this rule should be the highest and most delicate of all human pursuits, the science and the art of the mind. 2. The intellectual and moral treatment of insanity, presenting a subject of intellectual observation and experiment, in which little comparatively has been done, but which seems to promise results of the highest importance and interest. 3. The prevention of insanity in individuals in whom there exists the hereditary predisposition to it. He gave his reasons for being convinced that in such cases much might be done by a careful mental culture, and that irremediable injury might arise from the neglect of it. 4. Dr. Abercrombie alluded to the importance of mental science as the basis of a philosophical logic; and concluded his address by some observations on the dignity and importance of medicine, characterizing it as one of the highest pursuits to which the human mind can be directed, as it combines with the culture of a liberal science, the daily exercise of an extensive benevolence, and thus tends at once to cultivate the highest powers of the understanding and the best feelings of the heart.

Notice of some Experiments on the connexion between the Nervous System and the Irritability of Muscles in Living Animals. By Dr. J. Reid. *With Observations by Dr.* Alison.

Although physiologists are still divided in opinion as to the question whether nerves furnish a condition necessary to the irritation of muscles, (*i.e.* whether every stimulus which excites a muscle to contraction acts on it through the intervention of nervous filaments,) they have now very generally abandoned the once prevalent theory, that the irritability of muscles is derived from the brain or spinal cord, *i.e.* that muscles are continually receiving, through their nerves, from those larger masses of the

nervous system, supplies of a certain influence or energy, which enables them to contract; and that some of the statements of Dr. Wilson Philip, in particular, are generally regarded as decisive against this theory.

Dr. Wilson Philip found by experiment, that the irritability of a muscle of which the nerves were entire, was exhausted by applying a stimulus directly to the muscular fibres (sprinkling salt on them) even more quickly than that of a muscle of which the nerves had been cut, and where all communication with the supposed source of nervous influence or energy had been cut off; and he states generally that a muscle of voluntary motion, if exhausted by stimulation, will recover its irritability by rest, although all its nerves have been divided.

But in opposition to this statement, and in support of the old theory of nervous influence continually flowing through certain of the nerves into the muscles, it has lately been stated by Mr. J. W. Earle, that when the nerves of the limb of a frog were cut, the skin stripped off, and the muscles irritated by sprinkling salt on their fibres, until they had lost their power of contraction, although they did not lose their power much more quickly than when the nerves were entire, yet *they did not regain their power*, although left undisturbed for five weeks; while the muscles of the limbs of another frog, similarly treated, but of which the nerves were left entire, completely recovered their irritability.

It occurred as a fundamental objection to the experiment of Mr. Earle, that in the case where the nerves had been divided, the muscles had become inflamed; being found at the end of the five weeks "softer in their texture than natural, a good deal injected with blood, and with some interstitial deposition of fluid in them;" while in the limb to which the salt had been applied, but of which the nerves were left entire, and where the irritability was recovered, "although the colour of the muscles was rather darker than natural, their texture remained unchanged, and there was no interstitial deposition of fluid in them."

In these circumstances it might evidently be supposed that it was the inflammation and disorganization of the muscles, not the section of the nerves, which prevented the recovery of the irritability in the case where the nerves had been cut; and it became important to have the experiment repeated, with care to avoid such injury of the limb of the animal as should cause inflammation to succeed the section of the nerves.

With this view, Dr. Reid performed a number of experiments on frogs, in which the irritability of the muscles of both hind-legs was exhausted or greatly diminished by galvanism, after

the nerves of one leg had been divided and the lower part of the limb rendered perfectly insensible and incapable of voluntary motion, (but without stripping off the skin,) while the nerves of the other had been left entire. The state of the muscles of both limbs was examined after some days. The results of these experiments were not uniform; but in several, where every attention to accuracy seemed to have been paid, the irritability of the muscles in the palsied limbs appeared to be *restored as perfectly as in the others*; contractions being excited in them, in several instances, by the galvanism from four or even two plates, whereas they had formerly been irritated until they were no longer excitable by that from fourteen plates.

That the muscles which thus recovered their irritability had lost all nervous connexion with the brain or spinal cord was proved, not only by their obvious insensibility, but by afterwards cutting off the heads of the animals and forcing a probe along the spinal canal, which excited forcible contractions in all parts excepting the palsied limbs.

Dr. Alison's paper contained the details of several of these experiments; and he stated in conclusion, that as a *positive* result in such an inquiry must always outweigh a *negative* one, (particularly where a source of fallacy attending the latter can be pointed out,) these experiments appear fully to justify the assertion of Dr. Wilson Philip, that a muscle of voluntary motion may recover its irritability by rest, although all its nerves be divided; and that they afford, perhaps, more direct evidence than any others in support of the doctrine of Haller, now generally admitted in this country, that the property of irritability in muscles is independent of any influence or energy continually flowing from the nervous system, although, like every other endowment of living animals, it is subjected to the control of causes which act primarily on that part of the living frame.

Dr. Allen Thomson expressed a doubt whether these experiments warranted the conclusion drawn from them, not because he acquiesced in the theory to which they are opposed, nor because he called in question the accuracy of the results described to have been obtained, but because he knew that former experimenters had failed in producing such diminution or exhaustion of the irritability of muscles as had been found by Dr. Reid; and conceived it possible that some of the numerous fallacies to which such experiments are liable might not have been sufficiently guarded against.*

* A Committee, of which Dr. Thomson was a member, was appointed for the repetition of the experiments, which has performed the duty assigned to it.

The accuracy of Dr. Reid's statement as to the great diminution or apparent exhaustion of the irritability of the muscles under the influence of the galvanism, and the subsequent recovery of the power, notwithstanding the division of all their nerves, was satisfactorily established. It is to be remarked, however, that in these experiments, as usual in such cases, the limbs to which the galvanism was applied were kept moist by the same saline solution with which the galvanic trough was charged; and Dr. Thomson has observed, that when they are moistened with pure water, the diminution of the irritability under the excitement by galvanism is much less obvious. Hence he was led to suspect that the apparent loss of power in the muscles under that process might depend, not on the circumstance of repeated excitement, but on a degree, however slight, of injury to their texture by the action of the salt. This inquiry he proposes to prosecute further; but in the mean time it is certain that by the usual process of galvanizing a living muscle moistened by a saline solution, a very great diminution of its irritability may be effected, which may subsequently be regained, notwithstanding the division of all its nerves; and as the fact of its recovery, not the cause of its diminution or exhaustion, is the point on which the inference drawn from these experiments rests, that inference may be held to be sufficiently justified.

Notice of some Observations on the vital properties of Arteries leading to inflamed parts. By Dr. ALISON.

These observations were made with the able assistance of Mr. Dick, veterinary surgeon, on the arteries of the limbs of several horses, condemned on account of injury and inflammation there.

The immediate object of inquiry was, whether the tortuous and strongly pulsating arteries leading to an inflamed part are really endowed with a greater vital power of contraction than sound arteries; and the method taken to ascertain this was, to make a comparative examination of the condition of these arteries, and of the corresponding arteries in the opposite sound limbs, immediately on the animals being killed (by blowing air into their veins); and again after the lapse of 16 or 24 hours, when it is known that the tonic contraction, which takes place at the time of death, and is the indication of the only vital power which experiments authorzie us to ascribe to arteries, has relaxed.

The animals were killed, and the observations made, at different periods varying from twelve hours to twenty days after

the commencement of the inflammation, in the five cases of which an account was read. The extent of the inflammation was various. In all the cases, the artery leading to the inflamed part, when laid bare as high as the groin as soon as possible after the death of the animal, was larger in its whole length, *i. e.* had contracted less at the moment of death, than that of the sound limb. In two of the cases, where the inflammation was of long standing, and the coats of the artery appeared to have been affected by it, this vessel at the second examination appeared smaller than the artery of the sound limb, having not only contracted less at the moment of death, but dilated less after death, than the artery in the natural state. In the other cases the artery of the inflamed limb remained larger than the other at the second examination; and it was further obvious that its elasticity was impaired, for when slit open and smoothed out, it had less power than the sound artery of recovering the cylindrical form.

In all the cases, the artery of the inflamed limb retained after death a considerable quantity of blood, while the other was almost empty; and that this was not owing to inflammatory effusion, preventing the artery of the affected limb from emptying itself at the time of death, was proved, in two of the cases, by cutting across the vessel, immediately on the death of the animal, a little above the inflamed part, whereby it had full opportunity to rid itself of its blood, if it had retained the power to do so.

One of these observations was made in the presence of Dr. Yelloly, Dr. Clark, Dr. Fletcher, Mr. Broughton, Mr. Clift, and Mr. Bracy Clark; and it may be added here, that in a subsequent experiment, in which Dr. Alison and Mr. Dick were assisted by Dr. Fletcher, they obtained further proof of the loss of elasticity in the artery of an inflamed limb, by finding that after it had been distended by a given weight of mercury (in the way practised by Poiseville,) it had less power than the corresponding sound artery, to contract on itself and expel its contents when the distending force was withdrawn. But this last experiment was made too long after the death of the animal to justify an inference as to the strictly vital power of the vessel.

Dr. Alison stated, that it seems now generally admitted by microscopical observers, that during by far the greatest part, and during the highest intensity of inflammation, nothing but dilatation or relaxation of the *small* vessels of the inflamed part can be perceived. If the present observations shall be confirmed by others, they will show more distinctly than any statements

hitherto on record, that the same holds true of the *larger* vessels supplying an inflamed part. Now, there are two changes in the movement of the blood through the vessels of an inflamed part which seem well ascertained by many observations, viz. retarded movement or absolute stagnation (*stase du sang*) in many of the small vessels most affected, even during the height of the inflammation; and accelerated movement in the neighbouring vessels, with greatly increased transmission, in a given time, through the whole veins of the part. This last change may, perhaps, be reasonably ascribed to the relaxation of the vessels giving increased effect to the impulse from the heart; but it seems impossible to ascribe likewise to that relaxation of vessels, the former, which is just the opposite change in the movement of the blood; and yet no modification of the action of any of the vessels, except simple relaxation, can be detected.

The fair inference from these facts therefore seems to be, that the phænomena of inflammation are truly inexplicable by any changes which occur, during that state, in the contractile power of the vessels containing the blood; and that, instead of seeking for an explanation of these phænomena in the state of *contractions* of any of the solids, we ought rather to look for it in the state of the *attractions* subsisting during the living state among the particles of the blood, and between them and the surrounding solids. And this inference the author thinks might be supported by reference both to other facts in the history of inflammation, and also to many other phænomena of the living body both in health and disease.

Report of Progress made in an Experimental Inquiry regarding the Sensibilities of the Cerebral Nerves, recommended at the last Meeting of the Association. By Dr. MARSHALL HALL *and Mr.* BROUGHTON.

Some disagreement appears to exist amongst the results of the investigations regarding the sensibilities of the cerebral nerves, which demands further experimental inquiry. A series of experiments has therefore been instituted at the request of the Committee of the Medical Section, and the establishment of Messrs. Field in Oxford-street, London, was selected for the purpose of carrying the inquiry into effect; the horse and the ass, from their large size, being considered as the most favourable subjects for the free exposure of the nerves.

The properties of some of the cerebral nerves being admitted upon other grounds than experimental proof, this investigation was exclusively directed to the facial branches of the fifth pair

of nerves, the hard portion of the seventh, the vagus, the spinal accessory, the glosso-pharyngeal, the lingual, and the sympathetic nerves. Upon the properties of the first, second, third, fourth, sixth, and the soft portion of the seventh pairs of nerves no doubt or discrepancy exists.

It has long been known that the properties of the cerebral nerves are various. Thus, one nerve governs the function of motion; another that of some specific sensation, as of light or sound; and these properties are held independently of each other. To understand clearly the properties of nerves, it is also necessary to apply the test of experiment to their *roots*; for branches from two or more roots unite to form one nerve apparently, which may then assume two distinct properties, that is, the peculiar property of each root. This is exemplified in the origin and distribution of the nerves of the face.

The apparent discrepancies in the results of experiments probably depend much upon the indefinite manner in which certain physiological terms have been employed. Thus, *sensation* has been coupled *with* consciousness in some instances, and in others it has been supposed to exist *without* consciousness. In the present report the term *sensation* implies *consciousness*. It is considered as identical with *feeling*, and when violently excited it becomes *pain*. And this is manifested by *general and instantaneous* efforts or struggles. *These are, therefore, the signs of sensibility.*

Three modes of judgement have appeared as necessary to be kept in view in the present inquiry in reference to the above definition:

1. It was observed that when a nerve of unequivocal sensibility was pricked or pinched, an *immediate and general* struggle followed. The facial branches of the fifth nerve are examples.

2. That when a nerve as unequivocally devoted to motion is pinched, there is an *immediate contraction of the muscles which that nerve supplies, and of no other muscles.*

3. That on pinching the par vagum, neither of the phænomena above noticed occurs; but by continuing the compression for a few moments, an *act of respiration and of deglutition follows, with a tendency to struggle and cough.*

Of these three phænomena the first only is considered as indicating the property of sensation, or the power in the nerve subjected to experiment to transmit sensible impressions.

The movements in the third instance appear to arise from secondary causes, *the mechanical irritation of the nerve not being attended with immediate consciousness.*

1. *Experiments upon the Facial Nerves.*—These nerves go-

vern the actions of the face, and preside over the sensibilities of its different organs and surfaces. The first function is performed by the facial portion of the seventh nerve and a portion of the fifth. The second function is performed by the large portion of the fifth pair of nerves. Thus the fifth nerve possesses two distinct properties of transmission, one voluntary, the other sentient, in consequence of its having two distinct roots. One of these roots, the largest, has a ganglion attached to it, and is exclusively a sentient nerve. The smaller root has no ganglion, is insensible, and governs the motions of those muscles which it supplies. The first fact is easily gained by experiment, but the second is admitted upon other grounds, for the smaller root cannot be experimented upon in the living animal. It is to be observed that the larger root of the fifth nerve is divided into three branches, spread and ramified over the face, and frequently connected in its ramifications with branches of the seventh nerve; so that unless the experimental tests be applied to distinct branches, no certain response can be obtained as to their several properties.

Pricking or pinching the trifacial nerve was attended with instantaneous indications of consciousness; when its branches were divided, all sensibility ceased in the parts which they supply. The *lower* divided ends made no response when bruised, but the upper indicated sensation. The motions of the face, however, still remained unimpaired, until the seventh nerve was divided as near its origin as possible, when the organs which it supplies became permanently motionless. When this nerve was slightly pinched in its entire state, those muscles exclusively which it supplies were seen to be convulsed, without any *general* effort; when the compression was increased, and continued for a few moments, signs of uneasy respiration occurred. Pricking this nerve with a needle and cutting through it produced no struggle whatever, as is the case with the trifacial nerve. When the lower end of the nerve, after division, was irritated, no movement followed; but on compressing the upper end, the same signs were exhibited as when the nerve was irritated in its entire state.

2. *Experiments upon the Nervus Vagus.*—In the year 1820 Mr. Broughton experimented upon this nerve; the results were published in the *Quarterly Journal of Science of the Royal Institution.* It was found to be insensible when slightly pinched, pricked, or divided. The present experimental investigation confirmed this remark. It was also on the former, as well as upon the recent occasion, clearly shown that, when a forcible compression was continued a few moments upon the nervus vagus, a respiratory effort followed, and an act of deglutition, with a

cough and a struggle. In the recent investigation it was observable that when the nervus vagus was divided, mechanical irritation applied to the upper end of the divided nerve produced the same signs as when the nerve was entire. Every repeated compression of this nerve (as was also the case with the seventh) produced corresponding respiratory struggles; whilst a uniform, uninterrupted compression caused no repetition of the phænomena. An additional argument in support of the opinion that these effects are independent of any sensible property in the nerve itself is furnished by the fact that Dr. Marshall Hall has found precisely similar effects to occur in the turtle after its decapitation, on pricking the lateral spinal nerves, whether of the sentient or motory class.

3. *Experiments upon the Spinal Accessory Nerve.*—This nerve having been pricked without any response, was then slightly pinched and scraped; when the sterno-maxillaris muscle, the levator humeri, and other muscles of the neck exclusively were seen to contract at each application of this mechanical irritation. But when the forceps was applied firmly, and continued a few moments, similar effects were produced as with the vagus and the seventh. The branches of this nerve appeared to be equally destitute of sensibility with the root. The compression of the upper end, after dividing this nerve below its bifurcation, was followed by no effects, unless the pressure was made opposite the giving off of the anterior branch, when the same phænomena occurred as were exhibited in the entire nerve.

4. *Experiments upon the Glosso-Pharyngeal Nerve.*—When this nerve was pricked, scraped, or divided, no response was observed. The muscles of the root of the tongue were most probably set in motion by the compression of this nerve at intervals; but no opportunity occurred of bringing this part of the tongue into view. Neither in its entire nor divided state did any struggle arise from the continued compression of this nerve, which is therefore regarded as one simply of muscular motion.

5. *Experiments upon the Ninth Nerve.*—The sensible surface of the tongue is supplied by the ganglionic portion of the fifth nerve, whilst the muscles of its fore part are furnished with branches from the ninth nerve. No sign of sensation was evinced by mechanically irritating the trunk of this nerve, and its division was unattended with any sign of feeling or pain. But upon pinching it slightly at intervals, those muscles which it supplies, on the same side of the tongue, were convulsed. If the nerve was forcibly compressed, a slight gulp followed. When the nerve was divided, pinching the upper end of it was not followed by any muscular contractions.

6. *Experiments upon the Sympathetic Nerve.*—No experiments upon this nerve have hitherto exhibited any signs of sensation or muscular motion of any kind whatever. Its division is never followed by any visible effect.

Remarks.—By these observations some researches of other experimenters stand confirmed, whilst others are contradicted; the necessary consequence of discrepancies, often arising from the different modes of applying certain terms. Although the mysterious properties and actions of the nerves may never be completely unravelled, yet much has been effected by the successive and combined efforts of physiologists of different ages and countries.

The present investigation leads to the assumption, that one only of those nerves which derive their roots from the brain itself is, according to the definition laid down, a nerve of sensation. This is the larger and ganglionic division of the fifth nerve, whereby animals are enabled to examine by touch and to feel.

With regard to the other nerves subjected to experiment in this inquiry, none of them appear to possess in themselves any power to excite consciousness or feeling directly. Some of them are simply nerves of motion, and they transmit no other impressions but such as excite local muscular motion, limited to the muscles which they supply. Others, again, seem to possess a property of a different description from either of the two former kinds. One of these, the eighth for example, appears to be so intimately connected with the respiratory function as to be capable of influencing it in a most remarkable degree, without exhibiting any sign of sensation in itself, or of simple and direct muscular contraction.

It is a most remarkable fact, that when a nerve which influences respiration is divided, and the upper division is bruised or compressed for a few seconds, the same effects occur as when the irritation is applied to the entire nerve. This phænomenon affords matter of curious and interesting speculation with regard to the relations which subsist between the nervous and the respiratory functions.

The further pursuit of this inquiry may lead to some further development of facts hitherto exposed in some instances to doubt and controversial discussion.

Dr. Hall was necessarily absent at one of the experiments, that on the ninth nerve; but he feels perfectly satisfied with the joint testimony of Mr. Field and Mr. Broughton.

On the Effects of Poisons on the Animal Œconomy. By Dr. Hodgkin *and Dr.* Rüppell.

Dr. Hodgkin and Dr. Rüppell, who were appointed at the Cambridge Meeting of the Association to draw up a report for the Medical Section respecting some points connected with the effect of poisons, stated that they were not as yet prepared to present the results of their inquiries; but Dr. Hodgkin informed the Section that his colleague had paid very considerable attention to the subject, although his extensive materials were not put together in a form to be offered to the Association. Dr. Hodgkin also laid before the Section the Fasciculi published by Dr. Rüppell, and read a preliminary paper reporting the progress which he had himself independently made, illustrated by various drawings by C. J. Canton, and wax models by Joseph Towne.

The points alluded to in this preliminary essay were the natural structure of the lining membrane of the stomach; the differences which it presents in its normal and abnormal state in respect of colour, consistence, and equality of surface; the condition of the mucous membrane of the stomach with respect to follicular appendages; some indications which may be drawn from the situation of that part of the stomach which has been most injured by ingesta; and the different extent to which various noxious agents and their effects may be traced along the course of the alimentary canal.

The drawings and models exhibited the appearances observed in numerous human stomachs, occasioned by disease, congestion, arsenic, hot water, sulphuric and prussic acids; and the effects of hot water, alcohol, arsenic, corrosive sublimate, and oxalic acid on dogs or horses.

Inquiries into the Varieties of Mechanism by which the Blood may be accelerated or retarded in the Arterial and Venous Systems of Mammalia. *By Dr.* T. J. Aitkin.

The attention of the Section was particularly directed to four modifications of arterial distribution, as indicated, (1.) by the angle at which a branch comes off from its trunk; (2.) the direction of the vessel; (3.) the subdivision; and (4.) the formation of plexus.

In illustration of the first, or angle of origin, Dr. Aitkin exhibited a preparation of the aorta of the tiger, in which the superior intercostals arose at an acute, the middle at a right, and the lower at an obtuse angle; from which he inferred that the force

and velocity of the blood are rendered equal through the whole series. In speaking of the direction of the vessel, he adverted to the tortuous entrance of the internal carotid and vertebral arteries into the skull in the human subject, and showed that it is still more remarkable in the horse, which in feeding requires to have the head for a considerable time in the dependent posture. But the best examples of the tortuous, or serpentine, course are to be seen in the spermatic arteries of the *Mammalia*. This mechanism, the author contends, adapts the circulation to the various positions in which organs may be placed, and to their states of action and repose. In speaking of the third modification, or the subdivision into numerous long branches, he particularly alluded to the observations of Sir A. Carlisle with respect to the arteries of the sloth, and showed that a similar ramification is found in the hedgehog, both in the arteries of the panniculus carnosus and of the mesentery. Of the last modification, the plexus, he showed examples in the rete mirabile of Galen in the internal carotid, and of Hovius in the ophthalmic artery, of the *Ruminantia*. He inferred that this structure prevents valvular turgescence, which would otherwise occur during the long period these animals keep their head in the dependent position while browsing. He also showed that a rete mirabile exists in the ophthalmic artery of the seal and goose, and considered it probable that in them it is conducive to the alternate adaptation of the eye to vision in air and water. He described the remarkable plexiform arrangement which exists in the mesenteric arteries and veins of the hog; and instituted a comparison between those vessels in carnivorous and herbivorous *Mammalia*, concluding that these modifications are in conformity with the transmission of blood through the liver, the rapidity of the peristaltic motion, and the power of nutrition.

Observations on the Anatomy of the Blood-vessels of the Porpoise. By Dr. Sharpey.

1. The artery of the anterior extremity or fin of the porpoise, corresponding to the brachial in man, presents a peculiarity of distribution similar to that observed in the arteries of the limbs of slow-moving animals. The vessel, after crossing the first rib, divides into a great many long and small arteries, which run nearly parallel, but repeatedly anastomose, so as to form an elongated plexus, consisting at its thickest part of at least forty vessels. This plexus continues as far as the distal end of the humerus, where its component vessels again unite into five or more larger arteries, which run along the radius and ulna.

2. Convoluted arterial plexuses, similar to those in the thorax and vertebral canal of this and other cetaceous animals, (in which situation they were particularly described by Mr. Hunter in the *Philosophical Transactions* for 1787,) are formed also by several arteries of the neck and head.

3. Several arteries show a tendency to divide into long parallel branches, of which the arteries of the bladder, vagina, and uterus offer a striking example.

4. The mode of division of the posterior or caudal portion of the aorta differ somewhat from the description given by Cuvier, in as much as that vessel is not wholly resolved into small branches, which unite to form it anew, but is only diminished in size, and concealed in the midst of a plexus formed by its branches, from which, after becoming larger, it again emerges.

5. The veins as well as the arteries present in several regions of the body a plexiform arrangement, and in some situations plexuses of both kinds of vessels are associated or mixed with one another.

6. The artery corresponding to the internal carotid, which at its origin is as large as in man, diminishes in a tapering manner, and without giving off branches, till it enters the skull, where it is scarcely thicker than a pin.

On the Use of the Omentum. By Mr. Dick.

From a comparison of the structure of this organ in the horse and in the sheep,—in the former the omentum being small, the intestines are fixed, and undergo comparatively little change of place,—the author inferred that the omentum might serve, by interposition between the intestines and abdominal parietes, to facilitate motion.

On the Infiltration of the Lungs with black Matter, and on black Expectoration. By Dr. W. Thomson.

The author particularly noticed the cases of this singular infiltration, occurring in coal-miners, iron-founders, and other workmen exposed by their employment to the inhalation of carbonaceous gases and powders. He referred to a variety of published and unpublished communications on the subject, and exhibited a number of preparations and drawings illustrative of the appearances, nature, and seat of the disease.

On Excision of diseased Joints. By Professor SYME.

The author explained that his object was not to discuss the merits of the operation, to institute a comparison between it and amputation, to establish the principles which seem most conducive to its safe and effectual performance, or to enter into any more particular details concerning the different modes of procedure which are requisite for the different joints, but merely to prove by actual demonstration that the two great objections which have been urged against the operation, however specious in theory, are not supported by experience. These objections he stated to be, 1st, that the diseased bone could not be completely removed by excision, so as to afford a perfect and permanent cure; and, 2ndly, that the limb preserved by the operation must be nearly or altogether useless to the patient. In reply to the first of these objections, he produced a woman, 44 years of age, who eight years ago had the shoulder-joint removed, on account of caries in the head of the os humeri which had existed for six or seven years, and reduced her to an extreme degree of weakness. The head of the bone, completely hollowed out by disease, was exhibited, and the woman showed that while her general health and strength were quite restored, there was hardly any perceptible difference in the utility of her arms. He also placed before the meeting a boy who had his elbow-joint excised between five and six years ago, on account of caries which had existed twelve months. The articulating extremities of all the bones entering into the formation of the joint were exhibited; and the boy proved, by free and varied movements of his arm, that he retained completely the power of flexion, extension, and rotation of the elbow, without any diminution of strength. In reply to the second objection, he stated that it seemed to be grounded on the difficulty of conceiving how the tendons, after being cut away from their attachments, could again adhere to the bones so as to move them in obedience to the action of the muscles, and on the erroneous idea which generally existed as to the changes that occur between the osseous surfaces subsequently to the operation. In order to show that when tendons have their attachments divided they readily acquire new ones, so as to perform their usual offices, he brought forward a patient who had suffered Chopart's amputation of the foot for caries of the tarsus and metatarsus, and who consequently, having only the os calcis and astragalus remaining, had had all the tendons opposing the extensors of the ankle divided, but who nevertheless retained completely the power of bending

and extending the joint. In respect to the changes which take place between the osseous surfaces after the operation of excision, he stated that anchylosis could not be induced unless the limb was kept perfectly motionless; and that the bones almost invariably became united, not by any structure analogous to a joint, but by means of a fibrous substance possessing such thickness, strength, and flexibility as to preserve the shape and firmness of the limb, and allow a proper degree of motion in the seat of the joint. A specimen of this structure was exhibited in the case of an elbow-joint which had been dissected about twelve months after the performance of excision. Having made these remarks, he submitted to the meeting the positive evidence afforded by several persons in addition to those already exhibited, in all of whom the operation of excision had preserved limbs hardly if at all less useful than they were before suffering from the disease.

Abstract of a Registry kept in the Lying-in Hospital of Great Britain-street, Dublin, from the year 1758 *to the end of* 1833. *By the late Dr.* JOSEPH CLARKE, *of Dublin.*

This communication illustrated in a very striking manner the importance of thorough ventilation, and the great diminution of mortality among the children in this hospital since that object has been attended to. It appears that during the 75 years mentioned, relief has been afforded to upwards of 129,000 poor women; that in 1781 every sixth child died within nine days after birth, of convulsive disease; and that after means of thorough ventilation had been adopted, the mortality of infants, in five successive years, was reduced to nearly one in twenty.

STATISTICS.

Statistics of Glasgow. By JAMES CLELAND, *LL.D.*

The parochial register of births in Glasgow being so defective that no reliance could be placed on it, Dr. Cleland obtained the necessary information in the following manner:

Concerning Births.—On 6th December 1829, he addressed a letter to each of the seventy-five clergymen and lay pastors in the city and suburbs who baptize children, requesting to be favoured with the number they might baptize from 14th December 1829, to 15th December 1830, both days inclusive, being the year previous

to the last Government census. The letter was accompanied by a book, in which the sexes and the particular parishes in which the parents resided were to be inserted. He also requested the various Societies of Baptists, the Society of Friends, and Jews, and others, who do not dispense the ordinance of baptism of infants, to favour him with the above particulars relative to children born to members of their societies, and at maturity. He had the satisfaction of receiving returns from the *whole*; as also an account of the children of parents who, while disapproving of infant baptism, did not belong to any religious society; when it appeared that in the city and suburbs there were 6397 children baptized or born to Baptists, &c., and that of that number there were only 3225 inserted in the parochial registers, leaving unregistered 3172.

Concerning Marriages.—Although in Scotland there is no marriage act as in England, restricting the solemnization of marriages to clergymen of the Established Church, this ordinance can only be regularly celebrated by persons duly called to the pastoral office, and not until a certificate of the proclamation of banns has been produced.

Persons irregularly married are deprived of the privileges of the Church till they appear before the Session, acknowledge their fault, and are reponed. From this circumstance, in connexion with the solicitude of the female and her friends to have the marriage registered, the marriage register of Glasgow and its suburbs may be held as correct for all statistical purposes.

Concerning Deaths.—The deaths are ascertained by the number of burials. The burying-grounds in the city and suburbs are placed under the management of fourteen wardens. These officers, who attend *every funeral*, enter in a memorandum book, at the *grave*, the name, age, and designation of the person buried, along with the amount of fee received, and the name of the undertaker. Having taken these and other particulars, the wardens afterwards enter the whole in a book, classified conformably to a printed schedule drawn up by Dr. Cleland. At the end of the year they furnish him with an abstract from their books; and it is from a combination of these abstracts that he ascertains the number of deaths at the various ages. The abstract includes still-born children, and the deaths of Jews and members of the Society of Friends, who have separate burying-places.

Concerning the Population of Glasgow and its Suburbs.—Having been appointed to take the sole charge of conducting the enumeration and classifying the population of the city of Glasgow and suburbs for the Government census of 1831, the

author employed twelve parochial beadles, nineteen mercantile clerks, and one superintendent of police to take up the lists. Before the books were prepared, an advertisement was put in the ten Glasgow newspapers, requesting the inhabitants to favour him with their suggestions as to classification; and before the list-takers commenced their operations, bills were posted on the public places and dwelling-houses of the city, informing the inhabitants of the nature of the inquiries, and that they had no reference to taxes; and, moreover, that noncompliance, or giving a false return, subjected them to a fine. When the books were returned, the public, through the medium of the press, were requested to call at an office, appointed for the purpose, and to correct any omission or error which might have been made in their returns.

The list-takers having made oath before the Lord Provost that the name of every householder in the district assigned to them had, with the other particulars, been faithfully entered in a book, the author proceeded to classification, and to the formation of tables and abstracts for each parish, containing numerous details not required for the Government digest.

Bill of Mortality from 14*th Dec.* 1829, *to* 15*th Dec.* 1830.

A General List of Births, Baptisms, Marriages, and Burials, within the ten Parishes of Glasgow, and the Suburban Parishes of Barony and Gorbals.

Births and Baptisms detailed thus:

	Males.	Females.	Total.
Returns from Clergymen and Lay Pastors.	3281	3116	6397
Add still-born, from do.	246	225	471
Total	3527	3341	6868
Of this number there were registered only	1678	1547	3225
Number unregistered exclusive of still-born.	1603	1569	3172

The children were baptized as follows, viz.

By Clergymen of the Church of Scotland	3123
By do. of the Secession Church	664
By do. of the Relief Church	671
By do. of the Roman Catholic Church	915
By do. of the Scotch Episcopal Church, Independents, Methodists and other denominations, including births among Baptists, Society of Friends, Jews, &c.	1024
Total	6397

Of whom have died.

	Males.	Females.	Total.
Still-born	246	225	471
Under one year	463	414	877
1 and under 2,	316	307	623
2 — 5,	263	237	500
5 — 10,	134	119	253
10 — 20,	144	132	276
20 — 30,	189	145	334
30 — 40,	169	144	313
40 — 50,	184	164	348
50 — 60,	177	175	352
60 — 70,	168	171	339
70 — 75,	109	102	211
75 — 80,	55	58	113
80 — 85,	48	48	96
85 — 90,	24	26	50
90 — 95,	9	10	19
95 — 100,	3	6	9
100 —	0	1	1
Total	2701	2484	5185

Marriages engrossed in the registers of the City, Barony, and Gorbals.

In the City	857
Barony	691
Gorbals	371
Total	1919

Burials engrossed in the registers of the City, Barony, and Gorbals Burying-grounds.

	Males.	Females.	Total.
January	273	268	541
February	226	223	449
March	218	207	425
April	208	184	392
May	185	175	360
June	200	178	378
July	194	182	376
August	232	206	438
September	240	229	469
October	236	184	420
November	234	189	423
December	255	259	514
Total	2701	2484	5185

Total Burials within the City	1951
Total Burials in Barony Parish	1831
In Gorbals Burying Ground	1403
Total Burials in City and Suburbs	5185

Classified List of the Ages of Persons in Glasgow and the Suburban Parishes of Barony and Gorbals.

Ages of persons in Glasgow and in the suburban parishes of Barony and Gorbals, for the census of 1831.

Under 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 and upw.	Total.
Males, 15422	13127	10491	8489	15177	12179	8685	5549	3228	1090	260	26	1	93724
Females, 14855	12580	10720	12256	23008	14240	9329	6099	3692	1502	385	32	4	108702
Total 30277	25707	21211	20745	38185	26419	18014	11648	6920	2592	645	58	5	202426

Concerning the probability of Human Life in Glasgow.—The author states that he endeavoured to obtain from the medical gentlemen a note of the diseases of which their patients died during the period in which he had requested the clergy to give a note of the baptisms, but succeeded only with a small portion of the members of the faculty, and suggests that every attempt to accomplish this object, so interesting in a medical point of view, will fail, till a compulsory act regarding parochial registers be obtained.

That Glasgow is a place of average health for statistical purposes may be inferred from the daily state of the weather, which the author published in 1831, by which it appeared that the

average quantity of rain which fell yearly during thirty years preceding that period, amounted to rather less than twenty-three inches. But more particularly, the degree of health may be known, and tables formed for ascertaining the probability of human life, from a series of the Mortality Bills, where the age of the living and that of persons who have died are narrated in connexion with the population, and a table of longevity for Scotland which the author prepared in 1821; by which it appeared that, on an average of all the counties of Scotland, there was one person eighty years of age for every $143\frac{92}{100}$ of the population; while in the county of Lanark, with a population of 316,790, including 263,046 who live in towns, viz. in Glasgow 202,426, and in other towns 60,620, there was one such person for every $169\frac{71}{100}$, showing a degree of health in the population of Glasgow nearly equal to that of the whole of Scotland.

The following results have reference to Glasgow and its suburbs, which partake of a mercantile and manufacturing population, or something between Liverpool and Manchester, the town population being 198,518, and the rural 3908.

In 1831 the population was found to be 202,426, the burials 5185, and the rate of mortality consequently $39\frac{4}{100}$. The births being 6868, there is one birth for every $29\frac{47}{100}$ persons. The number of marriages being 1919, there are $3\frac{57}{100}$ births to each marriage, and one marriage for every $105\frac{48}{100}$ persons. The number of families being 41,965, there are $4\frac{82}{100}$ persons to each family. It is very satisfactory to know that with the same machinery in 1821, the population being 147,043, the burials 3686, the rate of mortality was ascertained to be $39\frac{89}{100}$, or in other words as near as may be to the mortality in 1831. By reference to the Bills of Mortality between the years 1821 and 1831, similar results will be found.

It appears from all the authentic Bills of Mortality the author has ever seen, that there are more males born than females, but taking the population above fifteen years the number of females preponderates. The following results for Glasgow are derived from the census of 1831:

Births—Males,	3527	Females,	3341	excess of Males,	186
Males under 5 years,	15422	Females,	14855	excess of Males,	567
Males under 10 years,	28549	Females,	27435	excess of Males,	1114
Males under 15 years,	39040	Females,	38155	excess of Males,	885
Males under 20 years,	47529	Females,	50411	excess of Females,	2882
Males under 30 years,	62706	Females,	73419	excess of Females,	10713
Males—entire Population,	93724	Females,	108702	excess of Females,	14978
Burials—Males,	2701	Females,	2484	excess of Males,	217

ADDENDA FOR 1831.

Description of Householders.—Married men 30,032. Widowers 1790. Bachelors 1437. Male householders 33,259. Widows

6824. Spinsters 1882. Female householders 8706. Total families 41,965.

Countries to which the Population belongs.—Scotch 163,600. English 2919. Irish 35,554. Foreigners 353. Total 202,426.

Religion of the Population.—Established 104,162. Dissenters, Episcopalians and Jews 71,299. Roman Catholics 26,965. Total 202,426.

Number of Paupers and expense of maintaining them.—The number of paupers in the city and suburbs being 5006, and the population 202,426, there is one pauper for every $40\frac{43}{100}$.

The number of paupers being 5006, and the sum expended for their maintenance or relief 17,281*l.* 18*s.* 0½*d.*, shows the cost of each pauper to be 3*l.* 9*s.* 0½*d.* If the sum for the relief of paupers were equally paid by the whole non-recipient population, the proportion to each would be *one shilling and ninepence and a small fraction.* The sum of 17,281*l.* 18*s.* 0½*d.* includes the entire expenditure of the out- and in-door paupers, surgeon's salaries, medicines, clothing and educating children, maintaining lunatics, funeral charges, &c.

The cost of each pauper in St. John's parish is 3*l.* 8*s.* 10½*d.* The poor in that parish are maintained or relieved on the parochial system introduced by Dr. Chalmers in 1820, *i. e.* by the Kirk Session from its own resources, without receiving any part of the general assessment for the poor, although the inhabitants of St. John's parish are assessed for the maintenance of the poor generally in the same manner as other citizens.

Analysis of the Report of an Agent employed by the Manchester Statistical Society in 1834, *to visit the Dwellings and ascertain the condition of the Working Population in Police Division No.* 2, *and in the first Subdivision of Police Division No.* 1, *of the Town of Manchester*. Communicated by the Society.*

The agent having been refused admittance into some houses, and the occupiers of others being absent and their dwellings closed, his report only extends to 4102 families, but which number comprises all the labouring population within this district into whose houses he obtained access.

The Report on the condition of the dwellings must be considered merely as the general impression of the agent, an intelligent Irishman, who was himself a hand-loom weaver, and who in this classification has been principally guided by the appearance of cleanliness or otherwise in the dwellings.

All the other entries are stated from the answers given by the parties themselves.

* The population of this portion of the town is (according to the census of 1831) 42,135 or 8932 families. It is a district inhabited more than any other in the town by the working classes and by those of the poorest description. It was on that account determined to commence the investigation in this quarter.

Country.	
English ...	2270
Irish	1761
Welsh	35
Scotch	30
Foreigners	6
	4102

Religion as professed to the Agent by the parties visited.	
Established Church...	2021
Roman Catholics	1473
Dissenters	591
Professing no Religion	17
	4102

Dwellings.	
Houses	3100
Cellars	752
Rooms	250
	4102

Report of the Visitor on the Condition of the Dwellings.	
Comfortable.....	1551
N.B. Of these houses 689 are reported to be well furnished.	
Not comfortable.....	2551
	4102

Rent of Houses, Cellars, and Rooms, per week.	s.	d.		s.	d.	
			not exceeding	1	0	45
Above	1	0	and not exceeding	1	6	320
	1	6	... ...	2	0	727
	2	0	... ...	2	6	992
	2	6	... ...	3	0	831
	3	0	... ...	3	6	434
	3	6	... ...	4	0	337
	4	0	... ...	4	6	84
	4	6	... ...	5	0	66
	5	0	... ...	5	6	2
	5	6	... ...	6	6	6
Average not exceeding ...				2	9	3844
Not ascertained						258
						4102

Classification of Individuals.	
4102 Families visited, consist of persons	19869
Lodgers	1165
	21034
Of this number, Children living in the Families,	
Above 12.....	3996
Under 12.....	8121
	12117
Children,	
Attending Day Schools ...	252
——— Sunday Schools	4680
	4932
Number of Parents who state that they can read	3114
Number of Persons who belong to Benefit Clubs	1163
Number of Persons in receipt of Wages.....	7789

Classification of the Employment of 7789 Persons in receipt of Wages.		
Employed in Factories.		
Spinners	141	
Carders	147	
Piecers.....	1087	
Throwsters.....	69	
Reelers	69	
Powerloom Weavers	608	
Dressers	60	
		2181
Employed in Warehouses.		
Warpers.....	20	
Winders	443	
Clerks	7	
Porters.....	144	
		614
Handloom Weavers.		
Fancy	3	
Silk	17	
Plain (Cotton)	2046	
		2066
Building Trades.		
Bricklayers.....	17	
Stonemasons	17	
Labourers	523	
Joiners.....	55	
		612
Calenderers and Dyers....	181	
Fustian Shearers	149	
Shoemakers	228	
Tailors.....	119	
Sempstresses and Dressmakers	149	
Washerwomen	136	
Other Occupations	1354	
		7789

Notice of the 'New Statistical Account of Scotland.' By Mr. GORDON, *Secretary to the Committee.*

There is now in progress and in the course of publication a periodical work descriptive of the parochial statistics of Scotland at the present time, under the title of *The New Statistical Account of Scotland.*

A similar work was produced upwards of forty years ago by the exertions of Sir John Sinclair, Bart., to whose enlightened enterprise so many of the most useful institutions in this country owe their existence or their improvement.

The two works resemble each other in the important circumstance that every parish has been treated by itself, and that the parochial accounts have been furnished by the ministers of the respective parishes. They resemble each other, also, in incorporating, as a relief to matters more strictly statistical, detached notices of the chief historical events, of the eminent characters, and of the remains of antiquity connected with the parishes. They differ from each other, 1st, in the arrangement, which in the new work presents the parishes placed together under their respective counties, while the matter of each parochial account is treated under the same heads in uniform succession; 2nd, in the greater expansion which the whole department of natural history, under the several branches of meteorology, hydrography, geology, zoology, and botany, has received in the new work; and, 3rd, in the statistical details themselves, which, from the changes that have taken place within the last forty years, are found to be so different from those of the former work as to render the present almost entirely new.

It may be added, that each parochial account in the new work observes the following general divisions: 1. Geography and Natural History; 2. Civil History; 3. Population; 4. Industry; 5. Parochial Œconomy. That the first and second of these divisions have the advantage of elucidation from county maps; that to each county there is appended a tabular summary of whatever particulars belonging to the several parishes are capable of being exhibited in a tabular form, together with some general observations applicable to the whole county, and not anticipated under individual parishes.

To this useful labour the clergy of the Church of Scotland have on this occasion been invited, not, as formerly, by an individual, but by the Society instituted to promote the interests of their sons and daughters; and it is honourable to the clergy that they have not only been cordially disposed to return to the

task, but that they have returned to it with a manifest increase of accomplishment for every part of its performance.

Three numbers of the work already published are now presented to the Association. Without specifying any portion of the statistical results which they have established, it may be noticed generally that the accounts are so uniformly complete on certain essential points, as to have furnished a set of tables, representing in every parish the quantity of cultivated and uncultivated land; the amount of agricultural produce; the different descriptions of the population; the ecclesiastical state of the parishes, as indicated by the various numbers of the several religious denominations, with the provisions for their respective clergy; the state of education, as shown by the number of teachers, and of the young in the course of receiving instruction; the number of the poor, and the amount of the provision made for them from the different sources of voluntary contribution, endowment, and assessment. At the same time, these are but the items which admit of being presented in a tabular form, and there is besides in each account a great variety of interesting notices on the several branches of natural and civil history.

Remarks on the Statistical Reports regarding Agriculture. By Earl Fitzwilliam, *F.R.S.*

The expediency of furnishing more minute details with respect to the agricultural part of statistical reports was suggested in these remarks. The statements ought to show not only the total amount of land in cultivation, but also the quantities allotted at the time of the inquiry to the various kinds of produce, the number and value of agricultural implements, the number of draught and other cattle, and similar details. Lord Fitzwilliam stated that he had succeeded in obtaining such returns for some parishes in his own neighbourhood, and observed, that accurate and minutely detailed information for only a small number of places would furnish more safe grounds for correct inferences than could be obtained from a more widely extended, but less precise inquiry.

The Rev. E. Stanley undertook to prosecute such an inquiry in his own parish (in Cheshire), and to furnish the results at the next meeting of the Association.

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Printed by Richard Taylor, Red Lion Court, Fleet Street.