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TO OUR READERS AND CORRESPONDENTS.

We have been favoured with Communications from Mr. Burnett, Mr. Kendal, and Mr. Jackson; they reached us too late for publication in the present Number.

We have seen Mr. Quarrill's new Table Lamp, and decidedly prefer it to any of its predecessors, as being perfectly sinumbral; of an elegant form; and, what is most important, simple in its construction and management. The want of a proper drawing and section has prevented our giving a more detailed description of it.

Our Correspondent at Tringham has furnished us with nothing new.

The Letter on Agricultural Chemistry is reserved.

In the Table of Contents to our last Volume, the Title of the following Paper was accidentally omitted:—

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THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

An Account of some of the Steam-Boats navigating the Hudson River in the State of New York. In a Letter from Mr. Renwick, Professor of Natural and Experimental Philosophy and Chemistry in Columbia College, to Captain Edward Sabine, R.A., Secretary of the Royal Society.

* * * * * *

You ask of me some further particulars in relation to the steam-boats on the Hudson, that I mentioned to you as so remarkable for their speed. I shall endeavour to give you all the information on this subject that is in my power.

Immediately upon the decision of the question between the representatives of Chancellor Livingston and Fulton, and those who contended for a free navigation, by which decision the exclusive grant vested in the former by the State of New York was set aside, several companies undertook the construction of passage-boats propelled by steam. Two of these were mere copies of the boats of Fulton, but lighter in frame, and propelled by engines more powerful in proportion; they, therefore, exceeded the boats of the old company in speed. Two others were constructed principally for the purpose of towing each a large passage-boat. These were fitted up in a splendid manner; and, from the comparative safety and comfort which they afforded, it was the general anticipation that they must obtain a preference. Various other boats, from other lines of communication, were also put upon the river; but no expectation seems to have existed at first, that it would be possible to make the passage to or from Albany between sunrise and sunset. All these boats were upon the low pressure principle, with condensing engines differing only in detail from the double engine of Watt.

Many months, however, had not elapsed, before an attempt was made to shorten the passage by the employment of more powerful means of propulsion. A boat was constructed upon a model apparently well adapted for quickness, being very similar to that of a fast sailing ship. This boat was furnished with an engine on the plan of Woolf, with two cylinders, one of which acted by high pressure, the other receiving steam of the first, as a condensing engine. This vessel, it was hoped by the proprietors, would be able to perform the passage between New York and Albany in about twelve hours. She did not, however, succeed in this; her average passages being at least sixteen hours. A similar attempt was made by means of a boiler generating high steam, communicating with a cylinder of more than the usual length, and acting then by its expansion; the steam being afterwards condensed. This boat also failed in realizing the anticipations of its proprietors.

The competition produced by the increased number of steam-boats, all of them much less costly than the boats of the Fulton company, had such an effect upon the price of the passage, that that association could no longer continue the contest; its boats were therefore withdrawn, and sold to persons who have applied them to other objects. When the representatives of Fulton had thus withdrawn from the contest, the Messrs. Stevens, sons of one of the original researchers for the method of propelling boats by steam, entered into the competition. These gentlemen had hitherto kept aloof from it, from highly honourable and delicate feelings, being unwilling to assist in destroying the prospects of the heirs and other representatives of Livingston and Fulton. When, however, they found that this company had abandoned the hope of maintaining a successful competition, and had withdrawn their boats, they felt no longer precluded from availing themselves of a privilege, now opened to all. Their first step was to bring round from Philadelphia a new vessel they had constructed there for the navigation of the Delaware. Upon that river no exclusive privilege had ever existed, and hence had arisen a continual trial of skill in the increase of the speed of steam-boats. This vessel, on trial, was found to be superior to any that had been before constructed. Placed upon the Hudson, the passage to Albany was readily made by her in the average space of fourteen hours. Leaving either place at sunrise, the distance was therefore performed during the summer season, before the daylight ended.

The introduction of this boat caused a complete change in the plan of travelling, which, instead of being principally performed during the night, was now rendered practicable during the day; a change extremely agreeable to voyagers in pursuit of pleasure, and convenient to men of business from its rapidity.

Some of the improvements in this boat I shall particularize when I speak of the last and most perfect boat of the same proprietors.

Contemporaneous with the introduction of this vessel upon the Hudson, the same gentlemen commenced the construction of another, upon nearly the same model, but with an engine of greater proportionate power. A faulty casting, however, rendered the engine incapable of working as well as it ought to have done, and no increase of speed was at first obtained.

For my own part, I must confess that I had come to the conclusion, that the speed now obtained was probably the maximum. I founded this opinion on the fact, that a wave unexampled in any former case was raised in front of these rapidly moving boats. The theory of Juan, showing that this cause of resistance, although insensible at slow velocities, increases with their fourth power, pointed out a final limit to the attainable speed. That the maximum was reached, I inferred from the fact that the second boat, although propelled by a more powerful engine, was not more rapid in her motion than the first. I was unaware at the time of the faulty part of the engine; but that I was in some measure correct is shown from the fact, that although the engine has been since put in perfect order, the acceleration bears but a small proportion to the difference of power.

Mr. Robert L. Stevens, however, viewed the subject in another light. Aware of the resistance growing out of the wave, he did not consider it insuperable, but conceived that, by a change in the figure of the prow, it might be in a great measure removed. The shape of the bow of the boats I have mentioned, departed in some degree from that formerly employed. Mr. Fulton, in his earlier boats, had employed flat bottoms, and prows nearly of the shape of a wedge with plane surfaces. I recollect, even at that early date, having combated the propriety of this plan in a conversation I had with him. The changes that he and his imitators subsequently made, were, however, rather grounded upon the necessity of increasing the strength of the vessels by regular curves in their moulds, than from a conviction of the error in the principle. The last boats built under his own directions, resembled in form vessels intended to be propelled by sails, but of a small draught of water.

Mr. Stevens, from experience, and a just view of the principles, was led to a different conclusion; and hence the stem of his vessels, which, above the water line, had the usual rake and curvature, began there to incline much more rapidly towards the plane of the keel than is usual, and thus the entrance of the vessel into the water partook at least as much of the inclined plane as of the wedge. But the change of form was too abrupt, and hence the enhanced height of the wave raised by a rapid motion.

In this state of the case, he instituted a set of experiments on the motion of figures of different forms through the water, at different velocities. The results of these, as he has stated them to me, are curious. The most remarkable is, that different forms are different in their good properties at different velocities. Upon the basis of these experiments, he commenced the building of a third boat, which I shall now proceed to describe to you.

The extreme length of this vessel (the North America) upon deck is 178 feet; her breadth of beam 28 feet; the depth of hold 9 feet. Her general figure I cannot better describe to you than by comparing it to the bowl of a table-spoon. The cutwater has a great rake, but in a uniform and regular curve; and all the curves upon the bottom are regular, and without any abrupt angles. The sternpost, to increase the power of the rudder, is vertical; an unusually large mass of dead wood therefore exists towards the stern, while there is but little near the bow. I have not considered it proper to apply to Mr. Stevens for a draught of the hull of this vessel, as it would be unfair to ask him to disclose what is his only safety from the imitation of his competitors. The North America is impelled by two condensing engines, each of the estimated power of 85 horses. These, with the boilers, are placed upon a platform, formed by a prolongation of the beams of the deck, until they meet the wheel guards, which are carried in a regular sweep from the stem to the stern of the vessel. The breadth of the deck, afore and abaft the buildings which inclose the wheels and machinery, is therefore considerably increased, while an uninterrupted passage is left between them from the stem to the stern of the vessel. An advantage similar to the last is gained in the cabins beneath, which, by opening folding doors, may be thrown into one suite from the cabin windows to the bow.

The wheels are $13\frac{1}{2}$ feet in breadth, and 21 feet in diameter. There is a peculiarity in their construction which I conceive to be one of the most important of the improvements for which steam navigation is indebted to Mr. Stevens. Experience had shown that a multiplication in the number of paddles (as is the case in an undershot water-wheel, and is there of value) is injurious in a steam-boat. The best arrangement is, that when one paddle is vertical, the preceding one shall be just quitting the water, and the succeeding entering it; in this way no more than two paddles can be in the water at a time, while a water-wheel works best when there may be four. There appears to me to be an obvious reason for the difference between a wheel propelled by a water-fall, and one acting upon water as a resistance to propel a vessel. In the first case it is advantageous to check the forward motion of the water; in the second, the paddle will act with most power upon water at rest, with respect to the surrounding mass. Now the more numerous the paddles, the greater the agitation that is produced; and each will in succession strike on water following in the wake of that which has preceded it, and which therefore acts as a less powerful resistance. But the paddles strike the water obliquely, instead of entering edge-wise. Each, therefore, meets a sudden resistance, that reacts as a shock upon the engine; and, in a small number of paddles, these shocks are not only greater, but, being less frequent, oppose a much more unequal action to the moving power. Both the boat and engine have been found to suffer extremely from this cause. I have been informed that it has been corrected in England, by inclining the plane of the paddle to the axis of the wheel, so that the edge of the blade enters the water first at one corner, and is immersed gradually. But in this construction the force is exerted obliquely, and constantly to a disadvantage; much power will therefore be lost.

To understand the improvement of Mr. Stevens, you have only to consider the water-wheel to be sawn into three parts, one of these to be removed back one-third, and another two-thirds of the distance between the original place of the first paddle and that which succeeds it. The water-wheel may, therefore, be considered as triple; and as each paddle will form a wake little broader than itself, those of each separate wheel will strike upon water at rest, in relation to the surrounding fluid. The force of the blow is, however, but one-third of what it is in a continuous paddle, and the succession so rapid as to oppose almost a constant resistance to the engine. Such a wheel, therefore, so far from rendering the motion irregular, acts as a fly, and that part of the machinery, of such vital importance in the boats of Fulton, is entirely omitted in those of Stevens.

Of the engines by which this boat is propelled, I have little to say: they are, in almost every respect, identical with the engine of Watt. But one essential difference, that I have noted, is, that the air-pump has more power than is usual. It will therefore keep up a vacuum in the condenser, even when the steam has a greater pressure than is usual in engines in ordinary situations. In the boats of the Fulton company it occasionally happened that, in anxiety to obtain speed, the steam-gauge was permitted to rise to 15 inches. This did not, however, cause an increase of power at all proportioned to the increased pressure; for an air-pump of the proportions of

Watt's engines was not sufficient to free the condenser from steam, and maintain a proper vacuum. In order to be provided for such a case, Mr. Stevens makes, as I have stated, his air-pump of greater power. In the North America, however, it has been unnecessary to employ it, for in two passages I have made in her, (one of them the most rapid she ever performed,) the steam-gauge never rose above eight inches. Nor do I consider that she has ever yet been brought up to her greatest speed, as even with this comparatively low pressure the steam was cut off at the half-stroke, and permitted to act by its expansive force. Mr. Stevens, I believe, is of opinion, that the boilers are sufficient to supply steam of 12 or 14 inches during the entire stroke of the engine, while the air-pump has power to maintain, at the same time, a vacuum in the condenser. If this be so, the speed may be enhanced, as the wave, that is at present raised in front of the boat, is even less than I have noticed it in front of others of not more than half the speed.

The boilers in all the boats of Stevens, as in those of Fulton, are of copper; and I do not apprehend that, with the highest pressure that can be given them, any danger is to be feared by the passengers. I do not consider that this is the case with any high-pressure boilers. The truth is, that both are liable to burst, from the natural imperfection of materials and workmanship. In the case of a boiler, where the material, supposing the safety-valve to be fastened down, will not bear an internal pressure of more than 8 or 10 pounds to the square inch (marked by from 16 to 20 inches of the steam-gauge), a small vent will discharge the steam, whose expansive force is far from excessive, while the temperature of the water is not such as to augment the volume of steam in any great degree. But when the pressure amounts to 60 or 70 pounds on the inch, and the boiler is proved to bear 100lbs., as in our high-pressure engines, no sooner is a vent given than the whole of the contained water is converted into steam, which expands itself with explosive violence.

I have to note another variation in the engines of the North America from those of Watt: it consists in the suppres sion of the parallel motion. The upper end of the piston-rod bears a cross bar, which works between guides formed of iron plates, screwed down upon upright posts. If nothing be gained in the working of the engine, much is in the fitting it up; for the parallel motion, of all parts of the engine, requires perhaps the most accurate workmanship.

The external appearance of the engines of the North America is less finished than the better class of English engines. There is, however, no real inferiority. The castings, made at the Westpoint foundery, are excellent; the boring of the cylinder and air-pump is perfect, and the fitting up, performed under the immediate direction of Mr. Robert L. Stevens, is not to be exceeded. I mention this, because the only specimen of an American steam-boat that has yet reached Europe, was most deficient in all these particulars. That vessel was, however, in every respect, far behind the better class of our steam-boats, even at that distant period (1816), and vastly inferior to those which are now constructed.

In our boats intended for the navigation of rivers, several points necessary to be observed in those intended for the ocean, may be omitted. It is, for instance, unnecessary that the engine should work under deck. Hence in all our engines the length of the stroke is greater than those described to me in the English steam-boats; in the latter, also, the position of boilers, and even of the engine, upon the wheel-guards, would be improper, and thus much of the comfort, that this plan affords to the passengers in the North America, would be unattainable. As the steam-boats on the Hudson never make use of sails, and as the waves rarely run high, every other property of a vessel for navigating the ocean, except stability (for instance, the capability of holding close to the wind), may be neglected, in the search for the prow of least resistance.

In the steam-boats on Fulton's plan the engine varies from those of Watt, and consequently from those of Stevens, in the suppression of the working beam; the reciprocating rectilineal motion of the piston-rod is changed into circular by means of two connecting rods, attached to a cross bar upon the top of the piston-rod; these take hold of cranks, or rather eccentric pins, in wheels upon the axes of the water-wheels. This plan has the disadvantage, that more power is lost by obliquity of action, than when a lever-beam and a connecting rod are used, as in the engine of Watt. When it is used in the body of the vessel, it has the advantage of compactness, occupying far less room.

In order to give you a proper idea of the velocity of the North America, some other circumstances require to be stated. The distance from New York to Albany has usually been estimated at 160 miles; the post-road between the two places is little less than this, as has been found by a recent measurement performed by the post-office department. It is, however, alleged, that the course on the river is not so much, and the surveyor-general of our state has recently published a statement of actual surveys on the river, that reduce it to less than 150. These, however, are the shortest possible lines that can be drawn from point to point over the several reaches. As steam-boats cannot follow these lines, but frequently cross the river to stop at landings, I cannot consider the actual distance as less than the first estimate; I shall, however, assume it to be 154 miles. The average passages of the North America for the last year, including stoppages, were performed in less than twelve hours; on one occasion in little more than ten. The delay at nine stated landings cannot be taken at less than an hour, which leaves eleven hours for a distance of 154 miles, or 14 miles per hour. As this average is taken from passages both up and down the river, any difference arising from the different rate of the flood and ebb tides, which at some seasons is perceptible, and any effect of current whatever, may be left out of view, and fourteen miles per hour be taken as her average speed through the water.

* * * * * *

Communication on the Structure and Economy of the Greenland Whale, made at the Royal Institution of Great Britain. By J. Harwood, M.D. F.R.S., Professor of Natural History in the Royal Institution.

This discourse was illustrated by means of a very extensive series of specimens, &c.

There is, perhaps, no part of the history of the animal world which is less generally known, to those who have not devoted particular attention to Zoology, than that of the Cetacea; nor certainly is there any, more justly entitled to our consideration, from the sublime examples which this tribe affords of Creative wisdom and power. I have, therefore, chosen the Greenland whale for our consideration this evening, because no individual can be expected to offer for our contemplation, more impressive illustrations of the Creator's attributes, than this stupendous piece of animal mechanism; and, especially, when, not contented with understanding its mere distinctive characters, we regard those conditions in its existence, and those curious modifications in structure, which have adapted its ponderous bulk to a medium, whose specific gravity is so like unto its own, and which afford to its progressive motion the widest geographical range.

Although our yearly intercourse with the cetacea during some centuries, has, in modern times, materially extended our knowledge of this gigantic race of beings, we should greatly err in supposing, that their remarkable submarine habits and economy escaped the attention of the observers of antiquity; and it would be an injustice to the memory of so true a philosopher as Aristotle, were I now to omit to mention, that the interest which was excited in his capacious mind by the wonderful characters of cetaceous animals, conducted him to a knowledge of the nature of these creatures, which is calculated greatly to excite the surprise of those naturalists whose opportunities of investigation have even been the most extended.

I shall, therefore, notice a few of his observations concerning them, which may prove interesting, from their accuracy, from their antiquity, and from the infant state of natural science at the period in which they were written.

"There are," he says, "some animals, which receive and return the water, for the same reason, as others which respire, receive and return the air:"—here he of course alludes to fishes, which, in the act of respiration, receive the water through the mouth by the expansibility of their fauces, and return it through the beautiful laminated surfaces of their breathing organs, or gills:—"but there are others," he adds, "which do so," that is, receive and return the water, "on account of the nourishment contained within it; and, since they receive their food in water, it is necessary that they should have an organ by which the latter (the water) may be returned or ejected; such animals, therefore, which employ the water in a manner analogous to respiration, have gills; but those sanguineous (warm-blooded) animals, which employ the water on account of the food it contains, have spiracles, or blow-holes." This, it will be observed, is a very interesting distinction between the fishes, and the creatures on which we are now treating.

Aristotle's observations on the sense of hearing, and on the voice of these animals, are also highly philosophical; after showing the incompatibility of voice with the structure of fishes, allowing, however, that many do produce certain sounds, he adds, in regard to the cetacea, "the dolphin likewise produces a stridulous sound, and murmurs when he comes into the air; yet not like these fishes, for the sound emitted by the dolphin is voice, since he possesses lungs and an air tube, although he cannot produce articulate voice;" and again, he says, in regard to his respiration, "when caught in nets, he is soon suffocated, in consequence of not respiring, although out of water he lives a long time, murmuring, and making sounds analogous to those of other animals which respire air."

After these and many other equally admirable observations on the part of Aristotle, it appears surprising that the cetaceous animals should ever have been erroneously associated with fishes in the works of more recent naturalists, from their mere possession of a fish-like form, and the consequent absence of hinder limbs; conditions which are rendered necessary by their fish-like progression.

Yet not only Ray and Willoughby, but even Linneus, in his earlier works, improperly placed them at the head of that class.

Linneus, however, afterwards followed Aristotle, in justly considering them as a tribe of creatures which resembled quadrupeds in disguise; since, unlike fishes, they not only, as we have seen, breathe the air by means of true lungs, but they closely resemble quadrupeds in much of their general construction, in their manners, in their intelligence, and in the energy of their senses. Their hearts, also, which propel warm, red blood, present no material modification in their structure from those of quadrupeds. Their other viscera somewhat resemble those of the ruminantia, and the size of their brain often even exceeds that of the generality of the mammalia.

Being, therefore, mammalia in their economy and their structure, they, in fact, only resemble fishes in inhabiting the same element, and in possessing that external fish-like form, which, being the best adapted for aquatic avocations, necessarily occasions differences in the details of their internal structure. The most obvious and striking peculiarities, which first attract our notice in the skeleton of the cetacea, are the enormous size of the head, in the whales; the almost entire absence of neck; the length and similarity of the bones of the spine; their ribs being comparatively few in number; the shortness of their arms; and the absence of hinder extremities, an os sacrum, and a true pelvis. Whales have, nevertheless, the rudiments of the latter, although the two bones which represent it, neither unite before, nor are they attached to the vertebræ.

The excessive shortness of their necks, although composed, generally, of only one bone less than the longest neck of a quadruped—as that of the giraffe, for example—renders any separate motion of their heads almost impossible, since the bones of the neck of the whale kind are excessively thin, and immoveably joined together. This, I am disposed to consider as a condition favourable to rapid progression, as that of birds is assisted by the immoveable state of the spine of the back, by which their centre of gravity is rendered less liable to be varied, and their bodies to be thrown out of equilibrium during their rapid flight; for, did the spine of the back of a bird possess great flexibility, its centre of gravity would be probably changed by every extra effort of either wing; and to counteract the same tendency, therefore, the necks of whales and of fishes have, probably, been rendered equally immoveable.

The Greenland whale may, I think, be considered as typical of the order cetacea, a tribe of creatures which, unlike fishes, generally possess only two fins, with the exception of the tail; and, although some species possess a third fin, on their backs, this latter possesses no bone in its composition; so beautifully is the analogy preserved between these animals and the rest of the class mammalia to which they belong. When, indeed, we examine the cetacea more critically, we find that these instruments, which present the external appearance of breast fins, by means of which they sustain their equilibrium, and perform gentle motions, owe their present fin-like form simply to the covering with which they are invested; for, instead of being composed of straight spines, like those of fishes, they conceal bones and muscles, formed very like those of the fore legs of land quadrupeds; but their hand alone appears externally, and we see it so enveloped in dense skin, that its fingers have no separate motion. But, as the several bones of the fingers are united together by means of intermediate cartilages instead of capsular ligaments, the fins, or, more strictly, the hands, possess great pliancy and strength, and enable the whale kind to spread them upon their sides, and on the breast; and, as Aristotle observed, in this way, to sustain their young beneath them, closely compressed to their bodies.

The fin, or hand of the common whale, is flat, and of much greater proportionate size than in many other cetaceous animals, which extension of the organs of equilibrium appears to have been required to compensate for the more unwieldy construction of the body of the creature. Yet, from the structure of the true and finely organised hand of the ape tribe, to the rude fin of the whale, we perceive no abrupt progression; since the fore extremities of the amphibious mammalia are precisely intermediate in their formation.

These beautiful gradations in organization afford some of the most interesting and apparent exhibitions of intention or design, which are presented to our notice in surveying the animal world; we may trace the gradual conversion of the hand or fore foot of the terrestrial quadrupeds into the fin of the whale, most obviously, by commencing with such quadrupeds as only occasionally frequent the water, in which the fin or web between the toes is short and imperfect; and thence proceeding in our examination, successively, through the otters, seals, walrus, the manati, dugongs, to, lastly, the whales, in which all the external appearance of a true hand is lost, though, internally, its structure yet identifies the fin with this organ.

The tail of a large whale measures about twenty-five feet across. It is composed of several layers of tendinous fibres, strongly matted together within an oily membrane; which structure imparts to it immense mechanical strength: it is also flattened horizontally, for the purpose of frequently and suddenly forcing the creature to the surface of the water to breathe; while the tails of fishes, on the contrary, are formed vertically, because their actions being performed chiefly in the depths, they do not require to rise frequently to the surface. But, in the whale, the tail, which is moved by immense depressor or flexor muscles, which are inserted into it, and form two large ridges beneath the body, becomes, from its enormous size and power, the most destructive instrument of defence with which any animal has been gifted. When whales are feeding near the surface of the water, this instrument acts with comparatively little force; for their hands or breast-fins are almost sufficient alone, to modify the movements of their bodies, and thus they swim slowly backwards and forwards, with the mouth generally wide open, and rise at each extremity of their short course to breathe. In playing on the surface, they also move in circles, and, occasionally, with the agility of the salmon, they may be seen to elevate their vast bulk almost out of the water; but, when the violent impulse by the tail, necessary to such an action, is differently directed, they dart like an arrow downwards into unfathomable depths, or, they rapidly extend their progress over vast tracts of the earth's surface.

But while we contemplate with surprise the voluntary powers of this creature in its native element, how great is our amazement in regarding the involuntary muscular efforts of its heart and arterial system! Mr. Hunter having first informed us, that he found the principal artery of the body to measure not less than three feet in circumference, and that it received from ten to fifteen gallons of blood at every pulsation of the heart. Therefore, as Dr. Kidd has observed, if we consider the heart of the whale not to exceed twenty pulsations per minute, at this rate of fifteen gallons received by the artery at every pulsation, we find, that not less a quantity than four hundred and thirty-two thousand gallons, or eight thousand hogsheads of blood, do literally pass through the heart of a whale during every twenty-four hours of the creature's existence.

I may, however, observe, that my friend, Dr. James Alderson, who has more recently had an opportunity of examining the heart of the same species of whale as the one to which Mr. Hunter alluded, although he found the aorta to be of equal size, supposes that the capacity of the left ventricle was not equal to the reception of more than eight or ten gallons of blood.

The heart of the whale, although much flattened, presents, otherwise, no important deviation in its structure from that of terrestrial quadrupeds; but, like that of other diving mammalia, and of the seals which I described on a former occasion, it is connected with an enormous development in the arterial and venous systems, in order to preserve it free from the oppression which would otherwise be occasioned by the returning blood; thereby to extend the intervals between respiration: to this end, the vessels, in various parts of the body, as Mr. Hunter observed, form, by their innumerable tortuous subdivisions, vast spongy receptacles; and, in other situations, the trunks themselves seem to be proportionately much enlarged. The proportionate quantity, also, of blood, in the whale, as in the seal, appears to be far greater than in land animals, which is, indeed, the case in all the aquatic mammalia.

I recollect having been surprised by an observation of an old Greenland captain, that the blood of all the animals of high northern latitudes was of a much darker colour than in those of more southern regions; it being, he remarked, in many, almost black; he alluded, especially, to the aquatic mammalia, which fell most under his observation, and such is literally the case in them. I have since observed the same fact to obtain scarcely less in the diving birds; and it is, perhaps, occasioned by the slow return of the venous blood to the heart, during frequent submersion, by which it probably acquires a superabundance, or an extra quantity of carbon. In ourselves, it may be added, that the same appearance of the blood is produced, by artificially arresting its progress in the veins; and that which is slowly drawn from the arm is, on the same principle, much darker than that which flows freely; a circumstance, even to the present day, often erroneously attributed to a morbid state of that fluid.

I shall now endeavour to describe to you another interesting peculiarity in the whale tribes. Beneath their smooth skins, the bodies of these animals are well known to be surrounded by an enormously thick membrane, which contains a prodigious quantity of fluid oil. This fluid oil, in like manner, pervades every part of the substance of their bones, which, unlike those of quadrupeds, are not hollow, but entirely spongy or cellular.

The blubber, or membrane, which contains the oil, varies, in the common whale, in its depth; it is two feet thick in several situations, especially across the back of the neck; but it even extends to three feet in thickness in the lip, near the angle of the mouth. It is comparatively the most abundant, and the oil is of the finest quality in young whales; hence, a sucking whale of nineteen feet long, and fourteen in circumference, has been known to yield six tons of oil, although its whalebone was not one foot in length, and far too short to enable it to catch food. In young whales, also, the blubber is almost white; in others it is found of a yellowish colour; and in some, apparently from their partaking of a peculiar kind of nourishment, it acquires almost the red appearance of the flesh of the salmon.

The blubber may, I think, be considered as a less dense portion of the true skin, consisting, in fact, as I have often seen at Hull, of a strong tendinous membrane, whose fibres interweave each other in every direction, and which contain the oil within them; but, when deprived of the oil, these fibres appear like an irregular network of tendon, differing in the fineness of its texture in different situations; it being most compact, where it is nearest to the surface of the body, and decreasing in its density as it dips downwards towards the muscles. In striking the back of the whale, therefore, the harpoon is plunged obliquely into this powerful tendinous network; which generally holds it so firmly, that I believe it is almost as common for the well-tempered iron to be broken as to be withdrawn; but, in destroying the creature, I may add, that its most mortal part, where the lances are afterwards applied, is a little below, and posterior to the origin of the fin, where the heart and the larger vessels are situated.

The greatest supply of oil, yielded by a single whale, of which I have been enabled to obtain a well-authenticated account, was the enormous quantity of one hundred and seventeen butts, or about forty-three tons, which was removed from a whale, struck by a person of the name of Pashby, who was harpooner to the Fanny, whaler, of Hull; and as the blubber is supposed to weigh about one-third of the whole, we here contemplate an animal body weighing no less than one hundred and twenty-nine tons.

Another whale, struck by a harpooner, from whom I received the account, yielded ninety-seven butts of blubber, and had whalebone which measured thirteen feet and a half in length, which is the length of the specimens of whalebone now before us; forty butts of oil, however, are considered a good average produce.

The necessity for this wonderful provision in the Greenland whale, to which I have last adverted, the abundance of its oil, is rendered more apparent, when it is known that the real specific gravity of the muscles of this creature is rather greater than that of the muscles of quadrupeds; but, by means of its oil, so nicely is its body balanced in the surrounding fluid, that it scarcely exceeds the specific gravity of the water. But this prodigious quantity of oil not only thus materially decreases its specific gravity, in which capacity it has been aptly compared to a cork-jacket, but it seems to have been intended as the most perfect of all the various kinds of clothing, with which the mammalia have been gifted; for, being a very bad conductor away of heat, it thus preserves the warm bodies of the whale kind from becoming chilled by the low temperature of the surrounding fluid. In diving birds, it is no less interesting to observe, that the same admirable precaution is had recourse to, though in fishes, whose bodies have naturally a low temperature, this being unnecessary, the oil is differently employed, and serves other interesting purposes in their economy.

But the blubber further assists, by its elasticity, in preserving the smoothness and rotundity of the body of the whale kind, which animals, as we see, have not only been deprived of external ears, or of other external appendages, which would tend to impede their rapid progression, but even the mammæ, instead of assuming their usual prominent form, are so flattened and extended beneath the skin as scarcely to elevate the surface; and on the same principle, the testes never descend from the lumbar region.

I must now direct your attention to the very remarkable exterior clothing of the whale. It is, in the first place, a curious fact, and one which is, perhaps, peculiar to the tribe, that those parts of the skin which are exterior to the blubber, in a young whale, are twice as thick as they are found to be in the adult, having measured an inch and three quarters in thickness.

Now these parts are generally called, from the analogy of their position only, I conceive, the cuticle, and the rete mucosum; to preserve which supposed analogy, anatomists are obliged to describe the rete mucosum of the whale as being three quarters of an inch in thickness. But after a careful examination of the recent skin of cetaceous animals, I cannot help believing that there is no analogy whatsoever between this substance called rete mucosum, in whales, and that of terrestrial quadrupeds. It appears to me to be a substance of a nature as peculiar to itself, as that of whalebone, or of ivory; and it is here, perhaps, destined to fulfil as peculiar a part in the animal economy, as those substances. It is of a dark colour throughout; it takes its origin from the outer surface, and, consequently, from the most dense portion of the true skin; it is of a sub-corneous texture, and consists of a dense congeries of parallel vertical filaments, having a great degree of elasticity. Immediately beneath the inferior surface of this substance, there is a black slimy fluid which is easily separated, and which is, perhaps, the only vestige of rete mucosum; and this substance is covered, externally, with a thin, smooth, black cuticle, which is easily split into detached horizontal laminæ.

The whale, then, has the blubber, which I consider to be the true skin and the cellular membrane united; a very indistinct rete mucosum; and, above this, a firm elastic substance, resembling a second cuticle, with vertical fibres; and which is itself covered by a common cuticle, having horizontal laminæ.

Whether this substance, just noticed, possesses sensation or otherwise, I have not been enabled to determine, but I could perceive no nervous filaments or blood-vessels, to enter its structure, either in that of the whale or sea unicorn, when placed under a high magnifying power; it is, therefore, probably, insensible. The blubber, on the contrary, or the true skin, from its vascular and nervous organization, is, doubtless, highly endowed with sensibility. Thus constructed, the skin of the whale is, as before-mentioned, peculiarly soft, smooth, and flexible; and although, as Mr. Scoresby has observed, the pressure to which it is liable in the depths of the ocean, is sufficient to force water through the pores of the hardest wood, yet its inherent qualities render it impermeable to the action of that fluid. All these parts of the external clothing are so pervaded with oil, that the latter affords nourishment to several species of small marine animals, which are generally found adhering to the skin; and in those parts of the seas where whales abound, an oily exudation floats on the surface of the water.

On such a scale of dimensions has the Creator been pleased to construct the Greenland whale, that I have myself seen jawbones of this animal, which have measured twenty feet in length; what is called a double oyster-barrel, appears to me to convey the most accurate idea of the size of some of its vertebræ. Its tongue, which is of an oval form, is sufficiently large to fill four butts, when cut into pieces, or to weigh two tons; and to yield one hundred and twenty-six gallons of oil. Of so enormous a size are its lips, and so much do they abound in blubber, that one alone has afforded sufficient of the latter to yield four butts, or two tons of pure oil; and you are aware that the body of this creature acquires from fifty to seventy feet in length.

The velocity of motion, possessed by so huge a body as that of the common whale, has always been a source of astonishment; but it is sufficiently obvious, that, having been destined to inhabit depths so profound, and so far removed from the air it breathes, this velocity of motion was a condition necessary to its existence.

It, however, very materially increases the danger attendant on its capture; from the awful accident of a coil of the line of the descending struck whale, entangling itself around any part of the body of the manager of the line, while it is run out; for, as the animal descends at the rate of from thirteen to fifteen feet per second, in this case, the individual so entangled becomes immediately dragged to a depth from which he is never able again to rise to the surface; and thus managers of the line are sometimes snatched from boats with such instantaneous velocity, as to almost escape the notice of all present.

That this species of whale is naturally very timid, is apparent from various circumstances. From the excessive fear into which it is thrown by the infliction of a wound, when reposing on the surface of the sea, it has, on several occasions, been known to descend with such incautious velocity, as to even fracture its massive jaw-bones, and occasion its death, by striking itself against rocks at the bottom. Nevertheless, when urged to resentment, which, as in all other animals, is most readily excited when under the powerful influence of parental attachment, the whale not unfrequently exhibits fatal illustrations of its tremendous muscular force. Thus, with the posterior half of its body quickly elevated above the water, it is enabled, with its broad semilunar tail, which has been seen to measure twenty-six feet in breadth, and one and a half in thickness, to instantaneously shatter to pieces a strong boat by a single blow. I have been assured by Captain Beadling, on whose word I have great reason fully to rely, that having once wounded a large whale, it instantly elevated its tail high above one of the boats, and struck it with such force as to completely cleave it asunder transversely: the men it contained, by leaping into the water, were nevertheless all fortunately saved by a second boat. There is a poor crippled object now living at Hull, who was shown to me by Dr. Alderson; he was formerly a boat steerer of the Diana, commanded by Captain Clifford, in which employment, a whale that was struck, ran out all the lines, and at a blow, clove the boat asunder, breaking the thigh, hip, leg, arm, three ribs, and the lower jaw of this poor man; and, afterwards almost miraculously, dragged the extremity of the boat, in which he lay, seven leagues along the surface of the water, without sinking, within an hour and three quarters; when he was picked up by the Dundee of Dundee. This is, however, evidently, a yet more interesting illustration of the curative efforts of the system, in our own species, than even of the powers of offence in the whale.

Another, and, perhaps, still more generally fatal mode of retaliation had recourse to by a wounded whale, especially if it be accompanied by a young one, although fortunately one of less frequent occurrence, consists in the creature tilting furiously, and with impetuous velocity, with the snout, against a boat, by which the latter becomes inevitably shivered to pieces and lost. When in the agonies of death also, by the rolling motion, which a whale often assumes, such blows have frequently been communicated to boats, by its widely extended fins, as to shiver them to pieces.

The extreme fidelity of these wonderful animals towards each other, and their affection for their offspring, is almost incredible. So fondly attached are they to the society of their brethren, that many instances are recorded of their assuming a passive floating position, on the surface, after offering much resistance; as though disdaining to survive the loss of their companions. Thus, when the Cyrus had captured six, out of a herd of seven whales, and they were supported around the vessel on the water, the surviving one rose, and thrust its head amongst its dead brethren, and remained immoveable, close to the vessel, while it was killed.

In general, the female is accompanied in her progress by her young one, though, on the contrary, she sometimes wanders very far from it; and yet, by some unknown impulse, highly calculated to excite our amazement, she has no difficulty in finding it, though perfectly silent, in the vast and trackless ocean, as often as she requires; and the same may be said of all the cetacea. But further, when her young one is hardest pursued and harpooned, she supports it under her fin, while she plunges with it for safety into unfathomable depths.

A young whale, having been struck by a harpoon from a

Hull vessel, being at the time at some distance from its mother, had run out some length of line, when the latter appeared in sight, and rapidly bent her course towards it. In vain did she use every usual means to induce it to leave the place of danger, while swimming by its side, as far as the line would allow, in circles around the boats, during the space of four hours; and within this time, on four separate occasions, the parent was observed, when on the surface, to throw one of her fins over the body of the young whale, and to endeavour to drag it away by all the force she possessed; she, lastly, in this way set off with it, in a straight direction, carrying away additional line, to the extent of seven hundred and twenty fathoms; but by that time, the young one became so much exhausted from loss of blood, that she necessarily abandoned it to its fate, and herself escaped, by pursuing her progress towards the ice, roaring and spouting with great vehemence; for here I may observe, that when a whale is struck with a harpoon, or is enraged by the loss of its young, it ejects the water through its spiracles with great force, producing a stridulous kind of roaring, which may be heard the distance of a mile.

This species of whale affords to us a sublime instance of contrivance, compensating its total want of teeth. I allude to the hundreds of plates of whalebone, which cover the roof of its mouth; and which, by their growth, increasing in length, and in breadth, often acquire twelve feet in length, and fifteen inches broad. There have, indeed, been some instances in which whalebone has attained fifteen feet in length; I believe there is at present a specimen of this kind in the Tower, which was obtained by a London vessel, and, doubtless, from a whale of enormous growth; since those whales, which afford whalebone of twelve feet, are themselves often more than sixty feet in length. The upper surface of the skull of a whale of this size, measured twenty feet eight inches long; and the creature itself weighed upwards of a hundred tons.

The roots of the two sides of the arch of whalebone, in the mouth of this animal, nearly meet at the top of the roof whence they grow, at the anterior part of the mouth; but they gradually recede from each other, as they are continued backwards, till they approach the throat, when they again approximate. This substance, called whalebone, which thus supplies the place of teeth, consists of a peculiar kind of horn. Its plates differ in their length and strength, in different parts of the mouth, but the outer row of plates are by far the strongest and the longest, especially those which are midway between the throat and the snout. Internally, supposing ourselves to be placed beneath the roof, and regarding it from below, from the lower edges of the outer plates, (those which they inclose becoming shorter and shorter, as their origin is more internal, or nearer the centre of the roof,) we see the lower edges of all uniting to form one inclining plane, extending obliquely upwards to the roof. And, as the fibres of every plate are loose and separate at its inferior edge, forming a deep pendent fringe, by the gradual splitting away of its substance in proportion as it is used, we perceive the entire vaulted sides of the roof of the mouth to be, in fact, by these means, deeply lined with a clothing of thick and coarse hair, whence the ancients gave to this species of whale the name of Mysticetus.

Now, beneath this vault of hair, lies the enormous tongue of the whale, and exterior to it, is the immensely high lower lip, which, when the jaws are closed, shuts up over all externally to the very origin of the whalebone above, so as to entirely conceal it from view. By means also of this formation of the lip, and the circumstance of the upper jaw shutting into a cartilaginous groove at the extremity of the lower one, the most perfect valve is formed, which any pressure from without, only tends to render more secure from the ingress of the water.

The fringe, which I before mentioned, produced by the whalebone, (as it is constantly and gradually extending itself in length, by the growth of the whalebone behind it, in proportion as it is worn away,) is thus always in a proper state of adaptation to the marvellous economy of the creature; for the most curious part of this beautiful mechanism is the net or sieve which it thus forms; an instrument which has been granted to this largest of creatures, for the purpose of straining or separating its minute prey from the body of water necessarily taken into the mouth with it, in feeding. For, in this whale, the mouth is of such enormous proportions, as to receive at once, even tons of water, and yet of such wonderful perfection is its filtering mechanism through these hair-like filaments, that it rarely allows the escape of the nourishing particles diffused therein, although they be no larger than peas; its food consisting chiefly of small medusæ, crustacea, and zoophytes.

(To be continued.)

On the Inland Navigation of the United States of America.

PART II.

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The success that attended the execution of the Western Canal of the state of New York, drew the attention of the inhabitants of Philadelphia and Baltimore to the subject of Inland Navigation. It was soon perceived that the trade of both these cities was affected by the diversion of a considerable part of the traffic of the country west of the mountains, to that new and more convenient channel. Each of these cities, therefore, entered eagerly into the search for channels equally advantageous; but these investigations have not been attended with any consequences of important value.

From Philadelphia three several routes have been examined; in one of these the summit is impracticable for want of a supply of water; the others involve an expense far beyond any probable return, in consequence of the number of locks that would be required to surmount the ridges. Hence it may be asserted that there is little probability of the opening of an entire canal from this city to the Ohio, or Lake Erie, although there is a strong probability that a mixed system of canals and railways will be successful. To this the legislature of the state have very recently directed their attention, and have made large appropriations for it. Two navigations, that will form important parts of such a system, have actually been completed.

Philadelphia lies between two rivers, the Delaware and the Schuylkill, which approach at that point to a distance less than two miles. The former is navigable for the largest ships, the latter for vessels of 100 tons. Immediately above the city the Schuylkill is interrupted by falls, and although from its volume of water it would rank in a high place among European rivers, the nature of the country is such, that interruptions of a similar character are frequent throughout its whole course. As many parts of the river are bold and deep, the Schuylkill navigation has been effected by using the bed in such places, and connecting them by canals and lateral cuts. To deepen the river and check its current, twenty-eight wears have been thrown across it. The whole navigation amounts to 108 miles, 46 of which lie in the ancient bed of the river, and the remaining 62 in the artificial channels. Besides 28 guard-locks at the wears, there are 92 locks overcoming a fall of 588 feet. So that this comparatively short navigation has a greater change of level than the Erie canal of the state of New York. The grand object of this navigation was to form a water communication with a vast coal field, of which we shall have occasion to speak hereafter.

At a distance of 58 miles from Philadelphia (measured on the Schuylkill navigation,) is situated the borough of Reading. From this a canal, called the *Union*, has been completed to the Susquehannah river. It is 71 miles in length. The summit level is 300 feet above the Schuylkill, and 210 feet above the Susquehannah. In the plan of the canal a difficulty was found in obtaining a supply of water for the summit level. This has been obviated by raising the waters of the Swatura by machinery, at such seasons as the other sources are usually scanty, and this will no doubt be effectual.

An important improvement in the structure of the locks of this canal was planned and carried into effect, by the very intelligent and skilful engineer (Mr. C. White) who superintended its construction. This improvement consists in the suppression of the breast wall, and making the upper gate nearly of the same depth as the lower one. The bottom of the lock has a slight slope, and the upper reach of the canal is gradually deepened until its bottom reaches the same level as that of the lower. As the breast wall is the weakest part of a lock, and adds considerably to the expense of construction, while it involves, in addition, the cost and inconvenience of lateral culverts in the walls, this improvement may be considered as very important; it is, in truth, the only change of real value that has been introduced into the structure of locks, since the time of the opening of the canal of Languedoc. We have seen a proposal for a similar change in the form of locks, made by a French engineer; and although he is, doubtless, no plagiarist, still it is proper to state that Mr. White's locks were not only planned, but built and in actual use before the French publication made its appearance.

The Susquehannah, we have stated, in the former paper on this subject, to be full of rapids and other obstructions, from the time it enters the state of Pennsylvania. It is practicable as a descending navigation for *arks* in times of floods, and an attempt was made, some years since, to mount against its current by means of a steam-boat. Although this vessel did mount the river, it appears doubtful whether the experiment will be followed by any useful results; for it would be impossible to convey in this manner any heavy lading.

There is, however, little doubt that a canal might be made in the valley of the Susquehannah, as far as its junction with the Tioga branch. The latter, running altogether on the western side of the great ridges of mountains, might readily be rendered navigable; and plans have been proposed, to connect it with the Gennesse River, and with the Seneca Lake, in the state of New York; by either of these, it would come into communication with the Erie canal, and thus with the lake of that name.

We consider this to be the best route by which Philadelphia can be brought by canals into competition with New York for the trade of the western country, unless some successful substitute be found for locks in inland navigation. It is, however, far more circuitous and distant than the New York route, but it has the important advantage, at certain seasons, of being earlier clear of ice, and closing later than the New York canals. It is probable, that some means may be found of lessening the distance, and for this purpose a canal route has been examined directly from Philadelphia to Harrisburgh, on the Susquehannah.

The legislature of Pennsylvania has recently adopted a great and general system of internal improvement by canals and rail-ways; and is thus the second state of the union that has followed the example of New York, in appropriating its revenues and credit to great public works. The debt contracted for such objects stands upon a very different footing from that which arises from warlike enterprises. While the latter impoverish a country, and diminish the means of liquidating the expenditure to which they give rise, the former increase and extend the sources of wealth, and provide ample means for the repayment of the cost of their construction.

The chance of the city of Baltimore being able to effect an advantageous and direct line of water communication with the states west of the mountains, is less than Philadelphia. It is, however, better situated to avail itself of the descending trade of the Susquehannah, or of any improvements made in the bed, or the valley of that river. Failing in the hopes of a canal, a plan for a rail-way from Baltimore to the Ohio River has been set on foot; the enterprise has been taken up by a company, chartered by the several states through which it is to pass, and the whole of the stock subscribed. It yet remains, however, to be ascertained by experience, whether a rail-way can ever be made to compete on equal terms with a canal navigation.

The states of Maryland and Virginia have earnestly sought a mode of communication with the Ohio, and by its branch, the Allegany, with Lake Erie, through the valley of the Potomac. In this the general government has also taken an interest, and a route has been carefully surveyed by officers of the United States Corps of Engineers. This investigation has shown that a summit level can only be obtained, by deep excavation, or by a tunnel of nearly six miles in length, and that this summit will be elevated 2486 feet above the tide water of the Potomac, and 1730 above the Ohio at Pittsburgh. We therefore consider ourselves warranted in saying, that although certainly practicable, it will, if locks be used upon it, involve an expense far beyond any that can be reimbursed by its revenue, or even by its public advantage. Notwithstanding this, a bill, authorizing a subscription to a company formed for making this canal, has passed the house of representatives, and will probably become a law. Within the state of Virginia, the sources of James River, which empties itself into the

Chesapeake, and of the Kenhaway, that falls into the Ohio, approach near to each other. James River is navigable for vessels of 125 tons as far as Richmond, the capital of the state. Partial improvements of the bed of this stream were made many years since, by a chartered company, and these are connected with the lower parts by a canal and fifteen locks, in the vicinity of Richmond. These have however been of so little value, that it is now proposed to make a separate canal, up the valley of the James River, and of its branch called Jackson's River. The mountains here appear to form an insuperable barrier to artificial navigation, and hence a railway must be resorted to, in order to convey the trade to the Kenhaway River. This last, it is reported, may be made navigable by wears and sluices. Much anxiety has been manifested by the intelligent population of this state, to press forward these improvements, and an engineer of high reputation, a pupil of the French Polytechnic school, has been employed, under the direction of a board of public works.

We shall here close our accounts of those canals, whether executed or projected, that are intended to form a communication between the sea-bord and the states west of the Allegany mountains. Those which we have mentioned are, in fact, all from which any important consequences are to be anticipated.

We proceed to notice the artificial navigations, the objects of which are more confined. These we shall consider in the order of the states, beginning at the north-eastern frontier, and proceeding south, and shall confine ourselves to those which are actually completed, or in a state that promises speedy completion, unless in cases where the importance of the enterprise, or some other cause of interest, shall render them worthy of remark.

In the new state of Maine no work of any importance has been commenced, or even projected. In New Hampshire, it has been proposed to unite the tide waters of the Piscatawny at Portsmouth, with the upper part of the Merrimack through Lake Winnesipiogee. This latter river has been rendered navigable for boats, as far as the confluence of Baker's river, 130 miles from the sea. Of this distance, twenty miles, as far as Haverhill in Massachusets, are navigable for ships.

In the state of Massachusets, the Middlesex canals are by far the most important artificial navigation. This work was commenced in the year 1793, under the direction of Mr. Weston, an English engineer, and in 1804 was opened for the passage of vessels. The canal enters the Charles river at the town of Charleston immediately opposite the city of Boston. From the tide it rises one hundred and four feet to the summit level, and descends thence thirty-two feet to the Merrimack. These changes of level are effected by twenty locks.

But one other artificial navigation has been actually commenced in this state: this is a canal from the town of Worcester to Providence in the state of Rhode Island. The most important advantage to be anticipated from the completion of this canal is the conveyance of coal, which is said to exist in abundance at Worcester, to a port whence it may be shipped.

Connecticut is without any other canals than those mentioned in speaking of the system extending parallel to the coast: and in Vermont, although various projects have been entertained, no canal has been actually commenced.

As New York was the state that furnished the first great example to the rest of what might be done by a well-combined system of artificial navigation; so this large and populous state has been more prolific than any other in the Union. Surveys of no fewer than sixteen, were ordered to be performed at the expense of the state by the Legislature at its session of 1826. Three of these routes have been more recently pressed upon the attention of the public; and to them we shall confine ourselves. The counties bordering upon the St. Lawrence possess a fertile soil, and were not behind any part of the state of New York in prospects of wealth and population, so long as the natural outlet of the St. Lawrence was open for the export of their produce. They even competed with the western counties, on more than equal terms, for the swarms of the New England hive, until the opening of the Erie canal. Since that period, they have been upon the decline; the tide of population is no longer directed towards them; and even those families that have settled, frequently leave them in pursuit of a more advantageous seat. So short, however, is the distance from the Erie canal, that an artificial navigation would speedily restore the equilibrium between these counties in the valley of the St. Lawrence and those in the west. An act has passed the Legislature of the state of New York, authorizing the formation of a chartered company to effect this communication, and the commissioners therein appointed are engaged in repeating and extending the original surveys with a view to attain exact estimates of the cost. Some of the natural circumstances are extremely favourable to the formation of this canal; the supply of water from the Black River, a tributary of Lake Ontario, and from Canada Creek, a branch of the Mohawk, is exuberant; the former stream is itself navigable for boats for a considerable part of the distance, and requires little more than a towing path (unless steam-boats should be found more advantageous) to make it a canal; and all the necessary materials are to be found in abundance. On the other hand, the elevation of the summit is very great, the whole amount of rise and fall being nearly sixteen hundred feet. Such, however, is the fertility of the country that will contribute the trade, and such the value of its pine forests which will instantly furnish a profitable article of commerce, that we feel assured that, even if locks be employed to overcome the elevation, a large interest will accrue upon the investment of the capital necessary to complete this navigation; while, if the resources of mechanics furnish any cheaper mode of obtaining a change of level, it must be prodigiously lucrative. The success of the Erie canal is in truth an earnest that this cannot fail to be not only useful to the country, but profitable to those who execute it. It is however yet questionable whether it will be possible to obtain capital for the accomplishment of this important undertaking. The state has in some measure decided that it will not for some years to come undertake any new enterprises; the country this canal is intended to benefit, is, from the causes we have stated, much impoverished; such too is the demand for capital in other parts of the States, to be employed in the innumerable branches of industry which the progress of internal improvement has called into existence, that little inducement exists to divert it to the accomplishment of enterprises of this character. Surplus wealth, beyond what is invested in lands and buildings, or is engrossed in manufactures and commerce, hardly exists in the United States, except in the great cities, and even there it has the facility of being invested in the local banks and other monied institutions, that have hitherto absorbed the capitals of the few who are not engaged in active business. Those who are willing to incur the risks of trade look for larger profits than a canal is likely to afford; those who seek a secure income without encountering the vicissitudes of commerce, have hitherto preferred the stock of banking institutions for the investment of their capital. Hence in many projects for internal improvements, the privilege of banking has been attached to the charter for canals, as a bait for subscriptions. It is hardly necessary to state that this heterogeneous association has not been, generally speaking, a fortunate one. The profits of banking are gradually falling, however, and must soon become so small as to compel the increasing permanent capital to be invested in enterprises of this nature. But, in the mean time, we cannot help wondering, that the capitalists of Europe, who have been for some years past searching in all directions for new modes of investment, should not have turned their attention this way. So little however does the character of these enterprises, or the resources and good faith of the states of the American confederacy, appear to be understood in the money markets of Europe, that we are inclined to believe that the most advantageously situated canal route would fail in obtaining subscribers, although South American mining stock would be eagerly taken up; and we know, that, at a time when Greek, Columbian, and even Poyais loans were sought with avidity, the state of Ohio could not obtain a loan for the execution of the important work we spoke of in the former part of this essay. The money for the last was indeed readily obtained in the United States, but at a rate of interest higher than is usually paid in Europe. There is, no doubt, great caution to be employed in determining between the different projects of canals with which the United States are teeming; nor are all the states equally capable of paying the interest of, or redeeming loans; but the discrimination may be effected by the exertion of ordinary prudence. The canal in question, for instance, is one whose cost can be ascertained within a trifle, in consequence of the experience attained by the engineers of the state of New York in constructions of the kind; its revenue is susceptible of ready estimate, and it is situated in a state that has attained, by the strict performance of all its engagements, the highest character for good faith and ability to comply with its contracts.

The second of the projects that has been recently agitated in the state of New York is the Chenango canal. This derives its name from a branch of the Susquehannah river, and is intended to form a communication between that stream and the Erie canal. A bill to construct this at the expense of the state failed of becoming a law by a few votes. A charter of the most full and liberal character might no doubt be obtained for its construction by a company; but we are not aware of its possessing equal advantages, or a probability of as great a revenue, as the one we have just spoken of.

The third of these routes lies between the head of Cayuga Lake and the Susquehannah at Oswego. Difficulties, arising from a scarcity of water and the height of the intervening land, have caused the plan of a canal in this direction to merge in that of a railway. A company has been chartered to carry this into effect, and will probably go into successful operation. Besides these embryo projects, two canals have been actually executed at the expense of the state; the first forms a communication between the Erie canal and Lake Ontario, by means of the Oswego river; the second between the same canal and the Seneca lake.

One private enterprise of great extent and importance has been nearly completed in the State of New York; this is the Delaware and Hudson canal. It enters the latter river near the mouth of the Wallhill at the town of Kingston, and extends in a south western direction, through the vallies of the Rondont and Nevisink, until within a short distance of the confluence of the latter with the Delaware, and for a distance of 64 miles. The ascent from the Hudson to the summit level is 535 feet, and the descent to the Delaware 80 feet. From the valley of the Nevisink it rises through that of the Delaware, and near its margin for the distance of 17 miles, and to a height of 148 feet. Here it crosses that river and enters the valley of the

Sackawasen, along which it is to be carried as far as the forks of the Dyeberry, about 20 miles. The canal was opened in April 1828 from the Delaware to the Hudson, the remainder is in a state of rapid progress towards completion. From the termination of the canal, a railway has been laid out, rising about 500 feet to a gap in the Moosick mountain, whence it descends 800 feet to the valley of the Sachawannock, a branch of the Susquehannah. At this point is an immense bed of coal, a portion of the great anthracite formation of Pennsylvania. The great object, indeed, of this canal is to bring a supply of this valuable fuel to the city of New York, and to those districts on the Hudson in which wood has become scarce.

As the coal of this region has been the source of a variety of projects of inland navigation, besides the canal we have just mentioned, it will be essential to the complete illustration of our subject, that we should describe this formation.

It may be traced from a point in Dauphin county, Pennsylvania, about fifteen miles north of Harrisburgh, the seat of the state-government. It thence extends about E. N. E. through the whole length of Schuylkill county, and incloses the sources of the river of that name. On the borders of Schuylkill and North Hampton counties it turns suddenly to the north, and proceeds in that direction, until it reaches the Susquehannah river, in the vale of Wyoming, when it spreads out on both banks of the river, and includes the whole valley. Here the formation resumes its original course, or one more nearly N.E., and when the course of the river abandons that direction, the coal can still be traced pursuing that azimuth, up the valley of the Sachawannock; along this it extends to the very source of that stream; and the last mine that has been opened is at Belmont in Wayne county, the north-eastern corner of the state of Pennsylvania. The whole length of this formation is about 110 miles, the breadth from four to eight miles. According to the investigations of the late Mr. Cist, of Wilkesbarre, the coal extends beneath the whole of this region, and is in many places from twelve to thirty feet in thickness. The supply is, in truth, vast beyond calculation.

The general character of the coal of this formation is what is called by mineralogists *anthracite,* and is similar to that of the

Kilkenny coal of Ireland, burning without smoke. Its properties as a fuel are, however, various, according to the situation and circumstances under which it is found. In some places it is dry, and composed of carbon nearly pure, the earthy matter amounting to little more than five per cent., and there being no other impurity. In others it passes towards the character of the adjacent carboniferous shale, and then leaves much ashes after its combustion. In others, again, it is saturated with water. This last variety burns much more freely than any other, and, when dug from beneath water, or from mines loaded with that liquid, it is found to assume a resplendent pavonine hue. The flame that attends its combustion is no doubt due to the decomposition of the water with which it is charged, while that found in dry situations burns away without emitting any gas heated so far as to become luminous. In the one case, it forms a pleasant and bright fuel for the open grate, while in the other it burns only in furnaces possessing a great draught; but it then furnishes the most durable and intense heat of any fossil substance.

Many of the mines furnish specimens of fossil charcoal, in which the ligneous structure is as marked as in that recently prepared from growing timber; and thus is afforded another link in the evidence, that all coal is of vegetable origin. In the shale and sandstone that accompany the coal, great quantities of vegetable impressions are also found. These appear to be identical, in genera and species, with those which accompany the bituminous coal of England. The shale that overlies the coal has a very peculiar character, containing much carbon, but no bitumen, and may hence form a new mineralogical species, *carboniferous shale.*

This great coal field has not hitherto been traced into the state of New York. Identity of geological position would, however, warrant the belief, that its continuation, or a separate but analogous formation, will be found on the western side of the Catskill mountains ; and, in corroboration of this belief, it may be stated, that the writer of this article found coal in place, near the Little Falls of the Mohawk River, on the western side of the mountain through which that stream forces its way, and which is a continuation of the great Allegany ridge, of which Moosick Mountains and the Catskills are parts.

The search for coal in this direction has, however, been retarded, in consequence of an erroneous impression that has been given of the character of the rock at the Little Falls. In the published geological survey of the New York canal, it has been classed as a primitive rock, while it is, in fact, a coarse-grained sandstone, retaining, indeed, the crystalline character of its parts in an uncommon degree, but readily distinguished, by the looseness of its aggregation, from the family of gneiss. It is probably similar to the sandstone used in many of the edifices of Thebes in Egypt, which was long mistaken for granite, although more close examination has shown that the latter material is only used in a few vast monoliths, and never as the material of buildings. As we have been led, in order to render the objects of several canals obvious, to mention this coal formation, it may not be irrelevant to state, that in the state of Pennsylvania there are other extensive coal fields; one of these has been lately discovered near the Tioga branch of the Susquehannah river; another has long been worked in the vicinity of Pittsburgh, at the confluence of the Allegany and Monongahela; both of these are bituminous in their characters. Coal of the same species abounds in many places on the banks of the Ohio river, in the states of Ohio and Kentucky.

As cultivation increases, and the wood is more frequently cut, not only does the space occupied by growing timber decrease in the Atlantic States, but the power of reproduction appears to diminish; the demand at the same time becomes greater, in consequence of the greater number of persons to be supplied, and the extension of manufacturing industry. Hence, in the great cities of the sea coast of the United States, fuel has for many years borne a price far greater in proportion than any other necessary of life. With the exception of a small district in Virginia, and beds of anthracite of very inferior quality in Rhode Island, and at Worcester in Massachusets, no coal has been found to the eastward of the first or primitive range of mountains. Hence a cheap and abundant supply of coal may be considered as almost essential to the continuance of the prosperity that has hitherto attended the progress of that portion of the American Union. The discovery of the great field of anthracite coal in Pennsylvania has hence been considered as likely to be attended with the most important consequences; while the formation of channels by which it could be readily conveyed to the markets, appeared to offer the most advantageous prospects for a profitable investment of capital. The coal of the vale of Wyoming may be conveyed by the Susquehannah to the tide-waters of the Chesapeake Bay. But that river does not at present permit an ascending navigation; and the supply, although cheap, is limited to what can be furnished by the species of vessel called in America *arks*. These vessels, rudely built of hewn logs, are broken up at their place of destination, and sold as timber, or even as fuel. If by no means costly, the quantity of them that can be prepared in a season is small. A similar attempt was made on the waters of the Lehigh, to supply the city of Philadelphia, but was, for the reason we have stated, found inadequate to the demand.

The Schuylkill navigation, described in the early part of this paper, was undertaken to open a more certain communication with these mines, and has been successful so far as Philadelphia is concerned; much coal has been shipped by sea to New York. But as a great part of the coal field is not more distant from the latter city than it is from the former, and as New York is likely to be a much greater consumer of this fuel, more direct modes of communication have been sought between it and the mines. The only one that is likely to be very speedily completed is the Delaware and Hudson canal; and such is the facility it will offer for the transportation of coal, that the best estimates appear to prove, that it may be delivered on the bank of the Hudson at so low a price, as not only to supply the demand of the city of New York, but even to supersede wood as fuel, in the very districts where it is now cut for the market of that metropolis. Wood is in truth so bulky, and requires so much labour to convey it to the place where it is used, that the farmers of those parts of the country to which coal can be carried by water, are already beginning to purchase coal, and abandoning the cutting of the wood that grows upon their own lands.

The Delaware and Hudson canal, to which we now return, was commenced at a period when the frauds and misrepresentations that marked the era of joint stock companies in

England, had extended to America, with a portion of the same mad spirit of speculation. But while various other companies were managed with a reference merely to the elevation of the price of their stock, this canal company was distinguished, in the most honourable manner, by a direct and faithful determination to carry into effect the great objects of their charter. It hence enjoys the highest character and credit; and when it became necessary to raise a further capital, to extend the line of communication beyond the point at which the original estimates ceased, the state was induced to pledge its good faith for the redemption of a loan of half a million of dollars. Such favour this company justly merited, from the honour and fidelity with which its business was conducted.

We shall close the present paper by mentioning another project for a communication with the coal region; the route of which lies partly in the state of New York. It was intended to pass in a direction nearly parallel to the Delaware and Hudson canal, from the mouth of the Pequest, a branch of the Delaware in Sussex county of New Jersey, to the Hudson in the vicinityof the village of Newburgh. Although the route has been surveyed, and found practicable by a very intelligent engineer, and a charter granted by the states of New York and New Jersey, no step has been taken towards its construction. It would afford, however, as convenient a mode of reaching the coal mines as the Delaware and Hudson canal, and has the advantage of entering the Hudson nearly forty miles nearer to New York.

In the former part of this essay, we paid what we felt to be a merited tribute of praise to the then governor of the state of New York, De Witt Clinton. Even before that paper reached England, that great public benefactor of the United States had ceased to live. He has left a space in the public councils of his country, that will not soon again be filled by one equally zealous for the improvement of his country, or equally fearless in promoting the facilities of internal communication, at the risk of the loss of political consequence, and popular favour.

On Malaria on Ship-board. By Dr. Mac Culloch.

If in the former papers on Malaria, to which you gave admission in your Journal, I took occasion to notice the production of this poisonous substance in ships, I submit to your judgment as to the propriety of entering on this particular branch of that question in more detail: partly on account of its great importance, partly also because of the very persevering mistakes which appear to have been committed on this point, and still more, as you justly remark, because that which was likely to have been passed with little notice in a general sketch of the entire subject, is more likely to attract the attention of those whom it may concern, when thus separated under a specific title, and treated somewhat more fully than was formerly admissible.

If it has not been an unvarying opinion that the fevers occurring in ships, and particularly those breaking out at sea, are of a contagious nature, or appertaining to Typhus, (to use a term now become popular,) I should find some difficulty, at this moment, in producing any opinions to prove that they were thought to appertain to the Remittent, or were fevers produced by miasma or malaria, and not by contagion; except at least in these very unquestionable, or unquestioned, cases, where the disease attacks the patient in a tropical climate, or other analogous country, in consequence of communication with the shore. On the contrary, I should, I believe, be safe in saying, that almost every fever, perhaps even every remarkable occurrence of this nature in a ship, has been viewed as an example of contagious fever, or as a true Typhus; while the treatment has of course been modified by that opinion.

Nor is this matter of surprise. I have shown in the Essay on Malaria, that throughout Britain generally this error has been extremely prevalent, for at least many years: while it might be curious to investigate the causes, whence it has arisen that we, of this day, had, on this subject, forgotten the knowledge of our predecessors, the Sydenhams and Lobbs, though it is an inquiry in which I ought not to indulge in this place. The fact, nevertheless, is such. The true remittent fever is not indeed unmarked or unknown: but it is most certain that the very great majority of cases are termed, as they are considered, typhus; while if navy practitioners take precautions against contagion under those occurrences, it has been very common among others to express surprise that the disease had not been communicated to the attendants. Sometimes, indeed, the practitioner imagines that his precaution has been the cause of stopping this anticipated but imaginary process; but it has also very often happened that where, from situation, from exposure to a common exciting cause, in an active and present malaria, many persons in one house have suffered, simultaneously, or rather in succession, the fever has been pronounced as propagated from one individual to another through a large family, when the truth has been that each was subjected to his own distinct marsh fever from a common exposure: and it is this which explains also that which has so often been a cause of surprise; namely, the occurrence of single cases of fever, in a numerous family, or in a populous neighbourhood, while perhaps no precautions have been taken against its propagation; just as it accounts for the innumerable instances in which the so called typhus fevers, received into hospitals, have not spread. Such fevers could not have been propagated, because they were not contagious, or were not typhus fever; while I need scarcely say to Physicians how very easy it is to mistake the continuous marsh fever for the true typhus, or, equally, that in which the remissions are slightly marked; and very particularly in the ordinary routine of practice, often hurried: an error, also, the more easy, should the prejudices, habits, and general impression of the practitioner on this subject have given his mind a general bias to this belief, the belief in contagion or in contagious fever as a common, or as the more common disease.

And that this has been a recent belief in England, or rather that it has been an opinion gradually spreading or accumulating for many years, must be well known to all observing physicians, while it would not be without interest to inquire how much it has depended on the recently augmented use of the term typhus, and its even popular adoption by the multitude at large. The influence of terms forms one of the most curious departments of the history of the human mind, and is the foundation of more fallacies than all else which that history can furnish. As long as the term in use was fever, simply, the disease might have been any fever; and it was then the business of the physician to ascertain what it was, whether it was a marsh fever or a contagious one. But the term typhus once adopted, became the substitute for examination and reasoning alike, as it became also the rule of practice: the association of ideas led necessarily to contagion; and hence unquestionably one leading cause of the rapid growth and progress of this error: an error which will not soon be eradicated, and may not possibly be entirely corrected until some change of the general, and particularly of the popular nomenclature takes place.

As a proof of this popular and general error, it is sufficient to open any monthly journal or inspect any newspaper, where we read currently of the prevalence of typhus "in this month," August and September, for example, and how "in this month," November, the number of cases is gradually diminishing. And these are the reports of physicians holding public situations in hospitals, from whom, if from any, we are entitled to expect more correct notions; particularly when they set up to be the recorders of medical statistics, to descend, possibly, to posterity, and corrupt the entire history of medicine. If it is from such authorities as these that Heberden and others have drawn their averages and deductions, we may well be cautious how we reason on them.

I am aware that more correct notions have for some little time begun to take place of this extensive error; but physicians know as well as I do that they are still very limited—so limited, that among men of any reputation, it would not be very difficult to point out the individual. They know, too, that such correct views, although promulgated by teachers, have very little effect on the general opinion and practice at present; and it is such teachers who will best know that in the Essay on Malaria, and in that on Marsh Fever, I have not overrated either the error or the evil, strongly as I may have pointed it out.

But if I have thus pointed out this most common and widely extended mistake, I ought to caution any reader of this paper from supposing that the writer of it is one of those who doubt or deny the existence of a typhus, or a truly contagious fever. It is difficult, indeed, to conceive how such a doctrine could have been promulgated by any one acquainted with practical medicine, or with the history of medicine: yet temper produces strange phenomena in human society; while the not uncommon tendency of mankind to fly off suddenly into opposite extremes, and not a little the love of paradox, added perhaps occasionally to a little desire for notoriety through whatever means, may perhaps serve to explain this recent aberration of opinions.

It ought almost to be unnecessary to say how important it is to distinguish between these two kinds of fever; the contagious and the non-contagious, typhus and marsh fever. And since a clear idea of this subject, as far as that can be conveyed in a paper of this popular character, is essentially fundamental to the special object of this brief essay, I must be permitted to enlarge a little on these general views; confining myself also as much as possible to that which, whether as matter of doctrine, or matter of practice and utility, can be rendered apprehensible to general readers.

Such readers ought, therefore, to understand that there are two fevers, of characters essentially distinct, if often very much resembling each other in their symptoms, or general appearances, progress, and effects; and that there are but two,—as I hope can be proved to the satisfaction of medical readers: all the eruptive fevers, together with all the symptomatic ones, or those which attend local diseases, being of course excluded: —two simple fevers; and one of these being the produce of vegetable decomposition or malaria, including consequently all marsh fevers, while the other, originating it is not always exactly apparent how, can be communicated from one person to another, which the former cannot. And this last is typhus; including many varieties, from a very slender disease to the most mortal jail-fever or putrid fever: while, under the former, are ranked every variety of ague or intermittent, together with remittent fevers, which are often as persistent or continuous as typhus or contagious fever, and often also assume the same character of virulence or putrefaction, with a course as short and as destructive; but the essential distinction always continues. These cannot be communicated from one individual to another.

Thus simply stated, it might be supposed by general readers that nothing could be more easy than to make the distinction in practice. Yet the fact has proved far otherwise, and even in much graver cases than the fevers of our own country, or even the fevers of ships; from causes, some of which I must allow my own profession to assign, as they would scarcely thank me for the attempt. To these readers, however, it will be interesting to know that on this have been founded all the confusion and all the disputes which so long occupied the world, and still occupy it occasionally, respecting the yellow fevers of the West Indies and of America, the fevers of Gibraltar and Cadiz, and the more recent ones of Barcelona and Alicante: while the confusion, the inconveniences, and the terrific mortalities which have resulted from these errors, I could not here undertake to speak of, without trespassing on at least the bounds of this essay, if not so very much upon its objects. Generally, however, I may venture to say, that as severe typhus and severe marsh fevers often put on the same aspect, so while the peculiar biliary affections caused by heat are common or general in hot climates, it has happened that these, occurring in each kind or class of fever, and forming an obvious symptom, as also giving rise to a popular term, have caused the name Yellow Fever to be applied indiscriminately to both. Thus there has been a yellow fever which was contagious, and a yellow fever which was not contagious, or a marsh fever; this last being the common endemic of the West Indies and other tropical climates, as of Spain, while the former has been a contagious disease, casually arising, or imported in ships, and propagated in the usual manner. That the yellow fever and the mortal fever of Gibraltar proved of this nature, and that, in the last case in particular, this contagious "yellow" fever had been mistaken for the non-contagious, or marsh "yellow" fever, with mortal consequences of the most tremendous amount, will form a sufficient illustration to ordinary readers, as to this particular error; while it will be a sufficient example also of the exactly reverse error, that the fever of Alicante, which was the non-contagious, or marsh, "yellow" fever, was mistaken, reversely, for the contagious one, and managed accordingly, with very inconvenient results, if not with similarly mortal ones.

And that the readers for whom I am here writing may apprehend generally what the consequences of these mistakes have been, and are, and may be again, I will state the most prominent points; facts that have existed, and which have been repeated, even very recently. In our own country and our own fevers, the consequences under these errors are comparatively trifling; yet the inconveniences, and even the mortality, are far from being so inconsiderable as a superficial thinker, or a person ignorant of medicine, would imagine. And what I have to remark offers another interesting, if painful example, of that class of fallacy arising from the influence of a popular term. The yellow fever; this term, like the word typhus, was sufficient: it was reasoning and observation united. "Yellow" fever had generally been a marsh fever: it was sufficient, therefore, that to a fever, to any fever with this symptom, the term yellow fever was applied, and the whole question became determined without examination. When will mankind be freed from the slavery to words?—when mankind learns to think and to reason.

When the "yellow" fever, being the typhus, or contagious disease, has been supposed the marsh, or non-contagious fever, the consequence has been neglect in intercepting communication, and in all those precautions which stop the progress of contagion: the consequence has been mortality, which might have been prevented by the simplest precautions, and that mortality diffusing itself from town to town, and from sea-port to sea-port, across half the globe. This was the lamentable case of Gibraltar, very particularly. When the "yellow" fever has been the marsh, or non-contagious fever, the consequences have been different: less mortal, it is true, but, perhaps, quite as vexatious, and, municipally as well as commercially, far more troublesome. Hence *cordons* drawn round towns, with other troublesome and expensive arrangements to check an imaginary contagion, destructive of personal liberty and commerce;—hence quarantine laws made and enforced against that which was not communicable, with great consequent commercial injury, and with great expense and inconvenience;—and hence, also, collaterally or incidentally, the excitement of a temper which, partly justified by the results as here stated, has generated a kind of party spirit against all quarantine regulations; and has even gone so far, in recent times, and in the hands of a few of violent tempers, or misapplied political feelings, or anxious for notoriety, or else delighting in universal opposition, as to propose, and persist in, the propriety of suppressing all quarantine or sanitary regulations, even in the case of the plague.

Of the last case to which I have here alluded, Alicante offers an example: while, if I dare not quote further illustrations, so I cannot venture either to enter into more minute details of the several grievous consequences which have resulted from the errors which I have here pointed at. It would require long detail to render these fully sensible to popular readers, ignorant of the facts and ignorant of medicine; and though such detail might be rendered very interesting, I dare not so far infringe on my allotted space. Such readers would also ill comprehend how the medical practice must be affected by correct or false views as to the true nature of such a fever; and if the differences required in the treatment are not always very considerable, there are many cases where they really are most important; while I need not suggest to any philosophical mind, that no physician can act correctly under vague, hesitating, or false views of the nature of the disorder which he is treating.

As far as our own country is concerned, the results of this error are evil in a less proportion, as the diseases, of whatever nature, are less numerous and less severe. I will pass over what relates to the practice or the medical treatment, though it is by no means unimportant. It ought to be obvious, also, that if a non-contagious disorder shall be judged a contagious one, the precautions which prudence would suggest on this view, and which a correct practitioner would follow, if too often omitted from ignorance or carelessness, are of a nature to be attended with great inconvenience, and often with considerable expense as well as alarm. Such is removal; such, separation, nurses, and much more; matters which, expressed in so many simple words, carry no weight, but the vexations and expenses arising from which will not easily be forgotten by those who have suffered from them. The reverse mistake, that of treating a contagious fever as if it was not contagious, has scarcely, perhaps, occurred much in our own country, because our errors are of the reverse nature: but while this would soon take place, should the sect, which admits of no contagious fever, make progress, it is apparent that the consequences would be most pernicious, since they would be the propagation of the disease: the same events, if on a smaller scale, which occurred to so terrific an extent at Gibraltar. How this affects the case of ships, I shall defer till I speak on this specific subject, that I may consolidate what appertains to that most important question, and the main object of this paper, into the most compact possible form. But there is one serious consequence of this prevailing error, *viz.*, that of confounding marsh fever with typhus; which, if less obviously apparent, I should be unpardonable in passing by, since it is, perhaps, equal in bad results to all else. If the vulgar, and, above all, the shallow and self-elected, critics, with which the popular literature of the hour abounds, suppose that fevers arise from no causes, or drop down from the clouds under magic or mysteries unappreciable by philosophy, it is not necessary to answer this class of modern philosophers. That effects arise from causes is no very profound discovery; and that effects may be regulated by regulating their causes, is not much more difficult to comprehend. Fevers, it is to be presumed, have their causes, like all other effects; and to know these, is to have made the first step, at least, in the management of the diseases in question. To command them, is to command the effect; to be able to modify them, is to modify it; to destroy them, or to intercept their action, is prevention; and prevention is health, as far as this disease, extensive in quantity and in evil, is concerned.

Let, then, an exact, accurate, and invariable distinction be drawn between contagious and non-contagious fevers; typhus and marsh fevers: and if there are but these two classes or species, as I hope to show still further than I have already done, we have acquired a command, to a certain extent, over the causes—while this command is prevention. How to prevent typhus, every one knows: to prevent marsh fever the rules are equally obvious; since it is to avoid the lands or circumstances which produce them. The object of the Essay on Malaria was to describe those, for the purpose of prevention; but that knowledge will become effective only when the distinctions between the two kinds shall be truly and invariably drawn, and when the people and their physicians shall have learned to admit, first, that fevers have a cause, and next, that the cause of those fevers, which are not typhus, is marsh miasma or malaria. How this question applies to ships, also, to the subject especially in hand, must be peculiarly apparent.

But the basis of the whole inquiry, the nature and causes of fevers, is so much in want of elucidation, and that elucidation is so necessary to a right understanding of the main question, that I must yet offer some remarks on this subject: while I also conceive that it will, in itself, not prove an uninteresting one to popular readers, thus treated; since, while they do interest themselves much about it, they are in a state of great confusion of mind relating to the whole subject. If people *will* think and act for themselves on the subject of medicine, and if they *will* control their physicians, it is, at least, of importance that they should think correctly. That, on this question of the distinctions and causes of fevers, even Physic itself is not very clear at present, I hope to show: while, whether I succeed in convincing my own profession or not, it is, at least, my duty to state my own opinions, and the reasons on which they are founded.

I need not touch on the causes of contagious fever; they are known, and not disputed. The question is, whether there is any other cause of fever besides those, except the application of marsh miasma, or malaria. It appears to me that no others have, at least, been *proved;* and if this be so, then it also follows that there are but two species of fever, contagious and marsh fever. For the purpose of establishing this merely, an inquiry into the causes of fever will be valuable; but it will be directly useful in another way. If, of any effect or effects, there are more causes than one, our power over these is checked or diminished: should we even suspect more causes than one, and without proving them, our confidence in our philosophical principles is weakened, and the results that might have been derived from them become more doubtful. In the case before us, it is most essential to ascertain, if this can be done, that, next to contagion, there is no other cause of fever than malaria; because we have then acquired a sure knowledge, at least, as to the mode of prevention, if not an absolute power in this matter. If, for example, in a ship we can control contagion, and if, in the same case, we can equally remove malaria, or its action, there can be no fevers in ships, because we have laid our hands on the only causes.

Absolutely to *prove* what I myself believe on this subject, is not possible; because it is an attempt to prove a negative in a science which is not an accurate one, and because it is an attempt to oppose established habits and prejudices, in a branch of knowledge which is especially governed by them. All that I can do, is to approximate the facts in as simple and logical an order as I can, and trust the effect to those who are in the habit of weighing moral probabilities; for of this nature is the present argument.

The power and effect of malaria are admitted: of this one cause of fever there is no doubt; but it is the custom in physic to say, that they are produced also by heat, or by cold, or by either of these united to moisture; or, further, by fatigue, errors or deficiency of food, the passions of the mind, and some few other causes, inducing what physicians call debility.

If it is a maxim in philosophy that superfluous causes ought not to be assumed, it is here worthy of remark, that all these causes of fevers were proposed or invented in the ignorance and infancy of physic; and when the very existence of such a subject, chemically, as malaria, was so little suspected, that the influence, even of marshes, was attributed to moisture, heat, putrefaction, animalculæ in the air, defective elasticity in the atmosphere, and any thing else which admitted of some well-sounding term.

It is also not an unimportant remark here, that to these very same causes, enumerated as the causes of fevers, were attributed inflammations, and, indeed, all other diseases. Mankind is naturally inclined to causation; and in this case physic has mustered everything which could be supposed capable of affecting the human body, hoping, perhaps, that if one failed, it was sure of at least including the real cause in the crowd. And, in fact, these words have always been used as a mere string of terms; not one definite idea having been attached to them, nor to their supposed powers of action: they are but a portion of that phraseology which has ever been the substitute for philosophy in physic; and not a very uncommon substitute also for reasoning in moral science. It is of some value, in such a case as this, to trace the origin and character of opinions; as we may thus often shake the structure which we cannot directly demolish.

If a single cause were always the sole agent in producing disease, there would be no difficulty in proving that not one of these was the cause of marsh fever, or of fever that is not contagious. Unfortunately, the condition of the subject to be acted on must also be taken into the account; as there are predisposing as well as exciting causes. And these last being sometimes obscure and unknown, while the former may be obvious, physic has not unnaturally committed the error of taking up with what was most obvious; while, in this case, ignorant or neglectful of the presence and power of malaria in all those least obvious instances which I have pointed out in that Essay, it has attributed to heat, or cold, or fatigue, or what not, as prime causes, that power which they possess but as secondary and assisting ones.

But as the very nature of this question does not allow me to prove the nature of the cause from any single fact, we must try to produce, from a wide mass of such, what could not be deduced from individual or separate ones; and should this be practicable, the proof is legitimate, because it is only thus that philosophy, in almost any case, arrives at truth. If I can prove that the one cause which is here assumed as the true one, acts as often as it is called into action, and that the power of the others is irregular and uncertain, as also that, in very plain cases, they do not act at all when present; and further, that when they do appear to be the agents, that other demonstrated cause is also present, or probably existent, it appears to me that the point in question is proved as perfectly as anything ever is proved in those sciences which do not admit of mathematical demonstration.

I may divide the imaginary causes which physic has assigned into two classes ;—those which depend on man himself, including injurious diet, fatigue, the passions, and so forth, are equally distributed, on a broad average, throughout mankind, everywhere, and at all seasons of the year, or in all climates. As to injurious conditions of temperature and of moisture, they are not amenable to the same universal average ; but they occur also under certain distinct sets of averages, entirely different from those which attend the existence and action of malaria.

Now, if what I have termed human causes were the causes of non-contagious fever, that should occur indiscriminately, and on some equable general average, all over the world. That, admittedly, is not the fact ; and I may surely, therefore, safely dismiss them from the list of causes which physic has registered. There is nothing wanting, even to demonstration, as to this branch of these supposed causes.

The second division of causes, consisting in modes of temperature, cannot be dismissed so briefly ; because physic, with its usual laxity of language, has even enumerated all the circumstances, without the requisite discrimination. If they were really causes of fever, it is difficult to see how any one should escape ; or, rather, there would be found a certain average, or certain averages, of fevers equably spread over certain average climates all over the world, which is not the fact. I must make the discrimination here, for physic, which it has not itself done.

The operations of temperature *must* consist in certain states, or changes, which can be defined. It may be continuous cold, or a mean heat, which we may, perhaps, safely take about 40° (I need not be accurate here) ; or it may be continuous heat, which, in a similar loose way, may be taken at 65°; or it may consist in sudden transitions from cold to heat, or the reverse. It is generally esteemed that the effect of moisture is dependent on temperature, or is a mode of the action of that cause: should it be thought otherwise, it may be distinguished into excess or defect. Such is a more accurate definition as to this set of causes. Let us examine their values.

Fever cannot be the produce of cold, because it does not occur as a consequence of cold climates or cold seasons; and, still more remarkably, when the causes of malaria are present, fevers which are non-contagious do not occur in winter. And, on the contrary side, they do not occur in those hot and sandy tropical regions, where there is no water, and little or no vegetation; and where the heat is generally far more extreme than in those of an opposite character. Again, with respect to transitions from cold to heat, this is a common occurrence in such countries as Canada and Siberia, in spring, and yet, if marshes are not present, no fever is the consequence. Of the reverse nature, or of sudden and frequent transitions from heat to cold, there can be no examples more complete than the burning deserts of Africa, where hot days are followed by extremely cold nights, and where, yet, fevers are notoriously not produced. If physic was in the habit of examining its facts before it drew its conclusions, it might have escaped many more unfounded ones with which it is filled, than this.

If the supposed effects of moisture in excess are dependent on its connexion with temperature, the same reasoning is applicable. If, on the contrary, mere moisture can act in producing fevers, by its excess, a fog, from any quarter, should be equally productive of these diseases; whereas I have shown, in the Essay on Malaria, that this never happens from the fogs that arrive from the west to us, from the wide ocean anywhere, from even on land in winter, when vegetation and heat are dormant, nor from the moist or foggy atmosphere of a mountainous region, where the fog is the ordinary cloud. Where fever is the apparent produce of such a moist atmosphere, it is where there are sufficient reasons to believe that it is the vehicle of malaria, and that it acts only as it contains that ascertained source of fever. As to defect of moisture, it can only act as producing evaporation from the body in an unusual degree; and it is thus a case to be argued on the general considerations already offered on this supposed cause.

Thus have I gone through all the supposed causes of non-contagious fever, having also, as I trust, shown that no proof whatever has been offered of their power in producing these diseases, and that there is not even a probability that they are the real causes which they have so long been supposed, though they may well be aiding ones, either as they render the body more susceptible of the action of these, or as they may be in some cases the very vehicle of the true cause, or the poison in question. And I cannot help thinking that, in any other science than physic, such reasoning would be satisfactory: unfortunately, this one has never yet guided itself by the ordinary rules of philosophy, nor been accustomed to the severity of logic; so that against its modes of faith, philosophy and logic are arrayed in vain.

But its errors are those of imperfect observation in this case, and are founded on a fallacy which it will not be very difficult to explain. Fevers, non-contagious ones, are proved to be produced where vegetable decomposition abounds most, or is most rapid; or wherever that unknown substance called Malaria, as contained in the air which has obtained this term from its effects, is present. And such fevers are also proved to increase with the increase and activity of these causes, formerly explained; to diminish with their diminution, and to disappear with their disappearance. This in itself approaches to a demonstration that here lies the real cause: in any other science, in any science which was not governed by phraseology and prejudices, it would be considered demonstration.

But the fallacy becomes plain. All the imaginary causes which I have been discussing, exist everywhere, and they must needs exist, therefore, where malaria does. They can always be traced, while malaria has been neglected; while, in our own country in particular, the medical world has remained ignorant of it, or has most unaccountably thought fit to forget what has been well known, if never so accurately known as it ought to have been. It is always easy to have recourse to an obvious cause: under the prejudices of physic, those which I have been arguing against have been selected, habitually and traditionally; and thus those which were but the accessory causes often not even that, have been invested with the title of original or true causes.

Here I think I may drop this subject, leaving these arguments to make such impression as may be their fortune. As I do not wish to enter here on the question of predisposing causes; and as I have shown that malaria is really present in thousands of cases where it is not now suspected, there will scarcely be any difficulty, with due attention, of tracing the real cause of those fevers which are not contagious. In the case immediately before us, I have shown that it exists in ships, even at sea, the produce of the vessel itself, and on the general principle of vegetable decomposition.

To conclude, if I do not, myself, perceive the defects in the logic of this argument, I can have no objection to see them pointed out, since my object is to discover Truth; never yet having comprehended what else there was worthy of the pursuit of a rational being. And on this ground I may venture, rather, however, for the sake of professional than strictly popular readers, to offer a few remarks, somewhat more specific, on the fevers of nosological and systematic writers.

I have stated two fevers, or two species (using that word in its widest sense) of fever, perfectly distinguished by their property of being communicated or not, and equally distinguished by their causes—exciting causes, in medical phraseology. The Plague is another fever of a distinct character: the eruptive fevers, smallpox, scarlatina, and so forth, form a different set of species; and, besides all these, there is a fever attending on local disorders—on inflammation or disorganization. The question is, are there any other fevers? any fevers besides these last, which are not modifications of the fever of contagion, or the fever of marshes? If there are any still received, I may safely now, I believe, reduce them to the modern simplicity, by ranking them under the Synocha and the Synochus of Cullen. May these not be reduced still lower, or ranked under one or other of the Two Fevers? I think that they probably can; and if so, we have but two fevers, and two causes of fever. As to the disorder itself, as it exists, it will then consist in nothing but varieties; and if this can be established, it is equally important, both in the view of practice and prevention,—to say nothing of the satisfaction which arises from generalization and simplification, and of the facility which these add to all our reasonings, and to our practice also.

The contagious fever, or typhus, varies exceedingly in the degree of severity; and this is also true of marsh fever. And while the term putrid fever has been applied to extreme cases of the former, slighter fevers, supposed to belong to the same species, have been popularly called low fever, and nervous fever, and so forth; while these terms have also been adopted by physicians. Referring to the above-mentioned remarks on the causes of fever, to what is here generally said on this subject, and to all that physic really knows about fevers, or determining not to follow the common lax language and reasoning of this branch of science, but to limit myself to that which is known and proved, such a low or nervous fever must belong to one or other of these species; namely, the contagious, or the marsh fever, because physic cannot prove another species and a third cause.

Now it is true that a fever of this mild description can be produced by contagion, and the proof is, that such instances will occur, in an epidemic period, among severe cases; while also such a mild disease will propagate itself, and even produce a severe case; and while, further, the very cause may sometimes be traced, for such an individual instance, in the exposure to a contagion. But I am very certain that if the very great majority of such mild fevers were carefully examined, they would be found to appertain to the marsh fever; while the error here is only a part of that general error of which I have been treating. And, but for this standing and almost universal error, physicians ought to have perceived this long ago, and might ascertain it every day. They should have believed it always, and ought to believe it now, because they can trace no contagion whence these fevers could have arisen, and because they are dispersed cases occurring everywhere, and without the existence of any epidemic to which they could belong. If it does happen that many occur in one neighbourhood, or even, perhaps, in one house, thus giving the false impression of an epidemic and a contagion, I have already shown how this is the consequence of exposure to a common cause.

But there are other circumstances indicating the same thing. There can be no fever without a cause; there are but two causes proved: if it is not contagion, it must be miasma, or malaria. And, as I have formerly shown, this cause is widely spread; while, very certainly, these low, nervous fevers will be found to occur or prevail chiefly or solely where that cause exists, and while also the very individual cause, the exposure, can itself be often traced with care. Further, this low fever will be found chiefly to prevail in the very period and season of malaria; as its range is that of summer, and chiefly of autumn, extending as far with us, often, as Christmas.

And, lastly, the disorder itself has a peculiarity of character which ought always to have explained its true nature and cause. It does not propagate itself, as I formerly remarked of these fevers generally; while physicians, pursuing the same systematic error, have, not without plausibility, according to their own views, attributed this, in all such cases, to the mildness or slight severity of the case or disease. Still more, the mild fevers arising from contagion, or the real *typhus mitior* of nosology, is commonly short in duration, while slender in point of severity. But the very reverse is the common character of the nervous fever which is most common; or its duration is commonly very great, even when it is so slight as scarcely to disable the patient. And, on this, I may remark generally, that if three weeks is a very general average duration for true typhus, the very mild cases will not last so long; while four, or five, or six being the much more common extent of remittent or marsh fever, so will that form the period of the very mildest nervous fever that exists, as arising from malaria. And whenever a very mild fever does last many weeks, there being no relapse, we may be quite sure that its cause is malaria; that it is a marsh fever, not *typhus mitior.* Thus, also, a relapse is rare in the very mild contagious fever, while it is so common in the marsh fevers, of whatever degree of severity, that we almost pronounce with certainty on its character, if there is even one relapse; most certainly and confidently, if there should be more than one, or if the symptoms should spin out to an interminable length.

There is one other fact, very common in these mild fevers; and it is, at the same time, both so remarkable, and so characteristic, that it is only wonderful how it should be so very generally, or rather universally, overlooked; why it did not explain, at all times, to every physician, the real nature of these low or nervous fevers, explanatory, or rather demonstrative, as it is. And it is not less remarkable, that it is a very common event in those extremely short fevers which have been called inflammatory fever, as I shall presently notice. It did not require this, among other examples of neglect, to prove how very mechanically physic is generally practised; what an utter routine it is in the hands of the great majority; how the mass follows whatever schools have taught or fashion may dictate, without inquiry or reflection. If any physician will watch these fevers when they are diminishing or terminating, he will generally find that the disorder assumes a distinct intermittent character; slight enough, it is true, as an intermittent fever, but still having a due proportion to the original continued one—exactly that proportion, in reality, which a regular and marked intermittent does to the remittent marsh fever, of which it is the representative, or the progress. This in itself is a proof of the nature of the first or original disorder; for it is most certain that the contagious fever, or typhus, is not convertible into intermittent, and does not terminate in this manner. That this has been supposed, I know; but it is only a continuation of the same general error—the original error of mistaking marsh fever for typhus—of mistaking the cases in question.

I do not say that this slender intermittent, as the termination or subsidence of a slender low fever, is not obscure; but I do say that every physician ought to be able to perceive it, while I have little doubt that, having thus been pointed out, they will be able to do so hereafter. And it is somewhat remarkable, while not unamusing, to observe, that it is perpetually cured by a sort of fashion or routine practice, while the practitioner himself does not perceive what he is doing; not very well knowing, indeed, what he intended to do by his prescriptions. I allude to the practice of administering bark, "bark draughts," after such fevers, and in what is deemed the convalescent state, on the general and mechanical notion of removing debility by tonics; the word debility and the term tonic having pretty nearly the same meaning—namely, no meaning at all. The cure, in this case, is not that of either convalescence or debility; it is, very simply, the cure of an intermittent; and thus also does change of air, as it is called, restore such patients, after long dragging on under what is called convalescence—on the same principle as it cures a marked intermittent fever, and, not seldom, by removing the patient from the original and perpetually-renewing cause of the disease. Such protracted convalescence, extremely common, not only after these slight cases, but after all fevers, is always, in fact, an intermittent, though never observed, and, perhaps, overlooked, partly because the original disorder was mistaken; and I need not say how valuable this view, and a greater correctness in distinguishing contagious from marsh fever, will become in practice, particularly when it is recollected to what an indefinite time the debility, often of mind as well as body, is frequently protracted, and how often, I am sorry to say, very injurious positive practices, to say nothing of neglect, are resorted to under false views of the nature of the evil. And I have little doubt, that while the supposed utility of bark in contagious fevers has been grounded on the great and common error which forms the fundamental object of these remarks, so have the interminable disputes on this subject proceeded from the fact, that some of the disputers have been treating the marsh fever, without being aware of it, while others have been, under equal confusion, referring to the true typhus, the contagious disease.

In illustrating at some length, but not more than was necessary, the mild or nervous fever, and in explaining that the *typhus mitior* of nosology is generally, or commonly, the marsh fever, or a modification of remittent, I have left little to say as to the only other fevers which require some explanation on the same ground.

The first of these is the synochus, of Cullen's nosology. I do not pretend to doubt that a contagious typhus fever may commence with one class of symptoms, and terminate with another. But for a marsh fever to commence with what are called inflammatory symptoms, and to proceed to, and terminate with the reverse, is so extremely common, that I must, at least, suspect that a very large proportion of the cases esteemed synochus have really been instances of this fever. And that, in practice, now daily in England, this is the truth, is perfectly apparent; and will be so to every one, who, hereafter at least, shall take the trouble to study fevers with a somewhat different care than they have hitherto received. That Cullen's own notions of fever were not very clear or definite, may appear a very bold doubt, particularly to those whose physic has been derived from Edinburgh; but it is not the only doubt, by very many, which arises on studying, after twenty or thirty years of far other studies, works which, in that boyhood of knowledge which continues to be perpetuated through successive generations, it would have been almost a crime to have not wondered at—to be wondered at now, in a very different manner. It is, however, an excuse for him, and one which I am pleased that I can make, that the comparative rarity of marsh fever in Scotland had probably deprived him of the means of forming very clear ideas on that subject, though in his day they were far from uncommon in Edinburgh; that he had formed his notions on systematic writings, of a very vague nature for the most part, and that seeing, habitually, contagious fever among the poor of his city, he had made this disease his leading base and ground of judgment.

The last of these diseases, of simple fevers, requiring notice, is the synocha, or inflammatory fever, in popular language. Its general characters are known; but what is the cause of a fever in which there is no topical affection, which is not symptomatic, and which does not arise from contagion? Cold, heat, any thing else which physicians please; but if still without local affection, what is its character, and whence does it arise? I do not pretend to say: but it is proper that they also who have defined and described such a fever, should refer it to some general principle; that the science of physic may not for ever wander about among words. This much is believed by those foreign physicians who have paid the most attention to marsh fever; namely, that an intermittent (if this term is here admissible), or a fever produced by a marsh, may be limited to a single fit. Of course, it may extend to two, or more; and thus the continued remittent (if I may coin a convenient phrase) may occupy any period, from one, to two, three or more days, onwards. Such a fever, in all its appearances, is the inflammatory fever, or the synocha of nosology; and its termination is very often that which I have already noticed as the nervous fever—or it ends in a slender intermittent: in this form, and this only, it has yet occurred to myself. If it had another character and another cause, I should be pleased to see that demonstrated, not asserted: though I must not terminate this slight remark on this surely obscure disease, without reminding my readers that I have never assumed malaria to be the sole and indispensable cause of even decided intermittent fever. It appears to be the far prevalent one, but it is not demonstrated to be absolutely exclusive.

I do not think that I could have dispensed with these preliminary remarks on fevers in general, in examining the question especially in hand, that most important and serious question, what are the prevailing, or ordinary fevers which occur in ships? Without these, all that I might have said would still have left a ground of evasion and cavil, or at least a demand on the general principles of the decision. That the remarks have run into some detail will, I hope, be compensated by their utility; and I trust, that even independently of their bearings on the main question, the mere excitement of such an inquiry, mere doubts as to what is received, will effect some good; while if they should be true, they cannot fail to be widely beneficial. I may therefore proceed to this question, as it relates to ships; and in a political and commercial view, to the naval service, and that of our vast commerce, and to the important consequences which flow from the health or otherwise of their crews.

It surely cannot be necessary to say much as to the particular necessity of health in the crews of ships, in whatever service; on the very peculiar and perilous consequences of bad health, or of sickness, disability, death, or prevailing mortality. On shore, a sick man finds his substitute in any service; and a dead man is so soon replaced, that death never concerns any one but the immediate dependents and sufferers. It is far otherwise at sea. For the sick, as for the dead, there is no substitute; and when a definite labour has been allotted to a definite number, every diminution of the number of labourers is loss or inconvenience—often, ruin. Merchants, owners, and commanders, will easily answer this question as it regards their own profits and losses; though the readers of this paper would scarcely even conjecture the answer, or might pass the whole subject with little notice. The very insurers at Lloyds can often answer it; for they know well how often the disability of a crew through ill health, through fevers, in fact, has been the cause of averages or losses, which they would gladly have prevented had they known how, and which it is no small part of the object of this essay to diminish or control. And I cannot help thinking, that even that great and respectable body of merchants will, before long, see their own interests in this matter—while not less open, as their history has proved, to the claims of humanity; and that whenever conviction shall reach them, they will, through the well-known means lodged in their hands, promulgate, or even compel a system of regulations for insured ships, analogous to those which I shall hereafter propose as to the naval service.

As to that service, in the case of the periods of war in particular, the inconveniences of bad health amongst the crews are matters of history; and no small volume, while a most terrific one, might be produced to show what have been the consequences of bad health in the navy. And this bad health, to use the popular term, is fever; or it is, at least, that in general: a sickly ship, in sea phraseology, is a ship with fevers. Formerly, the scurvy was an additional evil, now happily quelled. The history of the Havannah expedition, as given by Smollett, is one of those fearful records, of which naval history could furnish many more, if none perhaps so striking; and even the readers of Roderick Random may, from this tale, form a sufficient idea of what they have not consulted in the serious memoir of the same unhappy adventure.

If the miseries of Anson's most romantic and almost incredible expedition were not the exact consequences of fever, they will, at least, serve to prove what sickness can effect as to the service of the navy; and when the writer of this paper knew one instance of a merchant vessel, steered, heaven knows how, into the Havannah, by the captain, lashed to the helm by himself, in a fever, with part of his crew sick and dead below, and the remainder delirious and rioting on the deck; when an

English frigate picked up at sea a slave-vessel drifting under the guidance of a crew, of which every individual was blind, even to the slaves themselves; these are extreme cases, perhaps, yet to which parallels of some sort could be found, and from fevers alone, throughout the whole history of our commerce and of our naval service.

It cannot, therefore, fail to be a most important object to check or destroy the production and propagation of fevers in ships: since, if these are excluded, the crew of a ship is, now that the scurvy has disappeared through proper regulations, scarcely subject to any serious ill-health, or causes of mortality, at least, except from accidents. A ship at sea, barring this disease, is a far more healthy position than the shore, almost anywhere; and the events have proved it such in every case, as is apparent from the history of voyages of discovery beyond numbering. The reasons ought to be obvious: at any rate, I ought not to prolong this paper by pointing them out. Could that which I am desirous of enforcing be effected, as I think it can, the consequences even to commerce would be most beneficial; in the naval service they would be even greater; while a very little consideration ought to show to every one, what I should not here be justified in detailing, at the hazard of occupying another page.

And the mode of proceeding for this end must be to determine, first, what is the exact nature of the fevers which occur on board of ships; secondly, what are the causes of them; and lastly, having ascertained these, how they are to be removed, or their consequences prevented. A portion of this task is nearly accomplished in the preceding remarks on fevers: the rest will not occupy much space; and if what I have said, and shall add, should prove well founded, I cannot help thinking that a most important set of facts have been ascertained in medical and statistical science, and that the consequences will prove most widely beneficial.

No one can doubt that contagious fever occurs on board of ships, while the causes through which it may be introduced, and those which would make it spread, are obvious. I should be among the last indeed to desire to subvert this opinion, highly dangerous as I view that modern and yet limited dogma which has undertaken to deny the existence of contagion, even in the plague. I should be among the last for another reason—and that is, experience; a wide experience in the ordnance transport service during the war, both of the fact itself, and of the value of fumigations in exterminating the contagion. Yet it will be found, on a most careful examination, that this disease, typhus, forms a very small portion of all the fevers occurring on ship-board, and particularly of late years, since the great improvements which have been made in the economy of ships, the improved education of our naval surgeons, and the better understanding on the subject of contagion and its management, which has taken place in modern times.

Not to go over again all the reasons for this opinion, which can be extracted partly from this paper, and partly from the work on malaria, and that on marsh fever, it is almost a sufficient general proof of it, that the fever of ships occurs chiefly in warm climates, and in tropical regions of course most frequently; or generally in the circumstances where malaria is existent. To a certain degree, this has been familiarly known to naval surgeons from all times, as could not fail, in the case of crews, whether in boats or otherwise, subjected to the action of a pestilent marsh, a river, or shore. And while it has long been understood that the common "yellow fever" of the West Indies is not a contagious disorder, there could not have been any difficulty in perceiving that seamen on board, exposed to the same causes, must have suffered from a similar disease. Yet, in other circumstances, and where the disease has been precisely the same in nature, if perhaps differing in some appearances, it has been considered a typhus; a mistake far too easy to make, when the manner in which such a fever appears to spread in a ship is considered, when the characteristics of the two kinds of fever are often not to be distinguished, and when also, if Pringle is right, the marsh fever can, under confinement, even produce the contagious one, or is actually converted into it.

The particularly evil consequences of this mistake, in such a case, are apparent; though, perhaps, the actual treatment of the patient will not be much affected by the error. The cause remaining unknown, there can be no fit method of prevention adopted; and there *is* none: while much toil and inconvenience also are often incurred in attempting to control an imanary contagion. The great object of this paper therefore is to point out those causes, that they may be removed, and with that, such diseases prevented; and if it was first necessary to explain the differences in fevers, and to show how easily mistakes must arise, that object is now sufficiently accomplished.

There are two great and distinct causes whence ships are exposed to malaria; while if the one has been long familiar, serious errors have nevertheless taken place as to the power of this, and as to the modes in which the danger was incurred. The other has never, as far as I can discover, been pointed out in any medical or other writings, till it was indicated in the Essay on Malaria; while, from being the least suspected, and from its power of occurring in any ship, in any climate and season, and even at sea, it is the most important one. Against both, precautions are necessary; and against both, they are available; while, for both cases, they are different. I must explain both here in somewhat greater detail than I did in the Essay on Malaria, for the purpose of the two distinct sets of regulations which ought to be founded on them for the objects of prevention.

Communication with the shore, in a climate or country productive of malaria, is the cause generally known to medical men as generating fevers in ships. But the error here has chiefly been that of not attending to the distance to which this influence extended; practically also, that of neglecting such precautions in ordinary cases, as ought to be well known: which, in fact, *are known*, but are passed over from thoughtlessness, or from want of recent writings urging, or repeating that which many have forgotten, and others have not acquired.

I have shown distinctly, that malaria is currently propagated to distances of at least three miles; and I have given ample reason to believe, in the Essay on Malaria, (I mean in the book itself,) that this influence is very far more widely effective. I have indeed decidedly ascertained since, the instantaneous production of fever through a land breeze, at five miles, to a ship at anchor. Thus it is, apparently, that fevers occur so commonly in ships on nearing the tropical lands; and hence the calentures, (calentura) as they were formerly termed, of which we read in our old voyages under these circumstances. Such an event will happen chiefly under leeward positions as to the vessel, and of course will occur with land winds, where there are winds of this nature on any shore. And it is also evident how this will occur chiefly at night; because this is not only the period of the land wind, but because the mere influence of evening and morning, or of night altogether, in the production or propagation of malaria is very considerable, as I have elsewhere shown. Thus chiefly we explain the effects of dews in these climates, as the vehicle of the poison. And if a ship is ever so situated as to the land, as to have her decks covered with dew in the morning, that is in itself a proof that she is within the reach of danger, and ought to be moved; while I need not say that such dews are actually the perpetual causes of fever to the men of the night watches. And here also we may see the necessity of reducing those watches to the least possible number of men, if the circumstances of the ship do not allow her to leave her position, or weigh and stand out to sea at night.

And as to the land wind, I may give one general rule applicable to all circumstances of ships engaged in tropical climates, or in warm ones generally. It is always attended by that smell of land which is better known than it is easy to describe, and which many delicate or experienced individuals can perceive at great distances. And while I have no doubt, from the facts and reasonings given in the Essay on Malaria, that this substance can be conveyed as far as the smell of land is perceptible, it would be prudent, whenever that can be done, in such a climate, to weigh and run to sea, and particularly, of course, during the night, when the danger is augmented. And there are circumstances in which a vessel should not even wait for the breeze, but be at least a-trip and ready to get under weigh at the instant it comes to blow; since in one instance which I have noticed in that essay, in these very circumstances, and even where the captain of a frigate was habitually attentive to this precaution, to such a degree indeed as to order all the superfluous men below on this shift of wind, the armourer was seized with the fatal cholera, in the very act of clearing an obstruction in the chain cable, while others of the crew, unavoidably employed on deck, also died in a few hours of the same disease; the attack having attended, in an instant, the first perception of the land smell. And if this is an extreme case, it is precisely the one required as a proof of the truth and value of these remarks. In ordinary circumstances, the disease would have been a fever; but coming on less decidedly, and easily attributed to other causes, the same reasoning would not have been deduced from it. And if the mere delay of a few minutes in this case, arising from the accident to the cable, was, in the captain's own estimation, the cause of this loss, I must also remark, that the vessel was then about five miles from the shore. How much Blane and others have erred, and how dangerously, in fixing on one thousand, and on three thousand yards, as the utmost limit of the range of malaria in these cases, I need not say.

The other circumstances in which ships and their crews are exposed to the malaria of the shore, are more familiar and admitted; however much the necessary precautions are for ever neglected. I can afford to be comparatively brief, therefore, on these; and their enumeration will complete all that is necessary as to the first leading cause of fevers, or of marsh fevers, in ships.

The general cause of evil in this case, is familiarity with the shore; the landing of men, under whatever circumstances, in situations where malaria exists. The danger is evidently greater as the harbour or anchorage is most exposed to the effluvia of marshes or jungles; but I need not here repeat circumstances which were pointed out in the former essay. That it is also greatest at night, or between sunset and sunrise, has been further shown; so that, on both these considerations, additional precautions ought to be founded. And it has so happened, in general, from obvious enough causes, that most of the tropical towns and harbours are situated in the most unhealthy spots: while, not seldom, a bad one has been chosen where a salubrious one was equally convenient, or, as in the case of Batavia, artificial means have, by their adoption, rendered that which was naturally bad still worse. And the same indeed is too often true, even in Europe, as in the Mediterranean: the ignorance of the days in which these places were chosen, having combined with their convenience, while, in too many of them, modern nations have neglected those remedies or means of improvement respecting which the ancients showed so much anxiety.

In practice, it is the duty of the commanders of ships to avoid all that can be avoided respecting communication with the shore, through the landing of men, and particularly in pernicious spots, or at dangerous hours; while an enumeration of the former would comprise the geography of half the sea-ports of the globe. In ships of war particularly, where this really can be effected, men ought never to be permitted on shore upon leave at night, nor even officers, though the hazard to them is less; and when it is supposed, as is not uncommon, that the men affected with fever, have caught a contagious fever from improper communication, it will be found that the disease is simply the marsh fever thus induced. Thus, also, ships of war can procure their stores from the depôts and dock-yards, as for example, at Port Royal, Jamaica, and at St. Lucia, through launches or shore boats, by means of natives or negroes impassive to the effects of malaria; and in this way can such vessels often avoid anchoring, at least near to the shore, or even contrive to run out to sea every night. How far merchant vessels can attend to these precautions must depend on the nature of their particular affairs with the shore and on the strength of their crews; very materially also it will depend on the character of the captain—on his discernment, docility, humanity, and the interest he may feel for his owners. But I need not pursue what I have here said through details more minute, as the ramifications ought not to be difficult to conceive.

And this is not speculative matter: the practice in question has been tried by many enlightened and active officers, when their own discernment had taught where the danger lay, and with the most marked success. I need not quote more than two instances, though I could easily accumulate many more. The first is the case of Captain Smyth, well known through his account of Sicily, and his long and laborious surveys in the Mediterranean; and so successful did these several attentions prove, that he did not lose a man, or suffer from a fever during the many years he was employed there, in every harbour and on every coast of Spain, Italy, Greece, and Africa, and necessarily under the most intimate communication with places as pestiferous as any in the world.

This is a case comprising a long-continued train of the experiments and precautions in question, and nothing can be conceived more satisfactory. The only other instance which I shall quote, is, as a solitary case, not less remarkable. It is from Admiral Sir Henry Baynton, who informs me, that when in the command of the Quebec and Nereide frigates at Jamaica, he was in the habit, like others, of anchoring near the dock-yard at Port Royal, the pestilent marsh near which is well known, and that he invariably carried to sea a fever by which he lost many men. Perceiving the cause, and that cause the one which I have here stated; he determined to change his plan, when afterwards in the command of the Cumberland, 74, by anchoring at a greater distance; and though he was detained in port during most of the hurricane months, or the most sickly season, he, to his own great surprise, as he states it, but by using the several precautions which it is the object of this paper to point out, retained his whole crew in as good health "as if they had been in the British Channel," there not being one man on the sick list in a crew of 590 men: an instance, probably, scarcely known in naval records.

Here there is an example of what may be done by the combination of knowledge and attention on the part of a commander, even in the most unhealthy climates and situations; while the contrasts which I might easily draw between this and the histories of other ships in similar circumstances, would present a most extraordinary, and a not less fearful picture; though I must avoid what would give pain to many individuals still living, and even through those that are dead, to many more. Let those, however, who wish to see what such a contrast can be, read Smollett's account to which I formerly referred; and then ask themselves whether these speculations are visionary, or whether, on the contrary, the deepest blame does not attach to all those who have suffered such atrocities, as it will continue to do to all those who shall suffer them hereafter, where all possess the same power as the excellent officer whom

I have here brought forward as authority. And though I might refer to the journals of the several ships under his command, contrasting them with those of the same ships under other commanders, I wish to avoid doing this, from feeling that I cannot allot him the praise which he so highly deserves, without an implication of blame on his predecessors and followers in the same vessels. And the same reason induces me to suppress even some other names where I might equally have allotted praise; as, though the readers of this journal may not be aware of it, the records of the Admiralty, and the personal knowledge of individuals in the navy, would easily point out those officers to whose neglect there has been owing a loss of life, with inconvenience to the service, and an expenditure of the public money, implied in the loss of those lives, which it is most painful to think of, especially when we know how easily all this might have been avoided.

There is but one other modification of the connexion of ships with the shore in tropical and insalubrious climates, which seems to demand a specific notice, and for the sake of the precautions applicable to it, under this division of the general subject of the influence of malaria on the healths and lives of seamen. I allude to a particular class of service which, as perhaps most common on the coast of Africa, is best understood by this allusion. It is the sending boats on shore, for the purposes, among others, of wooding and watering. The consequences are but too well known: fevers of the worst class, and a very general or common mortality, often highly inconvenient, putting out of question all views of mere humanity. I have said, in the general Essay on Malaria, that much of this service ought not to be performed by good seamen, and would be a fitting labour for convicts; and that it is difficult to comprehend the policy which allots, as punishment to those who have forfeited their lives to the state, the best of climates and the most salubrious of occupations, while what almost amounts to a condemnation to death, is the lot of innocent and of valuable men.

And on this subject, as at least an easy and specific remedy, I have the experience of Captain Coffin to state; who, in these circumstances, applied for a party of negroes from the shore, and who, in consequence, preserved the lives of his crew, which he must otherwise have lost in the usual proportion. And his recommendation is, that a ship, or more, of no value, should be kept on the African coast, to be the receptacle of volunteer negroes for this particular service; a matter easily accomplished, and of which the consequences could not fail to be most beneficial. As to ordinary precautions, in the case of seamen, it should be an invariable rule to suffer no boat to enter a river, or to be on shore at all, between sunset and sunrise; and further, to prevent, as far as possible, any such boat from being on the shore, or in a river at low water,—as that is the period when, from the exposure of the mud, the malaria is active, while its presence is betrayed by the very smell.

Among other precautions, applicable to particular cases, I stated in the Essay on Malaria, and from African authority, that the lighting of fires in the service of cutting wood was found to be an effectual preventive; and it will be easy for any officer to see in what exact cases it may be applied. It is another general precaution, applicable to every case of this nature, every service of seamen on shore in a hot climate, never to suffer men to go on shore, nor even to be on the deck in harbour, further than as the watches are concerned, before breakfast, or without at least some allowance of spirits; since, in every case, this precaution has been found of great use, as the standing practice of Holland fully testifies. And, for the same reason, the smoking of tobacco, which, from obvious motives, is discouraged in ships, ought not only to be permitted, but commanded; since ample experience has also shown its great utility. To suffer no men to be unnecessarily on deck when near the shore, is a precaution to be deduced from what has already been said; and this rule offers a particularly obvious reason for reducing the night-watches, in particular, to the lowest admissible number. With respect to some of these circumstances, and very remarkably as to the dangerous influence of the morning, as well as the evening, the experience of India offers some very remarkable illustrations; as the great losses of men have always occurred in those regiments where a martinet feeling in the commanding officer (as the phrase is) has led to the regular drilling of the men before sunrise, that they might avoid the imaginary evil effects of the heat of the day. I use the term imaginary with little scruple, convinced that there is incomparably less danger at this period than at any other. There is always much alarm, it is true, at what is called a *coup de soleil;* an unlucky *term*, producing the same consequences on the mind as other terms do. I do not say that phrenitis, or, perhaps, common fever may not arise from this cause; but it is abundantly plain, from every case of this nature which I have found accurately described, that such an imaginary *coup de soleil*, or effect of a hot sun, is, in reality, most generally the fever in question, produced at a very different time of the day generally, and from a very different cause; though, from the natural effects of this prejudice and this *term*, easily attributed to the action of the sun.

I have but one suggestion more to offer in the way of prevention, as to this usual source of the action of malaria on the crews of ships. All the people of Southern Europe think that it enters by the lungs; and, in Malta, in Spain, in Italy, in Sardinia, perhaps, more widely, it is thought that the fever may be warded off in perilous situations, by stopping the mouth and nose: in Italy it is commonly held that a gauze veil is effectual. If this be fact, it is an easy remedy; if it is still to be proved, there can be no better opportunity of proving its truth or otherwise, than on the African coast, by obliging any one boat's crew to use this expedient, and by trying what the results would be as compared to those with another boat, without this precaution.

I have thus gone through, in as much detail as my space would afford, and in as much, I hope, as is really necessary for those who choose to reflect on this subject, the first great cause of the production of fevers in ships, from malaria; and the modes of regulation and prevention which are applicable to that cause: it remains to examine the other.

In the Essay on Malaria, I attempted to show that this poison was the produce of ships themselves, in circumstances independent of the land; the principle being the same, and the poison generated by the action of water on the wood of the ship,—an ordinary instance, in reality, of vegetable decomposition. I then quoted the case of sugar-ships as especially in point; and a further examination of facts has confirmed me in the belief, that the general fact is not to be questioned, and that this is the real cause of those fevers in ships, which are so commonly attributed to contagion deemed typhus; never suspected to arise from malaria, and consequently not supposed to be, by any possibility, remittent or marsh fevers, because occurring at sea, or in circumstances where no exposure to an unhealthy coast has taken place. Not to accumulate specific cases, which ought really to be unnecessary, I will here note but one remarkable fact bearing on this point, where an officer was suddenly struck with what is called apoplexy on the opening of a water-cask, and has remained partially paralytic for life: the first effect, and the whole subsequent disease, being precisely what occurs in France and Italy so frequently, from a sudden and transient exposure to a peculiarly virulent or condensed malaria.

This cause then, or bilge-water, is that source of malaria and fever in ships which may be deemed universal, because it can occur in almost any climate, and, under neglect, in any ship, even at sea, and without the least communication with the land. And if it should thus be produced, it can scarcely be guarded against, from the very circumstances of a ship; so that it is peculiarly necessary that the cause itself should be remedied *in limine.* And it is least of all surprising that the fevers occurring from this cause, this mode of the presence of malaria, should have been considered as typhus, and as the produce of a contagion, casually received into the ship, and continuing to act from adhering to the vessel itself or its furniture. It must always have appeared as the produce of the vessel itself, which it in fact was; while the unsuspected cause has led, in anxious hands, to fumigations, whitewashing, scowering, and all those other obvious remedies against contagion, which must ever have been inefficacious, as they, in fact, have proved; because no precautions of this nature could have checked the action of a poison generated every hour, and supplied as fast as even ventilation could dissipate it.

And if this very circumstance made it appear that the cause was contagion, so that has often appeared to be confirmed by the fever in question always appearing to commence in some particular part of the vessel, and to spread from that point. And while the fact was and is so, it is one that aids in confirming the very cause here assigned. It will depend on the quality and construction of the particular vessel where, exactly, it is to appear; but it will be found that such a fever commences about the cable-tier, or in some other place through which the air from the hold ascends, and that the men most exposed to the bilge-water are exactly those who suffer most from it.

I hope that I need not enter into much more detail as to this cause. It will naturally be a more active one in a hot climate than a cold, and with a crowded crew than a thin one: it will depend much on the cargo; and thus, as it is peculiarly notorious with sugar cargoes, has it also occurred very remarkably with coffee and with corn; some very remarkable instances of most destructive fevers from leakage with a corn cargo, being even familiar to those in any way acquainted with the ordinary commercial history of shipping. I have also reason to believe that a new ship is more subject to it than an old one, that is, if she should be leaky, or the pumps neglected; just as a new cask is decomposed by water, and spoils that more readily than an old one. Thus also, which is useful knowledge as matter of precaution, it occurs more readily with gravel or mud ballast, and less easily with iron; while it appears, also, that the malaria is most virulent in a ship which, from the abundance of vermin, of rats and cockroaches, for example, contains putrifying animal as well as vegetable matter. It has been suspected, but not proved, that on shore, in marshes and sewers, for instance, the addition of animal matter increased the production or virulence of malaria; but if that has not been proved, the facts in question may, at least, give some colour to the suspicion.

I have but one other remark to make on this cause, and it is one that, like all else, refers to matters of prevention. This is, that the closing or separation of a vessel by means of bulkheads, adds very much to the evil, and very obviously, by impeding ventilation and the dissipation of the poison. And this is so far from speculative matter, that were it not for the impropriety of mentioning names, now high in office as in rank, I could quote two pointed cases of ships of the line, with equal crews, and on the same services, on the coast of Brazil, where, while the one lost a large number of men by the remittent fever, obviously generated in the vessel, the other preserved her health; the difference of the two being, that the one was closed up by bulk-heads, and that the commander of the other, aware of their evil effects, had caused the whole to be cleared away.

I hope that I need not dwell longer on this particular source of the fevers of ships: the precautions and modes of prevention to which these statements lead will occupy but little space. Cleanliness and ventilation are the leading points; but to be conducted in a far other manner and on very different views from those in which they have usually been done, because directed to very different purposes.

It must be evident, in the first place, that the laborious system of scouring and whitewashing is useless, otherwise than it is advantageous for the sake of general cleanliness. It is equally plain, that fumigations, though with the mineral acids, must be without effect against a poison which is in a state of hourly production and renewal. Nor can fires have more than a temporary effect, since that must cease with the period of their action. The radical cure consists in cleanliness as to the hold of the vessel, for there the evil lies. This is the marsh, if I may apply such a term; the steady source of the malaria: and the remedy is as simple as it is easy; since it consists in nothing more than washing the ship by means of the plug, daily, or as often as that is necessary. And experience has amply proved the value of this practice. It was the rule of Admiral Baynton and of Captain Smyth, to continue this operation till the pump brought up water as clear as that of the sea outside, daily; and the success of these commanders, under this and the other precautions, I have already stated. In the reverse way, experience has proved the same thing; since, in some of the most notorious and destructive instances of fevers in ships of war, many of which I could name but must not, it was found that from neglect of this process, the hold and ballast were a mass of mud; and, what is more remarkable, that the character of the very same ship, as to sickness or health, had always changed with the change of commanders, just as the sailing qualities of a vessel has been known to do under similar changes..

What else may be said as to prevention in this case, relates to ventilation. Separations in a ship are often unavoidable; to a certain extent, always indispensible. But ventilation can still be effected through wind-sails, or other well-known means; and if the general principles here laid down should ever be admitted and understood in the Navy, or in ships generally, an attentive and able officer will find no difficulty, under any possible circumstance, whether as it relates to this cause or the former, in adopting such regulations of detail as the general principles indicate; and such as will doubtless prove efficacious in diminishing or preventing this long-standing and most active cause of mortality in the Navy, or in ships of whatever nature. Admiral Baynton well remarks, that his own crews were always far healthier than an equal number of people in any country town in England; and, in truth, it ought to be so; since, with the exception of these fevers, for ever incurred through the grossest neglect, it is not easy to conceive a situation more salubrious than that of a ship at sea, more free, at least, when we consider the usages and conditions of the inhabitants, and their modes of life, from mortal diseases.

I think that I need not proceed; because, if what I have said is not capable of producing conviction, I cannot conceive that anything will. In this case, I must trust to Time, the great friend to all improvement, who for ever effects what evidence and reason cannot. But it will be convenient to place, in a brief and simple summary, the chief regulations which have been here proposed, that they may be more conveniently committed to memory, should any one think that the entire recommendation deserves attention.

With respect to the possible effects of land or its malaria on ships, it would be prudent to avoid approaching this within a a certain distance, in tropical climates, and on low shores especially, unless in cases of necessity; and this distance to be limited to not less than three or four miles.

Whenever anchoring can be dispensed with in such cases, it ought to be avoided; and ships should be directed to be off and on, particularly at night, whenever the service admits of this, as the chief danger is between sunset and sunrise.

If, in certain harbours and ports, such as St. Lucia, for example, or Port Royal, stores can be sent on board by launches or otherwise, this should be done, to prevent the hazard from anchoring in a port.

Boats should never be sent ashore from sunset till after sunrise, and men should never have leave at night.

No man should leave the ship in the morning till after breakfast, or after a dram; and smoking, or tobacco in any shape, according to the practice of Holland, should be made the universal practice.

In any case of a vessel hovering or anchored on a tropical or other suspected coast within four or five miles, should the sea breeze, or any wind, change, or come to blow off shore, they ought to stand off or weigh immediately; and in these cases, also, all hands unnecessary on deck should be ordered below. The night-watches, in all these cases, ought to be reduced to the least possible number of men; and the men of these watches ought not merely to be permitted, but ordered, to smoke while on deck.

Boats employed in cutting wood in tropical rivers, should always arrive, if possible, and quit, during full water, as the fevers are produced during the exposure of the mud. If fires can be lighted during this service, that ought to be a standing order.

No boat to be in such a place at night.

It is believed that gauze veils prevent the malaria from attacking those exposed to it;—the least that can be said is, that it deserves a trial.

These are the general precautions; and it is believed that if adhered to, with such others as may be derived from the same general principles, a great portion of the mortality in ships in hot climates would be avoided. I will only add, that for African service it is recommended that negroes or natives should be adopted to perform those duties, in wooding and watering, that have proved so destructive to British seamen.

The other set of precautions relate to the production of malaria from bilge-water, or a foul ship, and they are, perhaps, more within the reach of positive regulations than the former.

The first general rule is this; that the ship should be washed every day, or as often as is practicable, by means of the plug, nor is the washing to be deemed effectual, till the water from the pump is as clear as that outside.

As, in any case of the production of malaria from the ship itself, its very construction renders it peculiarly effective, through confinement, every practicable mode of ventilation should be resorted to; while, as contagion is not the cause of these fevers, all the common plans of whitewashing, and so forth, usually adopted, may be dispensed with, as producing trouble and doing no good: and, in this view, all fumigations, from whatever materials, are useless; because the effect of them is temporary, and necessarily nothing, when the poisonous cause is in a state of perpetual production. This has been a leading source of deception and evil, and particularly so, as appearing to be founded on solid principles, while these were, in reality, false ones.

On the view of confinement, it is also proper that no divisions or bulk-heads should be suffered in a ship, if they can be dispensed with, as they concentrate and render the poison more active.

To conclude, if these regulations could be rendered effectual, it is believed that the greatest cause of mortality in ships would be removed; and if, in addition to this, every ship, leaving any port, were adequately fumigated by sulphurous acid, and the persons and clothes of newly-entered men, or men on shore on leave, were attended to, by the well-known means, together with such other equally familiar precautions as are recommended against contagion, it is believed that fever would shortly become unknown in the British Navy; and further, that as this is the only real cause of mortality, remembering always that dysentery and cholera also arise from malaria, and from the same causes, a ship would, in any case, be the healthiest of residences, and thus the mortality of the sea would become an absolute trifle to what it has hitherto been.

I may now conclude.—The past history of the Navy is a history of fearful or most injurious mortalities from fevers. The inconveniences to the Service, ill appreciated by lands-men, are but too well known to the Navy, and to the State also. The expenditure of life, and therefore of money, is not less known, at least, in the accounts, if placed, as usual, to the general average of unavoidable casualties. What regards humanity is not worth reckoning, it is to be presumed. If all this can be prevented, he who shall prevent it will have effected no small good, in many ways. The detail of past evils and future possible benefits would form a very curious and instructive picture; it would carry even an air of romance. In the merchant service, the evils are radically the same, but the effects are in some respects different. Humanity here, also, reckons for nothing, as long as the dead man can be replaced by a living one. Yet there is loss: wages are paid for services not performed; vessels are disabled from inefficiency of hands; and vessels are lost. The owners escape, it is true, for they are insured. Who is there to care?—the insurers. The insurers are so divided and diluted, that there is no one to care. The merchant service is little likely to do anything towards improving the health of ships. It must finally rest with the Admiralty, with the State. That when the State believes what is here detailed to be true, it will regulate accordingly, we cannot doubt; but we may safely doubt if this reasoning is adequate to command their belief.

Lineaments of Leanness. By William Wadd, Esq., F.L.S.

It may naturally be supposed, from the cases and comments on corpulence, that the "*fat* and *fair*" have not been the only persons who have consulted me; the man who knows how to reduce "the *fat*" ought to know how to "fatten the lean;" and, accordingly, I have occasionally been visited by "*quelques Anatomies Vivantes,*" and although Mons. ———*,

* This extraordinary production of nature, pronounced by the most eminent of the faculty in France and England, to be a "*great phenomenon,*" was brought, as we are told, to this country, at a considerable expense, to contribute to the advancement of science! The expense of keeping a skeleton we cannot calculate from any practical experience in this country; but we may presume it was not much; "a *recreative excursion,*" for a party of such persons, would, it may be presumed, not cost so large a sum as the convivial committee of City lands. Quere? which was advanced most by the skeleton's visit, the Englishman's philosophy, or the Frenchman's fortune?

the real living skeleton, never did me the honour of a visit, I have seen full as great curiosities as the said Monsieur, within the circle of my own acquaintance; and, in the persons of two of my most intimate friends, witnessed the most extraordinary instances of emaciation that the human frame could possibly exhibit. One of these was a gentleman about forty years of age; the other was one of the most lovely and beautiful of her sex, who, when she died, at the early age of thirty, presented the resemblance of an ivory skeleton, covered with thin parchment.

These cases were similar in appearance and progress; and each of them the effect of great organic disease, in the mesenteric glands and abdominal viscera. The first of these cases, was Major P——, who, after much military service, and much harder duty as regarded his health, in the service of conviviality and good living, became a barrack-master in Sussex. I had not seen him for a year or two, when one morning, he called me up, having suddenly left his quarters, "to seek my friendly advice, on matters of the utmost importance!" For some moments I could not recognize my friend,—I knew him not; how should I? an insane skeleton addressed me! It spoke of circumstances I knew, but in a voice I did not know. Never, in my professional life, was I more distressingly affected. I met the momentary difficulty of contending feelings as well as I could, and, as soon as circumstances permitted, deposited a living skeleton in the charge of his family. He lived a few weeks after, eating voraciously; and swallowed, or rather bolted, some large lumps of meat within a few hours of his death.

There are, however, cases of the absorption of fat, the causes of which it is impossible to ascertain.

A curious case is related by Halle in the 'Mémoires de l'Institut National,' of a young woman who gradually became emaciated, without any diminution of appetite, and without any specific complaint. At the age of twenty-one, the emaciation commenced; and from that time went on progressively: she had no fever, no cough, no sweatings, no œdematous swellings whatever; and the excretions were quite natural. She died at the age of twenty-five, having been confined to her bed only fifteen hours, and in these were included the usual hours of rest. The only peculiarities discovered, on dissection, were the almost total want of fat, and the obliteration, in a great measure, of the lymphatic system. The lacteals were invisible; all the glands were remarkably small; the inguinal glands, in particular, were quite shrunk, and the vessels leading to them were almost impervious.

Halle therefore concludes, that this case affords an example of atrophy, independent of any organic affection, except what resulted from the successive obliteration of the lymphatic system.

Two remarkable instances are mentioned by Lorry—one of which will sufficiently illustrate this remark.

A person advanced in years, and affected with melancholy, became, without any evident cause, in such a dry state, as to be unable to move without producing a horrid crackling noise in all his bones, even the spine, to such a degree, that (being a priest) he was obliged to give up saying mass, as the noise was so great as to astonish the vulgar, and make children laugh.

Sudden emaciation and absorption of fat, however the effect of diseased organic structure, or acute disease, does not properly belong to, or characterise that opposite state, or antithesis to *corpulence*, known by the term *leanness*, which is always attended by extreme tension and dryness of the cellular membrane, very frequently by weakness in the digestive powers, but not constantly, as we sometimes find thin and lean persons, eat more in quantity than others.

It is not eating alone, however, but digestion that gives strength and nourishment: yet digestion may be perfect, and assimilation of chyle into blood imperfect; for, that the quantity of nourishment does not depend on the quantity of food, is evinced, by the most voracious eaters being found among the leanest of their kind.

The act of eating gives rise to three subsequent processes,—digestion—chymefaction—and chylefaction. The production of fat seems to depend most on this latter process, and whether as Father Paul says, "the little we take prospers with us," or whether we fall off though fed on turtle, seems to depend on the facility of chylefaction; a process carried on out of the stomach, in the small intestines, a lower portion of the alimentary canal, to which the attention of modern physicians and physiologists has been particularly directed; and to which we may attribute the duodenal diseases, and discrepancies, now so fashionable.

There are many of the phenomena of digestion perfectly intelligible; there are others that are not so; and from the peculiar effects of certain alimentary substances, we are led to conclude, that there is a shorter road for some of the excretions, than by the lacteals and general circulation. And although we can very readily explain and account for various circumstances connected with digestion and chylefaction, there are many questions arising out of them, that an ingenious casuist may suggest, to which we can give no other answer than the doctors did to Voltaire, when he proposed on this subject the following question:—

> "Par quel secret mystère,
> Ce pain, cet aliment dans mon corps digéré,
> Se transforme dans un lait doucement préparé?
> Comment, toujours filtré dans ces routes certaines
> En longs ruisseaux de pourpre il court enfler mes veines?"
>
> "Demandez-ce à ce Dieu qui nous donne la vie—"

was the oracular answer.

"But what is the cause of my leanness?" said a thin gentleman, who would have given half his fortune for half of my fat; "what is the cause of my leanness?"—"Demandez-ce à ce Dieu!"—"Pho! demand a fiddle-stick's end!—I want *you* to tell me, sir—*you*, sir;—what is the cause of my leanness?"—"Well,—soyez tranquille—be quiet a minute: there is a predisposition in your constitution to make you lean, and a disposition in your constitution to keep you so." This explanation, about as satisfactory as Dr. Thomas Diaphoreus' explanation of the properties of opium—"quia est in ea," &c. &c., did not soothe the irritability of my lean inquirer, who became, if possible, more shrunken and wizened as his heat increased. Seeing the nature and temper of my antagonist, I went to book with him in another way:—"Why, sir, as to the causes of leanness, there may be many that an ingenious theorist might suggest;—I speak to you, sir, as *to a sensible man*."—The storm and heat began to subside; an oily word is like an emollient;—" I speak to you, sir, as a sensible man, and I am aware that it is not sufficient to talk to *you* in general terms, of constitutional peculiarities, digestive organs, and alimentary functions; you must have a positive specific cause; and, if possible, an explanation of that cause, as plain as the specification of a patent."—" Just so; that is what I want—you speak like *a sensible man*"—(the retort courteous)—" Every effect, sir, must have a cause; and I want to know whether the cause may be in the stomach, or any particular part of my inside, and if so, whether by particularly directing our attention to that part, wherever it may be, we can in any way alter its nature?"

The expectations of patients are sometimes very exorbitant, generally in proportion to their ignorance; sensible people give very little trouble. Hence it is not difficult to satisfy these exorbitant demands; for a foolish answer will always balance a foolish question. I do not recollect ever to have met the equal of this inquirer, except in a very pompous person, who kept a large circulating library, who doubtless thought " keeping a library, he himself was learned," and who, whenever my answer satisfied his great mind, always expressed his approbation by a condescending nod, with—" Aye! now, sir, you give *us* a physical reason!"

But " revenons à nos moutons:" finding my patient's mind was bent on *localities,* I suggested the intestinum cœcum for his consideration—the newly-discovered organ of fat! He had never heard of it; this was what he *expected of me;* (another retort courteous, for which I owed him one.) " This was news! What was it? how was it?"—" Why, sir, some are of opinion that the cœcum contains a certain ferment,—some that it is destined to secrete an important fluid,—others take it for a *second ventricle,* wherein the prepared aliments may be stored up, and so long retained, till a thicker and more nutritive juice may be drawn from them;—and how it is a depôt of fat you will find in the ' Philosophical Transactions.' "

He heard this very attentively, and having passed mutual compliments, and being *on very good terms with each other,* he favoured me with his unreserved opinion. " I see very clearly, sir, the application of this discovery to my case: this is an age of discoveries!—the quantity of fat diffused over the body must be in proportion to the quantity in the depôt: I must have a small cœcum! Now the question is—can we enlarge it?—Perhaps I have no cœcum!" We quite agreed upon the impossibility of supplying this defect; but as "there is more in heaven and earth than we dream of in our philosophy," my philosopher did not like to relinquish all speculation upon the subject. I considered the case beyond surgery. I am not sure that I might have been allowed to look at the caput coli,—though I have known an operation done on almost as frivolous grounds. But when I told him that, according to the account of the celebrated Hoffman, dogs became rapidly fat when their spleen was removed, and that Mr. Hunter once removed it from a wounded man, who did very well, there seemed to arise a lurking longing, as much as to say, "I wish Mr. Hunter had my spleen."

There is an asperity in the acute angles of some persons, that gives a most forbidding appearance,—every feature is sharp, and every variety of movement quick. Shakspeare makes Cæsar desire that he may have fat people about his person. It would be hard, on this authority, to condemn all persons who have the misfortune to be born with small cœcums and large spleens, and are meagre from causes they cannot control, "as fit for treasons, stratagems, and spoils." Yet it is clear that Cæsar liked a curvilinear *embonpoint* appearance in his body-guard, and thought there was most safety with a corpulent corps of household troops.

The lean are not less exposed to ridicule than the corpulent. A reverend doctor of divinity, of very ghostly appearance, was one day accosted by a vulgar fellow, who, after eyeing him from head to foot, at last said, "Well, doctor, I hope you have taken care of your *soul!*" "Why, my friend," said the amiable shadow, "why should you be so anxious that I should take care of my soul?" "Because," replied the other, "I can tell you that your *body* is not worth caring for."

Jonas Hanway, who was remarkably thin, was met by a man much inebriated, who approached him in so irregular a direction, that it might have been concluded that he had business on both sides the way. Hanway stopped when he came up to him, to give him his choice; but the man stood as still as his intoxication would permit him, without attempting to pass on either side. After viewing each other a moment, "My friend," said Hanway, "you seem as if you had rather *drunk too much;*"—to which the man replied, with considerable *naiveté*, "And you, my friend, seem as if you had *ate too little*."

I have stated, that good humour and the power of looking on the favourable side of things are among the concomitant causes of corpulency; and so they have been considered from the days of Solomon.—"A merry heart doeth good like a medicine; but a broken spirit drieth the bones."—*Proverbs.* Now the optics of some lean people are in so unlucky a perspective, as to throw a shade over every picture that is presented to them: to them the whole face of Nature is gloomy and ugly. It would be a blessed thing for such persons, if Dollond could alter their vision by the aid of spectacles. To fatten a man by impressions on the optic nerve would be a new feat in the philosophy of physic and surgery.

"Laugh and grow fat" is an old adage; and Sterne tells us, that every time a man laughs, he adds something to his life. An eccentric philosopher, of the last century, used to say, that he liked not only to laugh himself, but to see laughter, and to hear laughter. "Laughter, sir, laughter is good for the health; it is a provocative to the appetite, and a friend to digestion. Dr. Sydenham, sir, said the arrival of a merry-andrew in a town was more beneficial to the health of the inhabitants than twenty asses loaded with medicine." Mr. Pott used to say that he never saw the "Tailor riding to Brentford," without feeling better for a week afterwards.

From what has been said, it will appear that, next to my philosophical patient's notions of enlarging the cœcum, and lessening the spleen, the excitement of laughter ought to have a a place in the "Ars Pinguefaciendi." Mr. George Jones, mentioned by Granger, seems to have had this object in view in his "Friendly Pills," which were to make patients of all complexions *laugh at the time of taking them*, and to cure all curable complaints. Let us hope, for the sake of his Majesty's "lean lieges," that George Jones's recipe may start from some antiquarian pill-box, for the engraissing and beautifying that portion of the population. Let us also flatter ourselves, that although we do not now know our way to Mr. Payne's toy-shop for his three-and-sixpenny bottle of "Pinguefying Specific," a specific will be found amongst the arcana of modern chemistry*.

Amongst the most singular propositions for fattening the person, that our inquiries have furnished us with, that of flagellation is the most whimsical. In the "Artificial Changeling," we read that the Mangones, to make their bodies more fat for sale, "were wont to whip their posteriors and loins with rods, and so by degrees make them more fleshy;" and it is even said that this is noticed by Galen, as no contemptible stratagem to attract the nourishing particles to the outer parts.

The operation of flagellation has been, in former times, resorted to by ecclesiastical doctors, as well as medical; and some very curious secrets were laid open in the Abbé Boileau's "History of the Flagellants." But the work most to our purpose is that of Meibomius, "De l'Utilité de la Flagellation."

"Jerôme Mercurialis," says Meibomius, "nous apprend que plusieurs médecins ont ordonné la flagellation à des personnes maigres pour les engraisser, et leur donner de l' embonpoint.

"Galien citant à ce sujet les stratagêmes des marchands d'esclaves qui se servoient de ce moyen pour les faire paroître plus brillans de fraicheur et d'embonpoint, ne laisse aucun doute sur l'efficacité de ce remède. Il est certain qu'il fait gonfler la chair et attire à elle les alimens. Personne n'ignore que la flagellation avec des ortus vertes a le plus grand succès pour raffermir les membres et rappeler la chaleur et le sang

* When the Spectator was first published in the form of a newspaper, advertisements were attached to it, of which the following is a specimen:—

"An assured cure for *leanness*, which proceeds from a cause which few know, but easily removed by an unparalleled specific tincture, which fortifies the stomach, purifies the blood, takes off fretfulness in the mind, occasions rest, and easy sleep, and as certainly disposes and causes the body to thrive and become plump and fleshy, if no manifest distemper afflicts the patients, as water will quench fire, &c. &c.

"It is pleasant to taste, and is sold only at Mr. Payne's toy-shop; price 3*s.* 6*d.* a bottle with directions."

dans les parties qui en sont privées."—*Meibomius, de l'Utilité de la Flagellation, p.* 33.

He adds,—"Combien de nourrices, sans avoir consulté Jerôme Mercurialis, ni Galien, ont recours à ce stratagême qu'elles connoissent par tradition, et claquant les enfans sur les fesses, avant de les rendre à leurs mères, trompent par cet embonpoint factice et momentané, la confiance des tendres parens qui leur ont confié ces intéressantes créatures!"—*Meibomius, de l' Utilité de la Flagellation.*

One gentleman told me, that he understood mercury was very fattening. Mercury of itself cannot be said to fatten; for if it fails to cure the disease for which it is adminstered, the patient becomes thinner.

Those who refer all the difficulties to the stomach, and look for comfortable remedies in the "Cookery-books," would do well to visit Paris, where a restaurateur invites patients of this sort, by the following consolatory exhortation written over his door:—

Venite ad me omnes qui stomacho laboratis, et ego restaurabo vos!

This class of enquirers, who are generally great believers in the efficacy of milk, and cock-broth baths, gelatine, and potato-pie, and are ever on the alert to discover the most nutritious articles of food, should be informed of the notable example of the effect of chocolate, given by the industrious Dr. Mundy, who says that he knew a man in a desperate consumption, who took a great fancy for chocolate; and his wife, out of complaisance, drank it often with him: the consequence was, the husband recovered, and the wife had three sons at one birth!—*Harl. MSS.*

Notwithstanding the encouragement held forth by various remedial processes and specifics, the task still remains a difficult one—and we must even now agree with what the learned Bulmer said a century ago, "All bodies may be made leane, but it is impossible to fatten where vehement heat or driness is by nature; for one may easily subtract from Nature, but to add to Nature is difficult, when Virtue does not co-operate: all other creatures, if they have sufficient and proper food, will grow fat and befranked; whereas men, although they have the best aliment exhibited to them, will not in like manner be fat, the chiefe cause whereof, as to man, is imputed to his temperament."—*Artificial Changeling*, p. 478.

On the Curative Influence of the Southern Coast of England, especially that of Hastings. By William Harwood, M.D. 8vo. pp. 326. London, 1828. Colburn.

The subjects which the present volume embraces are very important, and calculated, we conceive, to excite great interest in the mind of the general reader, to whom the work appears to have been addressed.

Climate and medicine are each so powerful in their effects on disease, that we are often led to doubt which is the most active in its operation; and situations, whose position or other local circumstances give to them an atmosphere of any peculiar character, are salubrious, or the contrary, though the causes are often concealed from our view, no great difference being found to exist in the chemical constituents of the atmosphere, wherever it has been examined. The present state of our knowledge, therefore, confines our investigations of its varied effects on the constitution, almost exclusively to the quantum of heat and moisture which it contains; but even in these investigations we are far from finding ourselves free from considerable difficulties, since, in opposition to general principles, we have to encounter the powerful influence of habit and peculiarities of constitution; an atmosphere which, *à priori*, might have been supposed equally adapted to two individuals, being often found to suit the one, and not the other. It is, nevertheless, well known, that in many of the most formidable of our diseases, in whatever constitutions they may occur, a higher and more equally natural temperature than that which is usually enjoyed during the colder months of the year, is an important desideratum; and it becomes, therefore, a question of great interest whether, all circumstances considered, it is, in the majority of instances, more safe to endeavour to obtain this equability and elevation of temperature in our own kingdom, among our own comforts and friends, or to lose the advantages of the latter, by retiring to distant parts of Europe in its pursuit. In the former case it becomes an object of no less interest to the invalid to be made acquainted with the situations which are the most likely to afford it, and what are the natural causes on which it may be supposed to depend; and on this, as on other accounts, we consider the volume before us highly calculated to prove useful.

The first part of the work relates to the temperature of coast situations, and it enumerates those circumstances which may be considered as affording to sheltered parts on our own southern coast a higher and more steady temperature during the winter season, than any other portion of our island. These effects being chiefly derived from aspect, security from the effects of piercing winds, and from the influence which the temperature of the ocean exerts on the superincumbent atmosphere; that of the surface water of the sea being greater during this period of the year, than the temperature of the surrounding air. Dr. Harwood remarks—that

"To account for this difference, it appears that the impressions of heat, which are imparted by the sun's rays to the surfaces of the waters, and of the earth, are disposed of very differently; that heat which is received on the surface of the land, being slowly admitted, and feebly communicated to the dense earth below, loses much of its intensity by freely imparting it to the circulating air; while on the contrary, such rays of light and heat as fall on the surface of the ocean, without this sudden check to their progress, penetrate the bosom of the deep to a greater or a less depth, in proportion to its transparency. Thus their limits are confined to a few fathoms from the surface, and their influence becomes gradually diffused through this upper stratum of water. From hence, probably, and from that law which ordains that the cooler portions of fluid should remain at a depth proportioned to their coolness, or that of their superior specific gravity, the important result follows—that during the winter half of the year, the temperature of the surface of the sea is greater than the mean temperature of the air, tending to produce, by the well known property which heat possesses, of equally diffusing itself through contiguous bodies, that equality in the latter, which can only be expected to be experienced, in this variable climate, in sheltered situations on the coast; situations which, like detached islands, consequently experience comparatively little of that powerful change from summer to winter, which is felt on wide extended continents. Thus I may remark, that on the 8th of January last, when the thermometer stood at 35° on the Hastings' beach, I found it rise to 40° on being introduced into the surface-water of the sea; and on the 12th of February, the coldest day of the present year, when it stood, in the same situation, at 28°.5, on immersion, it rose to 39°.

"There is, however, another very efficient cause for the more elevated temperature of the ocean; I allude to the action of its currents, and the succession of its tides, by constantly mixing and combining that surface-water, which has, in various latitudes, been differently affected by the solar beams.

"Kirwan has given to the sea, between the latitudes 50° and 51°, which may be considered that of the south coast of England, a monthly mean temperature as follows:

January . . .	42°.5	April	52°.4
February . .	44 .0	May	58 .0
March . . .	50 .0	June	61 .0

July	63°.0	October . . .	50°.0
August . . .	62 .0	November . .	46 .0
September . .	57 .0	December . .	44 .0

I am, however, induced to think, that this calculation for the winter months is rather too high; yet if we deduct 3 or 4 degrees for each month, still, the powerful influence which so vast a surface must exert in equalizing the temperature of a superincumbent atmosphere, will be necessarily admitted; and this higher temperature of the sea, I may again remark, becomes, therefore, one demonstrable cause of the mildness of a coast climate, and one which could not be expected to operate equally far in the interior of the country.

"The effects of this cause in moderating the temperature of situations differently exposed to it, are, therefore, well exemplified by comparison; thus the temperature of Dublin compared with that of Warsaw—the one immediately influenced by that of the sea, the other probably very little affected by it, though both are nearly in the same parallel of latitude, is as given in the subjoined note*".

The effects of terrestrial heat, prevailing winds, and currents, on the temperature of our southern atmosphere, are next noticed; and, in the succeeding chapter, Dr. Harwood proceeds to point out those peculiarities which justly place the Hastings' coast amongst the several situations on the southern shore, where the benefits of a mild climate, and other advantages afforded by a proximity to the sea, are most observable. To this end we are furnished with a notice of its topographical and leading geological characters, and of its

	North Lat.	Mean Temp. of coldest Month.	Do. of warmest Month.	Mean annual Temp.	Extreme range of the Mean.
* Dublin	53°.21	37°.6	60°.3	48°.4	22°.7
Warsaw	52 .14	27 .1	70 .3	48 .6	43 .2

Petersburgh again, in lat. 59°.56, from its situation, is necessarily but little influenced by the ocean, and we consequently find the range of the thermometer as follows:

Mean of coldest Month	Warmest do.	Mean annual Temp.	Extreme range of the Mean.
8°.6	65°.7	38°.8	57°.1

But Pekin, which is situated in latitude 39°.54, or 20 degrees south of Petersburgh, probably from the important influence of the extensive Asiatic regions lying to the north and west, and the comparatively trifling equalizing power it derives from the Pacific, suffers a range of temperature still more remarkable, as follows:

Mean coldest Month.	Mean warmest Month.	Mean annual Temp.	Extreme range of the Mean.
24°.8	84°.2	54°.9	59°.4

North Cape, on the other hand, although having a latitude of 71°.0, or 31°.6 further to the north, from the influence of the ocean, by which it is almost surrounded, experiences a mean temperature, in its coldest month, of only 2°.7 less than Pekin, it being 22°.1.

other more important features. Of the position of the town of Hastings, we have the following passage—

"Of all the benefits, however, which the Hastings' coast offers to the invalid, there is none more obvious than the choice of situation it affords, adapting it either for summer or winter residence; many of its habitations being placed at an elevation of two or three hundred feet above the level of the sea; consequently, as the temperature of all places is so materially diminished in proportion to their elevation, that in this country, one of 270 feet is allowed to be equal in the difference of its temperature to an entire degree of latitude: and as these more elevated parts of the town of Hastings are moreover visited, during the summer months, by the then prevailing breezes, descending from the surrounding altitudes, these higher parts of the town necessarily receive from them a very diminished temperature, at those periods when coolness is most grateful. While on the other hand, the numerous habitations which are placed on the immediate beach, below the cliffs, being most effectually sheltered, at all seasons, from the more piercing winds, are no less suitably adapted for a winter residence. From hence it follows, that a proper degree of caution should be exercised on the part of invalids, lest by an injudicious choice, between situations so remote from each other in character, a summer or winter residence here, may lose some of its more important advantages.

"The most pernicious of all our winds, are the easterly and the north easterly; the latter of which, in this valuable climate, is the only one which can be considered periodical, as it visits us with great regularity, during a greater or less portion of the months of April and May, which, from this cause, are usually trying months to delicate constitutions.

"As, unfortunately, in no country in Europe are the pernicious effects of these winds more frequently experienced than in our own, it becomes of the utmost importance to observe, that such is the peculiar position of Hastings, that a considerable portion of it is most securely sheltered, by its natural bulwarks, from the searching and penetrating agency of these hostile winds. The more genial winds, on the contrary, which can alone visit these sheltered situations, are those which blow from the south, west, and south-west. During the winter season they often prevail many days or even weeks together, sometimes very powerfully, and usually waft to our shores a very sensible increase of temperature.

"It will also, I think, be generally admitted, that few coasts are recommended by so much natural beauty as that of Hastings, as in this respect it possesses an acknowledged superiority over any other within a much greater distance from the metropolis, and is indeed almost the only situation in its vicinity, frequented by invalids, that combines great beauty of inland scenery, with that peculiar to an extensive and highly varied line of coast; which circumstance, in connexion with its extensive distribution of those sources of interest calculated to excite pleasing and cheerful impressions, is of so much importance to the acquirement of health. In this point of view, however, the Hastings' coast is generally appreciated ; its surrounding neighbourhood, consisting chiefly of fine pasture, interspersed with much woodland scenery, and affording on its numerous accessible elevations, the most extensive and interesting landscapes. These are at the same time intersected by fertile dells and romantic rocky vallies, whose shelter and peculiarity of situation afford, by the many rare species of plants they contain, a rich harvest to those who are interested in the vegetable productions of our island."

Dr. H. has here subjoined a register of the temperature of Hastings, during the four last winter months, November, December, January, and February; and from this register it will be seen, to use the author's own words,

"That the coldest month we have experienced was February, which notwithstanding, I find, gives us a mean temperature of about 44° ; a striking example of the mildness of the late winter. A register of the same month in the year 1826, taken at Hastings, gives as the mean 43°.5 ; but even this is perhaps rather higher than the coldest month generally. Baron Humboldt makes the mean of the coldest month in Edinburgh 38°.3 ; Paris 35°.1 ; and Rome 42°.1. If, therefore, either of the former could be considered as a fair average, our winter mean temperature on the southern coast would prove higher than even that of Rome."

The work now assumes a more general character. Dr. H. proceeds to point out the more particular effects of various qualities of climate on the constitution of invalids, and he arrives at the conclusion that a climate which "is least liable to variation, and which unites a moderate degree of warmth, with a certain proportion of moisture, the usual properties of a sea atmosphere, is, in the generality of our afflictions, as conducive to improvement and health, as any to which we are exposed." This doctrine is satisfactorily supported in the elucidation which is given of the more obvious effects of other qualities in the air. The advantages which a sea atmosphere thus possesses, is attributed to the little irritation it occasions to the lungs, and to its healthful influence on the exhalents of the external surface of the body, on which it tends constantly to keep up a gentle action, while it does not too rapidly deprive them of their fluids, or the body of its heat.

In the chapter on the effects of climate, the author observes,

"This influence of climate, not only on disease already existing, but in its production or removal, as also in establishing its peculiar type, has been particularly noticed at all periods; but by few has it been more fully appreciated than by the great Hippocrates, who, in his labours on epidemic disorders, has left us, amongst other treasures, his admirable and persevering example in accurately tracing its effects; and indeed, the influence of atmospheric vicissitudes on the constitution, is one of the most important subjects of enquiry connected with the duties of the physician, from the great power which they exercise on the functions of animal life.

"The primary influence of the air which surrounds us, on the body, may be considered as resulting from a twofold operation: its action on the lungs, and that on the surface of the skin, and it is through the medium of each of these operations that its beneficial, or injurious properties, are imparted to the constitution.

"The effects of the atmosphere also exhibit themselves in a striking manner in the change of season, not only in the removal, but also in the production of diseases; and the same is not less observable, as I have already noticed, under the prevalence of certain winds; all which circumstances are highly interesting and important in a curative point of view."

In noticing the effects of a cold and dry atmosphere, the author remarks,

"The diseases to which this cold and dry state of the atmosphere chiefly predisposes, are inflammatory affections; and it is more especially productive of rheumatism, coughs, catarrhal fevers, and inflammatory disorders of the lungs and chest; all which are, therefore, more frequently met with in high elevations than in the valleys. In such diseases, therefore, this kind of atmosphere becomes pernicious, not only by the cold constricting the substance, and superficial vessels of the body, but by the irritation produced by its immediate contact with the vessels of the lungs; and, by the same operation, from its power of quickening the circulation through them; since the respective velocities of any fluid are inversely as the capacities of the canals through which it is propelled.

"There is, however, another cause, which usually renders such a state of the atmosphere injurious to persons much debilitated by disease; for as their afflictions incapacitate them from taking a sufficient degree of bodily exercise, the constricting force of the external cold, becomes superior to the enfeebled power of the circulation; and that of the exhalents on the surface, and the active functions of the latter, which are so conducive to health, become checked by the torpor thus induced, whence the whole frame necessarily sympathizes in the derangement.

"An atmosphere, however, which is very cold, and moist, is far more generally prejudicial to invalids than the former; for such a state of the air, so far from imparting appreciable advantages, is constantly succeeded by a great variety of disease. I have already observed, that the prejudicial influence of a cold and dry atmosphere on a debilitated system, although arising, in some degree, from the absolute abstraction of heat by contact, is chiefly communicated through the medium of its exhalent arteries, which, by the torpor they undergo, lose much of their energy, and consequently suffer a material diminution in the quantity of their secretion.

"When, however, cold is united with great humidity, a double cause operates in the production of this same result; for as the atmosphere can only sustain a certain portion of moisture in solution, or mechanical union, the slowness of its absorption of humidity is necessarily in proportion to the quantity it has already acquired. A diminished or suppressed action, therefore, of the exhalent vessels of the skin, becomes here a still more certain result than in the former case, and more especially where bodily exercise cannot be enjoyed.

"Another circumstance which tends to render a cold damp atmosphere more prejudicial than a cold dry one, and more especially, than one that is calm, arises from its more perfect power of conducting away heat, which Count Rumford, by numerous experiments, has shown to be the case; consequently, although the thermometer indicates the temperature to be the same, still its effects on the constitution are widely different; and debilitated persons feel more chilled by such an atmosphere, at a temperature of 35°, or 36°., than when the thermometer is down at 31° or 32°.

"Such an atmosphere, then, even on those who are naturally healthy, if it long prevail, can scarcely fail to be productive of more or less derangement of the bodily functions; which derangement is generally evinced by depression of spirits, indisposition to exertion, and most commonly, a sympathetic torpor and inactivity in the digestive function in its general sense; with vitiated or impaired secretions of the liver, and other glands.

"I have before observed that such an atmosphere as combines moderate warmth, with a slight degree of moisture, is, in the generality of diseases, perhaps more conducive to improvement than any other; yet, there is not probably a more baneful combination than when great heat and moisture are conjoined, and, more especially, when the air is at rest. This is too fully exemplified to us by its pernicious effects in tropical countries; where the air, in low and marshy districts, when confined and rendered stationary by woods, and consequently united with the unhealthful influence of perpetual vegetable decomposition, is productive of the most serious consequences to all who are exposed to its influence.

"The effects of an atmosphere thus surcharged with heat and vapour, on the constitution of man, is to relax the solids, to rarefy the fluids, and to increase the secretions on the surface; which, however, from the already saturated state of the air, is not readily removed; to lessen the powers of the circulation, and to diminish the energies of the body, giving rise, by their combination, to the various awful epidemic diseases, to which, fortunately, we are little exposed in this island.

"Yet, that a certain degree of moisture is necessary to constitute a healthy and restorative atmosphere, is evident, from a consideration of the deleterious effects of one without it; for air, destitute of moisture, cannot be breathed with ease or impunity, whether it be warm or cold; when any degree of irritability exists within the lungs, such an air generally becomes insupportable, and when united with much heat, is to all, productive of great oppression and uneasiness, as is experienced by those whose occupations expose them to its influence; while, on the contrary, if humidity be added to it, such impressions are speedily removed. It is, therefore, a common practice among such as are exposed to air greatly heated, by means of stoves, to have recourse to steaming the apartments.

" From such considerations then, may be I think deduced, the superior advantages which are afforded in many diseases, by a sea atmosphere, little subject to these extremes, advantages arising, not more from the absence of the irritation they occasion to the lungs, than from its healthful influence on the exhalents of the external surface of the body; on which it tends to constantly keep up a gentle action, while it does not too rapidly deprive them of their fluids, or the body of its heat.

" The salutary and invigorating qualities, however, of sea air, which have been so long experienced and acknowledged, have led to the idea, that other causes have an important share in the production of its peculiar effects; and thus they have been assigned to a difference in its chemical composition, from that of the land, while other authors, as Dr. T. Reid, have been contented to regard it as ' the most pure and healthful we possess,' without allusion to the causes which impart its salubrity. It is well known, however, that saline particles are wafted by it to considerable distances, and M. Vogel, of Munich, has shewn in a paper, published in the *Journal de Pharmacie*, No. 11, for Nov. 1823, that the sea air of our channel, holds in chemical combination, a portion of those muriates over which it is wafted, and a less proportion of carbonic acid than that of the continent of Europe.

" One quality of vast importance to its salubrity, is, doubtless, its constant agitation; by this means it affords to us, at each inspiration, a regular supply for our demands, pure and uncontaminated by noxious effluvia."

The next thirty or forty pages relate to the effects of warm and cold sea bathing on the constitution, and the circumstances which render it inadmissible.

Dr. Harwood then proceeds to take a comprehensive view of those maladies in which he conceives the operation of coast advantages to be most important, viz., various diseases of the chest, as consumption, winter cough, and asthma, indigestion, acute and chronic rheumatism, gout, the effects of loss of blood, and of other debilitating causes, and of mercurial medicines, diseases of the liver, scrophula, and many disorders incident to children.

This part of the work, which is by far the most voluminous, is executed in a manner which reflects highly on the professional talent of the authors, and exhibits a very extensive acquaintance with the opinions of ancient and modern physicians, on the broad basis of whose established opinions, rather than on novel hypotheses, he has, we think very judiciously, rested the reputation of his work, in support of the arguments he adduces. Although it is difficult to make a selection from this useful part of the publication, a few extracts may serve to illustrate the perspicuous style in which it is written ; and with these our limits will compel us to conclude our notice of a work, which, from the importance of the subjects it embraces, and the mode of treating them, must be considered a valuable acquisition to the public.

In the Chapter on Consumption, Dr. Harwood observes, in reference to low situations,

"Low situations having been found less obnoxious to consumptive complaints, and, on the contrary, a diminished atmospheric pressure, whether depending on meteorological variations, or on a removal from a lower part of the kingdom to one of a higher level, being thought to be prejudicial in such cases, many medical authors have supposed the advantages of the coast to be materially aided by the increased weight of its atmosphere.

"It has, indeed, not only been observed, that the proportion of these diseases materially increases as we ascend from a lower to a higher elevation, but cases are recorded in which their progress has been effectually arrested by a removal thence to a lower level.

"That such results, however, are dependent entirely on the degree of pressure to which the lungs are subject, is not clearly demonstrated; since the pernicious influence communicated, on high elevations, to pulmonic diseases, may arise, in a more especial manner, from the greater vicissitudes of heat and cold to which such situations are exposed; although I conceive that there is evidence amply sufficient for believing, that the greater or less degree of pressure in the atmosphere, is productive of very important effects on organs so immediately exposed to its action, and rendered by derangement or disease, preternaturally sensible to its effects.

"This would appear more particularly apparent from the observations of Mr. Mansford, on the degree of acceleration which the pulse acquires even at a comparatively trifling elevation; for, on ascending no more than 500 feet above the level of the sea, it seems that the heart gains an accession of several pulsations per minute ; while the resistance of the vessels is diminished in an equal proportion, as is demonstrated by the well known circumstance, that even the most healthy persons, on ascending great heights, not unfrequently experience a rupture of small vessels, which are distributed on the membranes most exposed to these influences; it is therefore easy to conceive, that injurious consequences may result from slight elevations, to invalids, the delicate vessels of whose lungs have either suffered partial disorganization, or have acquired an increased degree of susceptibility to disease.

"This increased rapidity in the circulation, and in respiration, is, nevertheless, by no means proportioned exclusively to the degree of elevation to which the individual is exposed, but is not less affected by constitutional peculiarities; hence evidently the cause, why so much discrepancy exists between the accounts given by authors, of the sensations they have experienced at different altitudes.

"Thus, although M. Saussure informs us, that on ascending Mont Blanc, he suffered from these effects in a very high degree, that his strength became exhausted, and that various febrile symptoms evinced themselves; and although Sir W. Hamilton felt great difficulty in his respiration on Mount Etna, and many have been attacked by hæmoptysis and other hæmorrhages, under similar circumstances; Dr. Heberden did not complain of any very material inconvenience from visiting the Peak of Teneriffe, and the same may be observed of several others, who have reached the heights of Mont Blanc and parts of the Andes; and even of those who have experienced the rapid ascent of balloons.

"But, notwithstanding these facts, when we consider, that in low situations, and with the barometer at thirty inches, we sustain an atmospheric pressure of fifteen pounds upon every square-inch, or thirty-two thousand pounds weight on the whole surface of the body, elevations, great or small, as well as changes in the state of the atmosphere, by each of which, this external pressure is often suddenly diminished several thousand pounds, necessarily exert a powerful influence on the delicate structure, and functions of the lungs, when the health of these organs is in any way deranged.

"From various experiments, which have been at different times undertaken, with a view to determine the effects produced by a light, and a heavy atmosphere on the function of respiration, we learn, that, although animals become subject to such serious inconvenience from the partial exhaustion of the air, within the receiver of an air-pump; on the contrary, by the condensing machine, they sustain a degree of pressure equivalent to the weight of three or four atmospheres, without apparent injury; and that, after an animal has been subjected to this extreme pressure, it seems to experience the most uneasiness in returning to that of its accustomed medium.

"In the first instance, there can be little doubt that the inconvenience does not depend more on the primary abstraction of the air, and the scanty supply of oxygen which so rare a medium can afford, than on the suffocating effects produced by the distension of the blood-vessels lining the minute air-cells of the lungs, by which the latter become diminished in their capacity; the absolute volume of air received, being small, in proportion as it is rarefied.

"In proportion also to the existing weight of the atmosphere, is the quantity of oxygen, which passes into the lungs, and there appropriated to the important end it is destined to fulfil in the animal economy; and the necessity for a quick succession of inspirations is diminished in the same ratio; while, on the contrary, respiration acquires equally an increased rapidity on high hills, and in air deteriorated by frequent inhalation.

"Dr. Wells took unusual pains to ascertain the influence of situation on consumption, and he has adduced many examples in corroboration of the comparative rareness of the disease under a heavy atmosphere. He remarks, that he was led to undertake this enquiry, from having heard, so long back as the year 1779, that it was common in Flanders to remove the consumptive to the low and marshy parts of the country for their benefit. Mr. Mansford has also collected numerous instances of the greater prevalence of consumption in high, than in low situations, and Drs. Darwin, Cullen, Beddoes, and others, have consequently advised the removal of invalids liable to this disease, from the higher to lower parts of the country; and this practice is more or less common in most kingdoms where the disorder prevails.

"At Aix la Chapelle, consumptions are said to be very rare, while at Monjoye, a mountainous country, only twenty-eight miles distant, this disease carries off a large proportion of the inhabitants. It is also said that the hill of Montmorency, near Paris, which is dry, sandy, and much exposed, is very productive of consumptive disorders, and that those who visit it, with any predisposition to these complaints, almost invariably derive unfavourable effects from the change; and the same remark applies, with no less certainty, to many of the hilly parts of our own country.

"The inhabitants also of the mountainous parts of Portugal and Italy, are very subject to consumption, while those of Finland, Denmark, and Holland, are much less liable to its attacks.

"There is, consequently, amply sufficient reason for supposing, that it is partly from causes of this kind, connected with a greater degree of exposure, that this disease has been found to be less common in low situations than in any other. This circumstance has given rise to the idea that consumption and intermittent fevers, cannot exist to a great extent in the same district; which latter opinion is, nevertheless, erroneous, as they are not only found in the same situation, but even in the same individual.

"Although, therefore, there is no sufficient reason for making choice of those more marshy districts which have been selected, on the continent especially, for consumptive patients, notwithstanding their tendency to produce intermittent fevers, we ought not to disregard the benefits arising from an increased weight of the atmosphere, in those situations where the latter disease need not be encountered.

On the subject of sea air in consumption we have the following observations,—

" But although the genial properties of a sea atmosphere to constitutions generally, is, I believe, fully acknowledged; it has been lately doubted, by certain physicians, if it be as well adapted for consumptive habits; and this being an inquiry of so much importance in reference to a residence on the coast, in these cases, demands further notice.

" That a sea atmosphere is less conducive to the production of consumption, than any other, may, I think, be inferred, from many of those kingdoms which are most exposed to it, being the least subject to the disease, as is particularly the case with Denmark. In the islands of the Mediterranean also, as in Malta, Minorca, and all those of the Grecian Archipelago, we are told, by Dr. Southey, and other authors, that consumption is of very rare occurrence.

" On the Alexandrian coast, it appears to be altogether unknown; while at Aleppo, which has an intermediate latitude, but which is situated at a greater elevation, and is more distant from the sea, it is said, by several writers on the disease, to be very prevalent. It is also a well known fact, and particularly mentioned by Dr. Trotter, in his *Medicina Nautica*, that consumption very rarely occurs in seamen, except under peculiarly unfavourable circumstances.

" That this disease is, nevertheless, too often met with on our own coasts, is equally certain, though it is there less prevalent than elsewhere; and it usually arises under the combined influence of crowded towns, a bleak and exposed aspect, and great humidity of soil, or under exposure to cold winds from neighbouring mountains; it is said also to be very common in the interior of the island of Iceland, but much less frequent on its coast.

" The advantage of a sea atmosphere, in those cases where this complaint already exists, is best inferred from general experience; and the most satisfactory proof of its adaptation, may be deduced from the numerous ages in which its benefits have been sought.

" Aretæus, who lived almost 460 years before the Christian era, is, I believe, the first who recommended sailing and a sea atmosphere in consumption: and although so many centuries have rolled away since his time, and so many publications concerning this disease have appeared, we find very few individuals who dissent from his generally received opinion.

" Dr. Duncan observes, that he has not seen, in his practice, any thing which tends to confirm the idea that sea air is injurious in consumption, and he recommends a residence on the coast; and, among many others, Dr. Gilchrist has published cases in which the greatest benefit has resulted from the effects of sea air."

In speaking of winter cough, Dr. H. remarks,

" From the whole character of the disease, therefore, it is sufficiently evident that the only effectual and reasonable mode of avoiding its consequences, consists in combining, as far as possible, the effects of an equable and elevated temperature during the winter months, with those means which are best adapted to impart strength; thus enabling the constitution to contend against the influence of the disorder. The advantages of pursuing such indications are not only exhibited by daily experience, and their adoption strictly enjoined by all the best writers on the subject, as Dr. Badham, Dr. Beddoes and others, but they have, I trust, been rendered sufficiently obvious in the preceding pages, to require very little in addition to what has been already observed.

"Although material benefit, in these cases, may be constantly derived from a careful attention to the degree of heat employed within doors, yet, as Dr. Buxton has very justly remarked, 'where a natural elevation of temperature can, without difficulty, be obtained, it is infinitely preferable to an artificial one,' as the invalid, in the former case, can adopt additional means of recruiting his health and strength, and chiefly, by exercise, in a pure and moving atmosphere; which very material advantage he is necessarily precluded from enjoying, during a confinement to his room.

"Although we cannot reasonably expect the perfect union of the most favourable of all natural means, in our own kingdom; yet, as there are situations which approach this combination so much more nearly than others, their influence may always be sought by patients suffering under these complaints, with the greatest relief and benefit.

"This remark of course applies, with almost equal force, to all the more sheltered situations along our southern shore, where, from causes already enumerated, the thermometer is necessarily much less liable to variation than in any other part of England; and where, during the more severe seasons, opportunities so frequently present themselves of taking exercise under the protection of the cliffs, and within the reflected influence of the sun's beams."

The utility of exercise in gout is thus enjoined by our author.

"So various are the modes by which this important antidote exerts it beneficial influence, that it would be tedious to enumerate them; I shall, however, briefly notice a few. By the stimulus it affords to the circulation, it increases the energies of the exhalents on the surface of the body, and, consequently, the volume of insensible perspiration; and, without expense of animal strength, if properly employed; it thus diminishes the mass of the circulating fluids.

"By the sympathy existing between these exhalents and the stomach, an influence no less beneficial is communicated to this organ, tending to impart a healthful action to its function. The salutary operation of exercise on the alimentary canal also, in increasing and preserving its peristaltic motion, with the effects on the viscera derived from the action of the abdominal and other muscles, as before shown, is of the highest utility.

"Exercise not only prevents the formation of those concretions which are frequently deposited around the joints in gouty disorders, but it enables the absorbents more readily to remove such as may already exist. It tends also to preserve that motion in the limbs, which is too liable to become impaired or destroyed by contraction of the tendinous structures in this disease.

"It is, moreover, an important means of correcting that acidity, which is almost an invariable attendant on derangement of the digestive organs, but more especially in gout, in which disease even the cuticular discharge is found by the chemical changes it produces on vegetable colours, to be of an acid nature; this beneficial operation it probably effects by increasing, not only the action of the exhalents, but also the flow of bile into the alimentary canal, which bile may directly tend to neutralize the existing acid, by its alkaline properties.

"To produce these useful effects, the exercise employed should be moderate, but it should be pursued with great perseverance during the absence of the paroxysm; no day being allowed to pass, without having recourse to it, when the weather and other circumstances will permit."

In the introduction to the diseases of children, are the following observations,—

"In many of the diseases of children, I am particularly anxious to call the attention of the reader to coast advantages, from the invaluable influence they are capable of imparting; and not less, from the conviction, that such benefits are too often overlooked by parents, who, in their tender solicitude, anxiously expect, from medicine alone, that aid, which can only result from the union of the most powerful means which nature herself has afforded.

"Independently of more humane considerations, the important effects which the disorders of childhood produce on the health and well-being of society, enforce the necessity of combining every possible advantage in their favour; yet I am convinced, that to the unfortunate neglect of the more natural remedies, and the resort to such only as are more readily attainable, may be attributed the development of many of those chronic diseases, to which we so often see youth subjected; and which, in later years, are injurious, no less to the comfort, than to the prospects in life, of the individual.

"The constitution of children renders them not only more susceptible to those external circumstances, connected with peculiar locality, which are productive of disease, but imparts to them the capability of deriving more speedy and greater benefit from natural sources, than is commonly possessed in after life; and, although, in every stage of existence, a pure and wholesome atmosphere tends so materially to the acquirement and enjoyment of health, to the young, this is far more essential; whilst every other means of contributing to its insurance, is no less imperatively demanded by them.

"These observations are supported by the fact, that during youth, the various functions employed in supplying nourishment to the body, are far more active than in the after periods of life, having now a double duty to perform, not only to sustain organization already existing, but to extend and mature it; whilst, on the contrary, in succeeding years, a supply, for the waste which results from constant action and employment, is all that is demanded from the functions of nutrition.

"As food is the source whence the nourishment of the body is derived, so the air, by the change it produces on this nourishment, through the medium of the blood, adapts it to the purpose of supporting our existence; the mutual operation, therefore, of food and air, in effecting the important process of nutrition, it being impossible that either should duly perform its office, without the perfect co-operation of the other, renders it evident, that pure and wholesome qualities in the atmosphere are as essential to the healthful development of the body, during youth, as the same qualities in the food which is employed.

"The constitutional demand for food is made known to us by feelings which cannot be mistaken; and, in childhood, these are even still more powerful and frequent, than in later periods of life.

"The want of wholesome air, however, does not manifest itself on the system so unequivocally, or imperatively, no urgent sensation being produced comparable to that of hunger, and hence, the greater danger of mistaking its indications; the effects of its absence are only slowly and insidiously produced, and thus, too frequently, are overlooked, until the constitution is generally impaired, and the body equally enfeebled.

"A child so circumstanced, although it neither suffer from pain or fever, loses the ruddy appearance of health; its countenance becoming pallid, and acquiring a certain anxious expression; it often ceases to grow in proportion to its years, and a degree of listlessness, and a morbidly increased, or a diminished appetite for food prevails; until, if recourse be not had to the only rational remedy, that of a removal to a more salubrious situation, disease, in some positive form, creeps on, as the natural result of this state of privation; as may be so constantly observed in those, naturally, healthy children, which reside in crowded and confined situations."

We fully coincide with Dr. Harwood in the belief that the efficacy of sea water is too much overlooked as a medicine in the present day; we shall, therefore, subjoin also his observations on its utility in scrofula.

"These disorders are frequently combined with an habitually confined state of the bowels, which greatly favours their progress, and impedes their cure; and this condition seems, sometimes, to be connected with constitutional fulness of habit.

" In all such cases, purgative or aperient medicines become especially requisite; and in the latter capacity, sea-water, which, from the earliest ages, has been esteemed an important remedy in these complaints, may be administered with very great advantage; the intention, generally, being not so much to produce a powerful operation on the alimentary canal, as one which is gentle, and continued, by which the action of the biliary and other secreting organs, may be regularly and mildly assisted.

" There are no purgative medicines, even of the most simple kind, whose use can be better regulated than sea-water; and there are none, whose frequent employment is less likely to be followed, either by languid action, or by that state of constitutional excitement, or, I may add, inflammation, which, not unfrequently, succeeds the too frequent use of more powerful purgatives.

" Calomel, notwithstanding its advantages, is, nevertheless, a medicine of this kind; for although, in these, as in so many other complaints, it is one of the most useful and eligible that can be employed, not more from its purgative, than from its other properties, it is yet one whose use cannot, with propriety, be very frequently repeated, or long persevered in. It is therefore an important consideration, that the peculiar properties of sea-water are such, as to allow of its more constant exhibition, either alone, or in alternation with calomel, or with other medicines, whose peculiar properties may indicate the utility of their employment."

We cannot, indeed, conclude the analysis of the work before us, without expressing our approbation of the clear, sensible, and temperate manner, in which it has been executed, and our high sense of the many valuable observations it contains.

On Ornamental Aviaries.

It is not a little surprising, that while every villa, and almost every cottage, has its greenhouse, or conservatory, comparatively few, even of our most splendid mansions, can boast of a well kept aviary; and yet, to the lover of natural history, or even to the mere admirer of animated nature, the latter is sure to afford constant amusement and gratification; and, in point of expense, its first cost is not so great as a conservatory, nor is the keeping it up more expensive; for while the one requires the constant attention of an experienced gardener, the wants of the other can be supplied by an old woman, or a boy; and the expense of seed will not amount to more than the difference in wages. Some humane minds have objected to an aviary, from a dislike to deprive the inmates of their liberty; this is, however, an objection more specious than real. Those who have directed much attention to the habits of animals, will confirm me in the assertion, that even birds, who have the power of locomotion to a greater degree than other animals, make excursions principally for the purpose of obtaining food; and I have observed the same pair of bullfinches on the same hedge, and started them within a few paces of the same spot, day after day, for many weeks; and that in winter, when it could not be from the attraction of their nest. I found a pair of hedge-sparrows had taken leave to enter my aviary, and, for some time, was unable to discover how they had gained admittance; but they had found a defect near the roof, through which they could enter, and they took advantage of it. They occasionally went out, but never to any distance; they naturalized themselves to the place most completely, and had a nest of eggs in the aviary. I believe if animals have plenty of food, and sufficient room for exercise, they require and desire no more. If, therefore, the proprietor of an aviary consults the habits of the birds he places there, and supplies them with appropriate food, he affords them ample compensation for the gratification they afford him, when hearing their lively song, and observing their sprightly movements; of course, there is great difference between the freedom of an aviary and solitary confinement in a cage. Few improvements have been made in the construction of aviaries, because few have expended property or pains in erecting them. The site of an aviary should be facing the south or west, and sheltered from the north and east. It should be principally open to the air, and should be constructed of wire almost entirely. But there is no objection, indeed it is rather desirable, that some parts of it should be covered with a roof affording shelter in winter, and shade in summer. A constant supply of fresh, and, if possible, running, water is exceedingly necessary for the health and comfort of the little inmates. The aviary should be well covered all over with turf, excepting the walks, which should be gravel. The perches should be most of them over the walks, for the facility of cleaning; and ample cover should be afforded by evergreens, such as the phyllerea, ilex, holly, laurel, Portugal laurel, laurustinus, yew, box, cypress, &c. If deciduous trees be planted, the leaves will soon be picked off, and the buds destroyed. If it should be intended to include foreign as well as native birds in the aviary, it should be so constructed as to be capable of being heated in the winter; and the best mode of doing this would be to have the aviary fronted with glass four or five feet from the wires, and the space between ornamented with plants, both because they would afford the best test of a proper temperature being maintained, and also this would combine the sources of gratification and amusement. Should only a few foreign birds be admitted, separate apartments may be so constructed, as to communicate with the aviary in the summer, and to be shut up and warmed with a flue in the winter. This is necessary even for the canary-bird, which is too delicate to bear our climate without suffering; excepting those green birds, which appear to have been less affected by domestication. In the gallinacii, nothing is so likely to preserve them from that fatal malady, the roup, as constant supply of fresh water. This disease has been shown, by Mr. G. Montagu, to be owing to a species of fasciola, which fixes to some part of the trachea, and which multiplies to such a degree as to cause death, either from suffocation, or, as seems to me, by inducing inflammation of the membrane of the trachea. That this theory of the disease is correct, is confirmed by my own observation; and I am, moreover, convinced, that the disease is generally induced by inattention to giving the young birds a supply of fresh water. I have little doubt, that the fasciola producing the disease is generated in stagnant or putrid water, and have found the best mode of preventing the disease was to have the vessels cleaned by scowering, or throwing in hot lime, every two or three days. Mr. Montagu recommends soaking the food in wine; I cannot say I have found this remedy answer my expectations. I know a farm-yard where the housewife is a most careful attentive woman, but she complains she can keep none of her chickens, on account of their being attacked with the roup. On examining the yard, I found the poultry were in the habit of drinking at a horse-pool, which received the drainings of the pigsties and ox-stalls, and this appeared sufficient to account for the prevalence of the disease. I found my young brood of pheasants and partridges always did well till they were shut up in my poultry-yard, or aviary; and as soon as that was the case they began to suffer from the roup.

But to return to the aviary—birds are either carnivorous, granivorous, or insectivorous *. The first class are not fitted for the aviary—the second always do well—the third require great care to keep them in health. I have found that the best food for constant supply is buck wheat, hemp, rape and canary seeds, and a mixture of barley meal and grated liver. The latter is particularly necessary for the lark tribe and the sylvias, and also for the merulidæ. Snails, slugs, and worms, should be frequently supplied also; and green food, such as groundsel, chickweed, lettuce, and water-cresses; also the seed of plantain, dock, and thistles occasionally. The seed should be provided in boxes, so constructed that a little only should fall down at a time. There should be several boxes, as the stronger birds are apt to tyrannise over the weaker, and keep them from their food; and each of the boxes should have several divisions of wire, or wood.

The enemies of the aviary are numerous: of these the cat is the most formidable; they will sit on the roof of the aviary for hours in a moonlight night, alarm the little inmates, and then pounce on them as they fly towards the light, and against the wire. I have sometimes found three or four birds in a morning killed by the cats, sometimes the head torn off, but often entire,

* These may be subdivided into those which live almost entirely on the wing, and whose food consists of those insects which they meet with flying, or those which live on the larvæ or eggs of insects which they meet with on the ground. The former are quite inapplicable for the aviary; for, independent of their being migratory, it would be impossible to supply appropriate food—the food of the latter may be afforded or imitated.

but pierced with the claws, and killed with terror. The wires of the aviary should not be more than half an inch apart, both to secure from this enemy, and also from that formidable one the stoat, or common weazel. The aviary also suffers much from rats and mice, the latter especially multiply exceedingly, from the abundant food they obtain; to prevent this as much as possible, the food should always be supplied in the boxes I have described, or on a table on a single pedestal. I had a circular tin-case, with holes at intervals all round; this was supplied by a large central box, from which the seed dropped down into all the partitions, so as to keep a constant moderate supply; this, supported on a pedestal, is not inelegant.

I will now shortly enumerate the birds most fitted for an aviary: of course, none of the genus falco or the genus strix can be inmates of the aviary; they are very pretty appendages to it, however, if they are braced and chained to a stand. I have kept the kestrel and the martin in this manner, and they are beautiful specimens of their kind. The genus lanius is also inadmissible; of the picæ, the jay is a pretty ornament, but too mischievous to be at large in the aviary; the other species are not sufficiently ornamental to compensate for their mischievous propensities. I have never possessed the chatterer, ampelis garrulus—it would, doubtless, be a great ornament to the aviary. There is a bird, which I have kept only for a short time, but which, I doubt not, a little pains and perseverance would preserve, and a beautiful ornament it would be to the aviary, viz., the kingfisher. There have been instances of this bird being rendered very tame, and if the stream which supplies the aviary were stocked with minnows and gudgeons he would be sure to thrive. The creeper is a very gentle little bird, and easily kept. Of the passeres, the starling is a handsome and very amusing bird; but his habits are so bad, and he is so destructive to smaller birds, that he cannot be admitted as an inmate of the aviary, except in a distinct apartment. The genus turdus is the great pride of the aviary: they are easily kept, soon become domesticated and familiar, breed most freely, and repay you by their song nine months in the year. I have had many generations born in my aviary: they require an abundance of snails and worms during the breeding season. The most beautiful are the

Turdus Viscivorus	Missel Thrush
——— Musicus	Song Thrush
——— Merula	Blackbird
——— Torquatus	Ring Ouzel — the three

first are the only ones I possessed.

Of the genus loxia, I have kept only the

Loxia Curvirostra	Cross Beak
—— Pyrrhula	Bullfinch
—— Chloris	Greenfinch;

but they may all be kept with great facility. The loxia curvirostra, the cross beak, is very ornamental and amusing; but their beak is an instrument of great mischief, and they commit much havoc by barking the trees, and destroying the woodwork of the aviary. The other members of the genus are easily kept, and the loxia pyrrhula, or common bullfinch, is one of the most beautiful of the inmates of the aviary. It is scarcely possible, however, to keep more than one pair, unless the aviary be very large, as they fight during the season of love with most unrelenting pertinacity. The genus emberiza are easily kept; but I have only had one species, viz., emberiza citrinella, or yellow hammer, a very pretty bird, although its note is rather unpleasant. The genus fringilla is the chief ornament of the aviary: of these I have kept specimens of the following, without any difficulty, for a very long time, viz.,

Fringilla Domestica	House Sparrow
———— Montana	Tree Sparrow
———— Cœlebs	Chaffinch
———— Montifringilla	Mountain Finch
———— Carduelis	Goldfinch
———— Spinus	Aberdivine
———— Cannabina	Linnet
———— Linaria	Redpole
———— Montium	Twite
———— Canaria	Common Canary Bird.

These birds all breed in the aviary, and by their familiarity and their lovely notes amply repay us for our trouble. They are too, almost all of them, gregarious, and many specimens of each kind may be kept in the same aviary; they are all of them granivorous, and only require a good supply of greens daily, to keep in perfect health. The genus alauda are kept with more difficulty, but they thrive well on barley meal and grated liver. They often suffer from the mice, who destroy their eggs, and sometimes even themselves, when roosting on the ground; of these I have only kept,

Alauda Arborea	Woodlark
——— Arvensis	Skylark
——— Pratensis	Titlark.

The genus Motacilla is easily kept, and the motacilla flava is one of the most elegant birds that are found in this island. It is a graceful and familiar little creature, and its colour very beautiful and ornamental. The genus Sylvia is kept in the common aviary with great difficulty; the greater number of them are migratory, and their food principally insects. They require a regulated temperature in winter, and insect food, which at that season is very difficult to obtain. The perseverance of Mr. Sweet has been rewarded by the domestication of many of them, and the beauty of their song, and the elegance of their movements, is sufficient incitement to make the attempt. They will, however, almost all of them, require a separate apartment. The following will succeed very well in the common aviary, and I have kept them very successfully on the common food of the aviary.

Sylvia Modularis	Hedge Warbler
—— Rubecula	Redbreast
—— Troglodytes	Common Wren
—— Regulus	Golden-crested Wren.

These are very pretty inmates for the aviary. It will be scarcely possible to keep more than one pair of the redbreasts, as he is quite master of the place, beating birds twice his size, and prying into every thing which is placed there; they are most prolific; but it is almost impossible to be sure of getting a pair, as there is but the slightest difference between the cock and the hen.

Of the genus Parus, the parus major, or great titmouse.—Parus cœruleus, or blue titmouse, are easily kept, and are very pretty birds. I have myself kept none others of the family.

Of the columba, the columba turtra, turtle-dove, and several foreign inmates, may be kept perfectly well, and they are exceedingly prolific, gentle, and easily domesticated. There is a white variety of the common Barbary dove, (which is common all over the south of Europe,) which is a very elegant and beautiful variety. The gallinacii are, perhaps, better kept in an open poultry-yard than in the aviary, except the gold pheasant, which is a proper appendage to it, and is tolerably hardy; but the silver and common pheasants, pinioned by having the last joint of the wing cut off when they are young, do better with a larger range than the aviary affords. The genus perdix, however, are very pretty birds for the aviary; as the

Perdix Cinerea	Common Partridge
——— Rufa	Redlegged Partridge
——— Coturnix	or common Quail,

but the latter are too delicate for this climate in general. The only other bird I shall mention, as suitable for the aviary, is the tringa vanellus, or common peewit, which feeds very well on bread crumbs, worms, and other insects. I have thus enumerated most of the varieties of our British birds, which are appropriate for the aviary. The continent of Europe and North America afford many beautiful specimens which would live in our climate; and if the taste for ornamenting the aviary were pursued with half as much zeal as for making acquisitions for the green-house, the varieties would, indeed, be great, and the pains well repaid; and if the aviary were constructed with only moderate regard to preserving equal temperature, such as is sufficient for the ericæ, proteæ, acaciæ, &c. &c., our aviaries might contain the most splendid specimens of the merulæ, tangaræ, cotingæ, &c., a far more interesting mode of preserving them than as stuffed specimens. It is to be hoped that the aviaries of the Zoological Society will be models to show how many living beauties may be naturalized in this country. But, if I might be allowed to make the observation, I think the design which is shown for the aviary in the Regent's Park, although exceedingly elegant in appearance, yet will not succeed for keeping the birds. It is too exposed and open to the north and east to afford any moderate degree of shelter to the birds during winter, and I fear many will thus perish.

My friend, Mr. Maliphant, architect, in Blenheim-street, has been so kind as to embody my ideas in a design for an aviary, which would, I think, combine all the advantages of shelter and ornament. The front and roof are proposed to be of glass. The centre-part and two wings may be either covered in with glass or patent zinc. A walk may be made within the glass, and outside the wire, as it is proposed to leave a space of five feet between the glass in front and the wire, which would afford room for a paved walk, and a bed for exotic plants; thus combining the beauties of the aviary and the conservatory. It would be advisable to have the wire at the roof within two or three inches of the glass. The same plan would do exceedingly well for a common aviary, in which case wire would be substituted for the glass; and the rooms at each end would be useful to contain delicate birds during the winter months.

A.

Observations on the Force of our Ships of War.

During the last few years, naval matters have been gradually coming before the public eye. Open and free discussion has, at last, made its appearance, and, as usual, has produced vast benefit in dispelling the mystery and darkness in which the construction, equipment, and economy of our naval force were formerly concealed.

It is a singular fact, that a Frenchman* should have been the first to have deemed our naval establishment worthy of being described and descanted on; but it is no less true, that we owe to a foreigner almost every information we possess on the subject, as well as the attention he has excited by

* Baron Charles Dupin—Force Navale de la Grande Bretagne.

his writings. That attention has been wonderfully increased; and we now see this once neglected but most important national topic introduced in almost every scientific or literary publication of eminence of the day; and even periodical works expressly devoted to its consideration, have been published. It is true, indeed, that much erroneous argument, and, consequently, many false conclusions have proceeded from those who are unacquainted with naval philosophy; but even these have been of service, for the detection and exposure of error is as important as the developement of truth.

The accession of His Royal Highness the Duke of Clarence to the office of Lord High Admiral has given a fresh impulse to the desire of improvement; and it is said to be in contemplation to make some very important innovations in the armament of our floating citadels; it is, therefore, solely with a wish to aid the intentions of His Royal Highness, that we intend briefly to examine the principles to which such alterations should be referred, and to point out how far we are justified in proposing an adoption of them.

The *first element* to be considered, in a ship of war, is, necessarily, its *force;* and this consists in its artillery; but there are two ways in which this force can be modified, viz.—

1st. By the *quantity* of guns mounted.

2dly. By the *quality* or *calibre* of the ordnance.

If we merely estimated the force of a ship of war by the *number* of its guns, we might be led into very great error. The famous Harry Grace de Dieu, built in 1515, was mounted with 122 pieces of ordnance *, a number exceeding even that with which our present first rates are established, but not more than thirteen of these were of the calibre of nine pounds, and upwards.

The calibre of a piece of artillery gives us a definite idea of its *individual* power; but this term alone does not furnish us with a correct notion of a vessel's force beyond that of carrying "heavy or light metal." It becomes necessary, therefore, that both the number and calibre of guns must be expressed, to give us a precise description of the fighting power of a ship.

* Charnock's Hist. of Marine Arch. vol. ii. p. 44.

In the earlier periods of the application of ordnance to naval warfare, it was usual to carry, not only various calibres on board the same ship, but even to have two or three different natures on the same deck. Much inconvenience and confusion must have unavoidably arisen on this account; but we find that it was not until the latter part of the reign of Charles I. that any attempts were made to remedy the serious evils inherent in such a disregard of system. The first regular establishment of guns for the various classes of ships of the royal navy is, we believe, that given by Derrick, in his Memoirs, and was made in the year 1677. We find in it an *uniformity* of calibre established for each respective deck; for instance, the lower deck of a three-decked ship of 100 guns was armed entirely with 42 pounders, or (as they were then called) cannon of 7 *; the middle deck with 18-pounders, or culverins; and the upper deck with 6-pounders, or sakers.

As may be naturally supposed, repeated attempts have been made to increase the force of our ships of war; not only by increasing the number, but also by increasing the calibre, of their ordnance; but the former alternative has, from the difficulties and expense attending it, (by requiring much larger ships,) been comparatively little resorted to; indeed, we find that our present largest first rates carry only *twenty* guns more than the first rates in 1677; and our largest two-decked ships of the line mount only *fourteen* more than the two-deckers of the same period. Hence it has been principally in the general increase of *calibre* of naval ordnance that the present superior force of our floating batteries consists.

In treating of cannon with relation to the qualities and capabilities required in them, as forming the armament of a ship, it will appear, on a due consideration, that there are two principal objects which should be attended to in the construction of a piece of sea-service ordnance, viz., facility of its service in time of action, and the influence which the guns possess over the sailing qualities of the ship: both these desiderata are dependent on the *weight* of the gun. This element not only governs the celerity of its service, but also has a great

* That is, cannon of seven inches, or whose bore was seven inches in diameter.

influence on the displacement and stability of the vessel; it is chiefly on the latter account that in ships of two or more decks, it becomes necessary to diminish successively the weights; and, according to the common constitution of artillery, the calibres in the upper tiers.

Since, therefore, it appears that the guns of the greatest weight should be placed on the lowest battery, it will be important to see how far experience has determined this *maximum* *, which, of course, must, above all other considerations, depend on the power and ease of management in time of action. Manual exertion is confined within comparatively narrow limits; but it is possible to construct a vessel that should carry much heavier ordnance than the heaviest now used. A mass of about 80 cwt. seems, from experience, to be the *maximum* of weight that can be allowed without losing the requisite celerity in the service of the guns. This we ascertain, not from our own practice, but from that of a foreign nation. The weight of a French 36-pounder and carriage is 83 cwt. †; that of our highest calibre of sea service long-gun and its carriage is only 64 cwt. ‡: we see, therefore, that the French retain, as manageable, a weight *exceeding* ours by 18¾ cwt., or nearly *one ton.*

Admitting, however, that our 32-pounder is equivalent in force to the French 36-pounder, and is otherwise as good a gun, it would have the advantage in a long action, since it could be served with much less fatigue and with fewer hands. This is certainly an important consideration; but the same recommendation, in a considerable degree, will be found to have been attached to a higher calibre of English ordnance now discarded from the naval service, excepting in the form of car-

* This quantity is much greater in the sea service, than in the land service, on account of the loco-motion which naval ordnance possesses in common with the ship on board which it is mounted. Garrison ordnance, however, from being stationary, is as heavy as that of the sea service; and, indeed, is generally, in the British service, supplied from it.

† The weight of the French 36-pounder used on the lower decks of their ships of the line, is nearly 71½ cwt., and the carriage weighs 11½ cwt., making together 83 cwt.

‡ The English 32-pounder gun of 9½ feet long, weighs 56 cwt., and its carriage 8¼ cwt., making together 64¼ cwt.

ronades. The 42-pounder gun, until the year 1793, formed the armament of the lower decks of all our first rates, and weighed 63 or 65 cwt.; so that even this heaviest of English naval ordnance, which was dispensed with on account of its *unmanageable* weight, was at least *one-third* of a *ton lighter* than the truly powerful gun which still forms the principal arm of the line-of-battle ships of the French navy, and the weight of which has very recently been adopted by an author* of great merit, as that which is sufficiently manageable.

We now proceed to make a few remarks on the calibres of ordnance used on board ship. The celebrated Robins, in a tract, first printed in 1747, entitled "A Proposal for increasing the Strength of the British Navy," fully points out the great augmentation of force to be derived from using higher calibres; and Muller, in his Treatise of Artillery, in 1768, makes a similar proposition. The fact is, that the larger calibres possess the great advantage of making greater breaches in an enemy's hull; their superiority of mass produces a greater momentum with a given velocity, and their ranges are greater even with a less proportional weight of powder.

The calibres of our guns should never be much less than those used on board the ships of other nations; they may be as much greater as possible. Experience has shown how much our 18-pounder gun, the common armament of our most numerous class of frigates, compromised their safety, when opposed to the 24-pounder of the American frigates, in the last war.

The Portuguese still use their 48-pounder, equivalent to 45.79 lbs. English calibre, for their heaviest ship gun, and the Dutch their 32-pounder, equivalent to the calibre of 34.54 lbs. avoirdupois: the Russians, Swedes, and Spaniards, use their several 36-pounders, respectively, equal to the calibres of 31.95, 33.73, and 34.42 lbs. avoirdupois. All these guns, except the Russian, are, therefore, superior to our heaviest calibre, the 32-pounder. But this last gun, which forms the arm of the lower decks of all our ships of the line, stands in a greater ratio of inferiority with the French 36-pounder, than even the

* M. Paixhans, Nouvelle Force Maritime, 1820.

18-pounder to the 24-pounder. The French 36-pound ball is equivalent to the calibre of 38.86 lbs. avoirdupois, being nearly *seven* pounds heavier than our 32-pound shot. This superiority of the French naval ordnance has been often felt by naval men. Captain Brenton, in his Naval History, repeatedly mentions the 36-pounder as giving the French navy a decided advantage. This advantage they have always retained in their two-decked ships of the line; but until 1793 our three-deckers, with their 42-pounders on the lower deck, possessed the superiority over the force of the same class of ships of the French marine. The 42-pounder gun rejected in 1793, though 7 or 8 cwt. heavier than the present 32-pounder, was lighter than the French 36-pounder, by 6 or 8 cwt.; and, before it was thrown out of the service, some means should have been devised to have rendered its management, at least, equally easy as the French 36-pounder gun, which, as we have already said, has been unequivocally declared to be sufficiently convenient in its service.

It may here be observed, that the Americans have adopted the calibre of 42 pounds for the guns of the lower decks of their three-decked ships; and we should not omit to mention that, with the present service charge, the point blank range of a 42-pounder is fifty yards more than that of the 32-pounder; and that at the small elevation of 1° the range of the former exceeds that of the latter by nearly 300 yards *.

As it has been most decidedly proved by Hutton's valuable experiments on military projectiles, that the weight of a gun of given length has no influence on the velocity of the ball, we have only to refer this element of a piece of naval ordnance, besides the power of management, to the attainment of a steady recoil; to the necessary length; and, lastly, to the required thickness of metal.

We suspect that much needless weight had been added to the 42-pounder, for we find that in 1768, that nature of ordnance weighed only 55¼ cwt. † Mr. Muller, whose work on artillery gives us this information, does not mention any objection

* Vide Naval Gunnery, by Sir H. Douglas.

† Muller's Treatise of Artillery.

to this weight, but that he thought it too great, and proposed a 42-pounder of 52½ cwt. We may, therefore, safely assume that it was a serviceable gun in every respect; and as it was ten feet long, a diminution of six inches in length might either have reduced this weight, or, retaining the same weight, have obtained a greater thickness of metal at the breech.

It appears, from the best authorities, that the weight of the 42-pounder gun has varied from 55¼ cwt. to 65 cwt., and the length from 9 to 10 feet. The greatest length now allowed for sea-service guns, is 9½ feet. The brass 9½ feet 42-pounders of the famous Royal George, which was, we believe, the last ship in our service armed with brass guns, weighed rather more than 61½ cwt. each. From these facts we may infer, that sufficient steadiness of recoil can be obtained when the ratio of the weight of the gun to the shot is greater than that of 147 to 1.

There cannot, therefore, be a doubt but that the 42-pounder might be again introduced into the catalogue of sea-service ordnance, with a weight of 61 cwt., or *half a ton lighter* than the present French 36-pounder gun: now, if we dispensed with the useless mass of metal about the muzzle, which only detracts from the elevation, depression and training of the gun, and renders its service *doubly delicate**, and disposed it about the breech, so as to give a more conical form to the piece, there would result the advantages of a greater projection of muzzle beyond the port, and a much stronger gun. This conical form has been fully proved to be the proper one for sea-service, as exemplified in the Congreve 24-pounder, which forms the present arm of the upper decks of our first rates.

* All gunnery operations at sea ought to be reduced, if possible, to an unity of purpose, and the mind of the gunner should not be distracted by two intentions, viz., hitting his mark, and preventing the *swell* of the muzzle from breaking away the side of the port in the recoil, and doing other mischief, on account of this contact taking place. The muzzle swell should be got rid of, and the chace carried right through, excepting at the quarters, where the swell might be left, so as to afford a sufficient hold for the muzzle lashing, when the gun is housed. Such a piece of ordnance *could not wood* or strike the side of the port in its recoil. The present ordnance could, at a small expense, be altered, to effect such a desirable purpose. It is true that the quarter sights would thus be abandoned, but at sea they have very rarely been of any service, and, with the present sights, are rendered wholly unnecessary in the operation of pointing guns on board ship.

Admitting, however, that the size of the shot of 42 pounds weight renders it too difficult for a man to manage with ease, when quick firing is required, and this appears to us to be the only feasible objection to it, we cannot imagine that the same objection can possibly be urged against the ball of 38 or 36 pounds weight. The diameter of the 42 pound ball is 6.684 inches; the diameter of the 38 pound shot 6.465, and that of the 36 pound bullet 6.35 inches.

We understand that it is contemplated to put 32-pounder guns of the increased weight of 63 cwt., in imitation of the Americans, on the lower decks of our ships of war of two and three decks, removing the present 32-pounders of 56 cwt. to the deck above; and mounting, in three-decked ships, guns of the same calibre on the upper deck, but of only 49½ cwt. Now, if this measure be adopted, for the purpose of obtaining an *unity of calibre*, we say that it will be done with much more weight than is necessary, and that, by again introducing a gun of 63 cwt. into the naval service, an opportunity offers of raising our *maximum* calibre to very nearly that of the French, by introducing the calibre of 38 pounds, with a gun 9½ feet long.

Although in establishing an unity of calibre with the maximum calibre as its base, we certainly increase the weight of metal thrown at a broadside; yet, before we decidedly pronounce it to be an increase of power, we must first inquire at what distance this modified force is effective. The fact is, that such a step requires some consideration, and should be referred to the principle, that none of the superior calibres of ordnance should be inferior, in point of range, to those it is intended to supersede. For instance, the guns of the middle deck of a three-decked ship should possess, with the proposed higher calibre, a range not inferior to the 24-pounders they are substituted for. We shall now proceed to examine how far this is practicable with a calibre of 38 pounds.

It has been already mentioned that the weight of a piece of ordnance, considered merely as a projectile instrument, is only referable to its influence on the recoil, and the strength required throughout the length of the bore to resist the action of the charge with perfect safety. Now, if we refer the weight of a 38 pound ball to 63 cwt., we should have a ratio of 1 to

186.31, which all experience has proved to produce a very steady recoil: in fact, the ratio of the ball of 32 pounds to the weight of the present gun of that nature, is less by only six times the weight of the shot.

But a question now arises of great importance. Are we to lay aside all the ordnance of the calibre of 32 pounds, merely to introduce that of 38 pounds, and thus entail a great loss on the country? We answer, no! The present 32-pounder gun of 9½ feet long, and 56 cwt., if *rebored* or *reemed* out to the calibre of 38 pounds, would still weigh 55.12 cwt., or 162.55 times its new shot, which is very nearly the ratio between the weight of the brass 42-pounder of the Royal George, and its shot. The 32-pounder gun of 8½ feet and 49½ cwt., might similarly be adapted to the calibre of 38 pounds, with a loss of only 81 lbs. of metal, which would reduce its weight to 142.28 times that of the ball, or nearly the ratio subsisting between the old iron 42-pounder of 55 cwt., and its shot, and which used to be fired with charges of half, two-thirds, or even three-fourths the weight of the shot, instead of the present lower charge of one-third the weight of the shot. This modification would cause but a very slight decrease of thickness of metal in these two guns; for the diameter of a 38 pound ball is 6.465 inches, and if we add to this the windage now adopted for all calibres of heavy ordnance above 12-pounders, viz., .15 of an inch, we have 6.615 for the diameter of the bore of a 38-pounder gun. Now the diameter of a 32-pounder shot is 6.105 inches, and with the *common* windage to which these guns are constructed, we shall have 6.41 inches for the calibre of the present 32-pounder gun: hence this gun can be reemed out for a 38-pounder, with an increase of calibre of .205 of an inch, or a *decrease* in *thickness* of metal of .102 of an inch; a quantity too trivial to excite the slightest apprehension of bursting*.

* In reeming out the 32-pounder for our purpose, the part of the bore about the charge might be left as it is, and thus preserve the original thickness of metal. The enlarged part of the bore being carried into it in the surface of a hollow conical frustum, so that the shot being forced into it would always have its centre in the axis of the bore,—an advantage too obvious to be insisted upon. This idea we owe to M. Gomer, a French artillery officer of distinction, about the middle of the last century; and M. Paixhans has availed himself of it in a similar manner, as we may see in his "Nouvelle Force Maritime."

From Hutton's experiments, we may conclude that, in different calibres of guns having the same length and windage, the ranges at the same elevation are nearly as the fourth roots of the charges directly, and inversely as the fourth roots of the weight of the shot; hence the range of a 38-pounder of 9½ feet long, with 10 lbs. of powder, is to the range of the 24-pounder of the same length, fired with 8 lbs., as 3.93 to 4.17, a pretty near approximation to a ratio of equality; but if we consider that the proposed 38-pounder has nearly one half less windage than that hitherto allowed to the 24-pounder, we may safely reckon on the usual full range of a 24-pounder being given to the 38 pound balls projected from the middle and upper tiers, with the charge proposed. Indeed, from some recent experiments * made on the windage of guns, such a result cannot be doubted.

If, therefore, it should, upon actual trial, be found inconvenient to employ the full service charge of one-third the weight of the shot for these converted pieces, and thereby have an *unity of range*, as well as calibre, to our proposed armament †, we may still secure to ourselves, with a reduced charge, all the advantages that arise from projecting a shot of 38 pounds to the same distance as we do at present those of 24lbs. and 18lbs. from the same decks. The same remarks apply in degree to the use of double-shotted discharges: and we may, in adverting to the use of two shot, remark, that the great uncertainty of hitting the object with double-shotted guns, excepting when *very close*, should always prevent a ship from throwing away its fire in double shots, at long ranges, with the full service charge. This is a fact, we believe, so fully established by careful experiment, and so generally admitted by those most conversant in practical gunnery, that we need not insist on it any further than by saying that it is better to

* Vide Sir H. Douglas's Naval Gunnery; wherein it appears that the range of a 12-pounder, whose windage was .1 of an inch, instead of .22 of an inch, (the common windage,) was rather greater with 1-6th less powder than the usual charge.

† This would be rigorously correct for two of our guns, as they are of the same length, and very nearly so for the three, as the gun of 49½ cwt. is only twelve inches shorter; and the ranges are nearly as the *fifth* roots of the lengths, with the same charges and calibres.

fire only *one* shot with *certainty,* than *two* with the chances of throwing away both.

We contend, therefore, that an experiment with the gun proposed of the calibre of 38, and the present 32-pounders bored out to the same calibre, as also the 32-pounder carronade similarly converted, should be made to ascertain whether it be not possible to introduce the calibre of 38 pounds into our naval service as the *sole* arm for all ships of the line and heavy frigates; and that such an opportunity as the present should not be allowed to escape us without making the attempt.

M. Paixhans, in his "Nouvelle Force Maritime," proposes the calibre of 36 pounds* as the only one for naval service; but, in adopting his suggestion, we are necessitated, in the smaller vessels of war, to make a sacrifice of range by employing lighter guns than we propose: thus, in the 64 gun frigate he would be obliged to employ the 36-pounder of the same weight as the 24-pounder it would supersede, but of less power of range, because projecting a heavier shot with only the same charge as the latter gun. This deficiency would become still more apparent in the next class of frigates, where he proposes to employ a 36-pounder of the same weight as the 18-pounder, which is the present arm; and it becomes a matter of grave consideration whether, in such cases, it would be prudent to give up power of range for the increase of calibre, as derived from his system.

In advancing our proposition, we do not pretend to originallity, as it is only a modification of that of M. Paixhans; but we possess a singular advantage over him, in point of *economy,* inasmuch as with only *one new* nature of ordnance we can obtain all the advantages to be derived from an unity of calibre, with a corresponding increase of force over our present sea-service ordnance, by taking the *maximum* calibre at a higher point; whereas this author is obliged to go to the great expense of introducing *two new* descriptions of ordnance, besides throwing aside those of 24 and 18 already in use. It may also

* This author adopts the present French 36-pounder, and two others; one of which is 142 times the weight of the shot, and the other 116 times.

be observed, that the 38-pounder we propose to obtain from the 9½ feet and 8½ feet 32-pounder, are much superior in relative weight to the two lighter and shorter 36-pounders of M Paixhans, and, therefore, capable of bearing larger charges, and producing greater ranges than his guns.

The additional weight accruing from this armament, in a ship of 120 guns, would be nearly 91 tons *, which would cause her to sink about 3½ inches more than with the common ordnance equipment.

Thus we have proposed a scale upon which our naval ordnance may obtain the *maximum* of simplicity, together with an *increase of force*, which, if we adopt the calibre of 38 pounds, will amount, in a ship of 120 guns, to nearly half as much metal again being projected at each broadside than is thrown by the usual armament †.

M. Paixhans has proposed, with great plausibility, the introduction of *shells* into naval artillery, and his system has been partially experimented on with much apparent success ‡; but although his proposition opens the door for a fresh modification of sea-service ordnance, we imagine that there are certain obstacles arising from the peculiar nature of naval warfare, which will render the adoption of it, to its full extent, very difficult. The great feature in it, besides that of explosion, is the property of giving hollow shot much larger calibres than our largest solid shot, with *less* weight than the latter, possess. For instance, the hollow shot of the calibre of 80 pounds, or 8 inches French in diameter, weighs, when filled, only 55 pounds French, or about 60 lbs. avoirdupois; the hollow shot of the calibre of 150 pounds, or 10 inches French in diameter, weighs only 110 pounds French, when filled, or nearly, or 119 lbs. avoirdupois.

It must, however, be recollected, that in augmenting the calibre to, perhaps, 10 inches, in the manner proposed by M. Paixhans, there arises a disadvantage in the loss of time; for

* This weight is estimated at the war proportion of ammunition.

† A broadside of sixty 38-pounders will project 2280 pounds of metal; a broadside consisting of sixteen 32's, thirty-four 24's, six 32 pounder carronades, and four 12-pounders, only amounts to 1568 pounds of metal.

‡ See No. 2 of the Naval and Military Magazine, for June, 1827.

the size of the projectile and its weight, become again so considerable *, that it will require *two* men to carry it; and if the sea be agitated, this inconvenience will be still more felt †. It is on this account, chiefly, that it requires to be decided by an actual experiment at *sea*, whether there would result any real advantage from having an entire battery, say of 68-pounders even, over one of 38-pounders. The former, we apprehend, could only fire, from their relative lightness ‡, *single* shot; whilst the latter would pour in double shot, and more than *twice* the weight of metal at each discharge, besides making two large breaches, instead of one. We do not say, however, that the 68-pounder may not be advantageously employed when partially adopted, as at present, on the lower decks of our ships of the line; but the practice brings along with it the evil of having *two* calibres on the *same* deck. We should prefer them in midships, and weighing as much as 55 or 60 cwt. Three or four, or perhaps half a dozen, of a side, although slow in their service, would prove, when discharged at a critical moment, for which they might be reserved, a tremendous auxiliary to ordnance already advanced to the greatest calibre, without sacrificing the necessary celerity in working, and powers of range; and if a separate magazine in midships were constructed for the ammunition of these pieces, confusion might be avoided in the hurry of action.

We shall abstain from making any further remarks on the subject of hollow projectiles for sea-service, because the idea has not yet been sufficiently put to the test of experiment, and certainly not at all to that of actual service.

The force of a ship of war is, as we have already said, the first element to be considered in its theoretical construction; and we might here proceed to explain its influence on the proportions and sailing qualities; but this will more properly be a subject for future consideration.

* The weight of an English ten-inch hollow shot, when filled, would be 95 lbs.; and when empty, about 85½ lbs.

† M. Paixhans himself seems to be fully aware of the magnitude of these difficulties.

‡ The new 68-pounder gun introduced by General Millar, weighs only 82 times its shot.

An Account of a New Genus of Plants called Diplogenea. By John Lindley, Esq., F.R.S., Professor of Botany in the University of London.

The genus which is the subject of the following observations forms part of a small collection of plants gathered in Madagascar, for the Horticultural Society of London, by the late Mr. John Forbes. By permission of the Society it has been allowed to be described and made public in this Journal.

The specimens consist of a few shrivelled branches with flower-buds and expanded flowers, but without fruit. From their appearance, it may be presumed that the plant to which they belonged was parasitical. The branches are brown, taper, fleshy, glabrous, very zigzag in direction, when young compressed, with a few dichotomous ramifications; the joints seem to have been rather tumid. The leaves are opposite, fleshy, spreading, glabrous, entire, oblong, retuse, tapering into a short petiole, triply ribbed, but otherwise destitute of all appearance of veins; their parenchyma, consist of large irregularly hexagonal cells, many of which are evidently filled with an oily fluid. The flowers are small, and appear in very short, axillary, fascicled racemes; their colour has probably been white. The calyx is fleshy and superior, with the limb falling off like a lid, and leaving a succulent dilated border behind; it adheres firmly to the ovarium on all sides, and when the lid has fallen, which happens at an early stage, resembles a truncated calyx; the coat of the tube distinctly abounds with receptacles of oil. The petals are four, lanceolate, acuminate, fleshy, involute at the apex, and having a twisted æstivation; they are inserted on the outside of a flat or concave fleshy disk, which occupies the summit of the ovarium. The stamens are eight, inserted in a single row on the outside of this same disk; their filaments are ligulate; their anthers in æstivation inflexed, ovate, acute, with two parallel cells communicating by a single pore at the apex, and having at their base two subulate, falcate spurs, or appendages; when the flower is expanded, the anthers acquire an erect position, and their lobes, which were before turned outwards, have an inward direction. The ovarium is inseparably connected with the calyx up to the point where it is covered by a flat or slightly concave fleshy disk; it apparently contains four cells, with numerous minute ovula attached to placentæ in the axis. The style is falcate, and thickened upwards; the stigma is a simple point. Of the fruit nothing is known.

It will have been already remarked that in many particulars this genus exhibits the common structure of Melastomaceæ, and that in fact it is very nearly related to, if not identical with, Conostegia. But there is a remarkable peculiarity in Diplogenea, which renders it impossible to associate it with any known genus of Melastomaceæ. This consists in the presence of receptacles of oil lying under the cuticle among the parenchyma, a character which it has been hitherto supposed that no Melastomaceous genus possesses, and which has been always employed as one of the chief distinctions of Myrtaceæ. I do not, however, think that the degree in which these receptacles exist in the genus under consideration will much invalidate the character of Myrtaceæ, because they are in too rudimentary a state to be actually identified with the transparent cells of that order; all that I wish to show is, that an evident tendency to produce oily secretions exists in Melastomaceæ, a tribe in which no such tendency has been before noticed.

In the absence of fruit, the characters of this genus can be only imperfectly traced; but the following will be sufficient to distinguish it from all that have been previously described.

DIPLOGENEA.

Nat. ord. *Melastomaceæ;* Conostegiæ *proxima.*

Calyx superus, limbo calyptriformi conico deciduo. *Petala* 4, lanceolata, in margine disci carnosi ovarium tegentis inserta. *Stamina* 8, circa discum inserta; *antheris* ovatis, basi bicalcaratis, poro apicis dehiscentibus. *Ovarium* calyci omnino accretum, 4-loculare, polyspermum, disco magno carnoso coronatum. *Stylus* falcatus, clavatus. *Stigma* simplex. ——Frutex *parasiticus? glaberrimus* (*Madagascariensis*). Rami *carnosi, dichotomi, junioribus compressis, Visci ferè habitu.* Folia *oblonga, retusa, carnosa, triplicostata, avenia,* receptaculis olei *intra parenchyma latentibus.* Flores *albi? parvi, in racemis brevibus, axillaribus dispositi.* Calycis *tubus receptaculis olei repletus.*

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1. D. viscoides.

Ad portum Sæ. Mariæ, Insulæ Madagascariæ, legit Johannes Forbes. (*v. s. sp. herb. Soc. Hort. Lond.*)

A Dissertation on the Nature and Properties of the Malvern Water, and an Enquiry into the Causes and Treatment of Scrofulous Diseases and Consumption, together with some remarks upon the Influence of the Terrestrial Radiation of Caloric upon local salubrity. By W. Addison, *Surgeon.*

Malvern has for a long period been justly celebrated for its pure and invigorating air, the excellence of its water, and the romantic beauty of its scenery. Dr. Wall, who wrote some years since a small work upon the efficacy of the Malvern Waters in many diseases, speaks highly of the benefits experienced from a residence at Malvern in scrofulous, nephritic, and many other complaints. Mr. Addison's work is scientific and ingenious; he attributes the many extraordinary recoveries which have occurred at Malvern, partly to the salubrity of the air, and partly to the purity of the water, which, from the analysis he has given of it, seems to contain much less saline or earthy matter than any we are acquainted with; and we think he has laboured, with considerable success, to prove that the continued use of a *pure water* may be a powerful means of removing and preventing many chronic disorders; his views of the causes of scrofulous diseases,—the circumstances which determine their seat or situation,—and the measures calculated to counteract a tendency to them are in our opinion, extremely creditable to his professional ability. The work contains many clear statements, with some accurate reasoning, which we can with confidence recommend to our readers. The last section treats upon a subject altogether new in medical science, though the facts to which Mr. Addison refers have been long known to the cultivators of chemistry. That the radiation of caloric from the earth will have a very great influence in the production of various diseases we are certainly much inclined to admit, and we feel induced also to believe, with our author, that the activity of malaria may very much depend upon this process. The remarks and observations which Mr. Addison has made upon diseases as they appear in tropical climates, certainly furnish a powerful statement in favour of the views he has taken. We earnestly recommend this subject to the profession of which Mr. Addison is a member; the conclusions he has drawn are, that *all those places where the radiation of caloric goes on with rapidity, will be found subject to great vicissitudes of temperature, to fogs, heavy dews, and other noxious precipitations from the air, whereby they are rendered cold, damp, and oftentimes extremely unhealthy*, while, cæteris paribus, *those situations where the* *terrestrial radiation is diminished will be proportionally warmer, drier, of a more equable temperature, and more healthy.*

We have been given to understand that this enquiry will be resumed by our author in a paper, which will shortly appear in one of the scientific periodicals.

On Mr. Ivory's *Investigations of the Velocity of Sound.* By Henry Meikle.

In the article on sound inserted in the *Edin. Phil. Jour.* for October, 1827, I had acquiesced in the theory of the late celebrated Marquis Laplace, so far as it appeared to go, and only suggested some small additions to it. But since writing that article, I have examined more closely the investigation of that eminent mathematician, given in the *Conn. des Tems* pour l'an 1825, and *Mécanique Céleste,* tom. v. page 119, and am now convinced that it is in itself objectionable in several respects, independently of any thing which I formerly hinted: so that my proposed amendments on this theory are as nothing compared with the thorough reform it would require; the result being neither deduced from correct principles, nor by means of an accurately managed calculus. The like objections attach to Mr. Ivory's view of it, given in the *Phil. Mag.* for July, 1825, p. 11. To this I shall principally direct my remarks at present, because it is better known in this country, and is given in a more detached form than that of M. Laplace, which, though essentially the same, and, in fact, the groundwork of the other, is curiously interwoven with some untenable speculations regarding heat*.

Considerable obscurity pervades Mr. Ivory's investigation, especially in laying down the first principles, which are both inconsistent and defective. Several of the most important circumstances are overlooked altogether; but, as will be seen from extracts which soon follow, the leading idea by which the

* In the *Conn. des Tems* for 1826, M. Poisson has treated the subject in a more general way, with the view of embracing cases where the medium is not uniform. The length of his Memoir would render it tedious fully to discuss its merits; but, so far as regards the ordinary case of sound traversing the horizon, it is not materially different from that about to be examined.

process is meant to be regulated is briefly this:—A minute cylinder of air, whose length varies without either changing its mass or diameter, is supposed to be acted on by an accelerating force, till it move over a small space z, and then abandoned to move *uniformly* with the velocity so acquired along a straight line x*. This latter motion is intended to represent that of sound, and its velocity is assumed, without either proof or probability, to be always the same, and, consequently, without either decrease or end, in air of the like density and pressure. It is further supposed, that the cylinder always moves over a space equal to its own length during the constant fluxion of time $d\tau$, and that it does so whether in passing over z or x.

Now without enlarging on the faint enough resemblance between this leading idea and the propagation of sound, it may be observed, before entering on further particulars, that either the space z, no matter how small, must be always of the same magnitude, and therefore the *intensity* or loudness of sound always the same in air of the like condition, which is contrary to universal observation; or else, the accelerating force must be everywhere inversely proportional to the space z. Without some condition of this nature, the final velocity with which the cylinder is projected, or the velocity of sound, cannot, as our

* This notion seems, in the first instance, to be borrowed from that usually given in elementary books on mechanics; where it is, in effect, shown that if a series of equal and perfectly elastic bodies, such as cylinders, be placed contiguous, having their axes in a straight line; and if an impulse be given to either extreme cylinder, it will communicate an equal impulse to the next, and this to the next, &c., till the whole series be run over. But to this is joined the assumption, that the velocity with which the impulse is propagated along the series is the same as the velocity of the first cylinder would have been, if alone, or projected by itself,—a coincidence for which I know no reason, nor can I believe it to be possible. But admitting it were true, since, as we shall presently see, the velocity of the projected cylinder must be proportional to the projecting force, how does this consist with the rate of propagation being likewise assumed to be ever the same in the same state of the medium? Some, perhaps, could tell us that the series of cylinders propagate the impulse, as if they were so many isochronous pendulums; but where is the proof? and I may again ask, how such a determinate velocity of sound can be aptly represented by the *precarious* velocity with which the cylinder may be projected? For, at all events, the calculus is conducted with reference to a projected cylinder. But supposing the investigation were to relate only to "the vibrations of a line of air," it would not be less objectionable; as, for instance, what could we make of the curious absurdity, to be shortly noticed, of the small cylinders of air being compressed till *infinitely dense*, at the turn of each vibration?

author assumes, be always the same in the same medium. For, to attain the same final velocity, the circumstances must be similar to those of a weight descending an inclined plane of a given height; where, abstracting from friction or other resistance, the accelerating force is inversely as the plane's length. But, in the case before us, the law of the force accelerating the cylinder must be of a very opposite description; for, as we shall afterwards see, in order that the velocity of sound, as deduced by this sort of investigation, may be independent of the intensity, or of the degree of condensation, the elasticity of the air would require to be either independent of, or to vary *inversely* as, the density, which are alike absurd; but here the elasticity is supposed to vary *directly* as the $\frac{4}{3}$ power of the density.

That the above are not the only serious charges which may be brought against Mr. Ivory's investigation, will appear from the following extracts; to which I shall subjoin some remarks, for the purpose of pointing out a few more of the tacit assumptions and undefined steps, which are not unfrequent, and for setting their merits and mutual relations, which are sometimes curious, in a proper point of view:—

"Conceive a slender horizontal tube of an indefinite length, containing air in a state of equilibrium; and let x, reckoned from a fixed point in the axis of the tube, be the distance of a small cylinder of air within the tube, the thickness (length) of which is equal to dx. Suppose now that the cylinder is pushed forward by some force to the distance $x+z$ from the fixed point, and that it occupies the length $dx+dz$ in the axis*. It is to be

* It is not, however, this movement of the cylinder over the space z that is considered in the sequel of the investigation; but its retracing of it occasioned by the natural tendency of the air to regain its equilibrium, and which accelerates the cylinder back over the space z towards the assumed point from which the distance $x + z$ was reckoned. A concussion or tremor is thus produced in the air, and propagated from atom to atom along the line x; and it is conceived that this tremor or sound moves *uniformly* along x with the velocity, whatever that be, which the cylinder has acquired during its acceleration over the line z. This supposed uniform velocity of the cylinder projected along x is further conceived to be the same with the velocity it happens to have, whenever its density equals the mean actual density of the medium. If so, how does this consist with the well known fact, that the series of aërial vibrations conducting sound through the atmosphere always get feebler and feebler as they become more distant

observed that dx is invariably of the same magnitude, whatever be the position of the small cylinder of air, and that dz alone varies in different places of the tube, and at different times. It follows, therefore, that x is independent on the time t, and z is a function of x and t. It is to be observed too, that the air is supposed to undergo very small condensations and rarefactions in proportion to its original bulk in the state of equilibrium; that is, dz must be considered as very small when compared to dx*. Let ϱ' denote the density of the air *in equi-*

from the sonorous body, and, consequently, the velocities of the atoms slower and slower at those similar points of their vibrations in which the densities of the cylinders become equal to the mean density of the medium? But ample reason may be given for the fundamental fact just stated, though Mr. Ivory has entirely overlooked both it and the reason. For admitting that the motion of the cylinder were, as he assumes, uniform in a tube, yet in the free air, sound is sent off as from a radiant point, in every open direction not opposed to the wind. Nay, sound reaches many a place by a curvilinear rout, even without being reflected. It is therefore plain, that the area of each wave or spherical shell of air, to which the tremor is communicated in succession, will increase as fast, at least, as the square of its radius, or of its distance from the radiant point. In other words, the number of atoms or the mass to be successively set in motion will, supposing the medium uniform, increase as fast, at least as the square of its distance from the sonorous body. This is a very different thing from saying off hand, that "the cylinder in motion has always the same mass." Hence, as might easily be shown from known principles, the motion of sound computed on projectile principles, instead of being *uniform*, ought to *decrease* as fast, at least, as the reciprocal of the distance from its source decreases.

Sir Isaac Newton's view of the subject is incomparably more consistent than the one before us. He supposed all the vibrations in the same uniform medium to be isochronous, or performed in equal times, however different their lengths, and, consequently, however different the velocities of the atoms at like points of their vibrations. Indeed, it is easy to see that there is no way in which the velocity of sound could be uniform, but by the vibrations, however different in length, being isochronous. Newton, and his earlier followers, were well aware of this circumstance; but vibrations of different lengths are quite at variance with, and cannot enter as an element into, the refined mode of viewing sound under the emblem of a projected cylinder, going on for ever, as the theory implies, without either decrease of velocity or of loudness. There is, however, no reason to think that every conceivable or possible law of elasticity in air would give isochronous vibrations; nor am I aware that such has been proved, from legitimate theoretical principles, to hold of even one particular law, far less of that which belongs to the atmosphere.—H. M.

* It would be difficult to reconcile almost any of these remarks either with each other, or with the very opposite principles acted on in the rest of this research. As, for instance, by strictly following up the leading principles of the investigation, it appears that dz, instead of being incomparably smaller than dx, must occasionally equal it; and that the con-

librio, and ϱ the variable density of the agitated cylinder; then, the masses of the two cylinders being the same, their densities will be reciprocally as the volumes: therefore

$$\frac{\rho}{\rho'} = \frac{dx}{dx + dz} = 1 - \frac{dz}{dx},$$

the powers of the small fraction $\frac{dz}{dx}$ being rejected *. This equation, it may be remarked, implies the continuity of the fluid †, since the cylinder in motion has always the same mass. Let P′ denote the elastic force of the air *in equilibrio*, and P the like force of the agitated cylinder; then, if we adopt the law of Boyle and Mariotte, we shall have $\frac{P}{P'} = \frac{\rho}{\rho'}$: and this equation would lead us to the result obtained by Newton ‡. But if, according to the observation of Laplace, we reason more

densation, in place of being trifling, must be *infinite*. For, here the length of the cylinder is $dx + dz$, which binomial is likewise used as the fluxion of z; no matter how curious and undefined the notation, which Laplace, however, avoids. But when the cylinder reaches its utmost distance from the assumed point from which $x + z$ is reckoned, and is about to return toward that point, its velocity $= 0$; and, therefore, the fluxion of the space $= dx + dz = 0$, and $dx = - dz$. Or, more properly, $dx - dz = 0$, and $dx = dz$. For in this case, the fluxion of the space, or the length of the cylinder, is obviously the *difference* and not the *sum* of dx and dz, because dx is constant. Hence, also, at the turn of the motion, the length of the cylinder is *nothing*, or its density is *infinite;* a consequence, though absurd, yet inseparable from the tacit hypothesis which makes the cylinder always move over a space equal to its own length, during the constant fluxion of time $d\tau$. It is therefore certain, that the length of the cylinder cannot consistently represent its velocity, or coincide with the fluxion of the space, as our author so conveniently assumes it to do, without offering the least reason for such illegitimate procedure. It is almost needless to add that the same assumption involves various other inconsistencies, or to remark that the shattering of windows and crazy buildings, the shaking of houses at considerable distances, the occasional deafening of persons, with many similar effects, could neither be produced by small vibrations, nor slight condensations; though *infinite* ones would be unnecessary.—H. M.

* Since, as we have seen, dz sometimes equals dx, this fraction is occasionally considerable, or even equal to unit; and, therefore, its powers cannot warrantably be rejected, either here, or again a little after in taking the fluxions.—H. M.

† True, a *continuity*, but only in *one* direction through the tube; whereas, in the open air, the continuity is in all directions.—H. M.

‡ We shall afterwards see this to be a mistake.—H. M.

agreeably to what actually takes place in nature, and suppose that the elastic force of the agitated cylinder is exerted while it retains the whole of its absolute heat, the preceding formulæ (D) * will furnish this equation,

$$\frac{P}{P'} = \left(\frac{\rho}{\rho'}\right)^{\frac{4}{3}} = \left(1 - \frac{dz}{dx}\right)^{\frac{4}{3}} = 1 - \frac{4}{3} \cdot \frac{dz}{dx}.$$

Take the fluxions making x only variable †, and divide by the equal quantities $\varrho\,(dx+dz)$ and $\varrho' dx$; then

$$\frac{dP}{\rho\,(dx+dz)} = -\frac{4}{3} \cdot \frac{P'}{\varrho'} \cdot \frac{ddz}{dx^2}.$$

Now, P is the elastic force of the air in the tube at the distance $x+z$ from the assumed point in the axis, and $P+dP$ is the like force of the air at the distance $x+z+dx+dz$; wherefore dP is the effective force urging the intervening cylinder towards the assumed point: and as the mass moved is equal to $\varrho\,(dx+dz)$,

* The formulæ referred to make the cube of the pressure vary as the fourth power of the density, which I consider to be the true law, though Mr. Ivory has since renounced it as incorrect, without giving any admissible reason ; but when he adopted this ratio, in the place from which he now quotes it, he did so for an *erroneous* reason, as I have hinted in the *Edin. Phil. Jour.* for January, 1827. However, I do not think such a ratio applicable to the investigation of the velocity of sound, especially in the supposititious case of the tube before us. For though, in favourable circumstances, sound be propagated in every open direction from the sonorous body, yet it does not appear that the air acts there exactly in its fluid character. Because sound which first passes through the tube, and then into the open air, does not proceed from the mouth of the tube, as from a sonorous body, in every direction, which it would do if the particles acted on each other with equal force in every direction. On the contrary, sound, as is well known, diverges but in a small degree after quitting a long tube which merely conducts it ; and I rather doubt if it would diverge at all, were it not for the friction or resistance which the vibrating particles suffer from their contact with air which is not in the direction of the tube. From this we should be led to infer, that the particles of air conveying sound through a narrow tube, especially the ideal one free from friction, only vibrate in the direction of the axis. If so, the elasticity of air conducting sound through the tube should not be estimated according to the above law, but more nearly as in the inverse ratio of the squares of the variable longitudinal dimensions ; because, as I have shown on a former occasion, the particles of air repel each other with forces inversely as the *squares* of their distances. But we have already seen that the actual case of the atmosphere is totally different from that of the tube.—H. M.

† This is a curious injunction, more likely to embarrass and mislead the reader than any thing else ; for the equation in hand does not involve x at all ; and, besides, Mr. Ivory, in the face of this strict precept, makes both P and dz variable.—H. M.

the quotient is the acceleration of each particle, otherwise expressed by $-\frac{ddz}{d\tau^2}$ *; wherefore

$$\frac{ddz}{d\tau^2} = \frac{4}{3} \cdot \frac{P'}{\rho'} \cdot \frac{ddz}{dx^2} \text{."}$$

Were every thing correct about this equation and the mode by which Mr. Ivory has obtained it, the velocity would obviously, as he in effect states it, be

$$\frac{dx}{d\tau} = \sqrt{\frac{4P'}{3\rho'}}$$

and since both dx and $d\tau$ are constant, the velocity would be uniform, and always the same in air of the same density and pressure. But another notable error and inconsistency have here evaded notice, by the manœuvre of twice rejecting the higher powers of dz, seemingly for the purpose of rendering the calculus manageable, though, as we shall presently see, there was no call or necessity for it on that account. Whether M. Laplace or Mr. Ivory were aware of this circumstance, I could not pretend to say; but one thing is certain, that further defects of the investigation become sufficiently apparent, when none of these powers have been discarded. For in this way we have

$$\frac{P}{P'} = \left(\frac{dx}{dx+dz}\right)^{\frac{4}{3}}.$$

* Viz. one of the usual differential expressions for an accelerating force. The second fluxion of the space being ddz, and the undefined symbol $d\tau$ denoting the constant fluxion of the time. It is from this step that it becomes more particularly obvious that the length of the cylinder is a measure of its velocity, being always equal to the minute space described during the constant moment of time $d\tau$. Not the shadow of a reason is either given or supposed necessary to assign why the length of the cylinder should not rather have had some other relation to its velocity than that just mentioned, which we have already seen to be impossible. But the gratuitous assumptions in this investigation are so numerous and important that they would have rendered it null and void as a mathematical production, although no inconsistency had presented itself. For were such assumptions to be tolerated in mathematics, there is no problem, however difficult, but they could solve with the utmost facility. A curious instance of their irresistible powers is noticed in the *Phil. Mag.* for Dec. 1822, where I have shown that the demonstration which Mr. Ivory supposed he had given of Euclid's 12th Axiom, in the number for March preceding, owes all its virtue to an assumption fully equivalent to the axiom itself, which was the very point to be proved!—H. M.

Take the fluxions, making dx and P' constant, which gives

$$\frac{dP}{P'} = -\frac{4}{3}\left(\frac{dx}{dx+dz}\right)^{\frac{7}{3}} \times \frac{ddz}{dx} = -\frac{4}{3}\left(\frac{\rho}{\rho'}\right)^{\frac{7}{3}} \times \frac{ddz}{dx}.$$

Multiply by P' and divide by $\varrho\,(dx+dz) = \varrho' dx$, as before, and we have

$$\frac{dP}{\varrho\,(dx+dz)} = -\frac{4P'}{3\varrho'}\left(\frac{\varrho}{\rho'}\right)^{\frac{7}{3}} \times \frac{ddz}{dx^2} = -\frac{ddz}{d\tau^2}.$$

Hence the velocity of sound should be

$$\frac{dx}{d\tau} = \left(\frac{\rho}{\rho'}\right)^{\frac{7}{6}} \times \sqrt{\left(\frac{4P'}{3\rho'}\right)}$$

which, though a very different expression from the former, is *uniform* or independent of the degree of condensation, because dx and $d\tau$ are constant; and yet it is affected by the intensity or degree of condensation, because ϱ is so affected.

We have thus, even when working more correctly, obtained a result which is evidently contradictory or absurd. Nor can it be admitted as an excuse, to say, that ϱ and ϱ' are nearly equal; for we have already seen that the principles acted on in this investigation imply that ρ may exceed ρ' in any proportion.

By using unit for the index of $\frac{dx}{dx+dz}$, we do not, when nothing is omitted, obtain Newton's result, as Mr. Ivory alleges, but the very different expression

$$\frac{\rho}{\rho'}\sqrt{\frac{P'}{\rho'}}$$

which is just as absurd as the other. Indeed, when in this mode of investigation, none of the powers of dz have been rejected, the velocity can never come out uniform or independent of the degree of condensation, and be at the same time real or possible. For, taking the only two supposable cases, – were the index $= 0$, neither the elasticity of air, nor sound, which depends on it, could exist; and were the index $= -1$, the elasticity would vary *inversely* as the density, which is a perfect contradiction, not to mention that the velocity of sound would come out an impossible quantity.

Any further evidence would be superfluous to show that this sort of investigation is not only inefficient, but full of error and incongruity, view it which way we will; and that it will be

K 2

alike unfortunate for this theory whether the motion of sound ultimately turn out, from experiment, to be uniform or retarded; for, independently of that, the result is anything but a fair logical deduction from correct data. I have as yet confined my remarks to Mr. Ivory's investigation in the *Phil. Mag.* for July, 1825. His other solution grafted on it, and given in that Journal for April 1827, is one way or other liable to all the abovementioned objections. The difference between his two solutions is owing to the innovations of his new law of condensation—an extraordinary production—the result of at least seven years' researches on the subject *. So far, however, from its being the *ne plus ultra* of science, as we should have reasonably expected, and as its author has more than once hinted, I doubt if, in point of absurdity, the like has been put on record since the dark ages. A few cases, where it leads to most erroneous results, are noticed in the *Edin. Phil. Jour.* for April, 1827; and I shall now state the law briefly, with an example or two of its unparalleled absurdity.

Let τ be the temperature on Fahrenheit's scale of an air-thermometer, when a mass of air begins to undergo a change of density; and ϱ the quotient obtained by dividing the density at the end of the operation by that at the beginning. Then, according to Mr. Ivory, the change of temperature due to such change of density is

$$i = \frac{3}{8}(448^\circ + \tau) \times \frac{\rho - 1}{\rho}.$$

Among the many extravagant and contradictory conclusions to which this new law leads, there is an obvious one notoriously at variance with observation,—that no compression could raise the temperature of air from the freezing to the boiling point of water; for, the greatest number we can substitute for ϱ, will always bring out i less than 180°, with $\tau = 32^\circ$. Hence, were all the air which invests our globe condensed into a point, its

* During the long period in which Mr. Ivory wrote on this subject, it is curious to observe that, though he is always right, and every one else wrong, he is incessantly changing his creed without giving the reason. Accordingly, this new law of condensation comes forth in February, 1827, without the least hint that the law of July 1825, was either repealed, or had ever existed.

temperature could not be thereby raised from 32° to 212°; nor could the ignition of tinder be caused by the heat evolved from air on its being condensed.

But though such results be out of all shape, yet the law now stated is, if possible, still more at variance with itself than with facts; for we soon obtain a very different result, by taking the condensation at several successive steps. Thus, by doubling the density of air at 32° Fah., or putting $\tau = 32°$ and $\varrho = 2$, we obtain $i = 90°$, which raises the temperature to 122°. Doubling again the density with $\tau = 122°$ and $\varrho = 2$, we get $i = 106°.875$. The density has thus been only quadrupled, and yet the rise of temperature, viz. $90° + 106°.875 = 196°.875$, exceeds the rise due to compressing all the air of the atmosphere into a point, at one operation*. I may remark, by the bye, that so far as is yet known, an elastic fluid cannot be liquefied by condensation, if it lose no heat.

If the air, which, by having its density quadrupled, has attained the temperature of 228°.88, be now restored to its original density, ought it not to be, in every other respect, restored to its former state? and, in particular, if it have neither gained nor lost heat, ought it not to resume its original temperature of 32°? So far, however, from this being the case, we shall find that by restoring the original density, the resulting temperature will be lower than 32° by the enormous quantity of 564°.6. Thus, putting $\tau = 228°.88$ and $\rho = \frac{1}{4}$, (for the law being general, must suit any initial temperature or density,) we obtain $i = -761°.48$. Hence, in place of 32°, the resulting temperature is $228°.88 - 761°.48 = -532°.6$ Fah., or 84°.6 below the *absolute zero* of those who, with Mr. Ivory, insist on an air-thermometer being the standard!

What a striking contrast between such absurdities and the following formula, in which not the slightest inconsistency can be detected, viz.

$$i = (448° + \tau)(\rho^{\frac{1}{3}} - 1).$$

* It is obvious that we may proceed in strict conformity to this law, and yet vary the result considerably; nay, in some cases, enormously, at pleasure, according to the number or magnitude of the parts into which we subdivide the whole change of density as taking place at successive steps,

This Mr. Ivory has rejected, without showing cause why. Most probably he did not like it after it was well known not to be his own production, and especially after I had advanced so much in its favour. Except the index of ϱ, it is M. Poisson's formula. The reasons why that index should be $\frac{1}{3}$ are given in *Edin. New Phil. Jour.* vol. ii. 333, 391, and iv. 101.

I formerly remarked that the notes of music afforded no satisfactory proof that sounds of all intensities are propagated with equal velocities. Another alleged proof, apparently of more weight, has been drawn from the experiments made in France in 1822; where the velocity of sound appeared to be the same, whether the guns were charged with two or with three pounds of powder. When, however, as was the case there, the powder has almost nothing to propel, a considerable proportion of it escapes without catching fire, and so much the more as the charge in the same size of gun is greater: so that the reports from the two and the three pounds of powder, might not differ materially in sharpness. Nor can I attach any stress to equal or uniform velocities deduced from hypothetical data; for we have seen that one of the most natural and obvious inferences from the projectile theory just discussed, is that the velocity ought to *decrease* rapidly; which is curious enough when we recollect that those who follow this mode of investigation assume, without hesitation, that it accords with a *uniform* velocity. No solution of the problem can be legitimate, if it take for granted an element so important, but so dubious, as *uniformity* of velocity,—an element which, I presume, can only be settled by experiment. For, I think it will be found that in solutions of this problem, the *uniformity* of the propagation of sound, or its independence of intensity, is, at best, assumed, or not provided for. In some cases, as we have seen, it may be got over quietly by inaccurate working; by repeatedly rejecting terms from the calculus, without giving any good reason, or showing that such terms could not have influenced the result.

If the velocity of sound be really greater when it is more intense, then all attempts to bring out a legitimate and definite mathematical result must necessarily prove abortive; because the intensity, being of an indeterminate character, cannot, as the supposed case would require, be made the basis of mathematical investigation. The great Euler, to whose sagacity we owe so much, was of opinion that the motion of sound is affected by its intensity. Lagrange, however, thought it easy to show, from theory, that this could not be the case. But, besides the particulars noticed in the present article, the circumstance, which I formerly mentioned, of wind exercising such an absolute control over the intensity of sound, and which our refined theories do not recognise, sufficiently shows how little confidence is due to theory in any such matters. The excess of the velocity of sound, aided by a very faint breeze, while passing from Montlhery to Villejuif, on the 22d June, 1822, over its motion on the preceding day, rather favours the opinion that wind adds more than its own motion to that of sound. Further experiments are, however, wanted to decide this point.

Description of a Regulating Valve for a Gas Establishment.

101, *Mount-Street, Grosvenor-Square,*
August 29*th*, 1828.

SIR,

The continued and increasing employment of gas, as a source of artificial light, confers a value upon every means which renders its distribution more convenient and economical.

As the consumption of gas, during the night is variable, it is desirable that the pressure at the gas-station should be regulated according to the demand. I have obtained from Mr. Eastwick (the very intelligent engineer of the Bath gas works) a description of a valve which enables him to adjust the flow of gas into the "main," so as to ensure an economical, yet sufficient, supply to the burners at the different periods of the night.

If you think it deserving a place in your Journal of Science, it is much at your service: I therefore enclose it for your approval.

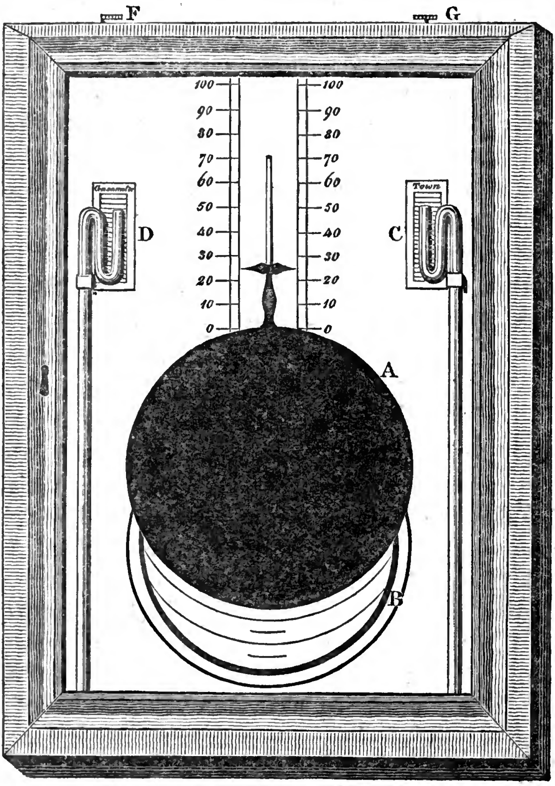
I am, Sir, yours, truly,
R. ADDAMS.

To W. T. Brande, Esq.

"SIR, "*Gas Works, Bath, June* 1*st*, 1828.

"AGREEABLY with your request, I send you a drawing of the index of a regulating valve, which I placed on the eight-inch main pipe leading from these works last summer, and which I had the pleasure of showing to you when you were in Bath in January last.

" The valve consists of a circular plate of metal, nine inches in diameter, sliding over the mouth of the main pipe, in a chamber. The face of the index is a representation of the valve itself; so made in order that the superintendent may know the precise position of the valve at any time.



" The black disc A is a thin plate of metal attached to a rod coming up from the valve behind the index frame, in which there is a slit for the pin which carries the index to pass. The portion of the circle B, which is uncovered by the disc, represents the aperture, or gas-way into the main pipe. D is a pressure gauge connected with the main on the gasometer side of the valve, and C, another pressure gauge, also connected with the main on the town side.

"There is a burner, supplied from the town side of the valve, placed before the eye of the person who adjusts the valve.

"From repeated inspection of the town lights at all hours of the night, as well as of the burner before the index, the requisite pressure is known and regulated; as the night advances, the valve is lowered more and more, and in the morning (when the lamps *ought* to be all out) it is depressed to one-tenth of an inch; that being sufficient to cause the exit of the gas in the lowest situations.

"The operation of the valve is so well understood, that I have been enabled to leave the management of it to the workmen ever since its erection.

"By this simple, yet effectual, contrivance, the saving of gas has been very great.

"I have never had occasion to elevate the valve more than $\frac{25}{100}$ths*, as shown in the drawing, this being sufficient for the escape of upwards of 7000 cubic feet of gas per hour, without the pressure of the gas being reduced more than $\frac{1}{10}$th of an inch, (viz. from one inch to $\frac{9}{10}$ths after it has passed the valve.)

"I believe you are aware that, from the nature of the coal used at this establishment, the retorts, after much use, become lined with a hard carbonaceous substance, of so considerable a thickness, as to diminish their capacity to one half and less, on which account a larger number of retorts *was* required; and from the imperfect conducting power of this incrustation, the decomposing process was slower, and additionally expensive. I am happy to inform you, that I have perfectly succeeded in removing the incrust in the following way: the retorts are left open, and kept at a good heat, by which the carbonaceous lining undergoes slow combustion, and in the course of a week, or more, according to the thickness, it is entirely burnt away.

"Believe me to be, Sir,

"Very respectfully yours,

"W. H. Eastwick."

"*To R. Addams, Esq.*"

* When the area of the lunaric aperture is = 14.733 square inches. R. A.

Transactions of the Horticultural Society. Vol. vii. Part II. 4to. London, 1828.

[Continued from No. III. p. 175.]

THE following are the principal contents of this Number, which is illustrated by four copper-plates, two of which are coloured, and many wood engravings.

XXIV. *Account of a Mode of managing Peach Trees in an early Peach House.* By Mr. Walter Henderson.

Of all the fruits that are forced into bearing at unseasonable periods, the peach is one of the most delicate, and which requires the greatest care and good management. Mr. Henderson, who has the reputation of being unusually successful, states, that the mode of treatment he adopts is the following:—

The trees are trained on trellis-work at a short distance from the glass; the house is warmed by a single flue running along the middle of the house; and by a pit between the flue and back wall, filled with decayed leaves, which are continually imparting moisture to the atmosphere.

The house is shut up about the first of December, and either gently warmed by fire, or not, according to the state of the weather. As soon as the buds swell, as much as possible of the wood that bore the previous season is cut away, and the younger shoots are tied into their places; not having previously been interfered with, but allowed to grow wild. The best shoots being selected, they are shortened according to their strength, care being taken *always to cut them down to a leaf bud.* The shoots are eventually laid in at the distance of from six to nine inches, and a great number of flower-buds are rubbed off, the strongest only being allowed to expand. As soon as the peaches are set, their leaves are gently sprinkled in the forenoon with water, once in every six or seven days; about the middle of March they are sprinkled in the afternoon. By this time the trees are producing the new shoots which are to bear a crop in the succeeding year: these are reduced in number by thinning and rubbing off, none being preserved except where there is room for them; such as are left are not tied down, but allowed to grow in their natural way, by which means the shoots on which the fruit is growing are not disturbed. As the season advances, the trees are sprinkled twice each week between four and five in the afternoon; this, however, is only done in warm, sunny weather. About a fortnight after the young peaches have stoned, the sprinkling is stopped, much more air is given to the house, and no fire-heat is maintained during the day; but if the weather is dark or wet, a little fire is applied at night; if the weather is warm and dry, the house is exposed to the air all night without fire-heat. By this management, the peaches acquire the unusual weight of half a pound each, and occasionally even that of ten ounces. The trees submitted to this treatment have undergone the same process for twenty-seven successive years, and are still in good health.

XXV. *Remarks upon the Comparative Advantages of Grafting Pears upon Quince Stocks.* By Mr. Thomas Torbron.

It is a well known law in vegetable physiology, that in proportion as leaf-buds, or as Darwin called them, viviparous buds, are produced by plants, flowers, or oviparous buds, cease to be developed, and *vice versâ.* Hence it is obvious, that whatever has a tendency to check the former, and favour the production of the latter, is beneficial to gardeners. Practice has shown that, by grafting fruit trees upon one kind of stock, the tendency to produce leaf-buds is increased, and that other stocks exercise a contrary influence. Gardeners in this country have long been well aware how to apply these facts to the cultivation of the apple, but they are little acquainted with the influence of the stock upon other kinds of fruits. The object of the writer of this paper is to show the benefit of grafting pears upon the quince-stock, instead of upon their own species. He states that the increase of produce by that means is on the average as 7.6 to 1 in favour of the quince; and in one case he found it as 15.1 to 1. Pears grafted upon the quince have also the merit of not occupying so much space as others; but it is to be doubted whether they be as long lived.

XXVI. *Description, with Plans, of a Hot-wall.* By Mr. John Hay.

Without the aid of heated walls, our friends in the North would have little chance of raising many of the good things in their gardens, which are produced with us by the climate alone. To them, we doubt not, these plans will be highly useful. They cannot well be explained without figures; we must, therefore, refer such of our readers as are interested in the matter, to the work itself.

XXVII. *Report upon the New or Rare Plants which flowered in the Garden of the Horticultural Society at Chiswick, between March,* 1825, *and March,* 1826. Part II.

This is a continuation of former reports of the same nature. The present paper comprehends twenty-seven species, with numerous varieties of hardy trees and shrubs, of which thirteen species, and ten varieties, are new. Several are highly interesting to lovers of gardening.

XXVIII. *On the Culture of the Mango and Cherimoya.* By Thomas Andrew Knight, Esq., F.R.S.

Upon a consideration of the failure which has generally attended attempts at cultivating these plants, and the circumstances under which the mango has succeeded so well in the garden of the Earl of Powis, Mr. Knight is led to the conclusion, that being plunged in a bark bed, or in some similar situation, where the roots can be kept in a constant state of humidity, and not exposed to the action of the air, is the secret which it was necessary to discover. This appears to Mr. Knight to be accounted for upon the principles explained by M. Dutrochet, in his work upon the influence of galvanism upon the motions of the fluids of plants; and he does uot doubt that the ill success of his former experiments arose from an excessive or injurious action of electric matter upon the roots of his plants, owing to the exposure of the surfaces of the pits to the air.

XXIX. *Some Account of the Mela-Carla, Mal-Carle, or Charles Apple.* By John Lindley, Esq., F.R.S.

This is an account, illustrated by a superb engraving, of a remarkable Italian apple, native of the territory of the Finale, in Liguria. By Gallesio, an Italian pomologist, it is stated to ripen in September, to keep well till the following spring, and even to remain fresh till the succeeding autumn. In October it is a pale yellowish-green, covered with a bright red on one side, and has a breaking, sweet, high-flavoured flesh; in November it becomes more tender, and finally its red colour fades a little, its green changes to a waxy yellow, its perfume diminishes, and its flesh becomes extremely delicate, without losing any part of its flavour. In short, it has no equal in beauty, tenderness of flesh, delicacy of flavour or fragrance.

Whether in this country it will acquire all these good qualities, remains to be proved. A south wall in a warm, dry soil is recommended for it.

XXX. *A Review of Fifty Kinds of Grapes, described by Mr. Speechly in his Treatise on the Vine.* By Mr. Joseph Thompson.

This is a capital review of the sorts of vines described by Mr. Speechly in his valuable treatise. Mr. Thompson has charge of the garden formerly under the direction of Mr. Speechly, and his observations have all the weight of the best authority. They do not bear curtailment.

XXXI. *An Account of the Species of Calochortus, a Genus of American Plants.* By Mr. David Douglas, A.L.S.

Three species of this very handsome genus are described by Mr. Douglas, from materials collected by himself for the Horticultural Society; one other is referred to, of which too little is known to enable it to be recorded; and Fritillaria barbata of Kunth is cited as being probably a fifth species. The only one in the garden is C. macrocarpus, a most beautiful plant, having the habit of Tigridia, with flowers of the same size, but of a deep violet blue. Three species are figured—one from a plant that flowered in the Society's garden, and two from dried specimens.

XXXII. *An Account of some Improvement in the Construction of Hot-beds.* By Thomas Andrew Knight, Esq., F.R.S.

By means of a simple contrivance of wooden or other pipes, Mr. Knight succeeds in keeping up a constant introduction of fresh warm air into the atmosphere of the hot-bed; a most important improvement, if we consider what the general nature is of the air of hot-beds.

An Attempt to prove that Ava was the Ophir of Solomon. By John Ranking, Esq.

Ophir, Aufer, Aufr, Afer *, is one of the most interesting and remarkable of the uncertainties in historical literature;

> "An *ignis fatuus* that bewitches,
> And leads men into pools and ditches †."

Peru, Hispaniola, Guinea, Armenia, South Arabia, Sofala, Ceylon, Malacca, Sumatra, have all had their advocates. The last treatise is by Mr. Bruce; who, following D'Anville and others, contends for Sofala: and which, says Dr. Robertson ‡, seems to establish the truth. The author of the article "Ophir" in Rees's *Cyclopædia* does not agree with that eminent historian; and the writer of the dissertation in the *Encyclopædia Britannica,* equally in doubt, ends his remarks thus:

Sub judice lis est.

Bruce's laborious calculations regarding the monsoons are

* So spelt by Dr. Doig. † Hudibras. ‡ India, p. 9.

found to be blunders*; and he has confined the cargoes to gold, silver, and ivory: *omitting* peacocks, monkeys, precious stones, spices, almug-trees, and ebony.

Buffon† insists positively that peacocks were not wild in Africa till they were introduced by the Portuguese, and that therefore Ophir could not be in Africa. Alexander the Great, when he entered India, is said to have been much struck with the beauty of the peacocks, never before having seen one‡. As to the first five places mentioned above, there are obvious insuperable objections to them all. The three last have, neither of them, ever been known to possess such abundant riches and ivory as were imported by David and Solomon. Four hundred and fifty talents of gold have been brought by one fleet §. Thrones, beds, and benches were constructed with ivory. "All thy garments smell of myrrh, aloes ||, and cassia; out of the ivory *palaces*¶." "The *houses* of ivory shall perish **." Whatever may be meant by *palaces* and *houses*, there can be no doubt that a considerable quantity of ivory was consumed.

Thus it does not appear that any one of the above places is free from objection, as not being known to have produced either ALL the objects imported by the Jews; nor, several of them, the great *quantity* of some of the articles enumerated.

The writer will now endeavour to prove that Ophir, or Afer, was no other than *Ava;* and if that country has always borne the name, which it now does, of Ava ††, and if it has formerly contained, or does still contain, all the articles described as forming the cargoes imported, is it not quite astonishing that that consideration and the name have never led one of the host of critics to the discovery of the *undisputed truth?*

Another remarkable circumstance attending that rich mart

* Rennell's Herodotus, p. 676.

† Sonnini's Edit. vol. xlii.

‡ Ælian.

§ 2 Chronicles viii. 18.

|| This means the perfume; it is produced in Ava, "the *aloexylum verum*, much valued for the grateful odour of its smoke."—Rees's *Cyc.* "Birman."

¶ Psalm xlv. 8.

** Amos iii. 15.

†† This word may be otherwise pronounced in that country; as it is spelt also *Aungwa.*—See Rees's *Cyc.* "Ava."

of ancient times, is, that it was probably known to, and invaded and conquered, by *land*, by the *Turks*, not many years after the death of Solomon, as will be seen.

It will now be shown that the country in question is noted for all the riches and productions with which the ships were laden.

" Pegu, in the 16th century, was visited by Gasparo Balbi, a Venetian jeweller; and he relates that the magazines of *gold*, *silver*, Ganza, *jewels*, &c., were under separate treasurers; and that the king was the richest in the world, except the Emperor of China. In the year 1600 the King of Pegu was slain by the King of Tangut, who laded six or seven hundred elephants and as many horses with gold and jewels; not regarding the silver, which, with all the artillery, was seized by the King of Aracan, to an immense amount. Bonferrus relates that the Peguans are descendants of Solomon's people. The largest *elephants* in the world are here found in abundance, and also *apes*, parrots, and *peacocks*." See Purchas (vol. i. 33 to 40), who is of opinion that Pegu is Ophir.

Ebony is produced in Ava,—the *ebenoxylum verum*, or true jet black kind. (Rees's *Cyc.* " Birman.")

Almug wood is said to be cedar, fir, cypress. (Rees's *Cyc.* " Almiggim.")

Josephus describes it as " pine wood in abundance, of such great size and beauty, that Solomon had never before seen any that was comparable; not like common pine, but with the grain of the fig-tree, only rather whiter and more glossy: and that it was used as pillars and supporters of the Temple and palaces, and also for harps, psalteries, &c." * Abundance of fir-trees grow at the present epoch in the kingdom of Ava. (See Rees's *Cyc.* " Birman.")

Spices. Pepper of several kinds †, ginger, cardamums, turmeric, three or four kinds of capsicum, cassia fistula, cinna-

* Josephus, Book viii. ch. 11.

† Peacocks feed on pepper. The writer, while shooting on the banks of the Luckia river in Bengal, flushed a flock of about twenty of these splendid birds in a field of grass, just high enough to hide them. A more beautiful sight can scarcely be imagined. One which he shot, had in his crop more than a hundred Chili pepper pods, the smallest and hottest kind known. The tail of the bird was full six feet in length.

mon laurel, nutmeg, spikenard, all grow in this country.—(Rees's *Cyc.* "Birman.")

Elephants and precious stones. No country produces such large elephants ; jewels of all kinds abound in this once opulent region. The reader, who has any doubts on these points, may have ample evidence in the 7th chap. of the *Wars and Sports of the Mongols and Romans.*

Josephus, speaking of Saphira whence Solomon had his gold, says that it was a country of India, and not an island, and that it is now called by the name of *Aurea* *.

Ava and Pegu are, in Ptolemy's map, named *Argentea Regio*, and *Aurea Regio*. The city of Pegu was anciently Sabara Civitas; Persain, or Basseen, was Barabouna Emporium.

Colonel Symes mentions that the analogy between the Birmans and ancient Egyptians, in many particulars, is highly deserving of notice; that Phra was the name under which the Egyptians adored the sun (before it was named Osiris), and a title for their kings and priests ; and that Praw, or Phraw, in the Birman country, imports lord, and is always annexed to a sacred building, and is a sovereign and sacred title, probably the same as Pharaoh. The temple of Shoemadoo † was founded 2300 years ago ‡.

" The ruins of the walls of Terrechetteree are of massive thickness, and may be traced through a circuit of ten miles. The enormous masses of brick pagodas in Pegu are of immemorial age, and approach nearer to the *pyramids* than any other relic of antiquity. The constant ornaments of the religious edifices are sphinxes, griffins, mermaids, and crocodiles, which are the exact symbols of the religion of the Egyptians. Would not this warrant the conclusion, that some

* Phil. Trans. 1767, lvii. 155.

† *Shoe* means golden. There is also a temple called Shoedagoung, or Shoedagon. " Dagon his name, sea-monster, upward man, and downward *fish*."—*Milton*, Book i. 462. It is well worth inquiry at Rangoon, whether the shape and rites of their god *dagon* have any analogy with those of the Philistines: and in this enquiry, it will be ascertained, whether there be any reference or connexion with the *fish* found in those seas called *dugong*. Some curious information may probably be obtained on this subject.

‡ Symes, 8vo. vol. ii. 62—76.

unknown cause exists for the similarity?—but this is a wild hypothesis, and very little borne out by probability." (*Two Years in Ava*, by Captain T. A. Trant. *Monthly Rev.*, Nov. 1827.) It is a curious circumstance that *mummy* is with the Birmans a favourite medicine *; but it may possibly mean the Arabian drug so called, which is used as a medicine also. The use of the *body* as medicine was first introduced by the Jews †.

The following subject is not *necessarily* connected with the above, but it is added because it is short, and probably new to most readers.

With regard to the Turks, their first great hero, Oguz, appears to have been a Siberian ‡. The Ottomans and the Moguls of the race of Genghis Khan claim descent from him §. Oguz was the grandson of *Mogul* Khan, the founder of that race. The exact epoch of Oguz is obscure; he is said to have attained the great age of 116 years. "When Cajumars, Prince of Chorassan, died, his son Haushang was in his minority, and the lords quarrelling for the reins, Oguz marched to Azerbijan, Irak, and Armeen, which countries he conquered ||." *Caiumaras* died, and *Husheng* succeeded to the throne, according to Sir William Jones (vol. v. 587), in the year B.C. 865; and this is, perhaps, the nearest approach to the know-

* Rees's Cyc. "Birman."

† Rees, "Mummy."

‡ Strahlenberg was informed by Tartars and Russians at Tobolsk, that to the south-west of that city, between the sources of the Tobol and Ischim, which few people frequented, there were great numbers of images cut in stone, of men and beasts, and that the ruins of several cities were discernible in those deserts; and that this was the place where Oguz Khan the Great had his residence.—*Hist. of Siberia*, p. 4. Tamerlane, when he was in this neighbourhood, acknowledged it to be the country of Oguz.

§ There is a great similarity of customs and ceremonies in these two people, who have often been rivals. They have each conquered Siberia and China, and all, or part of *India extra Gangem;* they both have *peacocks* as supporters for their thrones. The Chinese wall was built B.C. 221; and a century afterwards, Vu-ti, emperor of China, also conquered Pegu, Bengal, (probably Eastern Bangalla, described in Wars and Sports, ch. vii.) Siam, and Cambodia. He divided those countries among the generals who had conquered them; but they soon contracted the manners of the *Tartars*, and became the greatest enemies of the mother country. —*Du Halde, Wars and Sports*, p. 89.

|| Abul Ghazi, p. 19.

ledge of the epoch of Oguz that can be obtained; but the chronology of these heroes and events cannot be very correctly known; nor is the exact truth in that respect of much importance in this sketch. The 7th century B. C. is generally the epoch assigned to Oguz: some, perhaps, reckon from his birth, and others from his death. Solomon is said to have died about B.C. 975; therefore Oguz would undoubtedly be acquainted with the wealth of the Hebrews, and be inflamed with the ambition to pay a visit to the country which furnished such various and abundant articles of luxury and grandeur.

"The Turks had professed the true religion," says Abul Ghazi, "till the reign of Cara Khan, the son of Mogul and father of Oguz; but at this epoch idolatry had increased so much, that the son would destroy his parent, and the father his child, who showed an inclination to return to the true worship. When Oguz was born, his face shone miraculously like the *sun*, and he was continually pronouncing the word Allah.

When Oguz succeeded to the throne, he resolved to force the subjects of his grandfather, Mogul, to adopt the true religion *. He *invaded* Dsurdsut (Gurgut or Zinu) on the frontiers of Kitay †, and the booty was immense. He was seventy-two years at war, and brought the people back to the true religion. After which he *conquered* the empire of Kitay ‡, the city of Dsurdsut, the kingdom of Tangut, and Cara Kitay §,

* It is not unlikely that the introduction of the religion of Budda, or Boodh, was the innovation. Boodh was born, says Sir William Jones, B.C. 1027. The subject of Boodh's person and country is very obscure. "He had *curled* hair," says Sir William Jones, "was not a native of India, and was probably Sesostris." "I am inclined," says Herodotus, "to think that the Colchians were descended from the troops of Sesostris, because they are black, and have hair short and *curling*. They have also similar manners, the same language, and their manufacture of linen is alike." (Euterpe, civ. cv.) If the Peguans have preserved their early history, it will probably clear up some points relating to the Pharaoh's, Boodh, the Turks, and the Syrians.

† In the map with Abul Ghazi, Zinu is in north lat. 32°. At this epoch *Singan* was the capital of *Kitay*, which never means more than the northern half of China.

‡ B.C. 777. A torrent of Tartars distracted China.—Vide *Du Halde*, vol. i. p. 163.

§ Cara (black). Cathay is on the map in lat. 23 to 30, now partly Assam. Other territories have borne this name from the colour of the rich soil; it may in this case designate the *black* people.

about the lake Mohill*, where the people are as black as Indians. From hence, passing behind Kitay, he found on the sea-coast, among the mountains, very warlike people, whose khan was called Itburac, and who, with a good army, received him so bravely, that he was constrained to go back and post himself in an advantageous camp between two great rivers. Oguz and his chief officers carried their wives with them. Seventeen years afterwards, Oguz conquered the dominions† of Itburac, and put him to death. Cabul, Gazna, Cashmere, Damascus, Turquestan, Great Bucharia, Balc, &c. fell to this great conqueror, who, at his decease, made a just distribution of his empire among his numerous relations‡.

By the above short essay, some perhaps may be induced to *conclude* that Ava was the true Ophir. With regard to the navigation, it was more approachable than some of the places mentioned; and on that point those who have contended for Malacca and Sumatra will have nothing to oppose. The facts are too remarkable not to attract the attention of literary gentlemen in Bengal and Ava; and further enquiries in those countries will very probably lead to curious discoveries on this question.

On the Sap of the Rose Tree.

Communicated by R. Addams, Esq.

A FEW weeks since I had an opportunity of collecting a considerable quantity of sap from a rose tree, and I was induced to submit it to a chemical examination. The following are its characteristics :—

It was transparent, and colourless when viewed as drops, but in large portions it appeared a little opalescent. It was tasteless and inodorous. Specific gravity 1.001. It contained no uncombined acid or alkali.

* In the map, lat. 27°.

† A note says this means Tonquin and Cochin China; but the positions of those countries beyond so many large rivers, seem to warrant the conclusion that Assam, Ava, and Arracan, were the territories conquered by Oguz, as has been mentioned in page 89 of "Wars and Sports;" and this conjecture is strengthened by the circumstance of Boodh having introduced his religion first into Arracan, and thence into Ava.—*Rees's Cyc.* "Boodh."

‡ Abul Ghazi, (a descendant from Genghis Khan.) Vol. i. p. 11, ch. ii.

Sub-acetate of lead and oxalate of ammonia each occasioned a precipitate.

Twelve fluid ounces were evaporated, and afforded 7.25 grains of solid matter (*a*), which, being tested by sulphuric acid, evolved fumes of acetic acid.

To this (*a*) water was added and heat applied, a part only dissolved: the insoluble portion (*b*) weighed, when dry, 2.9 grs.; it was not acted upon by muriatic acid. Supposing it to be oxalate of lime, a part of it = 0.3 of a grain was heated to redness on platinum foil; it left a white powder possessing all the properties of lime. The remainder of the insoluble portion (*b*), viz. 2.5 grs., was boiled in carbonate of soda, and thus decomposed into oxalate of soda and carbonate of lime, the latter weighed 1.9 gr., containing 1.064 of lime, being, nearly, the equivalent of lime in 2.5 of *neutral* oxalate, or 1.094.

To the soluble part of (*a*) oxalate of ammonia was added, until it ceased to give a precipitate; this being separated, weighed 0.9 gr.; it was oxalate of lime, and equivalent to 1.097 of acetate of lime in the soluble part of (*a*). The remaining fluid yielded, by evaporation, a brownish viscid mass; this was digested in alcohol (sp. gr. 0.823), and it left insoluble matter, which, dried and weighed, was = 2.1 grains, and proved to be principally gum and extract.

The alcoholic solution, when concentrated, gave indication of potassa, by the application of muriate of platinum; it was then evaporated, and weighed 0.8 gr. When tested with sulphuric acid, the presence of acetic acid was manifested. It was now heated to redness on a silver capsule, then acted upon by water, filtered, and evaporated; it left a little more than 0.5 gr. of carbonate of potassa, equivalent to 0.7 gr. of acetate of potassa.

Therefore, collecting the results, it will be—

Oxalate of lime	2.9 grains
Acetate of lime	1.097
Acetate of Potassa . . .	0.7
Gum and extractive matter .	2.1
Soluble in alcohol (sugar, &c.?)	0.1
Loss	0.353
	7.25

I should not have considered the foregoing of sufficient importance to communicate, if there had not been some circumstances, relative to the collecting of the sap, which, independently, may be thought interesting, and which, perhaps, may justify my wish to see this inserted in the *Journal of Science and the Arts*.

The tree which supplied the sap in question is the *Rosa rubiflora*, growing in a garden at Hammersmith. July 29th, it was deprived of its branches by sawing off its head, leaving a stem $3\frac{1}{2}$ feet high, and $2\frac{1}{4}$ inches in diameter. I was informed the sap began to flow almost immediately after decapitation. I did not see it until the following day, when the exudation of the sap was so profuse, that I collected an ounce measure of it in forty minutes. The temperature of the air at the time was 67°. The saw-cut was made inclined to the horizon; hence the fluid accumulated at the lowest part of the section, and I occasioned it to drop into a suspended phial bottle, by a piece of twine fastened to the tree in such a manner as to act the part of a conductor.

The "bleeding" continued uninterruptedly, a few hours more than a week; during this period I procured 31 ounces, or nearly a quart; this, together with that which escaped before my attention was directed to it, and the loss by evaporation, probably exceeded three pints. The discharge diminished in quantity from the time I first observed it. The tree is now living, and vigorously protruding young shoots.

Having at my disposal an abundance of sap, I repeated the analysis upon six ounces collected on the fourth day, but instead of obtaining from it corresponding products, it furnished, by evaporation, scarcely one grain of residuum, consisting of about 0.25 grains of oxalate of ammonia, a trace of acetate of lime, the remainder being gum.

A third quantity = 6 ounces of the last portions collected, was also examined: in this trial the solid matter was quite equal to the last, but it differed materially in constitution, for nothing could be separated; it was entirely gum.

Some of the sap had been reserved, and by keeping, even three or four days, it is found to generate acid, its presence being detected by litmus paper; therefore, in all similar cases, it is advisable to examine such productions in their most recent condition.

September 6th, 1828.

Statistical Notices suggested by the actual State of the British Empire, as exhibited in the last Population Census. By Mr. Merritt.

(Read before the Literary and Philosophical Society of Liverpool.)

The Essay of Mr. Malthus, like the immortal work of Adam Smith, for some years after its appearance, rose slowly and almost imperceptibly into public estimation. Like its great precursor, it has at length attained the dignity of a class-book, and is now studied in schools and colleges; quoted by senators, and appealed to by writers, as an authority which scarcely any living author has hitherto reached. I am far from doubting the grounds on which this consent of public opinion has been obtained. I am satisfied that he has made out his main propositions with a certainty more nearly approaching to the demonstrative evidence of the exacter sciences, than can often be attained, or, indeed, expected, in subjects which are in their nature so controvertible. I am equally forward to admit that his dignified and dispassionate candour is equal to his clear and logical precision. The arguments of Mr. Malthus, however, it is well known, have been repeatedly and violently opposed, both here and abroad. It would have been wonderful if they had not. When we consider that the most assiduous and persevering research of some of the ablest men in Europe have scarcely established a single axiom in political economy which is not disputable, it is less surprising that his system has been attacked, than that his opponents have been so few and feeble as scarcely to merit the trouble of refutation.

This concession, however, is to be understood as applying only to the leading positions on which the system is founded: such as the natural tendency of population to increase beyond the ratio of subsistence; the consequence of this disproportion; and the inevitable alternative of preventive or positive checks, such as he has described. These principles being founded on the very constitution of human nature, I should suppose can never be shaken by any future discoveries or argumentations. They seem to have been at all times sufficiently obvious, and yet they have been so little recognised, that the manner in which Mr. Malthus has developed them, has advanced the science of political economy more than all the efforts of his predecessors, and has thrown broad sunshine on some of the most perplexed phenomena of civilized society.

But when this gentleman begins his examination of the remedies which have been proposed for these alarming evils, and especially when he brings forward his own grand propositions of practical alleviation, he then, I presume, enters on more debatable ground. This observation, perhaps, might be variously illustrated; but in the few remarks which are here loosely thrown together, I shall confine myself to two points of acknowledged importance—the question of emigration and the abolition of the poor-laws.

From the first appearance of this great work, it always struck me, that the chapter on emigration was the vulnerable part of the book. To use a vulgar, but very expressive parliamentary phrase, he always appeared to me a little disposed to *blink* that essential part of the enquiry. Apparently his object is to shew that the evils which have always preceded and accompanied emigration, are necessarily greater than those which they were meant to relieve. He has made a formidable array of the obstacles which present themselves to every new settlement, and has detailed some of the most disastrous attempts of this kind, which have been recorded in different ages. He has endeavoured to shew, that in almost every conceivable case, whether the object is to colonize an uninhabited country, or a territory claimed, but not occupied by other governments, the suffering and waste of life will, in most instances, exceed the operation of the positive checks which drove the superfluous population from home. On these grounds, therefore, emigration is not to be considered as a remedy, but as a substitution of one evil for another equivalent to it, with the gratuitous trouble and expense of a change of residence.

But these conclusions are not inevitable. The sufferings which usually attend the formation of a new settlement may be avoided or mitigated by that degree of prudence and foresight, without which no difficult undertaking can be expected to prosper. As the relief of an excessive population is a national affair, all governments are bound to consider it as such, and no expenditure of their revenues can be more useful and legitimate than that which is employed to maintain the due proportion between the number of the people and the means of their subsistence. It ought, therefore, to be a standing object of national policy, to provide the resources, and facilitate the means of a periodical emigration. Being thus in constant readiness, whenever the symptoms of a redundant population begin to manifest themselves, they can be promptly applied, before the positive checks, with all their horrible train, have made much progress. Most of the disasters which Mr. Malthus enumerates were the necessary effect of insufficient means, defective information, or rash enterprise. Many of the obstacles, moreover, which oppose or retard the establishment of new colonies, have disappeared, by the progress of colonization itself. In those parts of the world which admit and demand the greatest increase of inhabitants, the difficulties which attend a first settlement are already subdued. These communities then become strengthened by the admixture of new settlers, and the population diffuses itself by the mere expansive force of additional numbers.

It is sufficiently demonstrated that there exists in mankind a power of increase far beyond what was wanting to keep up their numbers to any stationary amount. Mr. Malthus not only admits but maintains that this prodigious power was given for the purpose of replenishing the earth; which, from reason and revelation, we have every reason to believe was originally peopled from a very small number. He therefore cannot deny that the command of "increase and multiply" is of human as well as of divine obligation, so long as any considerable parts of the earth remain unpeopled. Yet so imperfectly has this command been hitherto obeyed, that there is reason to suspect that no progress whatever in replenishing the earth has been made for the last 2000 years. Is not this a palpable contravention of the plainest designs of Nature and

Providence? Do we not wilfully retard, if we do not studiously promote the great scheme of creation, if we omit to furnish inhabitants wherever the means of sustenance are found? Does it not indicate some gross defects in human contrivance, when we contentedly labour under the dreadful ills of a redundant population, at the time when the greatest part of the habitable globe is wasting its annual produce in the desert air?

But projects of emigration on a large scale, it will be said, would entail on governments such an intolerable expense, that no nation would be willing to endure it. It is lamentable that the imbecilities of human management should thus encounter us at every turn. What an inconsiderable part of the sums habitually wasted in the pursuits of national ambition or resentment, would gradually people all the wilds of America and Africa! But as these diseases of our nature are, perhaps, to be reckoned amongst those which are the most desperate and incurable, it would not be wise to found any scheme of extensive good on the prospect of their removal. There is no need to reckon on any such chance of improvement in public affairs. Under the actual circumstances of the European nations, the means of carrying off their superfluous population might be provided without any sensible addition to their public burthens.

But the horns of Mr. Malthus's dilemma encounter us on every side. Supposing, what he is far from admitting, that all the herculean difficulties of this scheme of emigration could be finally surmounted, you only remove the evil for a century or two. It then returns upon you with more force than ever, because it is universal and illimitable. It is undoubtedly true, that the enemy, though defeated, is not destroyed: though driven from the field at present, it is only to recover strength for another struggle. But admitting this, we obtain, at all events, an indefinite postponement of the mischief; and when at last it actually approaches, and the world is completely filled, it will then be time enough to debate on the application of hazardous or desperate remedies. May we not hope that the same Providence, which gave to the fructifying power of man its superabundant energy, will provide in the progress of human civilization some remedy for its excesses. As this progress, when once in activity, proceeds with an accelerated motion, we may expect that the lapse of two or three more centuries will produce effects on which, at present, it is impossible to calculate. Before that time, we may hope that universal diffusion of instruction will enable mankind more effectually to understand their interests, and to regulate their passions. It is also probable that an increased taste for luxuries, and an improved sense of enjoyment, will render men more fearful of poverty and its consequent privations. They will then act more on foresight and calculation, and when that spirit is once aroused, the business is accomplished.

In fulfilling the grand primeval command of replenishing the earth—the express injunction of nature and revelation—there can be no doubt that the countries already peopled would be greatly benefited by the new nations which they successively sent out. The young and vigorous offspring would generally improve on the parent stock. The science of government, which is more in a state of infancy than almost any other, might be advanced, like the rest, by repeated and successful experiments. A degree of enterprize in this particular, which old states are afraid to hazard, can be undertaken by new governments without material danger. They are not encumbered by old, and corrupted, and feudal institutions, or checked by the fear of a wealthy aristocracy, or alarmed by the clamours of an ignorant populace, pent up in large cities. Besides these advantages, they start with all the lights, and all the experience of the mother-country, and of all other countries. In a society where the population is no where dense, where few are discontented and none are superfluous, the greatest degree of practical freedom may be safely essayed. We see this in the instance of America. A degree of licentiousness which would endanger the safety of an European state, is there found to be very compatible with public tranquillity. Her remote position, her security from hostile attack, her superabundant produce, and her consequent exemption from many of the vices and miseries of an old nation, admit a relaxation of vigour in the government, which in the perilous politics of modern Europe would inevitably prove fatal. In general, I very greatly admire the government of the United States; but it cannot, I think, be denied, that the superior happiness of their people is more to be ascribed to these circumstances than to anything in their positive institutions.

I shall now, in the second place, say a few words on Mr. Malthus's great remedy for the magnitudinous evil which he has so ably displayed—the gradual abolition of the poor-laws. The authority of this eminent writer, and of some distinguished individuals, in and out of parliament, who have adopted his doctrines, have propagated a general belief that the system of our poor-laws is the great radical evil of the country. After all the other trials which we have suffered and survived, this domestic sore is, it seems, that which is destined at last to consume our vitals: our system of parish relief is described as a sort of hydra, with a power of self-propagation so prodigious, that it must soon lay waste the whole land, and finally leave nothing to be devoured. All the mighty evils inseparable from the principle of population are, according to Mr. Malthus, increased by this system to a degree of tenfold aggravation. By it all the benefits of the preventive check of moral restraint in respect to marriage are stifled at their source. Yet by this alone can the multiplied horrors of the positive checks of vice and misery be prevented or retarded. No man, it is said, will be induced to put any restraint upon his inclination when he knows that the parish is bound to maintain all the children which his improvident marriage may bring into the world. For the same reason he never thinks of making any provision, in seasons of youth and prosperity, for those of adversity and old age. The increase of parish paupers diminishes their sense of shame, and degrades their habits of independence; and the disease in this way, like a conflagration, extends itself on all sides, and gathers strength by every extension.

The Report of the House of Commons labours to show that the theory of Mr. Malthus is more than borne out by fact and experience. They infer, from the rapid increase of the poor's-rates, which they describe to be in an accelerated ratio, that they must gradually absorb the whole property of the country, convert us into a nation of paupers, and finally reduce the functions of government itself to those of overseers and churchwardens. In confirmation of this alarming doctrine, they produce instances of some parishes where the rent of all the land is insufficient to support its poor, and where the soil has consequently been abandoned. It is not at all surprising that these reports have spread an universal anxiety, and a general sense of insecurity amongst persons of property. Every project for the abolition of the poor-laws is, of course, eagerly entertained.

It will not be difficult to show, without much consumption of time, that these apprehensions are as much exaggerated as the remedy they would suggest is inapplicable, unjust, and impracticable. The whole argument rests on a hollow foundation. About a twelvemonth ago I transmitted to a periodical work a statement respecting the gradual rise of the poor-rates, in which I endeavoured to show that the public were under great misapprehensions on this important subject. My object was to prove that the augmented amount of the poor-rates was not more than commensurate with the change in the value of money, and the increased amount of our population, especially of that part of our population which nourishes and sustains the mighty mass of pauperism. This position I endeavoured to support by a comparison of the sums expended on the poor, the value of money, and the extent of the national population, at a period of forty or fifty years ago and at the present time. These points, however, have been stated with much more accuracy and detail in a pamphlet published a short time since by a gentleman of the name of Barton. Adopting as a *datum* what I presume will not be disputed, that the value of money is to be estimated by the relative price of corn, he has reduced the contribution of every individual to the poor-rates into its proper value in wheat, and has found that the charge per head on the whole population of the realm was, in 1776, forty-four pints of wheat; in 1785, fifty-three pints of wheat; and in 1815, fifty pints of wheat. There is, therefore, a small advance from 1776, but a decline from 1785, in the real relative amount of our assessments to the poor.

But this statement, striking as it is, does not by any means show the extent of our misapprehensions. It is well known that the late rapid increase of our population has taken place principally in great towns, or in manufacturing and commercial districts. There is great reason to believe that the rural population has been nearly stationary; for though the cultivation of wastes, and the inclosure of commons, have necessarily caused an increase of agricultural employment, yet the reduction of small farms, and the improvement of machinery and implements, have diminished the demand for manual labour. Our increased population, therefore, has arisen in those classes of the community in which the seeds of poverty and misery most naturally take root. In almost all the districts of the country, purely agricultural, the poor-rates are comparatively low. The true inquiry, therefore, would be, not whether pauperism has increased, with reference to the entire mass of our population, but whether it is increased with reference to that part of it which furnishes the regular supply of indigence. On that comparison, which is strictly fair, we may safely assert that it has not increased; but, on the contrary, very considerably diminished.

Yet it is on this false assumption of the rapid and constant advance of the poor-rates, that Mr. Malthus, and a majority of the members of both Houses of Parliament, have founded their alarming list of grievances, and their still more alarming remedies. The irfundamental propositions are—that our system of parochial relief tends, inevitably, to create and extend the evils it professes to remove; that it destroys all self-respect, and extinguishes the spirit of independence amongst the poor; and that pauperism, when thus sustained, possesses an inherent power of self-propagation so immense, that it must soon swallow up the great bulk of our wealth, power, and population. Mr. Malthus, who usually applies all the phenomena of society to his great problem, maintains, as I have mentioned before, that all the evils of a redundant population derive their worst aggravation from this source.

The simple statement just exhibited will show, on the contrary, that all these calculations and anticipations are purely illusory. The relative portion of our collective wealth, devoted to the relief of the poor, is *not* increased, but diminished. The self-dependence of the poor, and their salutary terror of overseers and workhouses, is *not*, if we are to believe the evidence of facts, extinguished, or even impaired. There is no reason to infer, from past experience, that the poor-laws operate as a bounty on pauperism; that the prospect of relief creates the necessity for it; or that our support of the poor furnishes the supply of poor to be supported. Is it not perfectly astonishing that enlightened men can found such inferences as these, and a thousand others, on an inattention to two circumstances so well known, as the change in the value of money, and the amount and character of our population.

If these opinions were purely theoretical, and, like a new system of geology or cosmogony, framed merely to occupy the speculations of retired philosophers, they might be safely committed to the lapse of that *dull oblivious stream* which swallows up, in turn, the errors of successive ages. But such tenets as these are neither intended nor calculated to lie idle. Mr. Malthus boldly proposes to found upon them the most important innovation that was ever attempted in civilized society, and it is plain that the House of Commons are fast arriving at that state of mind which can contemplate it without horror. He would instantly commence a gradual, but complete, abolition of the whole system of poor-laws, by a public declaration, that no child, born after a given period, should be entitled to parish relief, in any case whatever. He thinks this would strike at the root of all the existing evils. By recreating a spirit of self-dependence, and by deterring the indigent from improvident marriages, it would bring his preventive check into full activity. The prevalence of moral restraint would then diminish so materially the existing stock of poverty and misery, that private benevolence would easily supercede the necessity of public relief.

But before we determine upon this desperate project, we must prepare our minds and our senses for such trials as they have never yet undergone, even in contemplation. We must prepare to see our fellow-creatures perish before our eyes, by thousands, of famine and disease. The idea, that the prospect of parish relief operates as an incentive to marriage, or that the removal of all such hopes would act as a restraint upon it, are, in my opinion, equally fallacious. It would be quite as vain, I believe, to expect from that source any considerable improvement in the moral conduct of the poor, or in their economical and prudential habits. If I may presume to mention the result of my own experience amongst the poor, which is not inconsiderable, I should say, that the most remarkable and uniform feature by which they are characterized is their reckless and incorrigible improvidence;—their total inattention to the casualties of futurity. Speaking on sober calculation, I do not believe that any reflection on the existence or abolition of the poor-laws would, in any important point, influence the conduct of a poor man in one case out of a thousand. Dr. Johnson's jocular account of the matter is, I believe, after all, pretty near the truth. "I am already as poor as I can be," (a young man says to himself,) "I cannot possibly be any worse, and so I'll even take Jenny." I would not, however, omit any justifiable means of augmenting and extending the preventive check; for though a system of emigration should be organized, complete in all its provisions, yet before this dernier expedient is resorted to, much previous suffering will always be endured. The suggestions of our townsman, Mr. Henry Booth, on this point, are highly deserving of attention. The necessity of restraining from such marriages as do not afford a reasonable prospect of providing for a family, ought, he thinks, to be inculcated by every practicable means, as a moral duty of the first class. He would have it enforced from the pulpit, and from the press; by private remonstrance, and public exhortations. None of these means, perhaps, ought to be rejected; but from none of them is any considerable effect to be expected. They must be estimated at a very small amount in any public or legislative measure which may hereafter be adopted.

The support actually awarded to the poor, though enormous in its aggregate amount, is barely sufficient to preserve them from immediate starvation. I use the expression *immediate starvation*, because it is *not* sufficient to prevent them from perishing by that lingering and imperceptible decline, which is frequently induced by excessive privations. Besides this scanty allowance, there is no other alternative but the workhouse; and their horror of both is so great, that, with respect to prospective influence, an entire abolition of parish relief would scarcely act upon their minds with any additional force.

A young couple, who marry with the ardent hopes and sanguine temperament of youth, disdain to contemplate the possibility of ever being reduced to depend on any other help than their own. It is a matter wholly aloof from all their calculations.

But the worst feature of Mr. Malthus's innovation is, that it reduces to one indiscriminate mass of immediate distress the profligate and the industrious; the young and the old; the sturdy beggar, and the blind and crippled mendicant. On this sweeping plan, a family, whose whole life has been an incessant course of steady industry, and who, on the approach of old age, are deprived of the fruits of their earnings by unforeseen misfortunes, and rendered incapable of labour by growing infirmities, are entitled to no more support from the country they have served and enriched, than the desperate spendthrift who never looked beyond the gratification of his appetites. The destitute widow, the hapless orphan, lameness, blindness, mental imbecility, casual insanity, and all the other innumerable infirmities of our common nature, which reduce the strength of manhood to the feebleness of infancy, are to be condemned to slow starvation, or to the forlorn hope of casual benevolence. I am well aware that any attempt to appeal to the charitable sympathies of our nature, in such a discussion, would be idle and impertinent. This grave question is not to be examined as a matter of feeling, but of calculation. I wish to make no appeal but to the results of plain facts and obvious experience.

The efforts of private charity, it has been alleged, would become so much more active and extensive by the abolition of the poor-laws, that they would quickly be found an efficient succedaneum for that pernicious system. There is, it has often been said, a fund of benevolence in the British nation, always adequate to every claim upon it which may successively arise. That this fund is very great, and that it seldom fails to rise with the occasion which demands it, I have had sufficient opportunities of witnessing. Without such an aid, many of the parishes of this kingdom, in the fatal winter of 1816, must have been half depopulated. But this resource is, in its very nature, precarious and incidental. As an auxiliary it may safely be depended on, but not as a principal. "He that runs against time," says Johnson, "has an antagonist not subject to casualties;" and the same may be said of him that strives against want. The supply and the demand being totally different in their nature, can never be made to quadrate with each other. They are things which are not in the same category. The most benevolent temper will become wearied of applications perseveringly continued, or impatient of claims which are never remitted, or negligent of wants which are diffidently urged. To count upon such a resource, as a regular and never-failing supply, correspondent to the cravings of human wants, would argue a gross ignorance of human nature, and of civilized society. In rural situations, where every individual instance of distress obtrudes itself on the notice of the neighbourhood, it might happen that private charity would often be found adequate to its object; but the case would be far otherwise in great cities. It is in these vast receptacles of poverty and crime that we are to seek for the deep and fathomless recesses of human misery. It is there where poverty retires to its cellar or garret to perish unseen; where no eye witnesses its decline, and no ear listen to its complainings. In great towns, the most unquestionable and self-evident claims could never depend on receiving that regular supply which, to the human frame, even for its essential wants, is absolutely indispensable.

But to all these objections, and many others which might be urged against the abolition of the poor-laws, a triumphant answer is given, by appealing to the instance of Scotland, and many other countries where no such system has ever been introduced. Here, say they, is a direct appeal to fact and experience, the only test which that class of reasoners who call themselves plain, practical men, will admit to be valid. That fact and experience are the surest guides of human conduct no one will attempt to deny; but we must always be sure that the experience we cite is applicable to the case in question, and that the facts which are forced into comparison are really analogous. The case of Scotland is, in many important respects, a complete anomaly in the history of nations. The system of parish education there established, diffuses amongst the lower classes those habits of mental application and moral restraint, which produce in manhood a character of prudence and self-command; for the sacrifice of the present to the future is at once the object and the means of all just education. This admirable institution, which has no parallelin ancient or modern times, could not fail to produce its proper effect, till the period when Scotland became a commercial and manufacturing nation. This is the true source, "Hinc illæ lachrymæ" of parish taxation. A system of poor-laws grows as naturally and necessarily out of the body-politic of a great commercial state, as *fungi* from a rich soil, or *tumours* from a diseased animal. This is the price and the compensation of our flourishing cities and our enormous trade. It is a price which cannot be evaded, without incurring the punishment which follows every gross violation of justice. In all the operations of nature, as well as in the affairs of man, a system of compensation is generally cognizable, and it is no where more palpable than in this instance. I do not mean to affirm that commerce and manufactures do not produce benefits which overbalance their concomitant evils, but merely to affirm that we cannot expect to receive these advantages pure and defecated. They not only, in their periods of prosperity, force up population beyond its natural level, and plunge it into distress by their perpetual vicissitudes, but they introduce every species of habit which is adverse to sober calculation and moral restraint. The great masses of people which they necessarily congregate, ferment with the leaven of intemperance and licentiousness, till the corruption becomes universal. Thus debilitated in body and mind, when the season of adversity arrives, it finds them for the most part helpless, and without resource. Nothing but the intervention of the poor-laws could save their families from the most wasting destruction.

This truth has been made evident even in Scotland, where trade and manufactures have already begun to produce their usual effects. In most of the populous districts, municipal regulations, analogous to the English poor-laws, have been generally established, and they are constantly extending themselves. A late eminent Scottish writer earnestly deprecates these fearful beginnings, and advises, at all hazards, to have the mischief checked in its bud. But such advice is vain, as well as pernicious. If we are determined to force the growth of our people in the hot-bed of our national wealth, we cannot abandon them in the weakness to which our processes may reduce them.

I am aware of only one more consideration on this subject which is deserving of much attention. Our contest with the poor-laws, it is said, is a "*bellum ad internecionem,*" and that we have to do with an enemy, which, if we do not destroy it, will eventually destroy us, and afterwards itself. According to the progress which the system is now making, we are told that, at no very distant period, the whole produce of the soil, and the productive labour of the country, must be absorbed in parochial taxation. This anticipation, I presume, is sufficiently removed by the calculations in a preceding part of this Essay, by which it is shown that scarcely any virtual increase has taken place in the poor-rates during the last half-century. Some persons, who are unwilling to go the whole length of abolishing the poor-laws, and yet are alarmed at the danger which they conceive to be impending, would limit the future amount of the poor-rates to a definite sum, which should not, in any case, be exceeded. Nothing could be more cruel and unjust than such a regulation, considering the fluctuations to which the value of money and the prices of necessaries are constantly liable. If, however, it is resolved to legislate further in this very difficult subject, I should prefer a statute which enacted that no more than a certain fixed proportion (the present amount of the poor-rates, for example) of *the annual rental* of the real property of the country should hereafter be devoted to the support of the poor. By such a law the danger of unlimited increase would be removed, and the changes would, in some degree, be provided for, which are constantly taking place in the value of money, of labour, and of commodities. I suggest this idea, however, with the greatest hesitation, not being, probably, aware of half the objections to which it is liable; but of this I am thoroughly satisfied, that the abolition of the poor-laws, in the present condition of the empire, cannot be attempted without the risk of greater miseries than have been witnessed in Europe since the revival of civilization, It cannot be too often repeated, that the great remedy for this evil, as for all other evils of modern society, is only to be sought for in the gradual and general education of the poor.

Proceedings of the Horticultural Society.

March 4th.

An account was read of the manner in which an orchard of cherries belonging to P. C. Labouchère, Esq., is protected from the attacks of birds. This orchard is of considerable extent, and is covered over completely with net-work, strained from poles to poles, which are placed among the trees; a noble instance of a disregard of cost in effecting a useful object. A paper was also laid before the Society upon the mode of training vines at Thomery, near Fontainebleau, where the famous grapes are produced that supply the Paris market. The method appears to consist in allowing the plants very little room to grow either with their branches or their roots, and in keeping the latter very near the surface of the ground; in the practice at Thomery, each vine is only allowed to occupy a space of about six feet, so that the walls are supplied by a multitude of plants instead of by a few, as with us. Several interesting varieties of seeds and cuttings were distributed; and the table was covered with a profusion of flowers and fruit. Among the latter, the most remarkable were some oranges from the open air, which had been produced in the garden of the Rev. J. L. Luscombe, upon trees protected in the winter with nothing more secure than wooden shutters.

March 18th.

A paper by Mr. Sharp was read, upon the advantages of heating hot-houses by the combination of steam and hot water. This it was proposed to effect by introducing steam pipes into troughs of water, by which means larger masses of heating fluid might be prepared at very considerable distances from the boiler. It was anticipated that in this method the advantage of permanent heat, which is attendant upon the use of hot water, might be combined with the power of heating rapidly and at points far more distant from the boiler than is practicable with water alone. The paper was illustrated by a fine model. Some asparagus of extraordinary size was exhibited; this had been procured in the garden of the Society in the open air, by heating ordinary asparagus beds with dung placed in the trenches, and putting wooden pipes about an inch in diameter over each sprout as soon as it made its appearance above ground. In this manner the shoots were twelve or fourteen inches long, and tender and eatable their whole length, a circumstance which never takes place in common cultivation. This plan is not, however, materially better than that of forcing the asparagus in open beds with dung linings, without the use of pipes.

April 1st.

A paper was read describing several new varieties of pears which had been raised by Mr. Knight. From this it appeared that the object of procuring fine-flavoured keeping pears, capable of bearing abundantly as standard trees, had been successfully accomplished; of the great importance of these varieties our successors will judge better perhaps than ourselves. Fresh specimens of the fine mountain Rhododendron of India, with scarlet flowers, were exhibited; they were from a conservatory, as have been all that have yet appeared in this country. This should convince the public that they have been deceived in supposing that this splendid variety will succeed in the open air in this country. It is true that the very name of Rhododendron seems to indicate something pre-eminently hardy, and it is also true that severe cold is endured by the Indian variety upon its native hills; but it must also be borne in mind that this cold, which is by no means so intense as that which we often experience, universally succeeds a season the isotherial temperature of which is almost tropical. The tables were covered with specimens of other flowers, and with choice fruits.

May 5th.

This was the first meeting after the unanimous re-election of the president and officers of the previous year. Mr. Knight was in the chair. Another fête was announced to take place at the garden on the 21st of June. Notices were read of the award of a number of medals of the Society by provincial horticultural societies. Among the subjects exhibited was an extraordinary fruit of the Madras citron, which had been raised by Mr. Wells of Redleaf. It was fully as large as a child's head, and excited much curiosity.

June 3rd.

Among a variety of beautiful flowers and fruit, with which the tables were ornamented, the most remarkable objects were some specimens of Persian melons, grown in the garden of Sir Thomas Frankland. These were of great beauty, and their flavour was as perfect as we imagine melons to be capable of attaining. It was stated that they also possessed the merit of being more easily cultivated than many of the Persian melons. When it is considered how far more beautiful and delicious these kinds are than the common red-fleshed, thick-coated, indigestible varieties with which our markets are supplied, it is really matter of surprise that the latter can find either cultivators or purchasers.

July 1st.

Mr. Knight laid before the meeting his observations upon the cultivation of the potatoe, the result of which was, recommending the plants to be very close in the rows, but the rows very distant from each other. He argued, that as it is a certain fact in vegetable physiology that the quantity of matter elaborated by the leaves and sent down by them towards the roots, depends upon their being exposed to as much light as they can have consistently with the due performance of their other operations, the placing such a plant as the potatoe in circumstances under which one half of the leaves is shaded and kept in comparative darkness by the other half, must of necessity be absurd. But by letting the plants be close in the rows, and the rows distant from each other, the greatest possible facility is given the plants for arranging their stems in such a way as to expose the whole of their leaves to the light. We have no space to enumerate the endless varieties of strawberries, pines, cherries, nectarines, raspberries, and flowers, with which the meeting room was ornamented.

July 15th.

We were much struck by the model of a bee-hive, which had been received from a Mrs. Griffiths, of New Brunswick, in New Jersey. It consisted of a square wooden box, opening at the bottom, and fixed upon a framed stand; a shallower box was adapted to the top of this, into which the bees were to work when the lower part was filled. The bottom of the box was in the figure of a truncated inverted pyramid, and sloped off so as to drain the interior effectually.

August 5th.

A long paper was read from Mr. Tredgold upon the theory and practice of applying hot water to heating stoves. The writer treated his subject in a philosophical, as well as practical manner, and fully described the mode to be followed of making the calculations required in determining the quantity of apparatus necessary to raise a house to a given temperature. We trust to have an opportunity of saying more upon this subject when Mr. Tredgold's paper shall have been printed in the Transactions of the Society.

August 19*th.*

An account was read from Mr. Knight of the method he practised in growing pine-apples without the aid of tan. It was stated from the chair that the paper had been accompanied by specimens of pines cultivated in the manner described, which were exceedingly well grown and high-flavoured. We chance to know this to be strictly true, having had an opportunity of seeing these pines; and we have no difficulty in stating that they were not only unexceptionable in every respect, but very uncommon specimens of excellent cultivation: they were handsome, heavy, well grown, extremely high-flavoured, and remarkably tender, which last quality all growers of pines know to be the most difficult of all to attain. We were particularly glad to hear this paper read, because it served at once to silence an ignorant clamour that has been raised against Mr. Knight's attempts at deviating from the routine of cultivation which certain persons have thought fit in their wisdom to prescribe. It has been pretended that pine-apples cannot be cultivated successfully without the aid of a tan-bed, as if there were some magic in that material, or as if they had such a medium to root in when wild. Nature has provided nothing for the support of pine-apple plants but heat, light, moisture, and the ordinary principles which all vegetables derive from the soil. These can be administered with the greatest accuracy artificially, and without a tan-pit; all that can be said of the latter is, that it is a clumsy contrivance to do that which we can effect far better without it. It may serve to screen the blunders of gardeners, or to save them trouble on one hand, while it certainly doubles both trouble and risk on the other; but nothing can be so absurd or unphilosophical as to say that such an agent is necessary to the cultivation of any thing.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

i. *Elementary View of the* UNDULATORY *Theory of Light. By* MR. FRESNEL.

[Continued from the Number for April.]

Of Double Refraction and Polarisation.

WHEN we throw a luminous pencil on one of the natural faces of a rhomboid of calcarious spar, it divides itself within the crystal into two other pencils, which follow different paths, and then present two images of objects seen through the rhomboid. This phenomenon has been distinguished by the name of double refraction, with many others of the same kind that are exhibited by other crystals, especially when they are cut into prisms, in order to render the separation of the images more sensible.

This bifurcation of the light, however, is not the most remarkable circumstance belonging to double refraction; each of the pencils, into which the incident rays are divided, is possessed of some singular properties which make a distinction between its sides. In order to describe the phenomena in question with precision, it is necessary to employ, and to explain, some particular expressions.

In such crystals, as exhibit the laws of double refraction in their simplest form, there is always a certain direction, about which every thing occurs in a similar manner on all sides; and this direction is called the axis of the crystal. It must not be considered as a single line: for there may be as many axes as there may be lines parallel to each other, and yet crystals of this kind are denominated crystals with a single axis, if, in all other respects, the optical phenomena are the same in all directions round it: so that the word is merely synonymous with a fixed direction. It must be supposed that the direction of the axis depends on the crystalline arrangement of the particles of the medium, and that it must hold, with respect to the faces, or their lines of crystallization, a determinate position, which is always the same for the same crystal, however it may be presented to the incident rays.

There are some crystals in which the perfect resemblance of all sides of the axis is not strictly observed, and in which there are consequently two particular directions more or less inclined to each, which are possessed of properties resembling those which belong to a single axis in the simpler form of the phenomenon: and these are called crystals with two axes; but we shall consider, in the first instance, crystals with one axis only, the optical properties of these being simpler and more easily understood.

A plane drawn through the axis, perpendicularly to the surface of the crystal, is called its principal section. The present object not requiring an explanation of all the different manners in which the rays of light are bent by the crystals, but merely of their mode of propagation in these mediums, and the optical properties which they acquire in them, we may suppose, for the sake of simplicity, that the incident rays are always perpendicular to the surfaces of the crystal, and contained in the plane of its principal section: and when it becomes necessary to study their progress in different directions with respect to the axis, we may imagine in each case that the surfaces of their admission and emersion are made perpendicular to these directions.

This being premised, we may observe, in the carbonate of lime, which has a very conspicuous double refraction, that one of the two pencils becomes oblique to the surface when the incident light is perpendicular; while the other proceeds without being bent, in the manner of ordinary refraction: and this ray is considered as *ordinarily* refracted, and the former *extraordinarily*: the pencils are also called respectively *ordinary* and *extraordinary*; and the images, which they form, *ordinary* and *extraordinary* images. A similar bifurcation takes place under the same circumstances in other doubly refracting crystals, such as rock crystal, but the separation is so slight that a considerable thickness is required to render it sensible. It becomes more easily observable, when the crystal is so cut, that the surface of emersion is inclined to that of admission, which causes the two pencils to emerge at different inclinations, and so become further separated as they proceed. But without entering into the details of experiments, which establish the laws of double refraction, it will be sufficient to explain the principal results to which they have led.

It is remarkable, in the first place, that, when the incident rays are perpendicular to the surface of the crystal, the deviation of the extraordinary pencil always takes place in the plane of the principal section; and in the next place, that this deviation vanishes whenever the pencil is either parallel or perpendicular to the axis.

It has been demonstrated by observation, that when the rays are parallel to the axis they not only follow the same direction, but pass through the crystal with the same velocity; and it is when they are perpendicular to the axis that their velocities differ the most, although they follow the same path. The velocity of the propagation of the ordinary rays is the same in all directions: and for this reason they are subject to the ordinary laws of refraction. The velocity of the extraordinary rays is different according to the angle which they make with the axis, and this velocity is determined, in the system of undulation as well as in that of emanation, from the flexure which they undergo at their admission or emersion in oblique directions, which enables us to find the proportion of the sines of incidence and refraction. The experiments of Huygens, of Dr. Wollaston, and of Malus, on the carbonate of lime, and the numerous observations of Mr. Biot, on rock crystal, in which the angular measures of double refraction have been carried to the greatest possible precision, demonstrate that the difference of the squares of the velocities of propagation of the ordinary and extraordinary rays is proportional to the square of the sine of the angle made by the extraordinary ray with the axis, if we compute the velocities according to the doctrine of emanation, as the celebrated author of the Mécanique Céleste has done: and in the theory of undulations, this same ratio is observed in the reciprocals of the squares of the velocities; for the velocities are always reciprocally related in the two systems. This important law, the discovery of which is due to the genius of Huygens, affords us, as its consequences, the facts which have been explained: the two kinds of rays possess the same velocities in the direction of the axis, because in this case the sine vanishes, and the difference of the velocities increases gradually with the sine, as we go further from the axis, until it becomes greatest in the direction perpendicular to it.

This difference of velocity is positive in certain crystals, and negative in others; that is to say, in the one class the ordinary rays advance more rapidly than the extraordinary, and in the other less rapidly. The carbonate of lime, or calcarious spar, affords an example of the first case, and rock crystal of the second.

Such being the general principles of the progress of the ordinary and extraordinary rays, we may now return to the physical properties which they exhibit after their emersion, when they are made to pass through a second crystal, capable, like the first, of dividing the light into two separate pencils. It may here be remarked, that the word *pencil* will be employed for a system of waves separated from another by difference of direction, or simply of velocity, though properly borrowed from the system of emanation, as implying a *bundle* of distinct rays.

We may first consider the state of the ordinary pencil which has been transmitted through a rhomboid of calcarious spar: and which, upon being transmitted through a second rhomboid, produces two new pencils of equal brightness, when the principal section of the second rhomboid forms an angle of 45° with that of the first: in all other positions the two pencils, and the images which they form, are of unequal brightness, and one of them even vanishes entirely when the principal sections are parallel or perpendicular: when they are parallel, the extraordinary image vanishes, and the ordinary image attains its greatest brightness; when perpendicular, the ordinary image disappears, and the extraordinary acquires its maximum of intensity. The extraordinary pencil, on the contrary, transmitted by the first rhomboid, exhibits exactly contrary appearances in passing through the second rhomboid: the ordinary image, that it affords, vanishes when the principal sections are parallel, and becomes brightest when they are perpendicular; and then the extraordinary image vanishes. Thus each pencil is unequally divided, except in the case when the sections make an angle of 45° with each other: but when they are either parallel or perpendicular, each of them will undergo a single refraction only, which is the same with the former when the sections are parallel, but of a contrary nature when they are perpendicular to each other.

It follows from these facts, that the two pencils, produced by the double refraction, have not the same properties in various directions about their axes or lines of motion, since they undergo sometimes ordinary and sometimes extraordinary refraction, accordingly as the principal section of the second crystal is directed parallelly or perpendicularly to another given plane. Supposing, then, that we draw right lines perpendicular to the rays in these planes, and conceive them to be carried by the system of waves in its progress, they will show the direction in which it exhibits opposite optical properties.

The name of polarisation was given by Malus to this singular modification of light, according to a hypothesis which Newton had imagined in order to explain the phenomenon: this great mathematician having supposed that the particles of light have two kinds of poles, or rather faces, enjoying different physical properties: that in ordinary light the similar faces of the different particles of light are turned in every imaginable direction; but that, by the action of the crystal, some of them are turned in the direction of the principal section, and the others in a direction perpendicular to it, and that the kind of refraction, which the particles undergo, depends on the direction in which their faces are turned. It is obvious, that some of the facts may be explained according to this hypothesis. But without particularly discussing it, and showing the difficulties, and even contradictions to which it leads, when closely examined; I shall only observe, that the differences of the optical properties exhibited by the two pencils, in directions at right angles to each other, may also be comprehended by supposing *transverse* motions in the undulations which would not be the same with respect to different directions: as they would if the particles of the medium oscillated backwards and forwards in lines perpendicular to the directions of the rays. But it is better to abandon all theoretical ideas of this kind until we have entered more fully into the phenomena.

It is not merely by passing through a crystal, which divides it into two distinct pencils, that light receives this remarkable modification; it may also be polarised by simple reflection at the surface of a transparent body, as Malus first discovered. If we throw on a plate of glass a pencil of direct light, inclined to the surface in an angle of about 35°, and then place a rhomboid of calcarious spar in the way of the reflected ray; we remark, that the two pencils into which it is divided by the crystal, are only of equal intensity, when the principal section of the rhomboid makes an angle of 45° with the plane of reflection, and that, in all other cases, the intensities of the two images are unequal: this inequality is the more sensible, as the principal section is further removed from the angle of 45°, and finally, when it coincides with the plane of incidence, or is perpendicular to it, one of the two images disappears: the extraordinary image in the former case, and the ordinary in the latter. Thus we see that the light reflected by glass, at an inclination of 35°, is similarly affected with the ordinary pencil, transmitted by a rhomboid with its principal section in the direction of the plane of reflection. The reflected pencil is said to be polarised *in the plane* of reflection; and in the same manner the ordinary pencil transmitted by a rhomboid is said to be polarised in the plane of the principal section of the crystal; and we are obliged to say, on the other hand, that the extraordinary pencil is polarised perpendicularly to the principal section, because it exhibits in that direction the same properties which the ordinary pencil possesses in the plane of the section.

The surface of water completely polarises light by reflection at the angle of 37°; and at the surface of other transparent bodies, in general, when the incidence is such that the reflected may be perpendicular to the refracted ray. For the discovery of this remarkable law, we are indebted to Dr.

Brewster. We are not yet certain whether this law is rigorously correct, or merely an approximation; but the latter supposition seems to be the most probable.

At other incidences, the polarisation is only partial; that is to say, in turning the rhomboid round, the image never wholly disappears. The images vary indeed in brightness, but their minima, which always correspond to the directions of the principal sections, do not become equal to nothing. In short, when the incident rays are perpendicular, or nearly parallel to the surface, the reflected light no longer exhibits any traces of polarisation; that is to say, the two images are always of equal intensity in every position of the rhomboid.

Many opaque bodies, which are not too highly refractive, such as marble, and black varnishes, are capable of completely polarising the rays which are regularly reflected at their surface; while other bodies perfectly transparent or semitransparent, but highly refractive, such as diamond and glass of antimony, never polarise it completely. But the metals are the least capable of polarising the light which they reflect, even in the most favourable circumstances. It is to be remarked, that the incidences, which correspond to the maximum of polarisation, approach so much the more to the surface as the reflective body is more refractive; if at least we may judge by the abundance of light reflected, when the body is completely opaque, like the metals.

Transparent bodies do not polarise light by reflection only, but by refraction also, and the more completely as their surface is the more inclined to the rays; but it is never completely polarised in this manner, unless it is caused to pass through several polarised plates in succession: and so many the more plates are required as they are the less inclined to the incident rays. Malus, to whom we are also indebted for the discovery of this mode of polarisation, demonstrated that the transmitted light is polarised in a direction opposite to that of the polarisation of the reflected rays; the one being polarised in the plane of incidence, the other perpendicularly to this plane. Mr. Arago has found, by some ingegenious experiments which afforded him a very correct test, that the quantity of light polarised by reflection, at the surface of a transparent body, is always equal to that which is polarised by refraction. The enunciation of this remarkable principle may be made still more general, if we say that whenever light is divided into two pencils, without any absorption, the same quantity of light, that is polarised in the one, is found to be polarised in a perpendicular direction in the other.

Having now studied the principal means of polarisation, we are next to apply ourselves to the singular phenomenon presented by polarised light, when it is thrown on the surface of transparent bodies; and it is to Malus also that these important discoveries are due. We have seen that the light reflected by glass at an angle of 35° was completely polarised: this property is universal, and independent of any anterior modifications of the light; and, in fact, light which has been polarised in any other manner is always found, like common light, after the reflection, completely polarised in the plane of incidence. Now we have remarked that a polarised pencil exhibited but one image in passing through a rhomboid of calcarious spar, the principal section of which was either parallel or perpendicular to its plane of polarisation; that is, the ordinary image in the former case, and the extraordinary one in the other; or the image of which the plane of polarisation coincides with the principal section: hence a pencil polarised in one plane cannot furnish, by any immediate subdivision, an image polarised in a plane perpendicular to it: and, generalising this principle, we must conclude that a polarised pencil, thrown on glass at an inclination of 35°, with a plane of incidence perpendicular to its plane of polarisation, is also incapable of furnishing any light polarised in the plane of incidence, since this is perpendicular to its own plane of polarisation: but the rays reflected at an inclination of 35° are always polarised in the plane of incidence; consequently the incident pencil, which is polarised in a direction perpendicular to this plane, can afford no reflection. This conclusion was justified by the important experiments of Malus; and in the case which we are considering, there is no reflected light, the whole being transmitted. But if, without changing the inclination of the plate of glass to the light, it be made to turn round the ray as an axis, and to assume different azimuths, reckoning the azimuth as the angle which the plane of incidence forms with the primitive plane of polarisation, as the word is used by astronomers: in these changes of azimuths it is observed that the reflected light begins to appear the more sensibly as the plane of reflection is further removed from that which is perpendicular to the former plane of polarisation: there is a maximum when it becomes parallel to this plane, and then the reflection diminishes till it disappears entirely, after half a revolution of the plate round the ray.

These phenomena are evidently analogous to those which have been observed in each of the two images produced by a polarised pencil which passes through a rhomboid of calcarious spar, when it is turned round the ray. It is also by the same formula that MALUS has represented, in both cases, the variations of intensity of the images and of the reflected light. If we apply the character i to the angle formed by the primitive plane with that of reflection, or with the principal plane of the double refraction to be considered; and if we call the maximum of brightness unity, the brightness of the image and of the reflected light will both be expressed by $\cos^2 i$.

We may examine this formula in the case of a polarised pencil passing through a rhomboid of calcarious spar; and making i the angle which the plane of polarisation of the ordinary image, that is, the principal section of the crystal, forms with the primitive plane, the angle formed with the plane of polarization of the extraordinary image will be $90° - i$; so that since $\cos^2 i$ represents the intensity of the ordinary image, that of the extraordinary image will be expressed by $\cos^2 (90° - i)$, or by $\sin^2 i$. When $i = 0$, $\sin^2 i = 0$; that is to say, when the principal section coincides with the primitive plane, the extraordinary image vanishes, and all the light passes to the ordinary image, because, in this case, $\cos^2 i = 1$. When $i = 45°$, $\sin^2 i$ and $\cos^2 i$ become equal each to $\frac{1}{2}$; and the two images are of equal intensity: lastly, when $i = 90°$, $\sin^2 i = 1$, and $\cos^2 i = 0$, which implies that the ordinary image vanishes, and all the light passes to the extraordinary image; and the same effects are repeated in the other quadrants. It is obvious that these consequences of the formula agree with the observations. In order that it should be considered as fully demonstrated, it would be necessary that it should be directly verified with intermediate values of i: but it has been subjected, in such cases, to several indirect criterions, which, without being perfectly decisive, very greatly increase the probability of its accuracy; besides that we are encouraged by analogy and by mechanical considerations to conclude that it is rigorously correct.

In examining the fundamental principles of the theory of undulation, we have found that the intensity of the light must be supposed proportional to the living force or energy of each undulation, or simply, for the same medium, to the sum of the squares of the forces of the different points of the undulation, and must consequently be proportional to the square of the common coefficient of these velocities: consequently if $\cos^2 i$ is the intensity of the light of the ordinary image, $\cos i$ is the common coefficient of the velocities of oscillation in this image, and represents their magnitude; and in the same manner, $\sin^2 i$ being the intensity of the light of the extraordinary image, $\sin i$ represents the velocity of the oscillations in the system of undulations which has undergone the extraordinary refraction. We see then that the decomposition of the velocities of oscillation of the primitive polarised pencil, which is resolved into two others at its entrance into the crystal, are proportioned exactly in the same manner as if the oscillatory motions, instead of being in the direction of the rays, were in a transverse direction, and either parallel or perpendicular to the plane of polarisation; for in this case the two velocities conceived to have been united, and to be separated, would be proportional to $\sin i$ and $\cos i$, according to the principle of the composition and resolution of the small motions of a fluid, which must be conformable to the laws of statics. The formula of MALUS appears, therefore, to imply, that the oscillatory motions of the ethereal particles are performed in directions perpendicular to the rays: and this hypothesis is rendered still more probable, by other remarkable properties of polarised light which remain to be explained.

Mr. Arago and myself, in studying the interference of polarised rays, discovered that they exert no influence on each other when their planes of polarisation are perpendicular to each other, that is to say, that in this case they produce no fringes, although all the conditions, which are commonly necessary for their appearance, are scrupulously fulfilled. I shall mention the three principal experiments which served to establish this fact; beginning with that which was made by Mr. Arago. It consists in causing the two pencils, emitted by the same luminous point, and introduced through two parallel slits, to pass through two very thin piles of transparent plates, such as those of mica, or of blown glass, sufficiently inclined to polarise almost completely each of the two pencils, taking care that the two planes, in which they are inclined, should be perpendicular to each other: in this case no fringes are observable, whatever pains we may take to compensate the difference in the paths, by causing the inclination of one of the piles to vary very slowly; although, when the planes of incidence of the piles are no longer perpendicular to each other, we always succeed in this manner in obtaining the fringes; and the same result is obtained with much thicker plates of glass, provided that proper care be taken to form and polish them very correctly; and to vary their inclination very slowly, in order that the fringes may not pass unperceived. In proportion as the planes of the two piles are further removed from parallelism, the fringes are weakened, and they wholly disappear when they are at right angles, provided that the polarisation of the rays have been tolerably perfect. It follows, from this experiment, that the rays of light, polarised in the same plane, interfere with each other in the same manner as rays not modified; but that this influence diminishes as the planes are separated, and disappears when they are at right angles.

A similar conclusion may be inferred from the following experiment. We take a plate of sulfate of lime, which, though Dr. Brewster has shown that it has two axes, yet, when divided into plates parallel to their common plane, affects the rays of light in the same manner as if it had one axis only in an intermediate direction; or a plate of rock crystal parallel to the axis, and of very uniform thickness; this plate we cut into two pieces, and place one of them on each of the parallel slits. Supposing now the halves to be so placed, that the edges which were separated remain parallel to each other, the axes will also be parallel; and in this case we observe but one system of fringes in the middle of the enlightened space, as before the division of the plates. But if we turn one of the pieces in such a manner as to destroy the parallelism of the axes, we form two other groupes of fainter fringes, one on each side of the former group, and completely separated from them, in the white light, when the plates of either crystal are about the twenty-fifth of an inch in thickness: and it is to be remarked, that the number of breadths of the fringes comprehended between the middle of one of these groups and that of the central group is proportional to the thickness of the plates, for crystals of the same nature, or in which the double refraction is equally marked, as in rock crystal and the sulfate of lime. In proportion as the angle formed by the axes increases, the new groups of fringes become more and more distinct, and acquire their greatest brightness when the axes of the two plates are perpendicular to each other: in this case the central group, which had gradually become fainter, altogether disappears, and is succeeded by a uniform light; so that we must conclude that the rays which produced it by their interference, are no longer capable of acting on each other. It is easy to infer, from the situation of these fringes, that they were formed by the interference of the rays which had undergone the same kind of refraction in the two plates, since, having passed through them with equal velocities, they must have arrived at the same instant at the middle of the enlightened space, the plates being supposed to be of equal thickness, and to remain perpendicular to the light; and these central fringes were consequently formed by the interference of the ordinary rays of the first piece with the ordinary rays of the second; and by that of the extraordinary rays of the first, with the extraordinary rays of the second.

The two lateral groups, on the contrary, depend on the interference of the rays which have undergone different refractions in the two pieces ; and the ordinary rays moving the most rapidly in both the crystals which have been mentioned, the left hand group of fringes must be formed by the combination of the extraordinary rays of the left hand plate, with the ordinary rays of the right hand plate, and the reverse for the right hand group.

We have now to consider the direction of the polarisation of the pencils which interfere, in order to determine the effects of the polarisation on the interference. It is natural to suppose, that the polarisation must be, as in thicker crystals, in the direction of the principal section, and in a direction perpendicular to it: but since this supposition is contrary to an ingenious theory of one of our most celebrated natural philosophers, it is necessary to confirm it by an experiment ; which may be done by cutting one of the edges of the plates obliquely, and obtaining a prismatic separation of the rays, which may then be directly shewn to be polarised according to the supposition: and if this were not reckoned sufficient proof, we might obtain it from the consistency of the solution which it affords, with the first experiment of Mr. Arago. It follows of course, that when the axes are parallel, the rays of the same refraction are polarised by each in the same direction, and those of each refraction are capable of interfering respectively in the middle. When the axes were at an angle of 45° the rays of contrary descriptions were capable of producing some effects on each other, as well as the rays of the same description: so that there were three groups of fringes. Lastly, when the axes are perpendicular to each other, the rays of the same refraction are polarised in directions perpendicular to each other, so that the central group, which was formed by them, disappears, while the ordinary rays of the left hand plate are then polarised in the same direction with the extraordinary of the right hand, which causes the right hand group, produced by these rays, to attain its maximum of intensity: while the left hand group acquire the same magnitude from the opposite refractions.

There is a third experiment, which still further confirms the inferences, which have been drawn from the first. I took a rhomboid of calcarious spar, polished on two opposite faces, which were carefully made parallel, and sawed it perpendicularly to these two faces, so that I had two rhomboids exactly equal in thickness, and in which the paths of the rays were consequently of equal lengths at equal inclinations. I placed them one before the other, so that the rays which passed perpendicularly through the one passed in a similar manner through the other: the principal section of the one was also perpendicular to that of the other, so that there were only two pencils which pervaded them, the ordinary pencil of the first being extraordinarily refracted in the other, and the reverse. Now it resulted from this arrangement, that the differences of the paths depending on the different velocities of the ordinary and extraordinary rays, were compensated in each of the pencils. The pencils crossed each other in a very small angle, so that the fringes which they would have formed must have had a much greater breadth than was sufficient for their being visible; and yet, notwithstanding that all the conditions necessary for the production of fringes, in common cases, were strictly observed, I could never succeed in obtaining them. While I looked carefully for them, with a lens in my hand, I caused the direction of one of the rhomboids to vary slowly, moving it sometimes to the right, and sometimes to the left, in order to compensate for the effect of any difference in the thickness, if it existed: but although I repeated this trial a number of times, I still observed no fringes; and indeed they were not to be expected, considering the experiments which have been related, since the two pencils emerging from the rhomboids were polarised at right angles.

It was not for want of a correct adjustment that the experiment did not succeed; since I easily obtained the fringes, if I employed light which had been polarised before its entrance into the rhomboids, when the polarisation is again changed after its emersion. It is therefore completely demonstrated, by the experiments which I have just related, that rays polarised at right angles cannot exercise any sensible influence on each other; or in other words, that their combination always affords the same intensity of light, whatever may be the difference in the routes of the two systems of undulations which interfere.

Another remarkable circumstance is this; that when they have once been polarised in rectangular directions, it is no longer sufficient that they be brought back to a common plane of polarisation, in order to exhibit appearances of influencing each other. In fact, if in Mr. Arago's experiment, or in that which I have described after it, we cause the rays which have been transmitted by the slit, and which are polarised at right angles, to pass through a pile of inclined plates, we perceive no fringes, in whatever direction we turn the plane of incidence. In place of such a pile, we may employ a rhomboid of calcarious spar: and if we incline its principal section in an angle of 45° to the planes of polarisation of the incident pencils, so that it may divide the right angles which they form into two equal parts, each image will contain the half of each pencil; and these two halves, having the same polarisation in the same image, ought to produce fringes, if it were sufficient to restore the plane of polarisation in order to renew the mutual influence of the pencils. But it is impossible to obtain fringes in this manner, except when the light has been polarised in some one plane, before it is divided into two pencils polarised at right angles.

When, however, the light has undergone the preliminary polarisation, on the contrary, the interposition of the rhomboid restores the fringes. The most advantageous direction, that can be given to the primitive plane of polarisation, is that which divides into two equal parts the angle of the orthogonal planes, in which the light is at last polarised, because, in this case, the light is equally divided between them. We may suppose, to assist the imagination, that the plane of the primitive polarisation is horizontal, the planes of the two polarisations which follow being inclined to it in angles of 45°, the one upwards, the other downwards, so that they may be perpendicular to each other. We may obtain this orthogonal polarisation either by means of the two small piles employed in the experiment of Mr. Arago, or by two plates, with their axes in orthogonal directions, or with a single crystallized plate: and this last case is the best fitted for our purpose, the others affording only phenomena which are precisely similar.

In order to divide the light into two pencils which intersect each other in a small angle, and which are thus fitted to afford fringes, the apparatus of two mirrors is in general preferable to the screen with two slits, because it affords more brilliant fringes; besides, it has here the advantage of giving to the pencils the previous polarisation required for the experiment. It is sufficient for this purpose that the mirrors should be of unsilvered glass, and inclined about 35° to the incident rays; and care must be taken to blacken them on the back, in order to destroy the second reflexion. We place near them, in the path of the light, and perpendicularly to its direction, a plate of sulfate of lime, or of rock crystal, cut parallel to the axis, and a tenth or twentieth of an inch in thickness; inclining the principal section in an angle of 45° to the plane of primitive polarisation, which we have supposed to be horizontal. The apparatus being thus arranged, we shall only see a single group of fringes through the plates as before its interposition, and occupying the same situation. But if we put before the lens a pile of glass, inclined either to the horizontal or to the vertical direction, we shall discover, on each side of the central group, another group of fringes, which will be so much the more remote from it as the crystallized plate is thicker. If we substitute for the pile of glass a rhomboid of calcarious spar, of which the principal section is in a horizontal or a vertical direction, we observe, in each of the two images that it produces, the two additional systems of fringes which had been before formed by the interposition of the pile; and it is remarkable, that these two images are complementary to one another; that is to say, that the dark stripes of the one correspond to the bright stripes of the other.

We see in this experiment a new confirmation of the principles which are demonstrated by the foregoing. The rays, which have undergone the contrary refractions, cannot affect each other; because, when they emerge from the same plate, in the case that we are considering at present, they are polarised in orthogonal directions; and consequently the groups of fringes on the right and left cannot exist, unless we restore their mutual influence by reducing them to a common plane of polarisation: and this is done by the interposition of the pile of glass plates, or of the rhomboid. The fringes thus obtained are so much the more marked, as the two contrary pencils which produce them are more equal in brightness; and it is for this reason that they are best distinguished when the principal section of the rhomboid makes an angle of 45° with the axis of the plate. When this principal section is either parallel or perpendicular to that of the plate, the rays refracted ordinarily by the plate, pass intirely into one of the images, instead of being divided between the two; and all the extraordinary rays pass into the other image: so that the rays of the separate descriptions do not interfere with each other; and the additional groups disappear: each image presenting only those fringes which are derived from rays of the same kind, that is, those which form the middle group.

These two groups of additional fringes exhibited by polarised light, in the first position of the rhomboid, furnish one of the most accurate modes of measuring double refraction, and of studying its laws. In fact, their eccentric position depends on the difference of the paths of the ordinary and extraordinary rays which have passed through the plate, and we may judge of the number of undulations that the extraordinary rays of the pencil have been left behind the ordinary rays, by the number of breadths of fringes comprehended between the middle of the right hand group and that of the middle group: and this difference in the paths is still better measured by the interval comprehended between the middle points of the lateral groups, which is twice their distance from the middle of the central groups. It is most convenient to employ white light for these experiments: first, because it is the brightest, and secondly, because it renders the middle point of each group more easily distinguishable. It is true, that we only measure in this manner the double refraction of the brightest rays, that is, the yellow; but this is precisely the mean double refraction, and besides, that of the other rays differs in general very little from this. And if we compare the thickness of the plate with the difference of the paths thus formed, we may compute from it the difference of the velocities of the ordinary and extraordinary rays.

With this apparatus, Mr. Arago and myself made an experiment on a plate of rock crystal parallel to the axis; and the result of our measurements gave us the same difference in the velocities of the ordinary and extraordinary rays as Mr. Biot had found by direct observation of the divergence of their rays in prisms of the crystal. Mr. Biot's method is equally accurate with ours, when it is required to determine the refraction of crystals which have great powers of separation, such as carbonate of lime, rock crystal, and sulfate of lime: but the method furnished by diffraction is far preferable for substances in which the difference is less sensible: for if we take a pretty thick plate, we can determine the difference of the velocities of the two kinds of rays with a degree of accuracy almost unlimited; and it is not even necessary that the plate should have any considerable thickness, in order that a very high degree of accuracy may be attained: for it is easy to perceive a difference of one-fifth of an undulation, that is, of four millionths of an inch, in the lengths of the two paths. The same experiment might be applied equally well to the purpose of verifying, in the most delicate manner, the law of Huygens, as relating to rays passing very nearly in the direction of the axis.

The agreement, thus obtained, between our results and those of Mr. Biot, is sufficient to show the multiplicity of relations which the principle of interference establishes between those phenomena of optics, which appear at first sight to be the most diversified in their nature.

We have supposed the rays of light to be polarised in the same manner in these crystalline plates as in the thickest crystals, that is to say, that the rays which are transmitted by the ordinary refraction are polarised in the principal section, and the others in a direction perpendicular to it. This hypothesis, deduced from the most direct analogy, ought not to be abandoned, unless it were found to be in positive contradiction with the phenomena; and, in following the consequences, to observe what pencils ought to influence each other, and to produce fringes, we have always seen the results of observation agree with it. Besides, the plates employed in our experiments, being always at least four hundredths of an inch in thickness, were capable of having their edges cut obliquely, and producing by these means the separation of the ordinary and extraordinary pencils, which are then found polarised in directions parallel and perpendicular to the principal section. It is not at all probable that this mode of polarisation should be determined by the very slight inclination of the two faces of the crystal, which divides the light into two distinct pencils when this angle is of only ten degrees: in short, a prism of glass of an equal angle gives but a slight degree of polarity to the light by the obliquity of its surfaces, which, even if it were more considerable, would only cause a polarisation perpendicular to the plane of incidence. Thus, if we consider the polarising action of the prism of crystal as generally composed of two parts, the one depending on the inclination of its surfaces, and the other on its double refraction, we can only attribute to the latter a polarisation of the two pencils in the directions which are parallel and perpendicular to the principal section, and we must conclude that they undergo the same kind of polarisation when the parallelism of the faces prevents us from distinguishing them, since this parallelism makes no change in the laws of the double refraction.

These consequences, however conformable they appear to the rules of analogy, have, however, not been admitted by Mr. Biot, who supposes light to receive, in thin crystallized plates, and even in such as are a tenth of an inch in thickness, a form of polarisation wholly different from that which it exhibits when it emerges from a crystal thick enough to separate it into distinct pencils. The opinion of so respectable a natural philosopher was of sufficient importance to induce me to establish, by some new experiments, the true direction of the polarisation of the ordinary and extraordinary rays in crystallized plates: but the results which I have obtained were always conformable to the general analogy of double refraction.

Having placed the two halves of a plate of sulfate of lime, about one twentieth of an inch in thickness, before two slits cut in a screen, and turning these plates in such a manner that their axes were perpendicular to each other, I examined, by means of a rhomboid of carbonate of lime, the direction of the polarisation of each of the two groups of fringes which they produced. We have seen that the right hand group results necessarily, according to the known laws of interference, from the combination of the extraordinary rays of the right hand plate with the ordinary rays of the left, since these latter move the more rapidly in the sulfate of lime: this group must therefore be polarised perpendicularly to the principal section of the right hand plate, since this is the direction of the polarisation both of the ordinary rays of the left, and of the extraordinary on the right, according to the actual arrangement of the plates: and since, besides, direct experiments on the interference of rays polarised in any plane, show always that the fringes are polarised in the same plane. In the same manner, the group on the left, resulting from the interference of the ordinary rays on the right with the extraordinary rays on the left, will be polarised perpendicularly to the principal section of the plate on the left. Now these consequences of our hypothesis are perfectly conformable to experiment: for we find when the principal section of the rhomboid, placed before the lens, is parallel to the axis of the right hand plates, the ordinary image contains no other than the left hand fringes, and the extraordinary image the right hand fringes; and on the contrary, when the principal section of the rhomboid is parallel to the axis of the left hand plate, or perpendicular to that of the right hand plate, it is the left hand group that has disappeared from the ordinary image, and the left hand from the extraordinary.

We see that the ordinary and extraordinary rays are here distinguished, not by their direction, as when the crystal is cut into the form of a prism, but by the difference of their effects of interference. Thus, for example, in the space occupied by the fringes of the right hand group, which result from the interference of the extraordinary rays of the right with the ordinary on the left, there arrive at the same time ordinary rays from the right hand and extraordinary from the left, which, being polarised in a common direction, necessarily influence each other, but produce no sensible fringes, on account of the too great difference of their routes, or on account of the too great distance of the point from the central stripe, which for these two pencils is on the left hand: for we have seen that, in white light, it is only possible to distinguish a very limited number of fringes, beginning from the middle stripe, and that beyond the seventh or eighth order the combination of the two pencils produces uniform light only. The ordinary and extraordinary rays of each plate are always found together at the same point of the enlightened space; but some of them form sensible fringes, by their interference with rays of a contrary description coming from the opposite plate; while the others constitute a white light only; and from this distinction we are able to examine them separately, and to determine the direction of their polarisation.

When two pencils which interfere are polarised exactly in the same direction, the fringes which they form possess the same character: but when the directions of their polarisation form an acute angle with each other, the fainter fringes, which they now produce, appear to be polarised at once in both directions, since they disappear from the extraordinary image when the principal section of the rhomboid is turned either in the first or the second direction; one of the pencils being excluded in either case, so that the interference can no longer take place, and the light must remain uniform.

Having shown that these phenomena of interference confirm the general hypothesis, it remains to be proved that they are inconsistent with the ingenious theory of *moveable polarisation*, the fundamental principles of which it is necessary to explain.

Mr. Biot supposes, that when a polarised pencil passes through a doubly refracting crystal, of which the principal section is situated obliquely with respect to the primitive plane of polarisation, *the axes of the luminous particles*, which had been situated in this plane, undergo, at their entrance into the crystal, certain oscillations, which carry them alternately to the right and left of the principal section, sometimes arriving at the primitive plane, and sometimes at another plane situated at the same angular distance on the other side, or at the azimuth $2i$, calling the angle formed by the two first planes i. For example, if the principal section makes an angle of 45°, with the primitive plane of polarisation, the axes of the particles vibrate through an arc of 90°, which is now $2i$. Mr. Biot supposes that these oscillations are repeated a very great number of times before the particles attain a *fixed* polarisation, which arranges their axes, so as to make them either parallel or perpendicular to the principal section: and a thickness of some tenths of an inch, or perhaps of some inches, is required, according to this able experimenter, in order that the moveable polarisation should become fixed in the sulfate of lime, at least while the parallelism of the two surfaces prevents the separation of the ordinary and extraordinary pencils, which is always accompanied by the fixed polarisation. But when the faces are parallel, and the thickness of the plate does not exceed the limit, the particles of light which pass through it, instead of being polarised in the principal section, and in the direction perpendicular to it, are polarised either in the primitive plane, or at the azimuth $2i$, accordingly as the last oscillation of their axes was directed towards the first or the second plane, and this whether it was finished or only begun at the time of their emersion; at least, according to Mr. Biot, they are affected by the rhomboid which is employed for analysing the emergent light, as if their last oscillation had been finished. The time occupied by one of these oscillations, or the thickness of the crystal in which each of them is performed, is supposed to be constant for particles of the same nature, but variable in the different kinds of light, in proportion to the length of the "fits" [imagined by Newton.]

Let us now examine the consequences of this, and consider the case of the two halves of a plate of sulfate of lime, about the tenth of an inch in diameter, placed before two mirrors of black glass in the path of the reflected rays. Let us suppose that the mirrors, disposed in such a manner as to produce the fringes, are inclined in an angle of 35° to the rays which proceed from the luminous point, so that they may be completely polarised by reflection before their introduction into the crystallized plates, as in the apparatus already described: and let us suppose that the axes of the two plates are perpendicular to each other, and each make an angle of 45° with the plane of reflection. According to the theory of moveable polarisation, all the emerging rays must be polarised in a direction parallel or perpendicular to this plane, which is that of the primitive polarisation: thus each of the two groups of fringes, which are observed to the right and left, results from the interference of the two pencils polarised both in this plane, or both in the direction perpendicular to it: consequently, if the two groups of fringes could exhibit signs of polarisation, it could only be in one or the other of these orthogonal directions: now the experiment is as opposite as possible to this consequence, since it is precisely when we place the principal section of the rhomboid in one or the other of these directions, that the two images of each group possess the same intensity: and in order that one of them may vanish, it is necessary, on the contrary, that the principal section of the rhomboid should make an angle of 45° with these directions, that is to say, that it should be parallel or perpendicular to the principal sections of the two plates. When it is parallel to the left hand plate, it is the left hand group that disappears from the ordinary image, and the reverse. It is obvious that the direction of the polarisation is the same as in the experiment last related, in which the incident light had not undergone any previous polarisation, before it passed through the crystallized plates.

Thus, whether we employ direct or polarised light, the ordinary and extraordinary pencils into which it is divided in passing through a crystallized plate, are always polarised, the one in the plane of the principal section, and the other in a direction perpendicular to it.

We have hitherto employed plates not less than a twentieth of an inch in thickness, and we have constantly found, in the ordinary and extraordinary rays, the same direction of polarisation as they manifest when they are separated into distinct pencils. It was, however, interesting to ascertain also, by means of interferences, whether the same mode of polarisation was also to be found in much thinner plates, such as those which give colours to polarised light, when it is analysed at its emersion, by means of a rhomboid of calcarious spar: for it is this production of colours that led Mr. Biot to a contrary supposition. For this purpose, I took a plate of sulfate of lime, about one hundredth of an inch in thickness, which exhibited strong colours, and yet was in no danger of having the different groups confounded: and having divided it into two pieces, I placed them in the manner already described. The two groups of fringes, instead of being entirely separated, as they had been when the plates were three or four times as thick, were mixed a little in the intermediate space; but it was easy, nevertheless, to distinguish in each of them the stripes of the three first orders, and to ascertain that the right hand group, for example, was polarized perpendicularly to the axis of the right hand plate; for when the principal section of the rhomboid was turned in this direction, it disappeared entirely from the extraordinary image; and when, instead of the rhomboid, a pile of glass, sufficiently inclined in its direction, was placed before the lens, the left hand group only was discernible, and was in this case perfectly free from the mixture of the colours of the right hand group, exhibiting the usual appearance of a single group. And when the experiment was made with two metallic mirrors, the slight polarisation which they occasion in the reflected rays, being destroyed by a pile of three or four pieces of glass, properly inclined, before their passage through the plates, the same direction of the polarisation is still found for each of the groups of fringes. It is therefore fully proved, that in one of these cases, as well as in the other, the thin plates polarise the ordinary and extraordinary rays in directions parallel and perpendicular to their axes.

Having shown that the hypothesis of moveable polarisation is contradicted by facts, whenever it is possible to distinguish the ordinary from the extraordinary rays, I shall now proceed to a particular description of the phenomena of the colours of crystallized plates, which led Mr. Biot to this hypothesis, and I shall show that it is by no means necessary to their explanation.

[To be continued in our next Number.]

ii. *Principal* Lunar Occultations *of the fixed Stars in the months of November and December*, 1828, *and January*, 1829, *calculated for the Royal Observatory at Greenwich. By* Thomas Henderson, *Esq.*

Date.	Names of Stars.	Magnitude.	Immersion and Emersion. Mean Time.				Apparent Difference of Declination.			Point of Moon's Limb.
				H.	M.	S.	′	″		°
Nov. 21.	δ^2 Tauri	4.5	Imm.	12	30	55	15	41	N.	6 R.
			Em.	12	43	1	14	56	N.	27 R.
26	A¹ Cancri	6.7	Imm.	11	1	41	5	4	S.	150 L.
			Em.	12	8	2	0	3	S.	52 R.
Dec. 21	*u* Geminorum	5.6	Imm.	9	1	20	4	16	N.	112 L.
			Em.	10	12	9	4	26	N.	42 R.
25	π Leonis	4.5	Imm.	10	19	42	8	6	N.	96 L.
			Em.	11	3	56	13	14	N.	11 L.
Jan. 8	ϑ Aquarii	4.5	Imm.	6	1	47	11	36	N.	17 L.
			Em.	6	59	1	0	16	N.	123 R.
,,	ϱ Aquarii	6.	Imm.	7	39	47	10	35	S.	94 L.
			Em.	8	7	52	16	16	S.	151 L.
18	λ Geminorum	4.5	Imm.	8	50	21	10	41	S.	167 L.
			Em.	9	51	21	8	35	S.	104 R.

The explanations of the columns are the same as in the preceding Numbers.

An error of 11 seconds in the computed difference of declination between the moon and star will be sufficient to convert the predicted occultation of δ^2 Tauri on the 21st of November, into an appulse.

An occultation of *o* Piscium may possibly happen on the 18th of November; for by calculation at $15^h\ 30^m$ mean time, the star is only 27 seconds distant from the moon's southern limb. The star will be occulted to places further north than Greenwich.

[To be continued.]

MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *Tulley's New Catadioptric Microscopes.*—Mr. W. Tulley, stimulated by the example and success of Amici, has invented a reflecting microscope, the optical principle of which is, we believe, entirely new and original; the objective part consists of an elliptic metal, and a perforated plane one of corresponding dimensions, forming an angle of forty-five degrees with it. A section of a cylinder is introduced into the hole of the plane metal, in such a way as to allow light to pass towards the elliptic metal, but to exclude it in every other direction, so that no false rays can enter.

The manner in which the instrument operates is as follows:—The object to be viewed is placed nearly whole in the plane, through which its rays diverge towards the elliptic metal, from which they are reverberated to the plane metal, which reflects them at right angles to the eye-piece, by which the image they form is viewed in the usual way (the object of the section of the cylinder is to exclude false light). This instrument has its good and evil properties, like all others. It has been duly executed by Mr. Tulley, and may be considered in its optical principle and its performance, equal to that of Professor Amici, over which, indeed, it possesses the advantage of being capable of receiving an unlimited angle of aperture. The objection to it is that the diagonal metal does not permit the approach of an object to the focus of the elliptic one, with the facility necessary for practical purposes. Only very small objects can be viewed, which must be mounted in a particular manner, having to be introduced as it were into the external part of the hole in the plane, where no latitude of motion can take place; sliders, aquatic live boxes, &c. are wholly inadmissible: these defects, will we are afraid, confine this instrument to the cabinets of the curious. There is a great difficulty in executing the plane metal, for that part immediately about the hole must be perfect, and the sharp edges of the aperture are a great obstacle to correct execution (which, however, has been conquered by Mr. T.). There are impediments also in the adjustment of the plane, which is apt to lose its figure, being made as thin as possible to admit the approach of the object to it.

The other microscope forms its image by one reflection only. Opaque objects are mounted upon a small arm, and presented to the focus of an elliptic metal inserted at the end of a tube, at the other extremity of which the usual eye-glasses are placed, while the illumination is effected from an aperture in its side.

Transparent objects are illuminated by means of light, furnished by a small plane metal placed diagonally behind them, being in fact exactly similar to that which is employed in the Amician microscope, to co-operate in forming the image, but which is here used only as an illuminator, being removed beyond the focus of the concave metal.

Any Amician instrument may, of course, be easily modified into this form, for it will be merely necessary to draw its plane metal further back, and to perforate a fresh hole in the side of the tube to admit the light, with some additional contrivance to present the object in its proper place. This reflector, considered only with reference to its optical principle and performance, is at once the most simple and the most perfect of the whole family of compound microscopes, but has nearly the same inconveniences as the other, relative to the application of objects. To those who regard not the difficulty at which they procure perfect vision, this instrument must be highly valuable, and probably will long retain a place among microscopes as a *verificator* or proof engiscope; for there can be no doubt that the vision it affords is of the purest and most unadulterated nature.

2. *Carpenter's Aplanatic Solar Microscope.*—This is the first solar instrument which has ever possessed *achromatic object-glasses regularly worked to correct diverging rays.* The experiment of converting telescopic object-glasses of short foci to the purpose of forming an image for the solar microscope has been often made, but of course without any good effect; it is as rational to expect that such glasses should answer both for divergent and parallel light, as that the same medicine should cure a diabetes and a dropsy. The pictures of microscopic objects given by the present instrument are totally freed both from chromatic and spherical aberration, and in consequence of which the coloured fringe which forms the outline of all objects shown with common object glasses is removed, together with that nebulous indistinctness which causes the image to appear a *mere shadow when inspected closely*, and, therefore, fit to be viewed *only from afar.*

The observer *may boldly proceed up to the very screen* on which the picture is formed by the achromatic glasses, and will find that the image instead of losing by this close scrutiny developes those minute details which were invisible at a distance. But it is chiefly when opaque objects are viewed that the incontestable superiority of the achromatic shines forth in all its splendour (especially if contrasted with the effects of common glasses, which, it is well known, give an image of radiant bodies, which is a mere jumble of aberration of both kinds, not fit for public exhibition.)

Those who fancy that aplanatic glasses are no better adapted to the nature of a solar microscope than to that of a camera-obscura, would do well to examine Mr. C.'s instrument; in fact the public voice has already decided the question,

The frame of this instrument is on a gigantic scale, the illuminating lens being a foot in diameter, with everything else in proportion.

The immense body of light condensed together by it gives a wonderful richness and vivacity of colouring to the image which, combined with its sharply-defined outline and vast dilatation, distends the faculties with surprise and pleasure, frequently surpassing the most lively anticipation.

Upon the whole there would be nothing to wish for or to find fault with about Mr. C.'s instrument if it possessed aplanatic object-glasses of sufficiently short foci to develope the tissue of animalcules and other regular microscopic objects. Nothing certainly can exceed the perfection of its combined double treble achromatic, the power of which is also admirably adapted for exhibiting *large popular objects*, but far too low to show those curious and difficult minutia which gratify a connoisseur. Such an objective as that famous deep sextuple one lately worked by Mr. W. Tulley, which demonstrates the most difficult test objects with such incomparable facility, would complete the effectiveness of this really respectable and scientific engine of public instruction and amusement. This deficiency is shortly to be supplied.

3. *Improvement in the Barometer.*—An improvement has been made in the barometer of Gay-Lussac by M. Bunten, which has been submitted to, and received the approbation of, the Academy of Sciences at Paris. Its object is to prevent the introduction of bubbles of air, which almost inevitably takes place when the barometer is carried either on foot or horseback, or in a carriage, in a horizontal position. It consists in expanding the glass in one part of the wide tube, so as to form it into a chamber, from the centre of which a capillary tube of a certain length descends perpendicularly, by which the mercury must necessarily pass, either when rising or falling. If a bubble of air enters, it necessarily moves up by the surface of the large tube, and is stopped at the top of the chamber, producing no error in the observation made whilst it is there. When the barometer is inverted the bubble escapes of itself. This invention, the reporters observe, does away with the only inconvenience attending the use of Gay-Lussac's barometers, without adding anything to their fragility.—*Revue Ency*. xxxviii. 536.

4. *Effect of the Moon upon Barometric Pressure.*—M. Flaugergues has added his efforts to those of the persons who have endeavoured to ascertain the effect of the moon's attraction upon the atmosphere of our globe, endeavouring to elucidate the subject by a very close and continued series of barometrical observations made daily at mid-day since 1808. The column of mercury in the barometer was 2.46 lines in diameter, and in the cistern 37.89 lines in diameter. The height was marked off to the $\frac{1}{150}$th of a line, and corrections made for capillarity, variations of the external level, temperature of the mercury both in the tube and in the cistern. The following is the table of the mean height of the mercury drawn

up from daily observations from 19th October 1808 to 18th October 1827, a period of nineteen years. The observations were made in the observatory at Viviers, 2° 20′ 55.″5 longitude east of Paris, and 44° 29′ 1″ north latitude. The basin of the barometer was 56.78 metres (186.3 feet) above the level of the Mediterranean. Mid-day was chosen for the time of observation, because the height of the barometer is not sensibly affected at that time by the sun.

Lunar positions.	Number of Observations.	Mean height in millimeters.
General mean height	6915	755.44
New moon or conjunction	234	755.39
First octant	234	755.37
First quadrature	234	755.37
Second octant	235	754.65
Full moon or opposition	234	755.23
Third octant	234	755.70
Second quadrature	234	756.32
Fourth octant	235	755.48
Northern lunistice	258	755.73
Southern lunistice	258	755.42
Lunar Perigee	252	754.72
Lunar Apogee	252	755.82

The conclusions drawn by M. Flaugergues are—1. That the barometer rises from the second octant when it is lowest, to the second quadrature when it is highest, and then again descends to the first point: the total variation is 1.67 millimeters (.0657 of an inch). Thus in a lunar day, the barometer is lowest when the moon is 135° from the meridian towards the east; *i. e.*, 9 hours $18\frac{3}{4}$ minutes of mean time before its passage across the meridian, or 6 hours $12\frac{1}{2}$ minutes after its passage. 2. The action of the moon is stronger when its declination is southern than when it is northern, contrary to the theory of Laplace. 3. The difference between the actions of the moon at the apogee and perigee, is 1.1 millimeter of the latter greater than the former. The author finally concludes that the number of rainy days is greater when the barometric pressure is diminished than when it is augmented.—*Bib. Univ.*, *Dec.* 1827.

5.—*On the Arrangement of Water Pipes in Streets.*—The effect of temperature upon iron pipes, used for the conveyance of water, and also some other circumstances have been investigated by M. Girard: he has arrived at the following conclusions. 1. According to the effect produced by change of season and temperature upon pipes of this metal placed in subterraneous galleries, they altered in length for each centesimal degree (1.8 degrees of Fahr.), 0.0000985, a quantity about $\frac{1}{9}$ less than it would have been if they had not been confined on their supports by friction. 2. Although this effect is less when the pipes are put in the ground, it is still sufficient to occasion rupture, leakages, and other unpleasant accidents. 3. If the joints are not made by bolts, but one end of a pipe is inserted into the mouth of the next pipe, then the space for the interposed substance should be as small as possible, and the substance one which swells when in contact with water. 4. The length of the joints should be considerable, both to prevent the escape of water and the flexure of the system of tubes. 5. To ensure tightness, the stuffing should be confined between a ring fixed to the end of the pipe, and a moveable ring sliding on the tubes. 6. That this precaution may be dispensed with by laying the pipes down in the coldest part of the season. 7. That pipes put into the ground should be supported at intervals by firm props of masonry, to prevent those inflexions which otherwise occur, and form ruptures. 8. That in large towns it is advantageous to place these pipes in subterraneous galleries, either such as are made on purpose, or else in the sewers. 9. That galleries have been tried advantageously for 20 years, and therefore should be resorted to, that those derangements of the pavement and inundations from broken pipes which are consequent upon the ordinary mode of proceeding, may, from henceforth, be avoided.—*Globe, April* 16, 1828.

6.—*New Razor Straps.*—A new kind of razor strap, invented by M. Ferrot, has received the name of *euthegone.* From the flexibility of leather, a round edge is given to the blade, for which reason paper is used in the new strap. Two kinds of very fine paper have been manufactured purposely, with fine and homogeneous pulp, mixed in the one case with fine emery, and in the other with very fine rouge. These papers are then steeped in melted tallow, afterwards pressed to give them a smooth surface, and then cut into bands, and mounted on pieces of wood properly shaped. Each strap has therefore two faces, one gray, on which the razor may be rendered very sharp, and the other red, which, polishing the edge, renders it extremely smooth. The razor must be laid very flat upon these straps—they improve by a few days use. When ineffectual from age, the surface should be rubbed with a very smooth piece of pumice, or with a little pumice powder on marble or ground glass; being then wiped with a piece of cloth, they are brought to their first state.—*Bull. Soc. Encouragement.*

7.—*On the Fusion of Tallow.*—The Council of Health at Nantes has been engaged in an investigation of the best means of fusing tallow, so as to avoid the injury and annoyance which arises from an abundant liberation of vapours, when the ordinary method is used. Much pains has been taken in acquiring all the information possible, and numerous experiments have been made both on a large and small scale. The best process which the Council has instituted appears to consist in using, according to M. D'Arcet's suggestion, a certain proportion of sulphuric acid, and operating in close vessels. By the use of the acid, the fumes always evolved are very much altered and ameliorated in quality, at the same time that the fused tallow is improved in quality and increased in quantity, the fusion very much quickened, and the use of a press dispensed with. By the use of close vessels, the fumes evolved can be either conducted to a fire-place to be burnt; or, if that may be thought dangerous, in consequence of the occasional boiling over of the melting tallow, can be conducted into a condensing apparatus, which is found readily to condense them.

M. D'Arcet uses 100 parts of crude tallow in small pieces, 50 parts of water, and 1 part of sulphuric acid, sp. gr. 1.848. In some small experiments a digester was used, having a pierced copper plate near the bottom to avoid the necessity of stirring; 1500 (3lb. 5oz.) parts of crude tallow, 750 of water, and 124 of oil of vitriol were used, and the fumes conveyed by a pipe into a fire-place; half an hour's ebullition completed the fusion. The infusible matter when pressed in a cloth, weighed only 96 parts, and was slightly acid. The tallow was white, hard, and sonorous, and *not* acid. Without the acid, the same effect was not produced in an hour.

A tallow manufacturer then tried the experiment with 2 cwt. of tallow, using the acid, but operating in open vessels; 92 per cent. of fused tallow was obtained, and 8 of loss occurred: in the ordinary way, 15 per cent. of loss occurred. In a second large experiment with acid, only 5 of loss occurred. The residue does not require the use of a press, but cannot be made into cakes for cattle, unless previously freed from acid by washing.

Experiments made on the condensation of the vapour was found to succeed very well, and thus all fear of injury from fire is avoided. The Council propose conducting the vapours into the drains of the works and so condensing them there; no annoyance being apprehended from the occasional return of the vapours into the building, as that effect can be counteracted by the use of stink traps.—*Ann. de l'Industrie*, i. 295.

8. *Method of hardening Plaster Casts and Alabaster.*—The following process is described by M. Tissot who has patented it in France. The piece of plaster or alabaster after being shaped, is put for 24 hours into a furnace. If the piece is only 18 lines thick, 3 hours in the furnace, heated up to the temperature required for baking bread, is sufficient; if thicker, it is left in for a proportionably longer time. At the end of the time, it is withdrawn with caution and cooled, after which it is put for 30 seconds into river water, withdrawn for a few seconds, and then again immersed for a minute or two, according to its thickness. The piece is then exposed to the air, and at the end of three or four days, has acquired the hardness and density of marble. It may then be polished.—*Bull. Univ.* E. x. 26.

9. *Injurious Colours.*—The Government of Lombardy has issued

a law, which, under penalty of confiscation, forbids the use of any venomous substance, such as arsenic, zinc, lead, and other mineral colours in the printing or dying of fabrics which are intended for clothing, or may come in contact with the human body. Many cutaneous affections, it is said, of which the cause has hitherto been unknown, are occasioned by the absorption of deleterious dying substances.—*Nouveau Journal de Paris*, 1828, p. 2.

10. *Method of preventing Milk from turning sour.*—Put a spoonful of wild horse-radish into a dish of milk; the milk may then be preserved sweet, either in the open air or in a cellar for several days, whilst such as has not been so guarded, will become sour.

11. *Internal change in the Position of Particles in Solids.*—If a certain quantity of the prismatic crystals of sulphate of nickel be enclosed in a bottle and then exposed to the heat of the sun, it frequently happens that though their external form is preserved, so that they may be measured, yet if broken they are found formed of a multitude of octoëdral crystals with square bases. This change requires two or three days. These crystals by analysis appeared to contain 2.93 parts per cent. less of water, than the prismatic salt containing 7 proportionals. This is another striking instance of the internal motions of the particles of solid bodies.—Mitscherlich, *Ann. de Chimie*, xxxviii. 65.

§ II. Chemical Science.

1. *Conducting Power of Metals for Electricity.*—The following are the results of M. Pouellet's researches on this subject, and are highly interesting, especially as regards the effect of alloys on the metals; for even small quantities of foreign substances exert great influence on the conducting power. The purity of the silver is expressed by the proportion of pure silver per cent. present in the alloy; the column of figures represents the conducting power:—

Silver of 98.6 . . .	860	Red copper . .	224
Red copper . . .	738	Brass	194
Silver of 94.8 . .	656	Iron	121
Fine gold	623	Gold of 18 car. fine	109
Silver of 80 . . .	569	Platina . . .	100

M. Pouellet finds, 1. That the conducting power is very exactly proportional to the section of the wires from the smallest diameter to that of three lines: 2. That it is in the inverse ratio, not of the length of the wire, but of the length increased by a constant quantity λ. This quantity λ, unchangeable for various lengths of the same wire, changes with the nature of the metal, and for each metal is in the inverse ratio of the section of the wire. M. Pouellet therefore believes that the conductibility is truly in the inverse ratio of the length of the wires, provided that the resistance opposed to the electricity in traversing the fluid in the cells of the pile and the different conductors which carry it to the experimental wire could be taken into account.—*Bull. Univ.* A. x. 59.

2. *Conducting Power of different Fluids for Voltaic Electricity.*—The following table is drawn up from the experiments of M. Foerstemann. The first column of figures indicates the specific gravity; the second, the quantity of electricity conducted by the different substances in equal times; and the third, the time required for the conduction of equal quantities of electricity.

Muriatic acid . . .	1.126	2.464	0.410
Acetic acid . . .	1.024	2.398	0.423
Nitric acid . . .	1.236	2.283	0.438
Ammonia . . .	0.936	2.177	0.459
Sol. muriate of ammonia	1.064	1.972	0.509
Sulphuric acid . .	1.848	1.737	0.575
Sol. potash . . .	1.172	1.709	0.585
Sol. common salt . .	1.166	1.672	0.598
Sol. acetate of lead .	1.132	1.560	0.632
Distilled water . .	1.000	1.000	1.000

Kastner's *Archiv.* iv. 82. *Bull. Univ.* A. x. 49.

3. *Influence over the electric Powers of Metals.*—In addition to the cases which have been pointed out by Avogadro, Marianini, De la Rive, and others, of a change produced in the electric powers of metals by various circumstances, the following striking one by Van Beek may be quoted. A plate of copper and a plate of iron connected by a platina wire were immersed separately in two vessels containing sea water, the portions of fluid being communicated by moistened cotton. Things were left in this state for forty-seven days, during which time of course the copper underwent no change; after that period, the platina wire was cut, and it was then expected that the copper would be corroded, as will happen to a plate of copper put in the ordinary manner into sea water for a single day only. This effect however did not take place; the copper remained bright, and the liquid clear, against all expectation, even though the cotton also was removed, and continued so after twenty days. This effect was not because the water had lost its power, for a portion of it, in which a piece of ordinary copper had been immersed, caused corrosion in a single day; and the copper itself being put into another portion of sea water, was instantly attacked by it. Hence it appears that the preservation of the copper for so long a time without action on the sea water must have been due to the mutual condition of the metal and solution, induced under the previous circumstances of an existing electric current, and not altogether to a change either in the one or the other.—*Bib. Univ., Mars,* 1828.

4. *On the secondary Piles of Ritter.*—A long experimental memoir

by M. Marianini on this subject is terminated by the following conclusions, which give the result of his investigations. 1. The electro-motive power in Ritter's secondary piles is not produced by the difficulty which they oppose to the passage of the electricity; since they acquire a polarity contrary to that of the voltaic piles with which they have been put in communication; because the more rapid is the current the more readily do they arrive at a given state of tension, and also because in varying the nature of the plates the piles also acquire more or less readily a certain electro-motive power. 2. The developement of piles of the second kind in Ritter's columns have little or no influence on their activity, for on turning and changing the humid layers which would make them, the polarity of the whole is not changed. 3. The polarity of these secondary piles arises solely from the alterations produced by the electric current on the metallic surfaces in contact with the humid conductors; for the plates being washed and dried still preserve their power of putting electricity in circulation if moistened pieces of cloth be put between them. The fact offers a ready explanation of all the phenomena produced by these piles.—*Ann. de Chimie*, xxxviii. 40.

5. *Comparison of the Tourmaline and bad electro-conductors.*—M. Becquerel has been engaged in examining the effect of heat upon bad conductors of electricity, and comparing them with the effects of heat upon the tourmaline, for the purpose of elucidating the electric states of the latter body, and of bodies in general. Heat which diminishes the conducting power of metals, increases that of glass, gum-lac, and bad conductors of electricity. The effects which are produced when the temperature of glass is diminished, were those first examined. A small glass tube, .04 of an inch in diameter and between three and four inches in length was suspended to a fibre of silk and hung in a glass cylinder, the bottom of which was closed by a metallic plate: by heating the plate the temperature of the tube could be varied, and the effects observed. When the tube is dry and at common temperatures, it is powerfully attracted by an excited stick of gum-lac held near it; but leaving the tube exposed to air, it soon becomes damp, and the attraction ceases. If again heated to 68° or 77° Fahr. nothing happens; for the heat is not sufficient to dissipate the moisture on the surface; but then, on removing the lamp and allowing the temperature to fall, peculiar effects are noticed; for the tube is immediately attracted, and continues to be so as long as the cooling proceeds. Again, raising the temperature to 77°, the tube is not only attracted, but acquires two poles, which disappear on the removal of the electrified gum-lac; whilst on the contrary, when they are produced immediately on the removal of the lamp, they continue during the whole time of cooling. When the temperature is raised to 212°, polarity is not occasioned in the glass tube under the influence of the excited electric until the moment when the thermometer begins to fall. When raised to

302°, polarity is not manifested until the temperature has fallen 2 or 3 degrees. In all these cases, it remains to the end of the cooling; but if at any time the temperature of the air about the glass tube is raised 2 or 3 degrees, polarity disappears. These effects are analogous to those produced by the tourmaline under the same circumstances of heat, except that in the glass they are determined by the excited electric, and in the tourmaline by the peculiarities of crystallization.

A small cylinder of gum-lac used in place of the glass tube was rapidly attracted by the excited electric at the moment of cooling from 68° or 77°; but the polar state was very difficult to produce, and continued for a very short time.

In these experiments the excited electric should have a nearly constant state; one of the poles of a dry pile answers the purpose very well.

M. Becquerel considers the theory of M. Ampere as accounting for some of these effects very well, but not for the permanency of the poles during cooling. The theory supposes atoms to have an electricity which is proper to them; which, acting as neighbouring bodies, decomposes their natural electricity or electricities, attracting that of a different name, and repelling that of the same name, as in the Leyden bottle; then these atoms become surrounded by an electric atmosphere, which partly hides the electricity proper to each, &c.

M. Becquerel finds that the electric effects of tourmalines vary with their length, and he concludes, doubtless, with their breadth; so that the intensity of the electricity developed may well be considered as a function of these two quantities. Small tourmalines become more highly and readily electric than large ones. Large tourmalines which could not be electrized by heat alone, when broken gave fragments readily rendered electric. Admitting that this law is applicable, however much the size is reduced, it results that the integral molecule, should acquire an intense electric polarity for very feeble variations of temperature.

Some facts induce M. Becquerel to suppose that the colouring matter of tourmalines may modify their electric properties.—*Ann. de Chimie*, xxxvii. 369.

6. *Electro-magnetic Current from heated Fluids.*—M. Nobili connects the two ends of his galvanometer wire with the saline solution in two cups, and then these cups with two others containing more of the same solution, by bundles of moistened cotton. Two small cylinders of clay are then made, and, when necessary, connected with the cups by moistened cotton, so as to constitute the ends of the arrangement. When one of these cylinders is dried, then strongly heated, and suddenly thrust two or three inches deep into the other soft and moist cylinder, the needle of the galvanometer deviates as much as 80° from its natural position. This

experiment is considered as bearing upon the theory of the globe, or at least upon that theory which supposes a central fire, since, by contact with the cold, damp superficial parts, the fire may be the active agent in producing magnetic currents.—*Bib. Univ.*, 1828.

7. *Purity of Metals tested by the Galvanometer.*—Mr. Œrsted proposes the galvanometer as an indicator of the comparative oxidability of metals, and also of their comparative purity. The metals in contact with the ends of the galvanometer wire are to be brought together with a portion of some proper fluid between them, and the relation of the two used, one to the other, will be indicated of course by the way in which the needle moves, and the extent of the motion. The needle is to be governed not by the magnetism of the earth merely, but by two magnets placed at a proper distance.

The quantity of copper in an alloy of that metal with silver, may in this way be instantly ascertained. A series of metallic plates are to be prepared, from the purest silver down to mere copper, and of known composition; then the piece of silver to be tried is to be applied to one or another of these, and both to the galvanometer, until that plate is found which has the same electro-motive power with the piece of metal: they will then both have the same composition. Muriatic acid slightly diluted is the fluid to be used: it may be placed between the two metals by a piece of linen or well-washed amadou. The surfaces of the two pieces of metal for contact with the acid should be very clean, and exactly of the same size.—*Jahrb. der Chemie*, 1828.

8. *Construction of Magnetic Needles.*—According to M. de Legey, steel for magnetic needles should not be selected from amongst springs, for such steel is formed of fibres more or less hard, which, by the action of the hammer, has had different directions and unequal hardness given to them. M. Legey prefers German laminated steel plate, from which he cuts a strip in the direction of the length, and then draws it out, so as to close the pores, till it is very brittle. From this plate he cuts the lozenge intended for the needle. All the operations should tend to lengthen the fibres in parallel directions. The steel is then to be hardened, after which it is to be moderately tempered, then polished on the wheel, and finally magnetized to saturation.

Before magnetizing the needle, it is examined, and usually found to have two poles. Whatever may have caused them, M. Legey regards the needle as more apt to receive magnetism, according to the position of these poles, than in any other direction, and therefore endeavours to preserve them in every operation to which the needle is subjected; thus, in the polishing it should always be done in the direction of the length of the needles, and the southern pole should be held opposite to the course of the wheel; a proceeding which it is affirmed preserves the position of the poles. When the needle is magnetized, the same attention to its previous state is to be given.—*Bull. de la Soc. Encourage*, 1827, p. 249.

9. *Alteration of Brass Wire in the Air.*—M. Cagniard de la Tour stated to the Academy of Sciences, that when long brass wires were stretched for some days in the open air, especially in wet weather, they became so brittle as to break with great facility when bent to a moderately acute angle.

10. *New Solar Phosphori*, by M. Osann.—The solar phosphori, prepared in the following manner, are described as being far more powerful in their effects than those previously known:—

1. Oyster-shells are to be calcined; the whitest and most porous are to be selected, to be cleansed from all impurities, and then packed into a crucible in the following manner. The bottom of the crucible is to be covered with a thin layer of finely pulverized sulphuret of antimony, then an oyster-shell is to be put in, this is to be covered with more sulphuret, after which, a second shell is to be packed in, and so on, until the crucible is full. The powdered sulphuret should be spread uniformly by means of a fine sieve, and each layer of it should be about half a line in thickness. The crucible being closed, is then to be heated red hot for an hour. When cold, the upper and lower shells, if spotted, are to be rejected, and the rest preserved. When exposed to sun light, and then taken into a dark place, it shines brightly at every part, with a greenish-white light. A red heat applied for a long time causes the light to be white.

2. If the powder used be red sulphuret of arsenic (realgar), instead of antimony, the light of the phosphorus produced, after exposure to the sun's rays, is blue, like that of a sulphur flame. The phosphorescence is not so universal as with the preceding, but takes place only upon the white parts. Points occur here and there producing light of a fine reddish purple colour. If heated highly for a long time, the light produced by phosphorescence is then white.

3. Arseniate of baryta and gum, made into a paste, and heated to redness for half an hour, produces a yellowish-gray substance, which by phosphorescence yields a red light; if heated more than half an hour, the light is yellow; if for a long time, the light is white.

Weaker phosphori are produced by using the following substances with the oyster-shells: mosaic gold, light bluish; cinnabar, light yellow; white arsenic, light yellowish-blue; blend and sulphur, light bluish. All the phosphori may be preserved in jars closed by bladder; even in the air they do not change rapidly: three weeks' exposure did not diminish their power. When the lime falls to powder, their effects are diminished. Those prepared with antimony and realgar lose in the intensity of the colour when long exposed to light, so that they should be preserved in blackened bottles.

Cold favours the absorption of light; heat favours the dispersion; boiling water destroys the phosphorescence. Exposed to solar light for a minute, and then taken into a dark place, some Bolognian phosphorus shone for 4 minutes; the third of those above, for 34 minutes; the first (with antimony) for 149 minutes; and at this period that prepared with realgar shone as brightly as it did one hour before. A red heat applied for several hours destroyed the power of the realgar preparation, very much weakened that with antimony, but did not affect that of the arsenic compound.

The light of an electric spark passed one inch above these phosphori makes them luminous. These phosphori shone even in the daylight, but their light then appears white.—KASTNER'S *Arch. Bull. Univ.* A. x. 50.

11. *Preparation of Iodine by* M. Souberan.—The following is the process recommended by M. Souberan, by which he has obtained as much as $\frac{1}{80}$th part of iodine from mother liquors, that would yield none by the ordinary process. The mother liquors from the soda works, are to be diluted with 4 or 5 times their weight of water, and solution of sulphate of copper added, until precipitation ceases. The deposit will consist of iodide of copper and sulphate of lime, and is to be separated. Large iron filings, or turnings, are then to be put into the liquid and agitated, until all smell of iodine has disappeared, by which process, the remaining portion of iodine will separate as an iodide of copper, mixed with metallic copper and the iron turnings, but easily separated by washing over. These two precipitates are then to be acted upon separately, in one of the following ways: 1. The iodide is to be mixed with two or three times its weight of peroxide of manganese, and a sufficient quantity of concentrated sulphuric acid, and then distilled, when all the iodine will rise with some aqueous vapour; or 2. The mixture of iodide and oxide of manganese is to be heated in a retort to a high temperature, when pure iodine will come over; the residue is pulverulent, and can easily be extracted without breaking the vessel.—*Ann. des Mines*, N. S. iii. 102.

12. *Action of Ammonia on heated Metals.*—We gave an account in our last volume of M. Savart's experiments on this subject. M. Despretz claims the honour belonging to a prior discoverer on the following points. The diminution in density of copper, iron, and platina, after these metals have been employed in decomposing hydrogen gas. This fact he had published in his lectures, and in printed leaves, as early as December, 1827; and also the following, that during the decomposition of bi-carburetted hydrogen by heat, fusible white crystals were obtained, volatile at a low temperature,

M. Savart then stated, and proved by the evidence of M. Dulong, Chevreul, and others, that his results were obtained in August and September, 1827.

13. *Method of collecting Air for Analysis.*—Chemists frequently have occasion to collect air from particular situations, for the purpose of analyzing it. When the air contains no substance soluble in water, a bottle filled with water being opened in the place, and the liquid poured out, becomes filled with the air, and may then be closed. If there be gases or vapours present, which act on, or are dissolved in water, as sulphuretted hydrogen or carbonic acid, then mercury is usually employed in place of water. M. Gaultier de Claubry thought that some saline solution might be found, which having little or no solvent power over these substances, might be used for the purpose, and ultimately found such a one in a saturated solution of sulphate of magnesia, made by dissolving in 1 part of water, 1 part of the crystallized salt, or half a part of the anhydrous salt, using a slight elevation of temperature for the purpose, and then allowing the liquid to cool. Experiments were made on the solvent power of this solution, and also of water and saturated solutions of sulphate of soda and nitrate of potash; the two latter were scarcely better than water, for being mixed with their bulk of carbonic acid, they dissolved nearly eight-tenths of it, whilst the sulphate of magnesia solution dissolved only two-tenths; and when tried with sulphuretted hydrogen, they dissolved above nine-tenths, and the sulphate little more than five-tenths.

Mixtures of air with a few hundreths of these two gases when agitated with the solution of sulphate of magnesia, lost very little of the gases, and only with difficulty. A bottle filled with the solution, and then opened in such mixtures of air, was filled with the mixture without any sensible change being produced on it by the solution. In experiments made at certain sewers at Paris, where air had to be obtained from depths and situations to which men could not pass, the use of the solution was found to give the same results as the use of mercury.

Sulphate of magnesia is a cheap salt, and may, therefore, be very useful in these and similar circumstances.—*Ann. de Chimie.* xxxvii. 380.

14. *On the Hypo-phosphites.*—M. Rose has been engaged in a general investigation of these salts, and his memoir is inserted in the *Annal. der Physik und Chemie*, 1828, p. 77. The hypo-phosphites of lime, baryta, and strontian, may be prepared by boiling the earths with phosphorus and water. In preparing that of lime, the phosphorus should not be added before the milk of lime boils, and the operation should be continued until all the phosphorus has disappeared, and the peculiar smell has ceased. Carbonic acid is then to be passed through to separate the excess of caustic lime, the

insoluble parts separated by the filter, and the solution evaporated under the air-pump, or in close vessels by heat. It then crystallizes with more or less water, according to circumstances, those obtained by heat having the least.

The hypo-phosphites of baryta and strontia may be prepared in the same way, and have the same general properties: these earthy salts are *insoluble* in alcohol.

The alkaline salts of this class may be made either directly or by mixing hypo-phosphite of lime with excess of the alkaline carbonate, filtering, evaporating to dryness, and digesting in alcohol, the alkaline hypo-phosphites are *dissolved.* The *potash* salt is the most deliquescent salt known to M. Rose, but may be dried under the air-pump by sulphuric acid; the soda salt is less deliquescent, and crystallizes in rectangular prisms. The salt of ammonia differs a little from the others, principally in the action of heat; for whilst all the other salts when heated first lose water, and are then converted into phosphates with the evolution of phosphuretted hydrogen, this allows ammonia to rise, and there remain hypo-phosphorus acid and water, which ultimately produce phosphuretted hydrogen and phosphoric acid.

By boiling the oxalate of magnesia for a long time with the hypo-phosphite of lime, filtering and evaporating the liquid, large regular nacreous octoëdral crystals of the magnesia salt were obtained, containing 54.92 per cent. of water. The salts of *alumine* and *glucina* appeared, when dry, like gum. Crystals of the cobalt salt were most readily obtained, and were exceedingly beautiful; they were red, octoëdral, efflorescent, and contained 49.35 per cent. of water.

All the hypo-phosphites are soluble in water.

The hypo-phosphorus acid was obtained pure and in quantity, by boiling the hydrate of baryta with water and phosphorus, until all garlic odour ceased; filtering the liquid, and decomposing it by sulphuric acid in excess, separating the precipitate, and digesting the clear fluid for a short time with an excess of oxide of lead; then filtering the sulphate of lead from the solution of hypo-phosphite, and decomposing the latter by a current of sulphuretted hydrogen. The acid freed from the sulphuret of lead was then concentrated, until strong enough to form the required salts.—*Bull. Univ.* A. ix. p. 37.

15. *On Pyrophorus.*—M. Gay-Lussac has made certain experiments on pyrophorus, with the intention of determining its nature more distinctly than has yet been done, and has arrived at a method of preparing one much more powerful than that ordinarily made from alum, yet producing its effects in the same way. A mixture of alum and lamp-black, heated in an earthenware retort, gave at first a mixture of nearly equal parts of carbonic and sulphurous acids, then pure carbonic acid gas; at a later period oxide of carbon appeared, and

ultimately predominated: small quantities of other unimportant substances were formed. The pyrophorus obtained burnt well, evolving much sulphurous acid, hence a poly-sulphuret of potassium must have been produced. The excess of sulphur must have been derived from the sulphuric acid of the sulphate of alumina, and evidently from the latter portions only. Evidently no free potassium can exist in the pyrophorus; and the same fact is proved by the action of water upon it, which produces no hydrogen.

Charcoal is not necessary to the pyrophorus, for when only so much was used as was sufficient to decompose the alum sufficiently, without leaving any excess in the pyrophorus, still the latter burnt well, and left a grayish-white residue. Nor is the alumine necessary; for using an atom of sulphate of potash, and 3 of sulphate of magnesia, instead of alum, good pyrophorus was produced.

Considering that the alumina and the magnesia were merely useful in dividing the sulphuret of potassium existing in pyrophorus, which is the essentially active ingredient, and that their places might be filled by charcoal itself; a mixture of 1 atom of sulphate of potash and 4 of lamp-black was made and heated; only an agglutinated sulphuret was obtained, which did not inflame in the air; but, doubling the proportion of charcoal*, a pyrophorus was obtained, so combustible, as not to bear transferring without difficulty and danger.

This pyrophorus produced no sulphurous acid during combustion, but a neutral sulphate of potash: dissolved in water it gave, with acids, sulphuretted hydrogen, and a deposit of sulphur. Hence it is not a single sulphuret of potassium, but a poly-sulphuret, and, consequently, a part of the potassium is uncombined with sulphur. This potassium is not free, for no hydrogen is produced in water, and therefore it must be combined with oxygen; nor, indeed, does the pyrophorus require a moist atmosphere, but burns equally well in dry air. The charcoal does not appear to be in combination either with sulphur or potassium, for the solution in water does not differ from that of a sulphuret without charcoal; and the charcoal which falls to the bottom of the solution has not that state of division which indicates an anterior state of combination.

Sulphate of soda in equivalent proportions gave an equally powerful pyrophorus, but sulphate of baryta did not. The new pyrophorus owes its superiority over that ordinarily prepared, to its more intimately divided state; to the absence of inactive earthy matter; and the smaller proportion of sulphur.

A high temperature did not improve this substance; an ordinary portable furnace was used, but great care was taken to prevent the access of air during the cooling.—*Ann. de Chimie*, xxxvii. 415.

* By weight about 1 of lamp-black and 2 of the sulphate of potash.

16. *Test of Potash by Nickel, before the blow-pipe.*—For an account of this test, see page 483 of the last volume. Berzelius commends it:—he says it is only necessary to dissolve the oxide of nickel in borax by heat, and to add to the vitreous matter before the blow-pipe a little nitre, felspar, or other body containing potash, to obtain instantly a blue glass. The presence of soda does not prevent this action. The nitrate or oxalate of nickel may be employed, but the absence of cobalt must be ensured.—*Ann. der Physik.*

17. *New Variety of Borax.*—Since M. Payen has published his account of a new kind of borax (see page 483 of the last volume), M. Buran has laid claim to the discovery, and a contest has arisen which could only be settled by a committee appointed on purpose. It appears that M. Buran has known the method of manufacturing this kind of borax and its nature for some years, but kept it a secret because it gave him a commercial advantage; such borax being in request by jewellers and others as he, until lately, had the exclusive power of making:—and so it is settled that M. Buran has the priority of the commercial discovery, but that M. Payen should be honoured as the true discoverer, since he only has advanced the interests of science by making that known to others which was not known to them before.

M. Buran has observed and now made known a curious fact, namely, that ebullition for many hours is necessary to obtain a large quantity of the octoëdral borax, a fact fully confirmed by the experiments of the commissioners. They also find that when the octoëdral borax is dissolved, it tends to give more octoëdral borax without boiling than ordinary borax under the same circumstances.—*Ann. de Chimie*, xxxvii. 419.

18. *Mutual Action of Nitre and Sal-Ammoniac.—Preparation of Nitrogen.*—From repeated experiments, M. Soubeiran finds that these salts when mixed do not give nitrous oxide on the application of heat, as might have been expected, supposing that muriate of potash and nitrate of ammonia had been formed, and as has, in fact, been stated; but that the results are chloride of potassium, water, chlorine, muriatic acid, a little nitrous acid and azote. The latter gas may even be prepared in this way; for the decomposition is easily effected, and a small quantity of the mixed salts yields much gas. The gas must be collected over water, and washed with a little potash, to separate all the chlorine and nitrous acid. The best proportions are one part of muriate of ammonia, and two parts of nitrate of potash.—*Jour. de Phar.* 1827, p. 321.

19. *Preparation and Properties of Aluminum, &c.*—It is now some time since M. Œrsted's discovery of an easy method of preparing a chloride of aluminum, and from that the metal aluminum was announced.* M. Woehler has since pursued this subject, and

* See vol. ii, New Series, p. 474.

added much to that which was previously known. Alumina which had been precipitated from alum by excess of carbonate of potash, and well washed and dried, was made into a paste with powdered charcoal, sugar, and oil, and heated in a crucible; a porcelain tube was filled with the hot product, and then placed in a long furnace; one end of the tube was connected with another tube, containing fused chloride of calcium, and this, with an apparatus for the evolution of chlorine, the other end of the porcelain tube entered a small tubulated receiver, furnished with a conducting tube. When the apparatus was full of chlorine, the tube was made red hot, and the evolution of chlorine continued. Chloride of aluminum was readily formed; it was long retained by the mass of matter, but a part passed over into the receiver as a powder, whilst a little escaped with the gas (oxide of carbon). After 1½ hour, the chloride obstructed the end of the tube, and on taking down the apparatus, more than an ounce was found in that part of the tube within the furnace. Part was an aggregated mass, and a portion in long crystals. It easily separated from the tube, was of a pale yellowish green colour, semi-transparent, lamellated, and distinctly crystalline. In the air it fumed a little, evolved the odour of muriatic acid, and rapidly deliquesced. In water it dissolved rapidly, hissing and producing great heat. Its fusing and vaporizing points are close together. It may be preserved in naphtha: heated in it it liquefies and collects at the bottom as a reddish brown liquid, on which potassium has no action.

The chloride may, however, be reduced by potassium in the following manner. Into a small platina crucible are to be put 9 or 10 small pieces of potassium, about the size of a pea each, and upon the man equal number of similar pieces of chloride of aluminum; the crucible is then to be covered, and the cover confined by a platina wire. Heat must be applied, gently at first: at the moment of reduction the crucible becomes red hot; then the heat of the lamp is to be increased, as the action diminishes, for a short time. The reduced mass is generally completely fused, and is of a blackish gray colour. When cold it is to be thrown into a large quantity of water; a gray powder separates, looking in the sunshine like small metallic plates: it is to be washed in cold water and dried. This is aluminum.

The reduction may be effected in a covered porcelain crucible. Excess of potassium should be avoided, as it tends, when oxidized, to dissolve a portion of the metal. The platina crucible used is but little acted upon.

Aluminum resembles platina in powder, but some particles have the colour and splendour of tin. When burnished, it looks like tin. Rubbed in a mortar it appears to be compressible, unites into larger scales with a metallic lustre, and leaves a metallic trace on the mortar. When heated in the air, it burns violently into a white hard mass of alumina. When the powder is thrown into a flame, each particle burns brilliantly. Heated to redness, in pure oxygen, the combustion dazzles the eyes, and the mass left is more or less fused; the fused particles are yellowish, hard as corundum and cut glass.

Aluminum is not oxidized by water, unless it be near the boiling point; then slow action takes place, and hydrogen is liberated. Cold sulphuric and nitric acids do not act upon it; hot concentrated sulphuric acid dissolves it *without* the evolution of sulphurous acid. The sulphuric solution did not by evaporation give the smallest crystal of alum. Solution of potash or ammonia cause oxidation of the aluminum, evolve hydrogen, and dissolve the earth formed. Aluminum heated in chlorine inflames, and chloride of aluminum sublimes.

Sulphur and aluminum combine at high temperatures to form a black semi-metallic sulphuret, which, by the action of water, evolves sulphuretted hydrogen, and leaves gray alumina. Sulphate of alumina cannot be reduced by hydrogen into a sulphuret.

A similar compound of phosphorus may be formed in a similar way, and has similar appearances. It is also decomposed by water, but not rapidly. A seleniuret, an arseniuret, and a telluret may be formed in the same way.

The chloride of aluminum and sulphuretted hydrogen combine, at an elevated temperature, and a very white sublimate is formed, partly in transparent pearly scales, and partly as a brittle mass. In the air, sulphuretted hydrogen is evolved, water attracted, and a chloride of aluminum remains in solution; by heat, from 30 to 40 volumes of sulphuretted hydrogen appear for one volume of the compound, and much must still be retained, as the affinity is exerted only at a high temperature.—Hensman's *Repertoire.*—*Phil. Mag.* N. S., iv. 148.

20. *Chloride of Glucinum.*—Œrsted showed generally that chlorides might be obtained by passing chlorine over an oxide and charcoal heated together, and in that way obtained a chloride of aluminum, from which the base aluminum has since been obtained. Rose has formed the Chloride of Glucinum by the same process. It very much resembles the chloride of aluminum, sublimes in white floculi having a silky lustre, is fusible by a low heat into brown drops, and is soluble in water.

21. *Metallic Cerium,* M. Mosander.—Previous to the time of M. Mosander, metallic cerium had not been obtained; and being obtained, it proves to be a substance nearer to the earthy than to the ordinary metals, for it decomposes water even at low temperatures. Whether it is a metal at all or not seems doubtful, if it be, as M. Mosander states, a non-conductor of electricity. He obtains it by decomposing the chloride by the vapour of potassium; a layer of sulphuret of cerium is put into a glass tube, heated, and converted into a fusible white chloride of cerium, by passing chlorine over it at a high temperature. The volatile matters are then swept

P 2

away by a current of hydrogen applied whilst the tube is heated, and then pieces of potassium are introduced, and the tube being again heated, their vapours are carried over the chloride, which is reduced more or less powerfully by the operation. The substance obtained is to be rapidly washed in alcohol, of specific gravity 0.85, to remove chloride of potassium, then pressed between paper, and dried in vacuo. It is a rose, or chocolate-brown powder, containing more or less oxide resulting from the action of the alcohol ; it generally resembles silicium in appearance; it smells of hydrogen ; in boiling water rapidly disengages hydrogen, and in cold water evolves it also, but more slowly; by friction it acquires a dull lustre; it is a non-conductor of electricity; when heated in the air it takes fire, long before the temperature has risen to redness, and burns vividly into oxide; heated with chlorate of potash, or with nitre, it detonates violently. It does not combine with melted sulphur, but burns in the vapour of sulphur ; it is not acted upon by phosphorus ; it burns vividly when heated in chlorine.

The sulphuret is best made by passing the vapour of sulphuret of carbon over it at a red heat; it is a red powder, having an appearance between cinnaber and minium ; it may be made by heating 1 part of oxide of cerium and 3 parts of sulphuret of potash to redness, for half an hour.

Carburet of Cerium.—The oxalate of cerium is to be decomposed at a moderate temperature, in a close apparatus, and the greyish-black powder obtained digested in muriatic acid; chlorine is evolved, and a heavy brown-black powder deposited, which, being washed and dried, is the carburet of cerium. Heated in the air, it burns vividly into oxide of cerium, without any appreciable change of weight.—*Ann. der Phys.* 1827, 406.—*Bull. Univ.* A. x. 64.

22. *Use of Chameleon Mineral for marking Linen.*—In many large establishments linen requires to be marked quickly, permanently and economically. The following is a process recommended in France: Prepare a chameleon mineral, by heating a mixture of 1 part oxide of manganese of commerce, and 2 parts of nitre, or common potash, to redness; the green substance obtained is to be preserved in dry bottles, as it changes in the air. When required for use, it is to be powdered, and mixed with its weight of pipe-clay, and then water added, to make a very thin paste. It is this mixture which is to be applied to the linen, either by a brush, or a stamp, or in the manner of stencilling, or even by a pen, if it be made thin, and used quickly. The green paste quickly changes to brown on the linen, and the latter being washed about half an hour afterwards, the loose particles and the potash are removed, and the marks left of a deep brown colour. This writing perfectly resists the action of alkaline lixivia, even though strong; it also resists soap and weak acids: hence the process may be useful to calico-printers. The operation depends upon the reduction of the manganesic acid in the chameleon

mineral to the state of oxide by any organized matter. The same circumstance renders it necessary to keep the substance from the contact of such bodies, and it is in its best state when recently prepared.—(J. D.) *Ann. de l'Industrie*, i. 309.

23. *Reduction of Oxide of Copper by Iron and Water.*—Mr. J. Malin, a worker of copper and iron plate, has had occasion to remark that iron chippings, which have accidentally fallen into a vessel of water in which copper-plates had been previously quenched and scaled, became, after some time, covered with copper, so as to have every appearance of metallic copper. This easy reduction, he thinks, may be useful to those who work in both metals.—*Franklin Journal.*

24. *Separation of Silver and Copper.*—The amalgam obtained at the silver works of Freyberg, leaves, when decomposed by heat, an alloy of silver, copper, and other metals; the latter used to be separated from the silver by boiling and dissolving the whole in strong sulphuric acid, and then precipitating the silver. Of late a process altogether new has been introduced. The alloy is now heated in a reverberatory furnace, exposed to air, so as to oxidize the copper, and is afterwards put into cauldrons of lead, and heated with dilute sulphuric acid, which dissolves the oxide of copper previously formed; the operations of roasting and digesting are repeated once or twice, and many precautions are requisite to obtain a good result, but these being attended to, the process is much more economical than the ancient one. The silver is not so pure, retaining about $\frac{1}{25}$ of copper; but this is of no consequence for ordinary uses. —*Ann. des Mines*, iii. 15.

25. *Solubility of Sulphate of Lead.*—Nitrate and acetate of ammonia dissolve sulphate of lead, the latter in considerable proportion, so as even to be useful in analysis for the separation of sulphate of lead from other insoluble sulphates. At the temperature of 55° Fahr., one part of sulphate of lead is dissolved by 969 parts of a solution of nitrate of ammonia, of specific gravity 1.29; and by only 47 parts of a solution of acetate of ammonia, of a sp. gr. 1.036. The same quantity is dissolved by 172 parts of nitric acid, sp. gr. 1.144. The dissolving power of nitric acid does not appear to be diminished by dilution; the best precipitate in such a case is free sulphuric acid, and not the sulphates of potash or soda.—Bischoff, *Jahrb. der Chem.* 1827.

26. *Use of Red Sulphuret of Arsenic, or Realgar, in Dyeing.*—M. H. Labillardiere states, that many useful colours may be obtained from this substance. The colours are compounds of the sulphuret with oxide of lead, and may, according to M. Berzelius's explanation, be considered as salts, in which the sulphuret plays the part of an

acid: for when the sulphuret is dissolved in an alkali, and then mixed with a solution of copper, lead, or iron, the sulphuret forms an insoluble precipitate, in combination with the oxide present. The oxide of lead is the substance which M. Labillardiere has used as his mordant; the acetate of lead, mingled with a little acetic acid, and thickened with roasted starch, is printed on the fabric. The bath is made by boiling three ounces of realgar, two ounces of potash, and one of slaked lime, in a pot with water, and being modified gives various colours. Thus calico, printed with acetate of lead, being immersed cold in this bath, with a certain quantity of ordinary potash, to be ascertained by experience, acquires a red orange colour; with caustic potash in certain proportions, a reddish brown is obtained; in larger proportion a brown colour is produced, if the temperature be not raised; if heat be applied, black and many other colours are produced, according to the quantities, the temperature, &c.

In other cases the cotton has been dyed all over, and then printed upon; subacetate of lead is the mordant to be used in that case. The method of printing on these colours, which resist mere acids and alkalies, is to print a thickened solution of chromate of potash, and then to pass the web through water acidulated by muriatic acid; the printed places become first yellow (chromate of lead), and in a few minutes after white, without any change being produced in the ground colour.—*Ann. de l'Industrie*, i. 178.

27. *Opaque and Transparent White Arsenic.*—Relative to the way in which transparent masses of white arsenic become opaque, M. Kruger has made a few experiments, which seem to show that the change depends upon the formation of a hydrate. A transparent piece put under a bell-glass containing air, confined over mercury, underwent no change in appearance or weight. Another piece put into air confined over water became opaque on the surface in a few days, and throughout in five weeks; at first it weighed 16.3 grain, and at last 16.4 grains.—Kastner's *Archives.*—*Bull. Univ.* A. x. 18.

28. *Reduction of Sulphuret of Arsenic.*—Sulphuret of arsenic is occasionally required to be reduced, when in very small quantities, in medico-chemical investigations. Berzelius remarks, that it may frequently be successfully performed, by putting it at the bottom of a small glass-tube, placing a small piece of steel-wire before it, and subliming it over the latter; the iron takes the sulphur; the arsenic condenses a little in advance. When the quantities are very small, this process sometimes fails; then Berzelius recommends the following:—The sulphuret is to be introduced into an open quill glass-tube, about four or five inches long, and being held obliquely, thus ⟍, is to be heated by a spirit-lamp, so that the hottest part shall be a little above the sulphuret, and the vapour be obliged to

pass by it; the operation should be conducted slowly; the sulphur will burn into sulphurous acid and escape, and the arsenic into arsenious acid, which will condense in the upper cool part in crystals. The tube is then to be softened in the lamp, and drawn out below the arsenious acid; a little piece of charcoal is to be introduced, and then the arsenious acid passed across it in vapour, to the narrow elongated part of the tube; it will be reduced by the charcoal in its passage, and metallic arsenic will appear. This process never fails.—*Annalen der Physik*, 1828, 158.

29. *On a new use of the Chromate of Potash, by* M. Kæchlin-Schouch.—This use of the chromate is to print a white pattern on a blue or green ground. A blue colour is first given to the cloth by means of the indigo-vat, more or less deep according to the green required; the cloth is then prepared with the aluminous mordant, and passed through hot water; it is then again prepared with an ungummed solution of bi-chromate of potash, consisting of 2½ ounces of salt to 4 pints of water. It is then printed with the following preparation:

Water thickened by roasted starch .	4 pounds
Tartaric acid . . .	10 ounces
Oxalic acid	6 ounces
Nitric acid	2 ounces

The nitric acid is unnecessary, except for delicate designs. The moment this substance is printed, the blue colour is destroyed; the cloth is instantly put into running water, and afterwards dyed in quercitron, or other dye stuffs.

This destruction of vegetable colour arises from the following general fact: whenever chromate of potash is mingled with tartaric or oxalic acid, or with a neutral vegetable substance and a mineral acid, as the sulphuric or the nitric, a strong action takes place, accompanied with the disengagement of heat and gaseous substances. The principal product of this reciprocal action is a new body having acid properties. During the effervescence which takes place, the mixture has the power of destroying vegetable colours. Carbonic acid is evolved during the decomposition; and when the mixture is made in a retort, there comes over a colourless liquid, slightly acid, having the odour of weak acetic acid, and reducing the nitrates of silver or mercury if heated with them (formic acid?).

When 9 parts of tartaric acid and 10 parts of chromate of potash are boiled with water, a neutral green liquid is obtained, which, being evaporated, does not crystallize, but become a brittle green mass. When acetate of lead is added to the solution, a precipitate is formed, which being well washed and then carefully decomposed by sulphuric acid, yields a very acid green fluid, uncrystallizable, and with alkalies forming either acid greenish violet salts or neutral green salts. Cold sulphuric or nitric acids do not act upon this substance; but, being heated, they decompose it. When the acid itself is calcined, it yields green oxide of chrome.—*Ann. de l'Indus.* i. 121.

30. *Chloride of Silver and Sodium.*—When pulverized chloride of silver is boiled in a nearly saturated solution of common salt, a compound of the two substances is produced, which crystallizes as the temperature falls. The crystals are not affected by light, and are decomposed by water. The solvent powers of the chloride of sodium over chloride of silver may be usefully employed in analysis. Similar compounds may be obtained by using the chlorides of potassium or calcium.—Wetzlar, *Jahrb. der Chemie*, 1827.

31. *Nitrate and Sulphate of Ammonia and Silver.*—The nitrate of silver and ammonia is easily obtained by adding ammonia to the nitrate of silver. The salt is very soluble, readily crystallizes, and consists of

Nitric acid . . .	264	1 atom
Oxide of silver . . .	550	
Ammonia . . .	180	2 atoms

The triple sulphate of ammonia and silver is formed in a similar way. It is very soluble, crystallizable, and consists of

Sulphuric acid . .	2160	1 atom
Oxide of Silver . .	6065	
Ammonia . . .	1940	2 atoms

Ann. des Mines, N. S., iii. 175.

32. *Artificial production of Ultramarine.*—A short time since it was announced in the journals that M. Guimet had succeeded in manufacturing ultramarine. The announcement has drawn from M. Gmelin, of Tubinjen, a note, in which he *describes* a method of making ultramarine, and expresses regret that, from an indiscretion of his own, he should have been anticipated. It appears, however, that his regret is without foundation; it is the more so, as every body will be ready to admit that he is the true discoverer of the artificial production of ultramarine, *i.e.*, provided it can be made by his process, since he is the first to publish that knowledge to the scientific world in such a manner as truly to constitute a discovery. M. Guimet, like M. Buran with borax, has resigned (as he had a right to do) the honour, which could only be obtained by a partial sacrifice of his pecuniary interests.

M. Gmelin was led to consider sulphur as the colouring matter of ultramarine, and from an observation by M. Tassært (*Ann. de Chimie*, lxxxix. 88) concluded that it might be made artificially. M. Tassært had remarked the formation of a substance like ultramarine in a furnace used in the manufacture of soda. The following is the method by which ultramarine may be infallibly prepared. Pulverised quartz is to be fused with four times its weight of carbonate of soda, the mass dissolved in water, and then precipated by muriatic acid; thus a hydrate of silica will be formed. A hydrate of alumina is to be prepared by precipitating alum by ammonia. These two earths are to be carefully washed with boiling water; the proportion of dry earth in each is then to be ascertained by heating a small quantity and weighing it. The hydrate of silica used by M. Gmelin contained 56 per cent., and the hydrate of alumina 3.24 per cent.

As much hydrate of silica is then to be dissolved in a hot solution of caustic soda as it will take up, and the quantity determined; then such proportion is to be taken as contains 72 parts of anhydrous silica and a quantity of the hydrate of alumina, equivalent to 70 parts of dry alumina added to it, and the whole evaporated together, being continually stirred, until it becomes a damp powder.

This combination of silica, alumina, and soda, is the basis of ultramarine, and is now to be coloured by a sulphuret of sodium in the following manner. A mixture of 2 parts of sulphur with 1 part of anhydrous carbonate of soda is to be put into a Hessian crucible, covered up, and then gradually raised to a red heat until it is well fused; then the mixture is to be thrown in very small quantities at a time into the midst of the fused mass. As soon as the effervescence occasioned by the water in one portion has ceased, another portion is to be added. Having retained the crucible at a moderate red heat for an hour, it is to be removed from the fire and allowed to cool. It now contains ultramarine, mixed with excess of sulphuret: the latter may be separated by water. If sulphur is in excess, a moderate heat will dissipate it. If all the parts are not equally coloured, a selection should be made, and then the substance reduced to fine powder.—*Ann. de Chimie*, xxxvii. 409.

33. *On an economical Method of dissolving Metals in Acids in the Manufacture of certain Metallic Salts, by* M. Berard.—The method which M. Berard describes is founded upon the rapid effect of oxidation which takes place when certain metals are exposed to air and moisture, or air and acids at the same time. These effects have long been known, but have not been hitherto applied: ordinarily, when metals are to be dissolved in acids, they are oxidized in the first place by air and heat, or else are converted into oxides during the act of solution, at the expense of the water or the acids, and the processes are often inconvenient and expensive.

M. Berard's method consists in granulating or laminating the metal, then putting it into vessels, so as to expose as much surface to air as possible; afterwards filling the vessels with the acid in which the solution is to be effected, which acid must be diluted; then withdrawing the acid, and leaving the moistened metal in contact with the air. The oxidation proceeds with such energy that much heat is generally evolved. After 10 or 12 hours, the acid is again put upon the metal, and being left for an hour or two readily dissolves the oxide formed: being again withdrawn, the oxidation recommences, and proceeds as before; and operating thus, in a very few days the acid is entirely saturated.

This process was proposed by M. Berard, sen., many years ago, for the solution of tin in muriatic acid; and at present three important applications of it are made in the manufactory at Montpellier, in the preparation of blue vitriol, muriate of tin, and acetate of lead.

In the preparation of blue vitriol, 4 or 5 leaden vessels are filled with pieces of laminated copper, as old copper, sheathing, &c. The metal in all the cases is moistened with weak sulphuric acid (sp. gr. 1.114 to 1.155), and left exposed to air for some time; then the first vessel is filled with similar acid, which, after a few hours, is transferred to the second vessel, then to the third, and so on in succession, until the hydrometer shews that it is saturated, or nearly so, with oxide. By this method a solution may be obtained, which does not require to be evaporated to less than one half before it will yield fine crystals; they are scarcely acid, and are consequently free from iron.

In the preparation of muriate of tin, the granulated tin is put into vessels of glass or earthenware, and muriatic acid poured upon it; action immediately commences, and hydrogen is evolved: if, after a short time, the acid is withdrawn and the vessels left open, at the same time that the moistening acid acts, evolving hydrogen, so great a portion of the oxygen of the air combines with the metal that the whole heats powerfully. On restoring the acid to the metal, it dissolves more oxide than it would have done without the use of the air, even though heat had been applied. By transferring the acid to be saturated from one vessel to another, a solution of proto-muriate of tin is quickly obtained, which merely requires a little evaporation to yield the salt in very fine white needles.

Acetate of lead is usually made by dissolving litharge in acetic acid, more or less diluted. In M. Berard's manufactory, the lead is granulated in as thin portions as possible, and a wooden vessel, as the half of a cask, filled with it; it is then moistened with weak acetic acid (distilled vinegar), and the tub covered with a board. In a few minutes, so much heat is evolved, that acetic acid is volatilized: more acid, then poured upon the lead, dissolves a large quantity of oxide; and by two or three such operations, not only a solution of the acetate, but of the subacetate may be obtained. The operation is so quick that three or four tubes suffice to supply a large quantity of the acetate. The solution is then evaporated in copper vessels, with the usual precautions: the mother liquor is evaporated with fresh solutions, and when, by these repeated operations, it becomes too coloured, it may be cleansed by the use of animal charcoal, like sugar.

M. Berard has no doubt that a similar process might be economically adopted in the manufacture of verdigris in the central parts of France, and also in the manufacture of ceruse, as at present carried on in Holland.—*Ann. de l'Industrie*, i. 78.

34. *Infusible Crucible.*—M. Deyeux has manufactured crucibles surpassing even those from Saxony in their infusibility. MM. Thenard, Lassagne, and Baruel testify to their superiority. 2½ lbs. of pure iron has been fused in one at once, without the crucible suffering any injury. The manufactory is at Mouchy Saint Eloy, department de l'Oise; but the depôt is at Paris, Rue Garanciere, No. 7.—*Ann. de Chimie*, xxxvii. 443.

Pure iron has often been fused in Cornish crucibles, at the iron works, without the vessels suffering injury, but perhaps not in so large a quantity as that above, merely because there was no occasion for it. The power of retaining fused iron, though a test of goodness, is not a sufficient test of superiority.—Ed.

35. *Sugar of Liquorice—Glycyrrhiza glabra.*—The peculiar principle in the root of this plant has been long known. Doberuner and Robiquet have given processes for its separation. The following is by M. Berzelius. The cut root is to be infused in boiling water; the cold filtered infusion is to have sulphuric acid added in small quantities, until no further precipitate is formed. The precipitate is a compound of the acid with the saccharine matter, and is to be washed at first with acidulated cold water, and then with pure water, until no free acid appears. The precipitate is to be digested with alcohol, which leaves certain impurities, and then pulverized carbonate of potash or soda is to be added to the solution, until it is neutral; the clear liquor is to be decanted and evaporated. It is desirable to have a small excess of acid present, for which purpose put a little of the alcoholic liquor on one side, to be added at last to the neutral portion, and then leave the whole at rest, that the sulphate of potash may separate before the evaporation is effected.

The saccharine principle is a transparent yellow mass breaking like amber. Being heated it melts, and burns with a bright flame and much smoke. In powder it burns like resin or lycopodium. It does not change in the air. Its aqueous solution is precipitated by *all the acids*, and the more completely the stronger is the solution. The precipitates have no acid taste, but are sweet; they dissolve in water, and gelatinize upon cooling, if the solutions are strong.

This substance also combines readily with bases forming soluble neutral solutions; those with baryta and lime are not precipitated by carbonic acid. This principle forms insoluble compounds with metallic acids and many metallic oxides. It combines also with many salts, causing their precipitation in some cases.

The saccharine principle of the root of the wild liquorice *(polypodium vulgare)* is altogether different in its qualities from the above substance.

36. *On the Preparation of Tannin.*—According to M. Berzelius, tannin is not in the very impure state, in an infusion of galls, generally supposed. He prepares it pure in two ways, either by the action of sulphuric acid, or of carbonate of potash. *First method.* A hot infusion of gall-nuts is to be filtered through a cloth, a very small quantity of weak sulphuric acid added, and the whole well mixed; the coagulum formed is to be separated, and the liquid filtered. Sulphuric acid, diluted with half its weight of water, is to be added in small quantities with agitation; the precipitate, after an hour's rest, acquires a half-fluid glutinous state; then the fluid is to be decanted, and carefully mixed with concentrated sulphuric acid, as long as any precipitate is formed. It is a compound of sulphuric acid and tannin, yellowish-white, and insoluble in a weak acid. Being put on a filter, it is to be washed with diluted sulphuric acid, pressed between bibulous paper, dissolved in pure water, and carbonate of lead in fine powder added to the fluid, until the free sulphuric acid is neutralized; ebullition for a short time also removes the acid combined with the tannin; perfect saturation is indicated by the deep yellow colour taken by the solution. The filtered fluid is to be evaporated carefully to dryness in an air-pump if possible; the brown extract obtained pulverized, and digested in ether, at a temperature of 86° Fahr. The ethereal solution evaporated yields a pale yellow transparent substance, which is pure tannin. It suffers no change in the air.

Second method. A concentrated solution of carbonate of potash is to be added to a filtered infusion of galls, only as long as a white precipitate is formed. The precipitate is to be washed on a filter with ice-cold water, and then dissolved in weak acetic acid. By filtration a brown matter is separated; the clear fluid is to be precipitated by acetate of lead; the compound of tannin and oxide of lead washed, and then decomposed by sulphuretted hydrogen. The filtered liquid is then colourless, and being evaporated under the air-pump receiver, gives transparent yellowish hard scales, which, treated with ether as before, yield pure tannin.

Pure tannin is colourless, but sometimes becomes coloured by alteration in the air; it is not deliquescent, is easily pulverized, and dissolves readily in water. By distillation it yields no ammonia, but a yellow oil and a liquid, which, on cooling, deposit crystals different from those of gallic acid; they have a hot taste, and colour or precipitate salts of iron of a yellowish or grayish green.

The combinations of tannin with acids, when exactly saturated, have no sourness, but a pure astringent taste. When pure, they are usually very soluble in water, and precipitated only by a great excess of acid. With salifiable bases, tannin forms very curious compounds. The neutral compound, with potash or ammonia, is little soluble in cold water, more so in hot, separating from the latter, as the temperature diminishes, in the form of a white powder, which, put in the filter, pressed and dried, looks like an earthy salt, and remains unchanged in the air. When moist it forms extract by means of the air. The combination with soda is much more soluble.

M. Berzelius then describes the tannin obtained from various other sources, as catechu, gum kino, cinchona, with their processes, and states that these kinds of tannin differ very much from each other.—*Ann. de Chimie*, xxxvii. 385.

37. *Vegetable Gelatine, and Albumen.*—M. Berzelius has lately examined gluten, and says that the gliadine and zymoma of Taddei are nothing else than the well known and ordinary principles of vegetables named above. Boil gluten with successive portions of alcohol until the latter ceases to become turbid upon cooling; mix these solutions with water, and distil; as the aqueous residuum cools, a glutinous coherent mass will separate, resembling gluten. It is *vegetable gelatine*, and the same substance as that separated by Einhof's process from barley, &c. The substance insoluble in alcohol is vegetable albumen.

Vegetable gelatine is grayish, yellow in colour, adhesive, glutinous and elastic, having no taste, but a peculiar odour. It dries into a transparent, shining substance. It dissolves in alcohol; if cold alcoho lbe used, a viscid foreign substance is separated, not gelatine. It dissolves in vinegar, leaving also a viscid insoluble matter; when precipitated by an alkali, it resumes its viscid state. The mineral acids, with the exception of the phosphoric, form glutinous compounds insoluble until the excess of acid has been removed. This principle combines with and neutralizes alkalies, forming solutions, which, when evaporated, yield a transparent matter. Earths and oxides form insoluble compounds.

Vegetable albumen is almost perfect in its resemblance to white of egg. It dissolves in alkalies, and when in excess, the solutions are neutral. It then coagulates slightly by heat, but the principal part is retained in solution; it combines with acids, and when exactly saturated the substance remains soluble, but excess of acid (except the acetic and phosphoric) precipitates it. Before the action of potash, the vegetable albumen dissolves feebly in vinegar or phosphoric acid, but by ebullition with these acids, it forms a transparent colourless jelly of considerable volume.

The azoted principle contained in emulsive seeds has been considered analogous to the coagulum of milk. Souberian has shown that that from almonds has all the properties of white of egg; it is, in fact, the same substance as vegetable albumen.—*Ann. de Chimie*, xxxvii. 215.

38. *Preparation of Piperine, by* Mr. Carpenter.—Digest one pound of coarsely powdered black pepper in one gallon of alcohol for ten days; distil off one half of the alcohol in a water bath; add

by degrees diluted muriatic acid, to hold the piperine in solution; then add water sufficient to precipitate the resin and separate the oil, a muriate of piperine remaining in solution; concentrate the solution by evaporation, and add pure potash to decompose it, and neutralize the acid; when the piperine, in consequence of the diluted state of the alcohol, and the absence of the muriatic acid, will be deposited in yellowish transparent crystals. The crystals may be obtained perfectly colourless, by carefully separating the oil and resin; but as there is no disadvantage in the colour (for medical use), the additional trouble and expense would not be compensated. The piperine in a colourless state is insipid and inodorous, but united with as much resin as enters into its crystallization, its taste is extremely hot, possessing, in an intense degree, all the pungency of the pepper, with a considerable portion of its odour.

The crystals were perfectly transparent tetrahedral prisms, with oblique summits, of a straw colour, and as large as the ordinary crystals of sulphate of magnesia.—*American Journal, Med. Scie.*

39. *Substitute for the Sulphate of Quinia.*—Bartholomea Rigatelli, a chemist of Verona, says he has discovered a substance which may be used instead of the sulphate of quinia; but the notice given of it is so imperfect, that it would not be worth attention except that it comes from a Committee appointed by the Academy of Verona, and may therefore be supposed to have some foundation. The committee report that the saline substance spoken of is obtained from an indigenous plant common to all Europe; that it is obtained in considerable quantities by a simple process; that it consists of an acid in union with a vegetable alkali; and that it contains nothing which can injure the health. The salt is friable, of an earthy appearance, and brick red colour, having a more astringent and bitter taste than the sulphate of quinia; its odour is slightly vegetable, but scarcely perceptible. When pulverised, the powder is white and very soluble in water. Multiplied observations have proved that it may be successfully used in place of sulphate of quinia, in every case where the latter has been found advantageous.—*Bull. Univ.* C. xiv. 101.

40. *Citric Acid from Gooseberries.*—Mr. Tilloy has obtained citric acid from this fruit, at an expense less than half the usual price of the acid in France. The gooseberries are to be bruised and fermented: the alcohol formed, distilled off, and the residue pressed to extract the liquid. The latter is to be heated, and carbonate of lime added as long as effervescence is occasioned; the citrate of lime is then to be collected, drained repeatedly, washed, and then pressed; it is still coloured and mixed with malate of lime: it is to be mixed with water until of the consistence of thin syrup, heated, decomposed by sulphuric acid, and the whole diluted with twice its weight of water. The fluid separated from the precipitate is to be

again treated with carbonate of lime; and now the precipitate, when collected on a filter, is to be well washed, pressed, and a third time decomposed by sulphuric acid. The clear liquor now obtained is to be boiled with animal charcoal, filtered, and evaporated. When sufficiently concentrated, it must be allowed to deposit, and the fluid, when poured off, be put into stoves heated to between 68° and 77° Fahr. Crude crystals of the citric acid will be thus obtained; they are to be drained slightly, washed, and recrystallized.—*Jour. de Phar.*—*Phil. Mag.* N.S., iv. 153.

41. *Nature of Aloetic Acid, or the Bitter of Aloes.*—M. Liebeg finds this substance to be a combination of carbazotic acid, and a particular substance having many of the properties of resins. The bitter of aloes may be formed in large quantity, by acting upon aloes with nitric acid of the specific gravity of 1.25. The substance obtained forms a purple salt with potash, but little soluble, and precipitating the salts of baryta, lead, and peroxide of iron, of a deep purple colour. When a solution of this salt was precipitated by acetate of lead, the water employed to wash the precipitate had a yellow colour, and deposited small crystals of the same colour. These crystals heated in water with sulphate of potash, gave carbazotate of potash, and from that carbazotic acid was obtained.

When aloes are heated with nitric acid of specific gravity 1.432, until the liberation of nitrous vapour ceases, and the liquid be mixed with a little water to separate a small quantity of bitter principle, then by neutralization with potash and evaporation, a large quantity of carbazotate of potash in fine crystals is obtained.

Wool, morphia, narcotine, and myrrh, did not give carbazotic acid by treatment with nitric acid.—*Ann. de Chimie*, xxxvii. 171.

42. *Preparation of Gallic Acid.*—The following is M. Le Roger's method. Gall-nuts are to be exhausted by repeated decoctions, the liquid obtained concentrated and precipitated by a solution of jelly; the tannin thus rendered insoluble is to be filtered out; very pure animal charcoal is to be added to the liquid, and boiled with it for eight or ten minutes and the whole filtered, when the liquid, on cooling, will give pure white and silky crystals of gallic acid, amounting, when the best galls are used, to one-fourth of their weight.—*Mem. de Geneve.*

43. *Volatilization of Alcohol.*—According to M. Soemmering, strong alcohol yields a weaker spirit at the commencement of distillation than it does afterwards. With weak alcohol, the weaker it is the more readily is its strength increased by distillation; on the contrary, the more concentrated it is, the more difficult is it to remove the rest of the water. When alcohol of specific gravity 0.796 is distilled, the weakest comes over first, and the product becomes stronger as the operation proceeds.—*Bull. Univ.* A. ix. 344.

The following experiments on this subject are by MM. Yelin and Fuchs. The first column contains the quantity of absolute alcohol per cent. in that which was experimented with, and the second the boiling point in degrees of Reaumer's scale.

0.94	60.58
0.95	60.59
0.96	60.54
0.97	60.48
0.98	60.48
0.99	60 52
1.00	60.62

From which it appears that alcohol containing 2 or $2\frac{1}{2}$ per cent. of water evaporates more readily than anhydrous alcohol, is, therefore, more volatile and more readily distilled. In another set of experiments alcohol of $98\frac{1}{2}$ per cent. was distilled at a moderate temperature, and the products received in eight successive portions; the following are the specific gravities of these portions.

1	0.7972 or 97.86 per cent. alcohol.
2	0.7970
3	0.7969
4	0.7966
5	0.7965
6	0.7964
7	0.7962
8	0.7959 or 98.32 per cent. alcohol,

so that after the strength of 97 per cent. is obtained, the weaker alcohol passes first, the stronger remains in the retort; the volatility of the alcohol is not, therefore, in direct proportion to its anhydrous state or to its lightness.—*Bull. Univ.* A. x. 81.

It appears, however, that none of the alcohol in the latter experiments was so strong as that put into the retort at the commencement of the experiments, consequently alcohol *stronger* than any of the products must have escaped. If this loss was due to inefficient condensation, then some of the other effects may have been influenced in the same way, and consequently the above results be more or less incorrect.—Ed.

44. *Concentration of Alcohol by Animal Membrane.*—A memoir was published some time since by M. Soemmering, on the evaporation of the water in diluted alcohol through a bladder, and the consequent concentration of the spirit. A second memoir by the same person has been published, in which the effects are more fully detailed, and especially when the alcohol is in contact with the bladder.

To strengthen alcohol or render it anhydrous, a bladder capable of holding 16 ounces is to be nearly filled with alcohol of specific gravity 0.85; it is then to be well closed, and suspended over a sand bath or before a heated stove, at an inch or more of distance; in the course of a few days the alcohol will be diminished one-fourth of its bulk, and have a specific gravity of 0.8. The bladder of an ox or a calf is to be used, prepared by being steeped some time in water, washed, blown out, freed from fat and adhering vessels, the two ureters effectually tied, and then turned inside out, that both sides may be cleansed. Being then blown up and dried, the surfaces are covered with a solution of isinglass ; one layer is put upon the internal surface, and two upon the exterior. The texture thus becomes closer, and the alcoholic concentration proceeds better.

The bladder should not be filled, but a small space left. It does not become moist to the touch, and allows no odour of alcohol. If the alcohol have a greater specific gravity than 0.952, the bladder softens and feels moist. Bladders prepared as above, may be used a hundred times or more: they gradually acquire a yellow brown colour and become stiff, but they are improved by a slight change. The air-vessel of the salmon will not produce these effects: alcohol of specific gravity 0.856 being put into one for 32 hours, lost a third of its volume, and was very much weakened. The air-bladder did not become moist, but the odour of alcohol was perceived near it.

Weak alcohol in bladders lost its water more rapidly than stronger spirit. In an experiment between water and alcohol, two equal bladders were chosen, and eight ounces of water put into one, whilst eight ounces of alcohol were put into the other. Both were equally exposed to a moderate heat : in the course of four days all the water had disappeared, whilst the alcohol had lost only one ounce of its weight.

If artificial heat is properly employed, absolute alcohol may be obtained in from 6 to 12 hours. Even solar heat will produce absolute alcohol.

Wine put into the prepared bladders acquired no bad odour ; it took a deeper colour, had more aroma, a milder taste, and generally became stronger. Oil of turpentine, put into a jar and covered by a bladder, lost nothing in four years. Concentrated vinegar lost half its volume in four months ; the other half was thick, and had no acid taste. Orange-flower water, under the same circumstances, lost a third of its bulk in several months, but had acquired a stronger odour, and had evidently lost none of its volatile principle.—*Mem. de Munich*, ix. 103.—*Bull. Univ.* A. ix. 322.

45. *Formation of Adipocire.*—Dr. Harlan of Philadelphia relates, that having occasion to macerate a cranium in the summer of 1824, he directed the head of a large fat negro, who had died of acute fever, to be placed in a barrel half filled with water and closely covered over. On examining the process about six weeks afterwards, he was surprized to observe the head floating buoyantly on the surface of the water, lying on one side. The gas disengaged during putrefaction, and detained within the cranium, had probably produced this effect. The upper surface, or that which floated *above* the water, presented a tumid appearance, and on cutting into

it, the whole substance down to the bone was found converted into adipocire. That portion of the head and face, on the contrary, immersed in the water was putrid and macerated.

"Those bodies," says the Doctor, "in which this change has occurred in the cemeteries of this city, such at least as have come under my observation, have been interred in a soil of clay with a layer of gravel or sand superimposed; the water percolating down to the clay, which confines it in the vicinity of the body, which rests on the water."—*N. American Med. Journal.*

§ III. Natural History.

1. *Mean Height of the Inhabitants of Paris, &c.*—During eight years, from 1816 to 1823 inclusive, the mean height of the young men found fit for military service has been 5 feet 2 inches $1\frac{1}{3}$ lines for Paris, and 5 feet 1 inch $9\frac{1}{3}$ lines for the suburbs de Sceaux and Saint-Denis; so that the mean height is higher in Paris than in the rest of the department de la Seine. The same fact has been remarked in the department du Rhone, between the town of Lyons and the suburb of Villefranche, in the years from 1806 to 1810 inclusive. From other facts of a similar nature also, it may be concluded that all other things being equal, the height of men is in proportion to their condition in life, or rather, perhaps, inversely, as the troubles, fatigue, and deprivations which occur in infancy and youth.—*Corr. Mathem.* iii. 161.

2. *Effects of the Tincture of Colchicum Autumnale on the System.*—Struck by the powerful and beneficial effects produced by this medicine in cases of gout and rheumatism, M. Chelius was led to search particularly for circumstances which might either give a reason for its good action or accompany it, and soon noticed a remarkable change in the urine, which he thinks sufficient to explain the whole. This change consists in a striking increase in the quantity of uric acid contained in that secretion. A person afflicted with gouty concretions at many of the joints, and especially at the knees, so as to be unable to move, took the colchicum wine; before its use, the uric acid, either free or combined in the urine, was 0,069; on the fourth day after the first employment of the medicine it had increased to 0.076; on the eighth day to 0.091; and on the twelfth day to 0.112: so that the quantity was nearly doubled in the short space of twelve days. Similar results were obtained in many other cases of the same nature, in which the analyses of the urine had been made.

M. Chelius thinks the English physicians give too large a dose of this medicine; he thinks it preferable to begin with 20 or 30 drops in half a glass of water, and to increase the quantity gradually, until gastric irritation is indicated. So used, he has never observed it to produce bad effects.—*Bull. Univ.*, C. xiv. 100.

3. *Gouty Inflammation cured by Vaccination over the diseased part.*—"A lady of hereditary gouty diathesis had been inoculated for the small-pox some 50 years ago," and had it severely. "Some time since, I was requested by her to vaccinate her servant girl, which I did, and successfully. She was herself labouring at this time under a severe attack of gout in her right wrist, which was swollen, and extremely painful, her system being feverish, &c. I inserted, with her permission, a portion of the virus into the affected part, with the view of ascertaining whether she could take the vaccine disease, and if so, what effect it would produce upon the gout. Somewhat to my surprise, and greatly to my satisfaction, she not only had the genuine disease, but the swelling and pain immediately left her arm, and long before the scab (which was green) had dropped off, she was as well and as comfortable as she had ever been in her life. The cicatrix remaining is of the genuine porous kind."—Dr. Theodore Coxe.—*N. American Med. Jour.*

4. *Effect of Chlorine in Chronic Affections of the Lungs.*—A bleaching establishment having been removed into a situation notoriously damp, where catarrhal affections were extremely common, M. Bourgeois was not a little surprised to observe that those employed in this establishment were less liable to these attacks than their neighbours. As chlorine is much used in such establishments, he attributed to it the preventive influence. It chanced that two people, one with chronic catarrh resembling phthisis, and the other with a vomica in the lungs, were perfectly cured after two or three months' residence in this establishment.

Chlorine has been used medicinally. M. Bourgeois prefers it disengaged from a mixture of oxide of manganese and muriatic acid; it is of course to be diluted with very much atmospheric air.—*Med. Journal*, lx. 173.

5. *Sting of a Wasp.*—The bulb of an onion or garlic, cut and applied immediately to the place stung, instantly removes the pain. *Recueil Industrielle*, vi. 216.

6. *On Insects inclosed in Copal.*—The insects contained in the different kinds of copal, which occur in commerce, are not less interesting than those inclosed in amber. M. Dalman has examined many of them very minutely, and has found,—1. That there is a complete analogy existing between these insects and those contained in amber. 2. That there are many new genera amongst them, and many new species of genera already known. 3. That some new points relative to the geography of insects may be deduced from them; for copal, which is always an exotic substance, contains insects belonging to genera which had been supposed to occur exclusively in Europe, such for instance as Pselaphus, Claviger, Aliochara, Chermes, Thrips, &c.

Copal resin perfectly preserves the most delicate part of the smallest insect, in consequence of which M. Dalman has been enabled to study the systematic characters of these small beings very minutely. He has made out three new genera and fifteen new species.—*Bull. Univ.*, B. xiv. 287.

7. *Reproduction and use of Leeches.*—The following experiments have been made, and conclusions drawn, by M. Pallas. The bottom of a box was covered with argillaceous earth to the depth of 6 inches, and then 200 leeches, which had been used six times, put in; they buried themselves in the earth. Five months after, a layer of earth 1½ inches deep was removed, and a conical hole found, with smooth sides, inclosing axiform cocoons of various sizes. On further examination, 73 cocoons were found, and a loss of 88 dead or useless leeches was remarked. In another experiment, the box being arranged as before, 200 leeches, which had not been used, were put in at the same period; at the end of five months only 14 cocoons were found, and 98 leeches were lost.

From these and other researches it is concluded,—1. That leeches which have been more or less frequently used, and placed in favourable circumstances, are more apt to reproduce than those which have not been used. 2. That the enormous difference above described depends upon the difference of nourishment. 3. That the time of increase in the climate of Pampluna appears to be from the 15th or 20th of August to the end of September. The atmosphere should be at least 59° or 60° F. Argillaceous earth is the medium preferred by the leeches. 4. Each cocoon usually contains 12 individuals. 5. The cocoons are principally formed of two kinds of substance; the internal is fibrous, dense, and very close, enveloping a very thin multilocular pellicle, which contains the germs; the exterior is very light, porous, and woolly, probably destined, according to M. Chatelain, to keep out moisture, and give lightness to the cocoon; but by M. Pallas considered as intended to protect the contents of the cocoon from sudden changes of temperature, of which the young leeches are very sensible. 6. That leeches may be applied again and again, and are then more apt to produce young. Between the 1st of January and 30th of September, M. Pallas used 35,611 leeches which had been used before.—*Mem. de Med. Militaire*, xx. 361.

8. *Red Viper of Dorsetshire.*—The Rev. Mr. Rackett states that a serpent, known to the gamekeepers of Dorsetshire under the name of the red viper, was recently killed in Cranbourne Chase. It does not appear to have been previously known to British naturalists, and is considered to be more poisonous than the common viper, but, fortunately, very rare. Mr. Racket describes it as of a marked red colour, and thinks it probably the Coluber Chersea of Linnæus.—*N. Monthly Mag.* xxiv. 403.

9. *Destruction of Grasshoppers' Eggs.*—Last year the Pacha of Egypt offered a reward, for all the grasshoppers' eggs that should be delivered to him, of 17 piastres per measure. By a letter from Acre, it appeared that in October last 40 garavas of 72 measures each had been sent in. The total quantity of eggs, estimated as above, would be worth 46,000 piastres to those who collected them, or about £40,000.—*Asiatic Journal,* 1827, p. 480.

10. *Loss in weight of Meat during cooking.*

4 lb. of beef	lost by	boiling	1 lb.
4 lb. ditto	. .	roasting	1 lb. 5 oz.
4 lb. ditto	. .	baking	1 lb. 3 oz.
4 lb. of mutton	.	boiling	14 oz.
4 lb. ditto	. .	roasting	1 lb. 6 oz.
4 lb. ditto	. .	baking	1 lb. 4 oz.

Jour. des Conn. usuelles. 1828, p. 256.

11. *Living Giraffes in Europe.*—Besides the two living giraffes at present in Europe, in London and Paris, a third has been sent by the Pacha of Egypt to the Emperor of Austria, and arrived some time since at Venice, accompanied by Arabs as keepers, and cows to provide its food. It was to pass the last winter in Padua, and then proceed in the fine weather to Vienna.

12. *Easy Method of preserving small Birds.*—It may be useful to travellers to know that birds to the size of a pigeon may be preserved from putrefaction by an easy process, and by a method which will effectually guard them against the attacks of insects. Carefully remove the abdominal viscera at the vent, by means of a wire bent to a hook at one end; then introduce a small piece of the antiseptic paste, and afterwards as much clipped cotton or tow as may be thought sufficient, with some of the paste mixed with it; remove the eyes, and fill the orbits with cotton imbued with the paste; draw out the tongue, which remove, and pass a wire from the mouth into the cavity of the cranium, merely to give the antiseptic access to the brain: bind a piece of thread round the rostrum, another piece round the body and wings; then hang it up by the legs, and pour in at the vent from half an ounce to two ounces, according to the size of the bird, of alcohol; let it be hung in an airy situation, and it will soon dry, without any unpleasant smell. The antiseptic paste is made by mixing 8 parts of finely-powdered white arsenic, 4 parts of Spanish soap, 3 parts of camphor pulverised in a mortar with a few drops of alcohol, and 1 part of soft soap.—*Med. Surg. Jour.* i. 196.

13. *German Method of procuring Flowers in Winter.*—According to the 'Recueil Industrielle,' the following method of expediting vegetation at will is practised in Germany. A branch, proportioned

to the size of the object required, is sawn off the tree, the flowers of which are to be produced, and is plunged into a spring, if one can be found, where it is left for an hour or two, to give time for such ice as may adhere to the bark to melt, and to soften the buds; it is then carried into a chamber heated by a stove, and placed in a wooden vessel, containing water; quick-lime is to be added to the water, and left for twelve hours. The branch is then to be removed into another vessel, containing fresh water, with a small quantity of vitriol, to prevent its becoming putrid. In a few hours the flowers will begin to appear, and afterwards the leaves. If more quick lime be used, the flowers will appear quicker; if, on the contrary, none be used, the branch will vegetate more slowly, and the leaves will precede the flower.—vi. 216.

14. *Chinese Method of Planting Branches.*—The following method is described as being practised by some Chinese retained by Count Linhares, in Brazil. The tree practised upon was a Brazilian myrtle. The branch to be separated and planted, already some inches in thickness, was surrounded by a band of straw, mingled with horse-dung, forming an envelope five or six times as large in diameter as the branch itself; then an annular incision was made below this part, and water was allowed to drop from a considerable height on to the wrapped part. The vessel is usually a cocoa-nut shell, pierced with very fine holes. In about two months the branch is separated from the tree and planted. To obtain rapidly growing trees the Chinese choose the upper smaller branches, but for more productive and better trees they choose stronger branches that are nearer to the earth.—*Bull. Univ.* D. ix. 342.

15. *On a Plant living entirely in the Air.*—This plant, which is described by M. Loureiro, in the 'Lisbon Memoirs,' ii. 83, is not the Epidendrum flos aëris of Linnæus, but a different species, which grows in Cochin China, and in one part of China itself. The calyx is small, oval, and of one flower; the corolla has five equal petals; the nectarium consists of two horizontal petals, of which the lower is oblong, fleshy, concave, and shaped like a boat; it is covered by the upper nectary, which rises and turns at one side into the form of a tube, whilst the other side extends horizontally. The stamina are two short elastic filaments united to the internal extremity of the lower leaf of the nectary; the anthers are oblate, simple, and covered; the pistillum consists of a three-sided, thin curved stem which supports the flower; the flower is of a yellow colour, larger than that of the jessamine, of an agreeable appearance, and pleasant odour. The root consists of intertwined bulbs. This plant is found in the woods, suspended from the branches of trees; being removed and hung upon a string, or some other support, it continues to vegetate, though slowly, and flowers every autumn. It is multiplied by producing each year new filaments, which send out roots, be-

come covered with leaves, and separating from the parent plant, still continue to vegetate and increase.

16. *Culture of Aquatic Plants in China.*—The Chinese take advantage of their lakes, pools, and rivulets, by cultivating different aquatic plants in them, many of which are considered as food. The government has planted these vegetables in the lakes, marshes, and uncultivated watery grounds belonging to the state, and the emperor has introduced them into all the canals of his gardens. These and other aquatic vegetables may generally be introduced into Europe, for they are not so sensible of changes in climate as those which grow in the earth.—*Bull. Univ.* D. ix. 367.

17. *Benzoic Acid in the Grasses.*—Benzoic acid has been found by M. Vogel in the sweet-scented vernal grass (anthoxanthum odoratum), and in the sweet-scented soft grass (holcus odoratus). It is these two grasses which communicate to hay the aroma peculiar to themselves.

18. *Eradication of Meadow Saffron.*—Colchicum, or meadow saffron, is highly injurious in meadows, in consequence of its poisonous qualities, especially when green. Instances are not at all uncommon of cattle, pigs, &c., being poisoned by it. It is not easily eradicated, propagating itself readily both by its seeds and roots, and the latter lying deep in the earth. The best method is to pull it up in the beginning of May, before the seeds are ripe. It is only necessary that the stem should be separated at the neck of the bulb, for it has been ascertained that then the plant no longer has the power of reproduction.—*Bull. Univ.* D. ix. 320.

19. *Native Arseniuretted Iron.*—A mineral substance was given to Professor Silliman by Mr. P. Baldwin, who said that it was from the Bedford county, Pennsylvania. Professor Silliman considered it as a new variety of native iron, and gave it to Mr. Shepard for analysis. The following are the general results :—

The fragment weighed 2 or 3 ounces, and, though injured in form, was evidently a crystal. It was ascertained to belong to the class of rhombic prisms, but whether right or oblique could not be determined. The natural planes were not good, and although the cleavage planes seemed quite perfect, they gave irregular results. The inclination of the primary planes are about 121° and 56°, those of the secondary (intersecting the base parallel to its greatest diagonal) to the primary 149°. With cleavage planes the angles were 120°, 121°, and 122°; cleavage parallel to the lateral planes is easily effected. The fracture in the other direction is uneven and sub-hackly. The original planes were dull, but fresh cleavages presented a fine metallic lustre, and a colour between silver-white and steel-gray. The substance breaks with difficulty: small masses often flatten under the hammer; its hardness is nearly that of ordinary steel; specific gravity 7.337, highly magnetic and polar; it melts before the compound blow-pipe flame, giving the smell of arsenic when in the exterior flame, and burning brightly like iron in the inner flame. No odour of sulphurous acid was perceived, and by chemical examination the substance was found free from sulphur.

When dissolved in nitric acid, black flakes of plumbago were separated, and, besides the iron, arsenic acid existed in the solution: no other metals were present. The proportions are as follows:—

Iron	97.05
Arsenic	1.55
Plumbago	0.40
Loss	1.
	100.00

20. *Muriate of Ammonia in Turkistan.*—M. Timkovski states in his Travels in China, (second part,) that there are mountains to the north of the town of Kutscha, containing numerous caverns, in which, during spring, summer, and autumn, flames appear, resembling at a distance lighted lamps, but difficult to approach. During the winter, when ice and snow abound, the flames disappear. The inhabitants of the country then enter the caves, and collect much sal ammoniac.—*Bull. Univ.* B. xiv. 220.

21. *New Minerals containing Selenium.*—Two mineral substances from Culebras in Mexico were given to Professor del Rio, and by him examined. One was red, like cinnabar, with a specific gravity of 5.66, the other gray, like light gray silver ore, with a specific gravity of 5.56. Both burnt before the blow-pipe with a violet flame, evolving an offensive smoke, smelling like rotten cabbage, and leaving a grayish white earthy matter. When heated in a retort, mercury, selenium, and a small quantity of sulphur rose, and a suboxide of zinc remained.

The gray mineral being analyzed in the moist way gave

Selenium	49.
Zinc	24.
Mercury	19.
Sulphur	1.5
	93.5

besides which were also 6 grains of lime from the matrix. The mineral is therefore a biseleniuret of zinc, united to a proto-sulphuret of mercury. The red mineral is stated also to be a biseleniuret of zinc, but united to a bisulphuret of mercury or cinnabar, which gives the red colour to it.—*Phil. Mag.* iv. 113.

22. *Common Salt on the Coast of Chili.*—The officers of the

frigate *United States*, who have returned from a voyage in the Pacific, gave Dr. Mitchell a piece of common salt from that part of the coast of Chili to the south of Coquimbo. An incrustation of salt is found along this coast 30 miles in length, and several miles in width. It has the appearance of that compact ice which forms on the surface of lakes and rivers in America, towards the middle of winter. Its thickness is about two feet. When a block of it is removed, the space is soon filled up by new salt. The great road runs for a considerable distance along the edge of this curious formation. It has frequently happened, that when mules, horses, and even men have died in this part of the route, their bodies have been perfectly preserved for a long time afterwards.—*Ann. Maritimes*, 1827, p. 617.

23. *Fall of an Aërolite and accompanying Phenomena.*—The fall of an aërolite, weighing 36 pounds, at Vaigou, one of the Sandwich islands, is described by M. Jégur Jékimof, a lieutenant who accompanied Captain Kotzebue in his voyage round the world, with attendant meteorological circumstances, which seem closely connected with it. It fell at 11 o'clock in the morning of the 14th September, 1825. A short time previously, the sky became charged with clouds, until the entire island was covered with a dense black veil. The fall of the stone was immediately preceded by a violent gust of wind from the N.W., and even at sea, sounds like those of thunder were heard. Immediately after these detonations, the aërolite fell in the middle of the village of Ganagauro, and broke into pieces on touching the ground. The Russian travellers gathered many of these pieces, one weighing 15lbs. They resemble the aërolites generally known.—*Bull. Univ.* A. ix. 325.

24. *Meteor exhibiting a peculiar Green Colour.*—"On the night of the 11th of February, (1828), between 11 and 12 o'clock, as I was crossing the East river between this city (New York) and Long Island, I observed a beautiful meteor, which was visible for about two seconds. Its course was from a point perhaps 5° below the zenith, towards the horizon, in a north-east direction. It described an arc of perhaps 20°, when it apparently exploded, but without any report that I could hear. Its colour was a singularly pure *grass green*, of a light shade; the trail which it left was of the same colour, and so were the scintillations which accompanied its apparent explosion. The latter were distinct, like those which accompany the bursting of a rocket, but by no means so numerous. Two gentlemen who were in the boat with me at the time also saw it."—*Mr. Silliman. Silliman's Journal.*

25. *On the Ascent of the Jung Frau.*

Sir, *Interlaken, Sept.* 12, 1828.

Thinking the following account of an ascent to the Jung Frau which has just been accomplished, may be interesting to your readers, I have taken the liberty of sending it to you for insertion in your Journal. I saw (with my telescope) the guides placing the flag on the peak, and have had the account from them this morning.

I am, Sir, yours, &c.

W. Lardner, M.D.

Monday, Sept. 8, 1828.

Christian Roth, guide,
Pierre Burman, chasseur de chamois,
Christian Burman, chasseur,
Pierre Roth, chasseur,
Ulric Widmer, berger,
Pierre Moser, chasseur.
Ildbrand Buregner, chasseur,

left Grindelwald, furnished with ice-pickers, cords, a ladder of 25 feet long, and a red flag, with an iron flag-staff nine feet long. They began their ascent on the glacier of Grindelwald, between the Grand Eiger and the Welterberg; they turned afterwards to the right, and continued their route till the evening: they slept in a large cave formed in the rocks, which perfectly sheltered them; it is situated at the foot of the Grand Eiger, and towards the south side, it is 73 feet long, and 44 wide; at the bottom is another small cave, out of which runs a rivulet. On the 9th, they passed over the summit of Fischerhorn, and descending again by the glacier of Alletsch, they slept the second night behind some rocks which had fallen from the Finisterhorn. Leaving the Finisterhorn on the right, and the Fischerhorn in front of them, on the 10th, turning to the right, they followed the crest which leads to the summit of the Jung Frau toward the Breithorn: there they found two crevices in the ice, which were so large, they were obliged to cross them with their ladder. Higher up they found the ice so sloping, that they were obliged to cut steps in it, which occupied them two hours; at last, about four o'clock P.M., they arrived at the highest plain, and between four and five, they fixed their flag on the peak, two feet deep in the ice. The evening of the 10th, they returned to sleep at the foot of the Finisterhorn, and the 11th in the afternoon, they returned safe to Grindelwald: they did not experience any great inconvenience from the temperature, which they say was even mild; the respiration was a little difficult. A fortnight before, a similar attempt had been made by the same men accompanied by a gentleman from Berne, and after remaining eight days upon the mountain, were obliged to return without having accomplished their object, chiefly for want of provisions; the cold was then much more intense, and had so much affected the skin of their faces that it was peeling off in large patches. In the first ascent their faces were unguarded; in the second they had a double green crape over them.

26. *On the existence of Active Molecules in Organic and Inorganic bodies.*—While Mr. Brown was examining the pollen of various

plants under the microscope, he observed distinct motion in the grains when immersed in water, consisting not only of a change in place, but of form also. Having observed this in the pollen of all the living plants he examined, he next tried to ascertain how long this property continued after the death of a plant, and found that plants dried, or immersed in spirit for a few days, and some even which had been dried for twenty years, and others not less than a century, still exhibited these active particles. Whilst making the observation with the ovula or seeds of the *Equisetum*, they were accidentally bruised, which very much increased the number of moving particles; and on bruising the floral leaves and other parts of mosses, they were also obtained.

With a view of ascertaining whether these active particles, obtained from such different parts of plants, were the supposed constituents or elementary molecules of organic bodies, different animal and vegetable tissues were examined; whether living or dead, if bruised in water, they gave moving particles, identical with those of pollen. They were also found in products of organization, as gum resins, vegetable substances, and even pit-coal. The dust or soot deposited on bodies, especially in London, is entirely composed of them.

As the particles were found in fossil and silicified wood, they were next sought for in inorganic substances, and were at once obtained merely by bruising a small splinter of window-glass upon the stage of the microscope. They were obtained in succession from rocks of all ages, each of the constituents of granite, travertine, stalactites, lava, obsidian, pumice, volcanic-ashes, meteorites, manganese, nickel, plumbago, bismuth, antimony, arsenic, and in every mineral that could be reduced to powder sufficiently fine to be temporarily suspended in water. In many cases the particles seemed to aggregate into linear arrangements or fibrils, consisting of three or four, and these also had motion.

Wood, linen, paper, cotton, wool, silk, hair, and muscular fibre, being burnt, gave the molecules as evidently in motion as before combustion.

The form of these molecules appears to be spherical, but modifications of it occur in certain circumstances; the diameter of the particles are from $\frac{1}{15000}$th to $\frac{1}{20000}$th of an inch.

The principal substances from which these molecules have not been obtained, are oil, resin, wax and sulphur; such of the metals as could not be reduced to the state of division necessary for their separation; and finally bodies soluble in water.

All these observations were made under a simple microscope, and, indeed, with one and the same lens, the focal length of which is about $\frac{1}{32}$d of an inch.—*Phil. Mag.* N. S., iv., 161.

METEOROLOGICAL DIARY for the Months of June, July, and August, 1828, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For JUNE, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Sunday	1	48	70	29.64	29.66	WbS	W
Monday	2	50	66	29.70	29.70	W	SW
Tuesday	3	52	66	29.77	29.70	WbS	SW
Wednesday	4	54	65½	29.50	29.30	WbS	WbS
Thursday	5	44	62	29.15	29.68	W	WSW
Friday	6	49	61	29.40	29.57	W	WSW
Saturday	7	46	60½	29.78	29.90	WNW	WNW
Sunday	8	45	67	29.98	29.98	WSW	WbN
Monday	9	49	67	30.02	30.03	WNW	WNW
Tuesday	10	48	68	30.03	30.07	WNW	WNW
Wednesday	11	55	70	30.07	30.07	WNW	WNW
Thursday	12	53	66	30.03	30.03	WNW	WNW
Friday	13	45	69	30.05	30.05	W	NE
Saturday	14	54	73	30.05	30.05	NE	ENE
Sunday	15	50	69	30.00	29.95	ENE	E
Monday	16	54	71	29.88	29.76	E	NE
Tuesday	17	55	72½	29.54	29.37	NE	ESE
Wednesday	18	50	71	29.37	29.44	NE	WbN
Thursday	19	55	75	29.70	29.74	SW	WSW
Friday	20	53	73	29.77	29.77	WSW	WSW
Saturday	21	58	70	29.71	29.64	S	SW
Sunday	22	53	73	29.60	29.71	W	NW
Monday	23	49	70	29.87	29.92	NW	NW
Tuesday	24	48	70	30.09	30.10	WNW	WNW
Wednesday	25	49	74	30.10	30.10	WNW	WbN
Thursday	26	51	78	30.17	30.17	WbN	W
Friday	27	52	79½	30.10	30.10	WSW	SE
Saturday	28	52	78	29.98	29.90	NE	NE
Sunday	29	57	77	29.82	29.81	E	E
Monday	30	50	74	29.81	29.80	NE	SE

For JULY, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Tuesday	1	57	75½	29.76	29.72	SW	SW
Wednesday	2	61	78	29.72	29.69	WbS	SW
Thursday	3	63	80	29.69	29.70	SSE	SW
Friday	4	62	79	29.58	29.68	SSE	SW
Saturday	5	60	77	29.68	29.68	SW	WbS
Sunday	6	54½	75½	29.68	29.68	SW	W
Monday	7	50	73	29.70	29.70	SE	SbE
Tuesday	8	57	76½	29.60	29.50	E	NE
Wednesday	9	59	64	29.42	29.40	NW	NbE
Thursday	10	53	73	29.47	29.67	NW	W
Friday	11	56	77	29.77	29.70	WSW	SSE
Saturday	12	58	64	29.46	29.39	W	WNW
Sunday	13	53	62½	29.24	29.30	WSW	WSW
Monday	14	49	69	29.35	29.33	W	SW
Tuesday	15	54	73	29.29	29.40	S	E
Wednesday	16	56	74	29.55	29.60	NE	SE
Thursday	17	58	76	29.54	29.54	SSW	W
Friday	18	59	74	29.48	29.40	SE	S
Saturday	19	59	74	29.40	29.40	WbS	SW
Sunday	20	50	67	29.30	29.20	E	NNW
Monday	21	53	73	29.29	29.33	W	WbS
Tuesday	22	57	72	29.33	29.43	WbN	W
Wednesday	23	53	73	29.50	29.50	W	W
Thursday	24	49	76	29.50	29.47	W	SE
Friday	25	57	75½	29.40	29.39	SW	SE
Saturday	26	56	74½	29.54	29.54	SW	SW
Sunday	27	52	67	29.60	29.70	NW	NNE
Monday	28	53	66	29.82	29.82	N	NW
Tuesday	29	47	64½	29.76	29.75	NW	NbW
Wednesday	30	44	62	29.75	29.78	WbN	W
Thursday	31	44	67½	29.82	29.82	WSW	WbS

For AUGUST, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Friday	1	50	69	29.82	29.77	W	SW
Saturday	2	53	68	29.61	29.48	S	S
Sunday	3	51	71	29.43	29.38	WbS	SW
Monday	4	55	70	29.38	29.40	W	W
Tuesday	5	53	70	29.40	29.40	W	W
Wednesday	6	43	69	29.40	29.27	SE	E
Thursday	7	54	68	29.27	29.33	E	E
Friday	8	53	73	29.41	29.47	S	WbS
Saturday	9	52½	68½	29.32	29.16	SE	SE
Sunday	10	54	67	29.40	29.46	SW	WSW
Monday	11	51	68½	29.50	29.48	WSW	S
Tuesday	12	53	69	29.58	29.63	WbS	WbS
Wednesday	13	44	70	29.67	29.60	WbS	SE
Thursday	14	49	67	29.52	29.52	NE	NE
Friday	15	44	66	29.70	29.73	NW	W
Saturday	16	41½	67	29.75	29.80	W	S
Sunday	17	48	72	29.70	29.63	S	S
Monday	18	50	68	29.68	29.78	WbS	WbN
Tuesday	19	51	68	29.91	29.97	W	W
Wednesday	20	51	70	29.94	29.84	WbS	SW
Thursday	21	55	71	29.72	29.62	SW	WSW
Friday	22	52	64	29.57	29.70	WSW	WbS
Saturday	23	50	68	29.82	29.90	W	NW
Sunday	24	46	76	29.98	30.05	W	W
Monday	25	56	75	30.10	30.13	W	NW
Tuesday	26	52	73½	30.13	30.14	NW	SE
Wednesday	27	45	69	30.14	30.12	SE	SE
Thursday	28	46	72	30.09	30.04	ESE	E
Friday	29	48	67½	30.04	30.04	E	E
Saturday	30	52	66½	30.04	30.04	ENE	NE
Sunday	31	56	67	30.02	29.98	NE	ENE

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TO OUR READERS AND CORRESPONDENTS.

We have received a paper from Mr. Meikle, "On the Relation between the Density, Pressure, and Temperature of Air, and on Experiments regarding the Theory of Clouds, Rain, &c.; with a Conjecture about Thunder and Lightning," which an accidental circumstance has obliged us to postpone. It will be found to bear upon his Reply to Mr. Ivory, which we now publish.

The deposit upon the Specimen from the margin of one of the Lakes of Killarney, is Argillo-siliceous. The Chalk contains Alumina, but no Magnesia. The Pyrites is common.

In Mr. Addams' paper on the Sap of the Rose, published in our last Number, p. 149, line 9 from the bottom, for "oxalate of ammonia," read "*oxalate of lime.*"

An account of the proceedings of the Royal Society, at their Anniversary Meeting, will be found in the *Literary Gazette* for the following Saturday.

The Drawing of an Ornamental Aviary, illustrative of the paper which we printed in our last Number, by J. C. Cox, Esq., reached us too late for insertion.

We request "X." to refer to our *private* letter upon the subject of Gas-engines. The whole affair is as we have there stated it, *et prœterea nihil.*

Our attention has been directed, by more than one Correspondent, to the operations lately carrying on in the Chelsea Water Company's Reservoir in the Green Park. We wish to see them terminated before we say more. In the mean time, we beg the writer, who dates from Paddington, and who deprecates the use of reservoirs, to recollect that, if these thousands of cartloads of filth had not been deposited where they now are, the whole mass of nastiness must inevitably have passed down the throats of the worthy persons who derive their supplies from the said Basin. The sooner the Reservoir in Hyde Park is looked after, the better. We should also like to know something of the state of the West Middlesex Reservoir at Kensington.

The Letter on the Burial of the Dead in the Streets of London, is rather intemperate, and we must decline its insertion, entirely agreeing, however, with the writer as to the indelicacy, impropriety, and, we may say, barbarity of the custom. We are surprised that he omitted the churchyard, as it is called, of St. Clement's in the Strand, where the burial-service is often performed in the actual street, and within hearing of the profane language and other abominations of the draymen, coal-heavers, hackney-coachmen, *et id genus omne;* and where the tomb-stones almost form part of the public pavement of that most public situation.

It is with sincere and deep regret that we announce the death of Dr. W. H. Wollaston, which took place on the 22nd of December, at his house in Dorset Street, Manchester Square. His loss will be severely felt by his numerous friends and acquaintance, and generally deplored throughout the scientific society of Europe.

THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

Facts towards a History of Eating, Drinking, and Sleeping.

By William Wadd, Esq., F.L.S.

I. Eating.

Philosophers, who have puzzled themselves how to define man, so as to distinguish him from other animals, have defined him as "animal risibile"—as a "buying and selling animal"—an animal that makes bargains. Some have even defined man to be a tree, bottom upwards, the brain being the root. The author of the "Sublime and Beautiful" settled the question in a different way: he said, "man is an animal that cooks its victuals!" thereby proving the justness of the proverbial axiom, "there is reason in roasting eggs."

These speculations, however, were all done away by another set of philosophers, who thought they had reconciled all difficulties, when they characterized him by his *stomach:* but alas! this theory, like the others, vanished, when it was discovered that the human stomach occasionally possessed the powers of the brute stomach.

This wonderful power of the human stomach, technically termed by doctors "*fames canina*"—"fames lupina and bulima," has been considered by some as a disease, and if we believe Forestus, is of very ancient date; for he records, that in Syracuse "there was an universal disease, called the 'hungry sickness,' in which people did desire continually to eat, and never were satisfied; of this," he says, "multitudes died.*" And Bonetus, has a chapter on the subject, "De Fame præternaturali."

* Forest. Obs. Med. Part 3.

It would appear from certain well-known facts, that this hungry sickness is to be found in some portion of the population of the present day; and from the strongly marked symptoms that occasionally manifest themselves in corporate bodies, we might further believe that some aldermen are of Syracusan descent.

That this disease was of great antiquity may be inferred from the circumstance, that the ancient poets made Hunger one of the Pagan deities; and from the place they assigned to the Pagan god, it would appear that they had adopted their notions from "*fames canina*"—for we are told—"ils la placaient à la porte de l'enfer, avec les maladies, les chagrins, les remords, l'indigence, et les autres maux, qu'ils s'étoient plu à diviniser."

A species of "fames canina" is also to be met with amongst schoolboys, differing from the Syracusan disease in respect to its affecting the *juveniles* most when most in health. We remember a gentleman offering a wager, that a boy taken promiscuously from any of the public charity-schools, should, five minutes after his dinner, eat a pound of beef-steaks.

There is another class of scholastic persons, adults, known in the literary world as poets and critics—who are also troubled with this complaint; which attracted the attention of one learned person so far, as to suggest the idea of "A Treatise on Τις αιτια βουλιμȣ, or what is the reason Critics are always hungry?"—ingeniously conjecturing, that a poor author is devoured by them, in proportion to the temporary action of this complaint on their stomachs.

Sauvages has enumerated seven different species of Bulimy; but in most of the instances which he has mentioned, it is rather to be regarded as a concomitant of other disorders, than as a distinct and specific affection.

Ruysch gives an instance of this complaint, which was connected with a dilation of the pylorus, in consequence of which, the food slipped through the stomach into the intestines, before there was time for digestion to take place: and it is recorded by Lieutaud, that upon opening the body of a patient who had died of a disorder, in which a voracious appetite was a leading symptom, he discovered a preternatural termination of the ductus choledochus in the stomach. In this case, the bile effused into the stomach seems to have kept up a constant irritation, by which the ingesta were expelled before digestion took place.

We have before paid our respects to voracious eaters: those whom we are now to introduce are inordinate swallowers, who may be said to devour, rather than to eat.

The first, and most notable of these, is the famed Nicholas Wood, whose excessive manner of eating, without manners, is described, in strange and true manner, about the year 1630, by no less a person than the celebrated water-poet, John Taylor, in a book entitled "NICHOLAS WOOD, *the great eater; or the admirable teeth and stomach exploits of Nicholas Wood, of Harrison, in the county of Kent.*" It is a very singular species of composition; and lest his prose should be taken for the flights of poetic fancy, he gives very cogent reasons why, in writing the memorable actions of Nicholas Wood, he tells nothing but plain truth, bare and threadbare, "almost stark-naked truth."

"First," he says, "I were to blame to write more than truth, because that which is known to be true is enough."

"Secondly, that which is only true is too much."

"Thirdly, the truth will hardly be believed, being so much beyond men's reason to convince."

"Fourthly, I shall runne the hazard to be accounted a great lyar, in writing the truth."

"Lastly, I will not lye, on purpose to make all those lyars that esteeme me so."

Our author then enters upon his history, and after pleasantly paraphrasing his name, and telling us, "that his mouth was a mill of perpetual motion, for let the wind or the water rise or fall, yet his teeth would ever be a-grinding;" and that his breeding would have been most mighty, if his education had been as his feeding, he proceeds to enumerate some of his exploits. "Two loynes of mutton, and one loyne of veal, were but as three sprats for him." Milo, the Crotonian, could hardly be his equal; and Woolner, of Windsor, was not worthy to be his footman.

"A quarter of fat lambe, and three-score eggs, has been an easy collation—three well-larded pudding pyes he hath at one time put down—eighteen yards of black-puddings (London measure) have suddenly been imprisoned in his *sowse-tub.*"

He was by no means difficult to please in cookery, nor was he nice in his palate. The peacock of *Samos*, the woodcock of *Phrygia*, the cranes of *Malta*, the pheasant of *England*, were all baubles with him. He was an Englishman, and English diet served his turn. "If the Norfolk dumpling and the Devonshire white-pot be at variance," says Taylor, "he will atone them; the *bag-puddings* of Gloucestershire, the *black-puddings* of Worcestershire, the *pan-puddings* of Shropshire, the *white-puddings* of Somersetshire, the *pasty-puddings* of Hampshire, and the *pudding-pies* of any shire, all is one to him; nothing comes amisse, a contented mind is worth all; and let any thing come in the shape of eating stuff, it is welcome."

Taylor seems to have had it in contemplation to make money of him, by exhibiting him at the Bear-garden, but Wood disappointed his scheme, and for very substantial reasons—first, as he was waxing old, and having lost all his teeth, but one, in eating a quarter of mutton, bones and all, he feared he should lose his reputation, though he could eat a fat wether, if it were boiled; and secondly, he feared, that if the king should hear of one who ate so much, and could work so little, an order would come to hang him*.

In more recent times, there has been some well authenticated cases in the public journals; and some are to be found in the *Philosophical Transactions.*

In 1700, there lived at Stanton, seven miles from Bury, a labouring man of middle age, who for many days together had such an inordinate appetite, that he would eat up an ordinary leg of veal, or a leg of mutton, at a meal. He would eat

* While *Charles Gustavus*, the successor to *Christina*, queen of *Sweden*, was besieging *Prague*, a boor of most extroardinary visage, desired admittance into the royal tent, and offered, by way of amusing the king, to devour a whole hog, of one hundred weight, in his presence. The celebrated old general, *Konigsmarc*, was at this time standing by the king's side, and though a soldier of great courage, being tainted in some degree with superstition, hinted to his royal master, that the peasant ought to be burnt for a sorcerer. "Sir," said the fellow, highly irritated by the observation, "if your majesty will but make that old gentleman take off his sword and his spurs, I will eat him immediately before I begin the hog."

sow-thistles, and various other herbs, as greedily as cattle are wont to do; and all he could get was little enough to satisfy his hunger. He voided several long worms. This case is related by Dr. Burroughs, in the 22d vol. Phil. Trans. Two other cases are recorded in the 43d vol. of the same Transactions, one by Dr. Mortimer, the other by Dr. Cookson. The subjects of this affection were in both instances boys. The first of them was twelve years old, and lived at Blade Barnsley, in Yorkshire. His appetite was so ravenous, that if he was not supplied with food when he craved it, he would gnaw the flesh off his own bones. When awake he was constantly devouring. Nothing passed his stomach; it was always thrown up again. In the space of six days he devoured 384 lbs. of liquid and solid food*.

The other boy was ten years old, and had been seized with a fever about fifteen months before, which continued for a fortnight, and was followed by constant vomitings. As in the former case, so in this: the food was no sooner swallowed, than thrown up again. In the space of six days, this boy devoured 371 pounds of meat and drink, besides one pound ten ounces of salt. After more than a twelvemonth from the first attack, he died, greatly emaciated.

Another case of canine appetite, accompanied with vomiting, is recorded by the late Dr. Lettsom, in the third volume of the Memoirs of the Medical Society. In this case, 379 pounds of solid and fluid aliment were taken into the stomach, in the space of six days.

A gentleman who had not been remarkable for his appetite, became, without any apparent reason, very voracious, so much so, that he could not dine out, without first allaying the cravings of his stomach. He consulted several medical men, without any alteration in his complaint. By accident, a book on the medicinal virtues of water, came in his way, and from the doctrines contained in it, he was induced to give up all other fluids, which in the course of a few months restored him to his former state of health.

* This boy, had he lived to manhood with the same propensities, would have furnished a singular exception in the history of Anthropophi—viz. a man eating himself.

From these observations, it is evident that the plan of treatment must be varied, according to the diversity of the morbid conditions with which it is connected. Thus, when it is the consequence of an immediate rejection of the food by vomiting, the irritability of the stomach should be counteracted by mild gelatinous food, by opiates, tonics, &c., aided by stimulant epithems, and other topical applications. At the same time, nutritive enemata should be injected into the intestines.

In like manner, when it is the consequence of the food passing off too rapidly (as in lientery), the remedies adapted to that condition of the body should be resorted to. If it proceed from worms, calomel, jalap, cowitch, and other anthelmintics, should be prescribed; if from mesenteric obstruction, rhubarb, neutral salts, and afterwards chalybeates; and lastly, when it arises from acidity in the stomach and intestines, bitters and absorbents should be given.

A false appetite, a craving that does not arise from the demands of health, but from the morbid piquancy of the juices in the stomach, is a state in which more is taken than can be digested—the food is devoured rather than eaten.

This condition of stomach has led to the notion that the parties have had to feed another animal besides themselves; and the lower orders do not hesitate to believe, that a large worm, and even a wolf, are occasionally inhabitants of that viscus.

II. Drinking.

Use a little wine, for thy stomach's sake.—1 Tim. v. 23.

So says St. Paul—and this seems to have been the opinion of the most ancient philosophers and physicians. A moderate use of it has been sanctioned by the wise and good in all ages. Those who have denied its virtues are those who have not been able to drink it. Asclepiades wrote upon wine, the use of which he introduced with almost every remedy, observing, that the gods had bestowed no more valuable gift on man: even the surly Diogenes drank it; for it is said of him, that he liked that wine best, which he drank at other people's cost—

a notion adopted by the oinopholous Mosely, who, when asked, "What wine do you drink, Doctor?" answered, "Port at home—claret abroad!"

Hippocrates, the father of physic, recommends a cheerful glass; and Rhases, an ancient Arabian physician, says, no liquor is equal to good wine. Reineck wrote a dissertation "De Potu Vinoso," and the learned Dr. Shaw lauded the "juice of the grape." But the stoutest of its medical advocates was Tobias Whitaker, physician to Charles the Second, who undertook to prove the possibility of maintaining life, from infancy to old age, without sickness, by the use of wine*!

It must, however, be remembered, that Whitaker was cordially attached to wine, and a greater friend to the vintner than to the apothecary, having as utter a dislike to unpalatable medicines, as the most squeamish of his patients: therefore, Dr. Toby's evidence must be taken with caution, independently of the courtly spirit that might have led him to adapt his theories to the times.

It has been questioned whether the use of wine was known to the antediluvian world; but there can be no doubt, in the corrupt state of man, that wine would have its share in his debasement, and it may be very strongly inferred, from the circumstance that Noah planted a vineyard, and, moreover, "that he drank of the wine, and was drunken" (Gen. ix. 20) —a sad stain in the character of a man who was "perfect in his generation;" and which also proves that, in the earliest period of the world, the very best of men were liable to fall into error and excess.

But the antiquity and propriety of wine-drinking is not matter of question. The Archbishop of Seville, Antonio de Solis, who lived to be 110 years old, drank wine; and even that wonderful pattern of propriety, Cornaro, did the same: but the question is about quantity. Sir William Temple was pleased to lay down a rule, and limit propriety to three glasses. "I drink one glass," says he, "for health, a second for refreshment, a third for a friend; but he that offers a fourth is an enemy."

As in eating, so in drinking, in the question of quantity—

* "Tree of Human Life, or the Blood of the Grape." 1638.

much depends on the capacity of the stomach. A very abstemious friend of mine, not long since, dined tête-à-tête with a gentleman well known for his kindness and hospitality, and not less so for his powers of bibulation. After dinner, at which a fair share of many excellent wines was taken, Port and Madeira were put on the table, and before the host, a *magnum* of Claret. My friend drank his usual quantum, three glasses of Madeira, during which time a great portion of the magnum had disappeared; and soon afterwards, being emptied, the host said, "I think we can just manage a bottle between us." The bottle was brought, and very shortly disappeared, without the aid of the visiter.

The same gentleman and Lord ———, at the Angel at Bury, fell in with some excellent Claret. They had disposed of six bottles, when the landlord, who did not guess or *guage* the *quality* of his customers (the bell being rung for a fresh supply), begged very gently to hint that it was expensive stuff, being fifteen shillings a bottle! "Oh! is it so? then bring up two bottles directly!"

We have nothing, however, in modern times, at all equal to the account given of some of the ancients. The elder Cato, we are told, warmed good principles with a considerable quantity of good wine*. But Cicero's son exceeds all others; so much so, that he got the name of *Bicongius*, because he was accustomed to drink two congii † at a sitting. Pliny, and others, abound in grand examples, that prove we have degenerated at any rate in this respect, for these convivials were neither sick nor sorry. Even that eminent debauchee, Nero, was only three times sick in fourteen years. "Nam qui luxuriæ immoderatissimæ esset, ter omnino per xiv. annos languit; atque ita, ut neque *vino*, neque consuetudine *reliqua* abstineret."—*Sueton. de Nerone.*

The Abbé de Voisenon, a very diminutive man, said to his physician, who ordered him a quart of ptisan per hour, "Ah! my friend, how can you desire me to swallow a quart an hour? I hold only a pint."

* Cato allowed his slaves, during the Saturnalia, four bottles of wine per diem.

† Two congii are seven quarts, or eight bottles!

Wine has not only been considered good for the body, but has, from the earliest period, been thought invigorating to the mind. Thus we find it a constant theme of praise with poets. Martial says—

> Regnat nocte calix, volvuntur biblia mane,
> Cum Phœbo Bacchus dividit imperium.
>
> All night I drink, and study hard all day:
> Bacchus and Phœbus hold divided sway.

Horace has done ample justice to it; and even Homer says—

> The weary find new strength in generous wine.

Upon the principle, no doubt, of expanding the imagination, we find, so early as 1374, old Geoffrey Chaucer had a pitcher of wine a day allowed him. Ben Jonson, in after times, had the third of a pipe annually; and a certain share of this invigorating aliment has been the portion of Laureates down to the present day.

At Dulwich College are preserved some of Ben Jonson's Memoranda, which prove that he owed much of his inspiration to good wine, and the convivial hours he passed at the Devil, a tavern then situated in Fleet street, near Temple Bar, on the site where Child's Place now stands. "*Mem.* I laid the plot of my 'Volpone,' and wrote most of it, after a present of ten dozen of Palm Sack from my very good Lord T——: that play, I am positive, will live to posterity, and be acted, when I and Envy be friends, with applause."—"*Mem.* The first speech in my 'Catalina,' spoken by *Sylla's Ghost*, was writ after I parted with my friend at the Devil tavern. I had drank well that night, and had brave notions. There is one scene in that play which I think is flat. I resolve to drink no more water with my wine."—"*Mem.* Upon the 20th of May, the King (Heaven reward him!) sent me a hundred pounds. At that time I went often to the Devil; and, before I had spent forty of it, wrote my 'Alchymist.'"—"*Mem.* 'The Devil an Ass,' the 'Tale of a Tub,' and some other comedies, which did not succeed, written by me in the winter honest Ralph died, when I and my boys drank bad wine at the Devil." Æschylus wrote some of his tragedies under the influence of wine.

Nor are the poets the only eulogists of wine. Some of the greatest names in history are to be found in the list. We find Mr. Burke furnishing reasons why the rich and the great should have their share of wine. He says, they are among *the unhappy*—they feel personal pain and domestic sorrow—they pay their full contingent to the contributions levied on mortality in these matters;—therefore they require this sovereign balm. "Some charitable dole," says he, "is wanting to these, our often *very unhappy brethren*, to fill the gloomy void that reigns in minds which have nothing on earth to hope or fear; something to relieve the killing languor and over-laboured lassitude of those who have nothing to do."

This observation of Mr. Burke's introduces it to our notice as a remedy—as a medicine, in the hands of the physician. Thus we find particular wines recommended by particular doctors, having a fashionable run as specifics:—at one time all the gouty people were drinking Madeira; and many a man persuaded himself he had a fit of *flying* gout, for the sake of the remedy*. Somebody, however, found out that Madeira contained acid, and straight the cellars were rummaged for old Sherry. This change was attributed to Dr. Baillie, who had no more to do with it than Boerhaave, as he has been known to declare. Sherry, and nothing but Sherry, however, could or would the *Podagres* drink.

Dr. Reynolds, who lived and practised very much with the higher orders, had a predilection for that noble and expensive comforter, Hoc! which short word, from his lips, has often made the Doctor's physic as costly as the Doctor's fee. He was of opinion, with the Poet—

> *Hoc* continet coagulum convivia;
> *Hoc* hilaritatis dulce seminarium;
> *Hoc* ægritudinem ad medendam inveniterunt.

Wine has also been recommended, by the highest medical authorities, as alleviating the infirmities of old age. "Le vin

* An eminent house-painter in the City, a governor of St. Bartholomew's Hospital, got a receipt for the Painter's Cholic (cholica pictonum), which contained all sorts of comfortable things—the chief ingredients being Cogniac brandy and spices. It did wonders with the first two or three cases; but he found the success of the remedy so increased the frequency of the complaint, that he was compelled to give up his medical treatment; for as long as he had the *Specific*, his men were constantly making wry faces at him.

est le lait des vieillards," says a French doctor; and so Horace before him:—

> Tu senum nutrix querelas benigno
> Lacte titillas.

A Greek physician recommended it to Alexander as the pure blood of the earth.

Though an excess in wine is highly blameable, yet it is more pardonable than most other excesses. The progressive steps to it are cheerful, animating, and seducing; the melancholy are relieved, the grave enlivened, the witty and gay inspired—which is the very reverse of excess in eating: for, Nature satisfied, every additional morsel carries dulness and stupidity with it. "Every inordinate cup is unbless'd, and the ingredient is a devil," says Shakspeare.

"King Edgar, like a king of good fellows," adds Selden, "or master of the revels, made a law for Drinking. He gave orders that studs, or knobs of silver or gold (so Malmesbury tells us), should be fastened to the sides of their cups, or drinking vessels, that when every one knew his mark or boundary, he should, out of modesty, not either himself covet, or force another to desire, more than his stint." This is the only law, before the first parliament under King James, that has been made against those swill-bowls,

> Swabbers of drunken feasts, and lusty rowers,
> In full-brimmed rummers that do ply their oars,

"who, by their carouses (tippling up Nestor's years as if they were celebrating the goddess *Anna Perenna*), do, at the same time, drink others' health, and mischief and spoil their own and the public."

Amongst other reasons for taking a little wine, a French gentleman offers the following:—"Un amateur de bon vin faisait jadis ce joyeux raisonnement à son confesseur, qui le gourmandait sur son penchant à boire, en lui annonçant qu'il ne ferait jamais son salut, s'il ne se corrigeait de cette passion: 'Mon père, le bon vin fait du bon sang; le bon sang produit la bonne humeur; la bonne humeur fait naître les bonnes pensées; les bonnes pensées produisent les bonnes œuvres; et les bonnes œuvres conduisent l'homme dans le ciel.

'*Ainsi soit-il,*' répondit le pasteur, converti à son tour par son pénitent."

An argument very much after this fashion was held by the learned Sir Thomas More. Sir Thomas was sent ambassador to the Emperor by King Henry the Eighth. The morning he was to have his audience, *knowing the virtue of wine*, he ordered his servant to bring him a good large glass of Sack; and, having drank that, called for another. The servant, with officious ignorance, would have dissuaded him from it, but in vain; the Ambassador drank off a second, and demanded a third, which he likewise drank off: insisting on a fourth, he was over-persuaded by his servant to let it alone; so he went to his audience. But when he returned home, he called for his servant, and threatened him with his cane. "You rogue," said he, "what mischief have you done me! I spoke so to the Emperor, on the inspiration of those three glasses that I drank, that he told me I was fit to govern three parts of the world. Now, you dog! if I had drank the fourth glass, I had been fit to govern all the world." With such authority, may we not say—

Bibe; si sapis, bibe.

The French, a very sober people, have a proverb—

Qu'il faut, à chaque mois,
S'enivrer au moins une fois.

Which has been improved by some, on this side the water, into an excuse for getting drunk every day in the week, for fear that the *specific day* should be missed. It would, however, startle some of our sober readers, to find this made a question of grave argument—yet, "whether it is not healthful to be drunk once in a month," is treated on by Dr. Carr in his letters to Dr. Quincy.

A French author has written a long éloge "*De l'Ivresse*," in which there is a chapter entitled, "*Qu'il est bon pour la santé de s'enivrer quelquefois.*" He sings in animated strains—

Buvez, mes chers amis, et buvez à grands coups.
Quels siècles de santé vous aurez devant vous!

Drink, my dear friends, and deeply too,
Ages of health you'll have before you.

Having said thus much on the subject of wine-drinking, both as it relates to health and conviviality, it seems incumbent on us to take some notice of the stronger potations resorted to in this country, by the middling and lower orders of society, in the joyous hilarity of public rejoicings, or the gay festivity of private merry-making*. Some "cordial drop" is required; and spirits being cheaper and stronger than wine, are resorted to in the shape of grog—rum-punch—or toddy.

Now, though we would, if we could, laud—like the philosophers and poets of old—good wine, and would gladly take our share of it, with the philosophers and poets of the present day, yet we feel very differently concerning dram-drinking. We are no advocates for the votaries of that power, who, of all the fabled divinities, treats his followers with most unkindness; who repays their libations with malady, their songs with degrading infirmities, their triumphs with defeat.

"Of all the contrivances to exclude the intruding demon Ennui from the mind of man, the most debasing and destructive is, the use of intoxicating liquors; that pernicious habit blunts all desire of improvement, deadens emulation, obscures the understanding, sinks the soul into sluggishness, renders men insensible to the love of reputation, familiarizes them with the idea of contempt, and extinguishes every enjoyment, but that maudlin delirium excited by spirituous liquors, which soon hurries them to their graves."

PUNCH.

Punch was first made by the English at Nemle, near Goa, where they have the *Nepa die Goa,* commonly called arrack.

* Sir Joseph Banks used to tell a story of his being at Otaheite with Capt. Cook, when it was accidentally discovered to be the King's birthday, on which it was suddenly agreed to have a jollification; every soul on board got fuddled, except three men who were on duty. The next day they came on deck, and begged to speak to the captain. "Well," said the captain, "what have you got to say?" "Please your honour, you were all drunk yesterday, all except we three; will your honour be pleased to allow us to get drunk to-day?" Sir Joseph, who was standing by, was so tickled with the oddity of the request, that he begged they might be indulged, and that he would subscribe two bottles of rum, and two bottles of brandy. The boon was granted, and in less than three hours, these messmates balanced accounts, being as drunk as their hearts could wish.

This fascinating liquor got the name of *punch,* from its being composed of *five* articles—that word, in the Hindostanee language, signifying five. The legitimate punch-makers, however, consider it a compound of *four* articles only; and some learned physicians have, therefore, named it *Diapente* (from Diatesseron), and have given it according to the following prescription—

Rum, miscetur aqua—dulci miscetur acetum, fiet et ex tali fœdere—nobile Punch.

and our worthy grand-fathers used to take a dose of it every night in their lives, before going to bed, till Doctor Cheyne alarmed them by the information, that they were pouring liquid fire down their throats. "Punch," said he, "is like opium, both in its nature and manner of operation, and nearest arsenic in its deleterious and poisonous qualities; and, so," added he, "I leave it to them, who, knowing this, will yet drink on and die."

Now, we cannot but think this philippic rather strong, and applicable only to strong punch, such, perhaps, as made such terrible depredations on the noble faculties of Sophia Western's waiting woman, Mrs. Honour. Fielding, who understood the effects of this liquor exceedingly well, evidently hints, that the punch, in this case, must have been made of bad rum, from its making "such terrible depredations on her noble faculties;" for he says, "as soon as the smoke began to ascend to her pericranium, she lost her reason, while the *fire* in the stomach easily reached the heart, and *inflamed* the noble passion of pride!" p. 68. All this only proves that the mixture was not secundum artem, nor the dose properly proportioned.

We argue thus also, from our own personal experience. Who, that has drunk this agreeable accompaniment to calapash, at the City of London Tavern, ever found themselves the worse for it? They may have felt their genius inspired, or their nobler passions animated—but *fire* and *inflammation* there was none. The old song says—

It is the very best of physic.

and there have been very excellent physicians, who have confirmed the opinion by their practice. What did the learned Dr. Sherard, the grave Mr. Petiver, and the apothecary Mr.

Tydall, drink in their herborizing tour through Kent? Why—punch! and so much were they delighted with it, at Winchelsea, that they made a special note in their journal, in honour of the *Mayoress*, who made it, that the punch was not only excellent, but that "each succeeding bowl was better than the former!"

Captain John Graunt, in his Observations on the Bills of Mortality, says, that of 229,250 persons, who died in twenty years, only *two* are put to the account of *excessive drinking*. But, perhaps, if the matter were truly stated, a great many of the dropsies, apoplexies, and palsies, ought to have been placed under that head. It is not impossible that those who had the charge of rendering these accounts, might have entertained the opinion of old Dick Baldwyn, who stoutly maintained that no man ever died of drinking. "Some puny things," said he, "have died learning to drink, but no man ever died of drinking!" Now, this was no mean authority; for he spoke from great practical experience, and was moreover many years treasurer of St. Bartholomew's hospital.

III.—Sleeping.

All agree in the value and necessity of sleep—

> Sleep, that knits up the ravell'd sleeve of care;
> The birth of each day's life, sore labour's bath,
> Balm of hurt minds, great nature's second course,
> Chief nourisher in life's feast—

as Shakspeare has it—all of which is confirmed by philosophers, and poets, as well as by Sancho's homely opinion, that "it wraps round the heart like a blanket," for which he very emphatically exclaims, "blessed be the man who invented it."

With some, going to bed, and going to sleep, are synonymous terms; these persons, in nursery language, are said to "sleep like a top;" whilst others "sleep like a watch-dog," and count the clock from midnight till morn: amongst the most profound adepts of the former class, may be reckoned the guardians of the night.

Though the necessity of sleep for the refreshment of the body be admitted, yet it is possible for a person to sleep a long night through, and be none the better for it, as is the case with those who are troubled with that most horrid of horrors, the nightmare. A ticket porter, who has been all day with a heavy load on his shoulders, does not feel half so much fatigued, as the person who has been carrying an imaginary chest of drawers on his sternum all night. Thus—

> When man o'er-laboured with his being's strife,
> Shrinks not to sweet forgetfulness of life,

but, *dreams*—he owns, with Hamlet, "there's the rub."

A question has been raised, how much sleep is required, and how long it is necessary to be in bed, for the purpose of rest and refreshment. Eight hours have been allotted for the labourer, and six for the scholar and gentleman.

Very few gentlemen, however, are satisfied with this scale; and a capacity for sleeping makes the greater part of this class of the community inclined to double the period. The capacity for sleeping, like the capacity for eating and drinking, is to be increased by indulgence. Much depends upon habit. Some people can sleep when they will, and wake when they will; and are as much refreshed with a short nap as a long one. Sea-faring people have this property from education. I have known persons who have never indulged in a second sleep. One gentleman, who entertained a notion that a second nap was injurious, invariably got up as soon as he awoke, no matter how early the hour—winter or summer.

Others, again, will sleep for four-and-twenty hours. The celebrated Quin had this faculty. "What sort of a morning is it, John?" "Very wet, Sir." "Any mullet in the market?" "No, Sir." "Then John, you may call me this time to-morrow." So saying, he composed himself to sleep, and got rid of the ennui of a dull day, in the arms of Morpheus.

One gentleman, in the Spectator, used to sleep by weight. "I allow myself, one night with another, a quarter of a pound of sleep, within a few grains, more or less; and if upon my rising, I find I have not consumed my whole quantity, I take out the rest in my chair."—No. 25.

A lazy old woman used to apologize for lying in bed, by saying that "she lay in bed to contrive." Strange as this old woman's excuse was, it was an example followed by one of the most extraordinary geniuses of this country, viz. Brindley, of whom it is recorded, that when any great difficulty occurred in the execution of his works, having little or no assistance from books, or the labours of other men, his resources lay within himself. In order, therefore, to be quiet, and uninterrupted, whilst he was in search of the necessary expedients, he generally retired to his bed; and he has been known to lie there one, two, or three days, till he had obtained the object in view. He would then get up and execute his design without any drawing or model.

There are different kind of sleepers, as well as different kinds of sleep: some cannot sleep *from* home—others cannot sleep *at* home; some can sleep on a board, and snore on a carpet; while others tumble and toss on a soft bed, as if the down disconcerted them.

Some again cannot sleep in a noise; others cannot sleep out of it. A miller awakens the moment the mill stops; and a tradesman from Cheapside cannot sleep in the country, because "it is so plaguy quiet."

Somnambulists, or sleep-walkers, usually sleep with their eyes open; but without vision. Shakspeare, who may be considered very good medical authority, makes Lady Macbeth a somnambulist with her eyes open—"but their sense is shut." This is not always the case, however, and there is a singular exception, in the instance of Johannes Oporinus, a printer, who being employed one night in correcting the copy of a Greek book, fell asleep as he read, and yet ceased not to read, till he had finished not less than a whole page, of which, when he awoke, he retained no recollection.

There are many curious histories of sleeping prodigies on record. The *Philosophical Transactions* have several: in one, a man slept from August till January. There is a case, read before a society of physicians, in 1756, of Elizabeth Orvin, who began her sleeping fit in 1738, by a four days' nap, and for ten years afterwards, never slept less than seventeen hours out of the four-and-twenty. Dr. Brady relates, that some strange methods were resorted to, to rouse her—such as rubbing her back with honey, and in a hot day exposing her to a hive of bees, till her back was full of bumps;—making a pincushion of her, and performing acu-puncturation, with pins and needles ;—flagellation, and "other odd experiments," which the Doctor informs us he thinks better " to pass over in silence," all of which might as well have been spared, for she was very sulky, and good-for-nothing, when she was awake. This sulkiness, however, should be noticed, as being connected with the complaint. Previously to this somnolent disease, many of the persons have become uneasy, sullen, and surly. In all, the mind has evidently been affected ; and in some, where there has been extreme abstinence, their waking hours have been characterized by decided mental aberration.

A lady in perfect health, twenty-three years of age, was asked by the parents of a friend to be present at a severe surgical operation. On consideration, it was thought wrong to expose her to such a scene, and the operation was postponed for a few hours. She went to bed, however, with the imagination highly excited, and awoke in alarm, hearing, or thinking she heard, the shrieks of her friend under the agony of an operation. Convulsions and hysterics supervened, and on their subsiding, she went into a profound sleep, which continued sixty-three hours. The most eminent of the faculty were then consulted, and she was cupped, which awoke her ; but the convulsions returned, and she again went to sleep, and slept with few intermissions for a fortnight. For the next twelve months, she remained perfectly well. The sleeping began again without any apparent cause, which, in irregular periods, continued for ten or twelve years, the length of the sleeping fits being from thirty to forty hours, diminished in duration as time went on, till she got well. Then arrived irritability, and total want of sleep, for three months, which was succeeded by aberration of mind. This state continued about six months, when, to the relief of her friends, her sleeping fits returned, and were very regular in their periods, both as to arrival and duration.

Her usual time for sleeping was forty-eight hours. She would in the intermediate day be very well, till twelve at night, when she went to bed. Sometimes she would awake for a few minutes, take some warm fluid, which was always kept ready with a lamp ; but found any effort to remain awake unavailing, and the bare notion of attempting it gave her great horror.

Amongst the sleepy people of modern times, the case of Elizabeth Perkins, of Morley St. Peter, in Norfolk, should be noticed as a case somewhat resembling that just alluded to. For a considerable time she was very regular in her times of waking, which was once in seven days, after which they became irregular and precarious, and, though of shorter duration, they were equally profound; and every attempt at keeping her awake, or awaking her, were vain. Various experiments were tried; and an itinerant empiric, elated with the hope of rousing her from what he called "her counterfeit sleep," blew into her nostrils the powder of white hellebore, being a very powerful sternutative; but the poor creature remained insensible to the inhumanity of the deed, which, instead of producing the boasted effect, excoriated the skin of her nose, lips, and face.

Buonaparte was polite enough to say to a gentleman, "J'irai dormir vîte pour vous;" from which we may conclude. that he possessed some of the properties of the man who advertised, in the Spectator, that he intended to sleep at the Cock and Bottle, in Little Britain.

The following account of this affair is from a scarce tract in the British Museum:—"The sleepy man awakened of his five days' dream; being a most strange and wonderful true account of one Nicholas Heart, a Dutchman, a patient of St. Bartholomew's Hospital, in West Smithfield, who sleeps five days every August: and you have a true relation how his mother fell in one of her sleeps on the first of August, she then being near the time of her labour; and on the fifth day she wakened, and was delivered. As soon as he was born, he sleeped for five days and five nights: together with the true dream which he and his mother dreamt every year alike. But what is more particular than all the rest, he gives an account of one Mr. William Morgan, who he saw hurried to a dismal, dark castle; and one Mr. John Paimer, he saw him going into a place of bliss: these two men were patients in the hospital, and dy'd while he was in his sleep. London: printed by Edward Midwinter, at the Sun, Pye Corner, Smithfield." We have here given the whole of the title, which tells nearly all about this sleepy set.

A Geological and Geographical Sketch of the Island of St. Christopher. Communicated by I. C. Lees, Esq.

The island of St. Christopher is situated in 17° 16′ N. lat., and 62° 31 W. long.; it lies about S. E. and N. W. To the eastward of it is Antigua, distant about fifty miles; to the northward, St. Bartholomew and St. Martin, distant about thirty miles; to the westward, St. Eustatia and Saba, the former distant about ten, the latter about fifteen miles; Nevis is to the S. E., distant about eight miles from the town of Basse-terre, but not more than one mile from the extremity of the island, which stretches in a narrow neck of irregular hills in that direction, and terminates in a nook, somewhat resembling the head of a violin. It is one of the most beautiful, and formerly the most productive, of the sugar colonies; its original Indian name, "Liamuiga," signified the fertile island.

This island is entirely composed of volcanic matter, in some places alternating with submarine productions. The principal mountain is situated at the western end of the island; it is an exhausted volcano, called in books of navigation, charts, &c., Mount Misery; the inhabitants, however, do not call the whole mountain by that name, but only a part of it, which consists of a large aggregate of rock on its summit, forming the N. W. side of the crater. The summit of this mountain is 3711 feet above the sea; it appears, as far as I could judge, to consist of large masses of volcanic rocks, roasted stones, cinders, pumice, and iron-clay. The whole extent of land, to the sea-shore on either side, may be considered as the base of this mountain, as it rises with a pretty steep ascent towards it; but from the part which is generally considered the foot of the mountain, it takes a sudden rise of an average angle of about 50°. To the east another chain of mountains runs, of a similar formation, though of inferior height. On the summits of these there are no remains that indicate their having ever possessed a crater: so that whether any of them have originally been volcanoes, or whether they have been formed by an accumulation of matter thrown out of Mount Misery, it is difficult to decide. That the low lands have been thrown from the mouth of the volcano is evident, from the regular strata of volcanic substances of which they consist; these too are interspersed with masses of volcanic rock, and other stones, some of the lesser ones entirely roasted through, and some of the larger ones to certain depths from their surfaces. Masses, also, of iron-clay, inclosing various pebbles, which have been burnt into a kind of red brick, are abundantly found in many places. There is scarcely any thing that can be called a path, or even a track, to the mouth of the crater of Mount Misery; indeed, there are but few whose curiosity is sufficiently strong to induce them to undertake this expedition. The common course for those who do, is to take a negro man as a guide, with a cutlass, or large knife, to clear away the underwood, and form a kind of path as he goes on. The ascent is very irregular, in some places being gentle, in others almost perpendicular; in which case the hands are obliged to assist the operations of the feet. In wet weather, the ascent of this mountain is extremely laborious, as a great part of it consists of clay, which then becomes so slippery as to render the getting up almost impracticable. About half-way up on the south side, and in a very pretty, romantic situation, there is a natural spring of remarkably cool water. On the north side, at about the same height, there is a water-fall, which, though small and insignificant in itself, has a pleasing appearance, as it rushes over the rocks, and through the trees and shrubs. This mountain is thickly clothed with wood, which in many places not only excludes the rays of the sun, but produces a sombre, gloomy appearance; this, with the occasional plaintive coo of the mountain dove (the only sound heard at this height), creates in the mind sensations of pleasing melancholy. In some parts an open space suddenly appears, from whence the whole country below bursts unexpectedly upon the view, which has, as may be supposed, an extremely fine effect. The thermometer, on the top of the mountain when the author visited it, stood at 65, being a difference of 15 degrees from the low lands, where it stood at 80°. The descent into the crater on the north and east sides, is perfectly perpendicular; on the south and west sides, it slopes at an average angle of not more than 18 or 20 degrees from the perpendicular, consequently, persons descending are often obliged to let themselves down by clinging to projecting corners of rocks, or the branches and roots of shrubs, which grow all the way down; nor is this mode of travelling particularly safe, for should any of these give way, the consequence would probably be highly dangerous. The bottom of the crater, which, as nearly as I could estimate, is about 2500 feet below the summit of the mountain, and contains about forty-five, or fifty acres, may be said to be divided into three parts: the lowest side (to the south) consists of a large pond or lake, formed entirely by the rain-water collected from the sides of the crater—accordingly its extent is greater or less, as the season is wet or dry; the centre part is covered with small ferns, palms, and shrubs, and some curious species of moss; the upper part, to the north, is that which is called the Soufrière. The ground here consists of large beds of pipe-clay, in some places perfectly white, in others of a bluish or black colour, from the presence of iron pyrites. These are intermixed with masses and irregular beds of grey cinders and scoriæ, pumice, various kinds of lava, lithomarge, and fuller's earth. Amidst these beds of clay, there are several hot springs, small, but boiling with much violence, and emitting large quantities of steam. A rumbling noise is heard under the whole of this part of the crater. The hot springs are not stationary, but suddenly disappear, and burst up in another place. The ground in many parts is too hot to be walked upon: a great quantity of sulphuretted hydrogen gas is likewise emitted, which is exceedingly disagreeable to the smell; and occasionally such a volume of it arises, as is almost suffocating, and resembles much the smell of rotten eggs. The watches of the author and his companion during his visit, and every article of gold or silver about their persons, were in a few moments turned perfectly black, from the effect of this gas.

The water from the springs is strongly impregnated with the sulphates of alumine and iron; it is, indeed, almost a saturated solution of them. In every direction about these springs, pure sulphur is crystallized in minute but beautiful crystals on the scoriæ; it is also found in a state of sublimation. Crystals of alum are also abundant. The quantity of iron and sulphur about the springs, I think, renders it easy to account for them, from the well known action of iron upon sulphur in moist places. The heat occasioned thereby would be sufficient to cause the violent boiling of the water; and also to sublime part of the sulphur, and to convert another part of it (assisted by part of the oxygen of the water) into sulphuric acid, which latter, acting upon the clay and (with the assistance of water) upon the iron, forms the alum and the sulphate of iron. Water is composed of oxygen and hydrogen; when it is deprived of any part of its oxygen, a correspondent portion of hydrogen is set loose. I have said above, that the heated sulphur absorbs part of the oxygen of the water, and forms sulphuric acid; another portion is absorbed in the formation of the sulphate of iron. A considerable quantity of hydrogen is consequently set at liberty, which, dissolving part of the sulphur, forms the sulphuretted hydrogen gas already described. I found some pieces of clay, red on the outside, and bluish-green within. These were, doubtless, at first, pieces of perfectly white clay, which being saturated by a solution of green sulphate of iron, of course assumed the colour; but in a very short time, green sulphate of iron absorbs oxygen from the atmosphere, and becomes converted into the red oxide: the red colour to a small distance from the exterior of the clay had been occasioned by this action of the atmosphere; but the interior, being defended from it, retained its green impregnation.

It is well known to those who are at all acquainted with the rules of perspective, that when a person stands immediately contiguous to the base of a perpendicular height, it appears to bend over him, and the greater the height, the more the deception is increased; this is eminently the case in this crater: for, when you stand close under Mount Misery, it appears to be rent from its natural position, and to hang directly over your head, as if it were instantly about to fall. For the same reason, all the sides of the crater, when you are at the bottom of it, appear more overhanging or perpendicular than they really are; and as there is, almost continually, a heavy, dense body of clouds, resting on the summit of the mountain, the mouth of the crater appears to be shut up, and the possibility of getting out almost precluded. This, with the associations which are naturally formed in a person's mind, when he recollects that he is in the bowels of a volcano, gives the whole a gloomy and awful aspect. A musket, fired from a particular spot in this crater, afforded an interesting illustration of one of the laws of sound; for the report was repeated distinctly seven times, and the two first and two last echoes were considerably louder than the intermediate ones—appearing as if the sound had struck at seven points, in its passage round the sides of the crater, commencing with those nearest to where it originated, and returning to the same spot, after having performed its circuit. The ascent from the crater is, as may be supposed from its steepness, extremely laborious.

To reach the summit of the rock of Mount Misery, it is necessary to take a different course from that which leads to the crater, commencing on the opposite or north side of the mountain. This is an extremely fatiguing undertaking. About one-third part of the way up in this direction, there commences an extraordinary natural path, lying along the top of a ridge of rock, very remarkable for its thinness and height, being mostly not more than from two to four feet wide on the top, while its sides descend almost perpendicularly to the depth of from one to two hundred feet; thus, as it were, forming a gigantic wall, up the centre of an immense ravine. All along the top of this ridge, however, narrow as it is, there are trees and shrubs in abundance, most of the large ones hanging over the precipice as if they could scarcely remain a moment in their situation. This ridge continues, I think, about two-thirds of the way up the mountain, and in the part above it, there is a very sensible change in the air and in the appearance of the vegetation. The former begins to feel very cool, and the latter becomes stunted and low. Proceeding on, these changes are still more observable; the traveller is now above the regions of those plants which have hitherto adorned his path. The cabbage-tree no longer waves its graceful branches, nor does the rough but beautiful tree-fern rear its stately form upon these heights; the ground is covered with low, succulent plants, not higher than the knee; most of them are so formed as to retain, in the interstices of their leaves, a considerable quantity of water, with which they are abundantly supplied by the heavy vapours, which almost continually clothe the summit of the mountain. The ascent here becomes almost perpendicular, in some parts perfectly so; in consequence of which, it requires the constant exertion both of the hands and feet to climb up it. In laying hold of the plants to drag himself up, the water is thrown from them all over the traveller, so that he is in a continual shower-bath; this water, too, is very cold, which quality, being assisted by the sharpness of the air, absolutely makes the teeth chatter. The weather had been rather moist, for a short time previous to the author's ascent; perhaps, therefore, he saw a larger collection of this water than he would have done in a dry season. It may, however, be fairly concluded, that even in dry weather the rivers receive some supplies from the moisture which these plants attract from the clouds, as they pass among them.

The way up the parts of the mountain I have just been describing is not very safe, as the roots and plants by which it is necessary to cling are very slight, slippery and rotten, and the breaking of them frequently exposes the climber to a considerable fall. All the mountain hereabouts is covered, to a considerable depth, with a rich, black, vegetable mould, formed by the long accumulation of decayed plants. The summit of the rock appears but small from the country below, and seems to end in a sharp ridge; but when you are near its summit, it has a gigantic appearance, and on the top of it are several acres of level land. The view from it is most extensive and beautiful; the whole island of St. Christopher is seen below, like a miniature picture; the ships in Nevis roads appear almost under you, as does the island of St. Eustatia; the sea is as a sheet of blue glass, and the distant ships are like white specks upon its surface. The islands of Saba, St. Martin, St. Bartholomew, Martinique, Barbuda, Antigua, Montserrat, and Redondo rock, are also very plainly seen. The effect of this extensive view is greatly heightened, by its being occasionally disclosed in a kind of coup-d'œil; for, as before observed, the summit of the mountain is almost always wrapt in clouds, which now and then break, and suddenly disperse by a gust of wind; so that from being enveloped in a thick gloomy fog, which prevents you from seeing more than a few yards before you, you find yourself in an instant, and almost as if by magic, in a most brilliant sunshine, surrounded by a prospect magnificent and extensive in every direction. In descending, the author travelled a very considerable distance, and with great rapidity, by sitting down and allowing himself to slip over the long, smooth, wet leaves of plants, of a liliaceous appearance, with which the upper part of the mountain is thickly clothed.

Brimstone Hill is about two miles from the foot of the above described mountain, three from the west end of the island, and half a mile from the sea shore on the south side. On it are the principal fortifications of the island. It is composed of a mass of madrepore limestone, and covered by many kinds of volcanic substances, heaped together in great confusion. There are, in many parts of it, abundance of the red and yellow oxides of iron; and I am told that, in digging the foundations for the fortifications, some masses of native vermilion were found. This singular hill appears more like a stupendous artificial mound, raised for a fortification, than like a production of nature. It is seated on a gentle slope, but does not appear at all to have been formed at the same time with the plain it rests upon, as the latter has not the least slope towards its base, but runs in its natural inclination from the mountains to the sea; while Brimstone Hill rises abruptly from it, at about an angle of 55 degrees, to the height of 715 feet, and is formed almost entirely of white madrepore limestone, whereas the circumjacent plain is composed of strata of cinders, pumice, terrass, and other volcanic matters; it must, therefore, either have been raised up by some subterranean explosion, or have been hurled from the mouth of the volcano. In some of the intermediate spaces between the heads of the mountains, are plains of some extent, nearly flat,—these are called *levels*. They are well covered with grass, and are free from shrubs or wood, so that, when they are accessible, they form good pastures for cattle. The principal of these is Spooner's level, situated about the centre of the range of mountains. It is a plain of considerable size, which, from the height at which it is situated, is in an atmosphere considerably cooler than that of the low lands. It is well covered with grass; and having in various parts of it clumps of trees, bears a strong resemblance to a park in England. The mountain heads which surround it, covered with their thick, dark foliage, and magnificent ferns, have a very fine effect, and give a degree of grandeur to the scenery, which is heightened by the vicinity of the clouds that gather round the mountain tops. These, sometimes meeting, form an arch that entirely excludes the rays of the sun; and then suddenly dispersing, occasion a surprising and instantaneous transition from gloom to brightness. Close to the level, there is a curious and large ravine, called Nine-turn Gut—Nine-turn, from there being nine windings in the path leading down it—and Gut, from the rather coarse appellation which is commonly given, in this island, to a ravine. In the centre of this ravine, and bordering one side of the path, is a large chasm, of so great a depth, that a stone thrown into it cannot be heard to strike the bottom. All the mountains in this country are scored by deep ravines; and frequently, after continued falls of heavy rain, the water rushes down them in such quantities, and with such force, that it does considerable damage to the plantations below. The little streams of water, which they dignify here by the name of rivers, are formed by the rains, which fall in the upper part of the mountains, and which, gradually penetrating the strata of earth, are collected in two or three of the largest ravines, and run in small rivulets to the sea. Such are Old Road River, and Cayon River; in a very dry season they are nearly exhausted.

The upper stratum, on the low lands near Basse-terre, is a soil so full of black volcanic cinder, and minute fragments of augite, that in damp weather it appears quite black. The stratum of soil about the hills, and on the western parts of the island, is nearly free from these substances, and is, consequently, of a lighter colour. This upper stratum of soil reposes on a bed of terrass, which, being free from the least particle of decayed animal or vegetable substance, is totally unfit for the support of vegetable life. In some places, this terrass is of a very considerable depth; in others it is only a thin stratum, resting on, or alternating with, grey or black cinders, pumice or scoriæ. These strata of black and grey cinders and scoriæ, with small crystals of augite and quartz, interspersed with masses of different sorts of volcanic rocks and lava, compose the eastern side of the island. Towards Sandy Point, more pumice occurs; and the strata of black cinders and grey ones, or pumice mingled with scoriæ (which is called here Botheration), become very distinct and well defined. More to the north, towards Deep Bay, there are some solid blocks or currents of lava. There is a remarkable chasm or hole, just above Sandy Point, called *Tomber* Hole, from the circumstance of a French dragoon, with his horse, having fallen into it and been killed, at the time the French had possession of the island. This hole is perfectly circular, and of nearly an acre in extent. It appears as if a portion of land had suddenly sunk, by the giving way of some subterraneous cavern, to the depth of 80 or 100 feet. The sides are perpendicular, and the bottom is covered wtth trees and shrubs: its sides exhibit regular strata of black and grey cinders, and pumice: a large quantity of vegetable mould appears to have collected at the bottom of it.

In the narrow neck of land which stretches from the south-east of the island, there is an extensive natural salt pond, from which considerable quantities of salt are occasionally gathered. Near the pond, there is a large quantity of sulphate of lime, which occurs in lamellar, semi-transparent masses: of this no use is made. The principal anchorage of the island is in Basse-terre roads, on the south-east side, which is much exposed to the south and south-west winds. Now, the hurricanes almost always commence from that quarter, so that vessels lying in the roads, on the appearance of a gale, generally endeavour to put to sea: those that cannot do so, run the most imminent hazard of being driven ashore. At Deep Bay, on the north-west side, there is a bay, sheltered by an extensive reef, through which there is one narrow passage, but not sufficiently large to admit any other than small-sized vessels. The getting in is certainly rather dangerous; but when it is once accomplished, a vessel is much better secured from the violence of the sea than in the other parts of the island. On either side of the passage, through the reef, are large rocks, called the Dogs, over which the sea dashes with great violence, when the wind is from the northward. The reef consists of common massive quartz and carbonate of lime; it is covered with coral, branching out in every direction, and having an exceedingly beautiful appearance in calm weather.

List and Description of Mineral Substances found in the Island of St. Christopher.

Common massive Quartz, white, with reddish brown veins, shining vitreous lustre, fracture uneven, scratches glass, strikes fire with steel. Found on the reef at Deep Bay, and in some other parts of the island.

Vesicular Lava, found in great abundance in the crater of Mount Misery, and in many parts of the island; of a blackish, glassy appearance, with white specks, full of holes and cavities.

Limestone, hard and compact, of a light brown colour, dull, fracture flat conchoidal, passing to uneven, yields to the knife. On the reef at Deep Bay, and in some other places.

Sulphate of Lime, or *Selenite,* is found near the Salt Ponds, white, shining pearly, semi-transparent, yields to the nail, occurs massive, with straight lamellar structure.

Olivine, and Leucite embedded in black Lava, is one of the most common rocks in all parts of the country. It is very hard and fine grained, of a dusky black, full of glistening white specks, and uneven fracture.

Leucite, embedded in red Lava, particularly prevalent in the cliff at Old-Road Bay, giving the whole a red appearance; colour, several shades of red, with white and brown specks; hard, fracture uneven. A great quantity is found in a state of decomposition, forming a red mould.

Vesicular Lava, light reddish brown, with white specks, rough and friable, full of holes and minute pores.

Three varieties of *Lithomarge*:—1. perfectly white, very soft, falling readily to powder; 2. brown, speckled white; 3. flesh red; both harder than the first: found in the crater, and the last in some other places, particularly on the hills north-east of Basse-terre. This is dull, earthy, unctuous to the touch, adheres strongly to the tongue, and contains a great quantity of magnesia.

Fullers' Earth is found in most parts of the island, of a light yellowish, brown, and olive-green colour. It occurs massive, fracture earthy, uneven, yields easily to, and receives a polish from, the nail, and falls to pieces in water.

Volcanic Rock, of a bluish grey colour, passing into pumice; very common.

Shale, of a light ochrey yellow, or pinkish brown colour; fracture large, conchoidal, earthy, and uneven; dull, opaque, meagre; adheres firmly to the tongue, yields to the nail.

Masses of small stones and sand, embedded in iron-clay, and burnt into a kind of red brick.

Pipe Clay, nearly pure, is found, in large beds, in the crater of Mount Misery.

Alum and *Sulphur*, crystallized, are also found in the crater.

Alumine, full of minute iron pyrites, giving it a bluish black colour, abounds in the crater: these are easily separated by dissolving the clay in a glass of water.

Alumine, mixed, more or less, with yellow ochre, is found on many of the hills, particularly to the eastward.

Greenish brown Lava, very thickly speckled with leucite, is often found to be the interior of a stone that appears dirty red without.

Vesicular Lava, greyish white, very full of pores and black glittering spots, is very common.

Leucite, in porous black lava, occurs in large masses. This, as well as some of the other lava rocks, is called "fire-stone," and is used for building the furnaces of boiling houses, &c., where great heat is required.

Augite appears in great abundance wherever there is a black soil. It occurs in small broken prismatical crystals, shining, transparent, blackish green, translucent, scratches glass with ease.

Terrass, of a reddish brown colour, forms the first stratum under the vegetable mould in most parts of the island, particularly the east end. It is used with lime in the composition of mortar.

Soft white Carbonate of Lime, containing madrepores, composes the principal part of Brimstone Hill.

Siliceous Sand, consisting of minute particles of dark green, yellow, and white transparent quartz, mingled with powdered shells, is that which is found on the sea-shores. The dark-coloured quartz predominates so much as to give the sand a blackish appearance.

Pumice, Scoriæ, and *Volcanic Ashes,* are everywhere abundant.

In many places, the volcanic rocks are met with, in all stages, from incipient to absolute decomposition.

Remarks on the Discovery of some Fossil Bones in France. By John Ranking, Esq.

The bones discovered at Breingues, in the *Département du Lot,* have been noticed by M. Cuvier. The following description is by M. Delpon :—" In various points of the calcareous portion of Quercy, there are remains of an entrenchment formed of blocks of stone, in straight lines or circular inclosures. The most remarkable of these inclosures occupy the summit of two mountains of the Commune de Breingues, in the circle of Figeac, one on the right, and the other on the left bank of the Selé. On the rocks of the right bank, there are several cavities or grottoes, before which some vestiges of buildings are seen ; a circumstance which presents itself in the greater number of grottoes with which the rocks along the Lot, the Selé, &c., are perforated. In 1816, the population of Breingues, in hope of finding concealed treasure, were occupied in digging among these grottoes, and came to one, the entrance to which was choked up with earth : here, at the depth of three feet, they found the bones of a human body, and an iron instrument resembling a fork with two prongs. They then dug in a perpendicular direction to the depth of eighteen metres ; but the natural cavity, which hitherto was in a straight direction, here presented three cavities also filled up with earth and stones. On coming near the first grotto, they were arrested by three large stones placed above one another by the *hand of man.* These stones had evidently been long exposed to the open air before they were removed thus far under ground, each being of a reddish colour on one of its faces ; like all those which are at the present day raised from the surface of the ground; the opposite face was covered with mosses and byssi. Here, instead of treasure, they found a prodigious quantity of bones, some mingled with earth and stones, and *others very carefully placed in narrow fissures of the rock.* Several heads of a species of deer, now unknown, and other bones, were discovered, without any mixture of earth, in a small cavity, covered over with a rude slab placed with great care. Here and there, the mass of stones and common soil was interrupted by small quantities of an alluvial earth of clay and sand, like that which the river Selé deposits at the present day: but no current of water could have brought them there; they had been formed by men, since some were pressed, regularly arranged, and surrounded with very white calcareous stones, which must have been soiled by water, had it deposited these alluvial matters so regularly. The elevation of this grotto, being 300 metres above the river, precluded the idea that the waters of the Selé could have reached it. The other galleries presented nothing but bones placed in the same manner; the whole together would have formed a mass of more than twenty cubic metres. Some of them were encrusted, and others inclosed in a calcareous breccia with a crystalline paste. The greater number were so well preserved, that they looked as if the flesh had been recently detached from them; but as soon as they were exposed to the external air they became scaly and whitish. Among them were a skull and three teeth of a rhinoceros, three species of deer now unknown upon the globe: the horns have some resemblance to those of a young rein-deer, the fragments of the horn of a large deer, equally unknown, but allied to the common stag, the humerus of a large ox, and a horse's femur. M. Delpon infers from the existence of these foreign animals on our soil, a diminution of temperature; and in an historical view he supposes the bones are remains of the sacrifices of the Druids. We are of opinion that they are of a date much anterior to the Druids, and even to the *establishment of the human species* in these countries; and that their regular arrangement is the result of the superstition of the first inhabitants, or the amusement of herdsmen." —*Bulletin Universel, Nov.* 1825*.

* Edinb. Phil. Journal, April, 1826.

Monsieur Delpon has, perhaps, been led by the circular disposition of the stones to suppose the Druidical origin; but the Druids were not very likely to possess rhinoceroses.

Quercy is now named Département du Lot; Cahors, on the banks of that river, was the capital of the *Cadurci*. *Uxellodunum*, so called from being high and lofty*, is a few miles east of Cahors, by the river Selé†, which runs into the Lot. There is every probability and evidence that these bones are of Roman origin.

"Julius Cæsar was informed what resolution the people of Usseldon (Uxellodunum) had taken; wherefore, ordering Q. Calenus to follow after him by moderate marches, he went before with all the cavalry to Caninius; on his arrival, he found Usseldon so well invested, that the enemy could not escape. Cæsar was informed that they had plenty of corn, and was, therefore, resolved to cut off their supply of water. There was a river that divided the plain below, which almost surrounded the craggy hill upon which Usseldon was built, on every side. The stream ran so low that it could not be drained; but the descent to the river was so steep, that the besieged could not approach it without being wounded by the Romans; Cæsar, therefore, guarded the place easiest of approach, with archers, slingers, and engines. Close under the walls, for 300 feet, where the river did not run, there was a plentiful spring. Here, with great labour, Cæsar began to cast up a mount, but many of his men were wounded. A mount was raised sixty feet high, and a tower of ten stories was constructed as high as the top of the spring, not to the top of the walls, for that was impossible. Engines were planted to play upon the access to the fountain; and now the cattle and several men perished with thirst. The enemy filled barrels with grease, pitch, and bits of wood, and rolled them blazing upon the battery, fighting furiously at the same moment. The works took fire wherever the barrels were stopped. The Romans exposed themselves to the flames, and the showers of darts; Cæsar

* Camden's Britannia, vol. i., p. 5.

† Bowen's ancient map. Sanson inclined to think that Uxellodunum was Cahors itself; but Bladen denies that. See his "Address to the Reader," p. 9.

finding many of his troops were wounded, ordered his cohorts to ascend the hill on every side, as if to scale the walls, which alarmed the enemy, and gave time to quench the flames. The spring was now diverted from its course; and after an obstinate resistance, the enemy, in despair, surrendered. Cæsar, finding a severe and striking example was necessary, ordered the hands to be cut off of all those in Usseldon who had borne arms against him *."

If we add to this, that there was an *amphitheatre* at Cahors †; the improbability that the bones of the horse, ox, and some of the deer, being of what are termed of *extinct* species; and that no large animal was more frequently exhibited than the rhinoceros,—who can doubt that this collection of bones, so evidently placed by design, in the careful positions described by M. Delpon, is the produce of Roman sports?

"In *Auvergne*, now Puy-de-Dome, principally in Mount Perrier, near the Issoire, there have been found very lately, in volcanic tufa, bones of thirty species of animals; and a large proportion of them prove to be extinct and hitherto unknown quadrupeds. Among them are an elephant, a small mastodon, a rhinoceros, hippopotamus, small tapir, many of the *genus cervus*, two bears, three panthers, a hyæna, a fox, an otter ‡. The bear is similar to the extinct species found in the hyæna cave at Torquay, in Devonshire, and those in the Val d'Arno, discovered with the remains of an elephant ‖."

These animals are all similar to such other collections, in numerous places where Romans resided, or where ruins of amphitheatres exist.

The Arverni were powerful opponents of the Romans. "Vercingetorix was grandson to the commander of Gaul, and his father had been put to death by the Romans; he rebelled, was elected king, and entered into a league with the people of Paris, Poictou, Quercy (at each of which there are the remains of an amphitheatre), and other places. When Cæsar with his army arrived near Gergovia (said to be Clermont), Vercingetorix had encamped his numerous troops on all the

* Cæsar's Com. by Bladen, b. viii. c. ix.
† Rees's Cyc. "Cahors."
‡ Quarterly Rev., Sep. 1826, p. 511. ‖ Quart. Rev., Oct. 1827, p. 402.

hills round about, which made a dreadful appearance. All his chief nobles attended every morning at his levée, and no day passed without his sending out detachments to skirmish with the Romans. At length, after a hot encounter, in which the Romans lost forty-six centurions, and Cæsar himself lost his sword, the Romans retreated *. The brave Gauls, with 248,000 troops, carried on the war, but were defeated, and their king was captured †."

The Arverni claimed a common descent with the Romans; and when we consider their power, bravery, and character, it seems improbable that they should not have been amused with the same grand sports, as their confederates enjoyed after they were subdued. We find that Vespasian indulged the Britons with two amphitheatres, in the part of the island where he met with the greatest opposition, having fought thirty battles.

With respect to these fossil bones, the hippopotamus is African, the tapir probably Asiatic, (as the Romans had the power of procuring it from Sumatra,) and the bears are perhaps African, numbers from that quarter having been often mentioned in their games. The fox and the otter were, in all probability, natives of the spot. Thus, this collection, like numerous others, consists of animals of the three parts of the earth then known; and when we compare all the difficulties attending the mysterious and marvellous conjectures regarding their origin; and consider that most questions or facts in geology are still subjects of difference in opinion among geologists themselves, how can such theories for a moment be placed in competition with the well-known custom of the Romans, to amuse their vanquished people with the very animals in question; the only difference between the fossil and living kinds, being in the *species*; and that difference being accounted for, by the Romans not having procured their wild beasts and elephants from those countries that have supplied modern naturalists with the specimens from which the inference has been drawn, that the fossil kinds are extinct?

Gibbon, c. xxxviii., says the sword was displayed as a great trophy, and afterwards, when Cæsar saw it, he laughed.

† Cæsar's Com. by Bladen.

The volcanic character of Auvergne and the neighbouring regions, as described*, is truly interesting and wonderful, and almost unknown to the French themselves, in general, although the visible devastations are equal in magnitude to some of the recent convulsions of the earth in Iceland!

With respect to the epoch when these extinct volcanoes were in action, it is, perhaps, impossible to come to any certain conclusion. Gaul was, for nearly five centuries, subject to the Romans; but, like Britain, its history during that period is unknown, except the descriptions of actual wars or revolts. There were twelve hundred walled towns in Gaul when the Romans conquered it, but such fortifications were thenceforth not permitted†. There are, at least, six centuries, since the Christian era, of what may be termed a blank in the history of France, when no such event as a volcano would be registered; and therefore, as far as history is concerned, these convulsions may have happened as well after as before the Roman conquest. If such bones as those in question be found under volcanic tufa, it is not an indifferent proof of those volcanoes not being so ancient as Cæsar's invasion, especially as some of the streams of lava are quite fresh in appearance.

"In the cavern of Oiselles, near Besançon, there have been found remains of bears (ursus spelæus) of all ages, tigers, hyænas, and other carnivorous, besides herbivorous, animals. The bones are marked by the teeth of hyænas, like many others discovered in England, &c."—*Annales de Chimie et de Physique*, Oct. 1827.

"Near the church of Notre Dame, in Besançon, is a triumphal arch, to Aurelian, on which are seen several mutilated figures of men and animals. This was one of the most magnificent places the Romans had in Gaul, and many superb remains of their buildings are still visible; but after the death of Julian, it was almost destroyed by the Germans, and a second time by the Huns, commanded by Attila‡." When

* Quarterly Review, Oct. 1827. "Scrope's Geology of Central France."

† Mezeray (Introduction). Many of the walls round the towns of Gaul were composed of double palisades of trees, filled with earth or stones.

‡ Rees's Cyclopædia. "Besançon."

Julius Cæsar was at Besançon (Vesontio), he placed a strong garrison there; but his troops were so intimidated at the descriptions they heard, from the merchants, of the neighbouring Germans, that the whole army made their wills. The panic was so great, that Cæsar threatened to make the tenth legion his life-guard, and with them alone to go in search of the enemy*.

More than three hundred years afterwards, Aurelian, on his recovering Egypt from Firmus (who had seized the remains of his friend, the unfortunate Zenobia's kingdom), wrote to his people: "The tribute of Egypt, which that wicked robber had suspended, will now come entire to you. Entertain yourselves with the pastimes and shows of the Circus, while we are taken up with the necessities of the state." This Firmus kept up the commerce with India, and was enormously rich (the Romans traded thither with one hundred and twenty ships *annually*); and hence we may easily account for *tigers*' and *hyænas*' bones being found at Besançon, as the triumphal arch is dedicated to Aurelian.

If the mummy of Firmus himself had been found, he might have afforded a puzzle whether *he* was of an extinct *tribe.* Vopiscus describes him "of large stature, with prominent eyes, frizzled hair, blackish visage, body fair enough, but very hairy, scars and wounds on his face, and stronger than the gladiator Tritannus; for he would bear a smith's anvil upon his breast, as he lay bent back upon his hands and feet, and permit any one to strike upon it with force. He ate so much, that some say he would devour an ostrich in a day—(a young one, no doubt!)—he drank little wine, but much water†." Firmus had two elephants' tusks, ten feet long. These tusks, having been designed by Aurelian to make a chair for a golden statue of Jupiter, covered with jewels and a robe of state, the indignant Vopiscus adds, "But the lewd Carinus‡ presented these *Indian*§ rarities, with two others, to a certain

* Bladen's Cæsar's Commentaries, b. i., ch. xv.

† Flavius Vopiscus, Augustan Hist.—"Firmus."

‡ Britain formed a part of the government under Carinus, during the absence of his father, Carus, in Persia.—*Vopiscus.*

§ These immense tusks were, probably, from Pegu, that country producing the largest elephants known. An officer lately travelled into the lady, for feet, or posts, of her bed. I say no more, because we of this age know her, and for posterity it signifies nothing."

Thus fossil bones of beasts, such as were exhibited by the Romans, according to their history, are one of the sure guides to trace the residences and wars of those powerful conquerors.

On the Junction of Granite and Sandstone in Sutherland, and on the Lignite Formation of that District. By J. Mac Culloch, M.D., F.R.S., &c.

The appearance on which the title of this paper is founded, has so rarely been observed, that every instance of it would, under the present circumstances, be deserving of record, were it even not attended by the peculiarities here occurring, which render this example interesting in no common degree. Only one other instance of it has occurred in my experience, and that is in Aberdeenshire; but, in that case, the smallness of the space exposed where the junction takes place, is such, that nothing of importance can be discovered:—it stands as a bare example of a rare fact in geology. In the case about to be described, there is the most perfect exposure of the whole junction throughout a space of many hundred yards; while all the rocks are as clean as if recently cut by a tool, and absolutely free of any incumbrance capable of introducing obscurity or doubt into the observations. This junction is, at the same time, perfectly accessible, throughout a considerable space, even to the hammer of the geologist; so that nothing but incapacity or prejudice, on the part of an observer, can lead either to a misapprehension or misstatement of the circumstances attending it.

Having, on many occasions, attempted to demonstrate the igneous origin of granite, I shall, probably, in describing these

mountains of Aracan, from that city eastward; and upon the *sand* of a river bank he saw six wild elephants, but could not get very near them. On examining the impression of their feet upon the *hard* sand, he found one of them to measure upwards of twenty inches at the smallest diameter. The writer of this note conjectures the Mastodon to be a native of Ava (their fossil bones having been found there lately), and that they are *mountain* elephants. This curious subject, so highly interesting in natural history, is well worth inquiry in Aracan and Ava.

facts, be exempted from the suspicion of attempting to support that doctrine by a prejudiced selection of facts; since some of the phenomena accompanying the appearances to be detailed, might, with very little effort, be so represented as to support the opposed opinions, and may indeed, for aught that I know, be used for this purpose by an observer, who, having adopted those, may (and really without any absolute mala fides) view them in a different light from myself. To those, in reality, who still believe that all rocks have been formed successively from solutions of the earths in water, this appearance may probably afford matter for triumph, more particularly from the apparent gradations which it presents between granite and sandstone. It has been, censoriously, said to be the policy of a strenuous partisan to suppress, if he cannot mutilate and misrepresent, facts so hostile as these may at first sight appear; but he deserves only compassion who does not feel that there can be no pleasure equal to that derived from establishing a truth in science, though he should, for that, renounce all the opinions for which he has contended or written.

I shall not pretend to follow up the facts which I shall now proceed to describe, with any very detailed reasonings, since they do not seem, in the present state of our information, to admit of much that would prove of a satisfactory nature; but I may, nevertheless, repeat what I have so often urged, that, on the question which respects the igneous origin of granite, there is a vast body of evidence derived from facts of the same nature as those which have established the igneous origin of trap. The present is, in some of its circumstances, a solitary difficulty—possibly, what is popularly called an exception: and, on the nature of such exceptions, or rather of appearances not concordant, in all particulars, with the predominant examples, we have no right to pronounce any judgment, unless perfectly informed respecting all the circumstances which might have modified or affected the usual results.

It cannot fail to be known to those in the least acquainted with Scottish geology, that there is an extensive tract of granite in Sutherland, one part of which interferes with the boundary of the neighbouring county of Caithness. It is equally well known, that a large portion of this latter county consists of a sandstone, of such characters, and occupying such a situation with respect to the primary rocks, that it must be considered as an example of what is called the old red sandstone. An account of this tract, which is in many respects interesting, may possibly form the object of some future communication; but it is unnecessary here to enter into any details respecting either of these rocks, further than is required for elucidating the objects of this memoir. It is sufficient to say, in general, that as the sandstone is unquestionably the lowest member of the secondary strata, so the granite resembles, in its geological position, all other granites in Scotland, being in many, if not in every part of its extent, inferior to all the primary strata, and, in most places, covered by extensive tracts of gneiss.

With respect to the mineralogical characters of both these rocks, I must also add, that the granite undergoes numerous variations of composition; exhibiting, in some places, that modification which is by some esteemed particularly *genuine*, while, in others, it is found under a great number of different aspects. The sandstone *formation* is somewhat complicated, yet less so than that of the great central district of Scotland. It is sometimes grey and arenaceous, or else compact and of the same colour. In other places it is red, of various tints; while it is, in some situations, simple, in others argillaceous, and, in a few, calcareous. Almost every where it is remarkable for containing interposed beds of various argillaceous schists, sometimes so abundant as to exceed in quantity the arenaceous strata, and even to occupy alone entire tracts of the surface. These schists present great diversity of character. They sometimes resemble common clayslate; more frequently they are like the coarser varieties, or the greywackes, which accompany the fine clayslate. It very seldom happens that they put on the appearance of the shales found in the white, or upper sandstones, or those which occur in the red sandstone of Arran.

I have observed, above, that the junction of the sandstone strata with the subjacent mass of granite, is visible for a considerable space; and, where it occurs, the sandstone beds incline to the eastward of North at about an angle of 40 degrees—perhaps less. It is important to remark, that the strata are even, or lie in straight planes, without any sensible incurvation; the upper surface of the granite, on which they repose, corresponding, of course, to them in its outline. In many places that surface is perfectly even, or the lines of junction are so straight at the section, that a ruler may be laid on it. In a very few, only, there is some slight irregularity in the surface of the granite, and there the superincumbent strata are affected in a corresponding manner, being bent in such a way as to enter its cavities. No fissures, or other marks of disturbance, are visible in the sandstone, nor is it penetrated by any granite veins. Every thing, in short, seems to prove that the sandstone has been deposited on the granite.

So far, there is nothing to cause surprise, although this junction presents an example of a rare and not uninteresting fact, namely, the total absence of the primary strata in this place, and the deposition of the secondary on a surface of granite, where either no primary strata ever existed, or where they have been removed prior to its deposition.

It may, perhaps, also be inferred, by some geologists, that there is thus proved a possibility, or a fact, which has been questioned by the majority of observers in this science; namely, that stratified rocks may be deposited from water, even at high angles, and that all stratification is not necessarily performed on planes nearly horizontal. But this conclusion does not by any means follow from the present appearances, nor is it rendered more probable in this case, where granite is the immediate substratum, than in those where stratified rocks lie below. In many examples in nature, bodies of strata, continuous and level for long spaces, are found elevated at much higher angles, in positions, and under circumstances, where they could not possibly have been deposited from water. But these facts must be well known to all geologists.

If the geological circumstances apparent at the junction of these two rocks are thus free from any peculiarities leading to useful or interesting conclusions, it is not so with the mineralogical appearances. It is in the peculiarities of structure—in the approximate parts of the two, indeed, that the chief interest of this place consists. It is impossible to render these thoroughly intelligible without the series of specimens collected from them. An attempt, indeed, has been made to render the facts more clear by means of some slight sketches; but specimens cannot be drawn, nor mineralogical varieties of character represented, in this manner; nor, indeed, can any thing but a sight of the parts, in their native place, convey either an idea of their true nature, or the impression of that interest which they are calculated to excite.

The body of granite exhibits every where throughout its chief face, which extends upwards in some places for 100 feet and more, a solid mass, marked only by the indications of a vertical fracture, or, in some places, by an appearance of a vertical laminar structure on a large scale, such as is familiar to those who have seen the granite faces in Glen Sannox, and in many other parts of Arran; those in Mull; or those about the sources of the Dee, and the other precipices of the Braes of Mar. In some spots, it has even an irregular prismatic appearance; and all of these are circumstances which it is necessary to point out to those who may visit this spot; because, where it is most easily accessible, it presents appearances of a very different nature, immediately to be mentioned, and the observer might therefore overlook, or neglect those parts of more difficult access, where it appears under the unquestionable characters just described.

Where the mass of this granite approximates to the sandstone, this laminar or vertical structure first disappears, and it then displays solid masses rounded by the action of the weather, and not satisfactorily distinguishable by the eye from the neighbouring sandstones, which have been subjected to the same influences. At length, as it comes close to the sandstone, it begins to resemble a stratified rock; and, at the first view, appears so like the sandstone near it, that the observer begins to question the existence of any granite, and to conclude that the whole is a mass of sandstone, of which the lower bed is occasionally thick, and, like the conglomerates which often attend this sandstone, less distinctly stratified. In one place, but for a space not exceeding a few feet, the appearance of stratification is perfect. Here the surface of the granite touches the sandstone by a level plane; and, immediately following that junction, are three or four laminæ of an argillaceous nature, or of a shale imbedded in the granite, so as to divide it into three or four corresponding laminæ. I must add, that the whole thickness of this species of stratification, or laminar structure, does not much exceed a foot.

As this remarkable point is the most obvious and easy of access of the whole, the first impression it conveys serves to confirm that suggested by the more distant view just mentioned, namely, that the granite is regularly stratified and graduates into the sandstone.

The appearances are indeed such as, on a first view, not to seem to admit of any doubt; and, unquestionably, an observer who had predetermined that this was the ordinary relation of granite and sandstone, would quit the spot with a thorough conviction in his mind; and imagining a complete demonstration of it to be here found, would enter it accordingly in his note-book. Yet a more accurate investigation will show that the appearance is very partial, and that no real indications of stratification in the granite, beyond that mentioned, exist. I must remark, however, that the particular spot now cited is not the only example of this laminar appearance on the surface of this granite; since, in one or two other places, where that surface is bare, there adhere to it portions of laminæ, or small strata, resembling these already mentioned.

The mineralogical nature of these rocks is even more remarkable than the geological circumstances that have been detailed.

The granite has, in many parts, the most ordinary aspect and characters; but where it approximates to the sandstone, it consists of quartz and felspar. In some places only, these minerals are equally intermixed in large grains; in others, large grains of each are united in a sort of general basis, or paste, of finer sand. In one variety, of remarkable appearance, the large grains of felspar are of a high red colour, when that mixed with the quartz in the paste is white. The effect of this peculiarity of structure can scarcely be appreciated in description; but the rock approaches so nearly in general aspect to the finer conglomerate, or gravel-stone, of the strata which consists of the same materials, that, in the detached specimens, it is at first sight difficult to know which is the substance under examination.

Another remarkable variety contains schistose clay, which is either in small particles, or in larger, resembling fragments, or else merely communicates a grey tinge to the stone. These specimens similarly approximate in character to that variety of the sandstone which contains small fragments of shale.

The hesitation and uncertainty produced in examining these specimens, particularly when found detached and taken up from among the surrounding fragments, is even greater than that arising from the contemplation of the geological appearances before described. If the mind is turned towards the consideration of the granite at that particular moment, they are placed with the granite series; if, on the contrary, the sandstone has been the object last contemplated, we feel inclined to arrange them in that division. And yet, on a very minute and careful examination, I doubt not that every one who is habitually and extensively conversant with rocks under all their uncommon modifications, will assign to each specimen its true place; from internal, and if slight, yet essential, characteristics, which the experienced eye can discriminate, but which the pen cannot describe.

With respect to the mineralogical characters of the sandstone, I shall limit myself to those varieties which are found at the immediate junction. These are the only ones that are interesting in the present case, and the description of others would unnecessarily extend this paper.

The first stratum at the only part of the junction which is perfectly accessible, is a very compact, reddish sandstone, resembling quartz rock, consisting chiefly of quartz sand, with a few small fragments of felspar. This graduates into a sandstone of the same colour, without fragments. Then follows a purple schist, which is in some places arenaceous, in others fine and fissile, succeeded by a blue shale resembling greywacke. After this there occur, in irregular order, a series of sandstones, red, purple, blue, and brown, intermixed with the same blue and purple greywacke shales. In some places, the schists predominate; in others, the sandstones.

This, however, is not the order of succession every where. In many parts of the junction there are found conglomerates of various kinds, but generally of a moderate-sized structure, or consisting of small fragments. The first of these to be mentioned, consists of quartz and felspar, compacted to a state as hard as the granite which I have already described as of the same composition. A coarser conglomerate consists of small fragments of granite, or of quartz and felspar, cemented by argillaceous schist. Other varieties contain fragments of argillaceous schist, or various fragments united by sand; and some are purely formed of fragments of schist with an argillaceous cement. I must however observe, that the proportion of these conglomerates to the finer sandstones at this place, is very considerable.

Such is the mineralogical description of the several rocks at this place, as far as it seems necessary to notice them for the objects in view. But I must not conclude it without remarking, that there are many more varieties of conglomerate present in some part or other of this junction; which, by reason of the altitude and precipitous nature of the cliffs, are inaccessible in their native places. This is a necessary conclusion from the quantity and variety of the fragments found on the beach, which present all those resemblances to some of the specimens of the granite already noticed.

From these facts, two questions in the first instance arise, namely, whether they are sufficient to prove that the granite is stratified, and whether the apparent gradation between it and the conglomerate, which it so much resembles, is real. Circumstances so important in geological science must not be passed over slightly or decided on hastily; since resemblance, in many other cases than this, does not constitute identity.

That the great mass of the granite is not stratified, appears from its solid continuity downwards, and from the peculiar fracture and structure which I have already described. The appearances at the junction are too limited to justify any conclusion, even with respect to the immediate surface; and they are at best but equivocal. An analogy may set this difficulty in a clearer point of view. Where the red sandstones of the west coast of Sutherland follow immediately on gneiss, the first layer, as I have shown in my work on the Western Islands, is an obscure conglomerate, consisting of gneiss, broken, and re-united to the body of the rock. Such is the union, and such generally the obscurity of the fragments, that it is often not possible to determine where the gneiss terminates and the conglomerate commences. A similar appearance and gradation, where micaceous schist is followed by the sandstone, occurs in Argyllshire.

It is easy to understand that, under similar circumstances, the fragments of a subjacent granite might form a compacted conglomerate, not distinguishable from the original rock; and it is equally easy to see, that the alternation of layers of this substance, with layers of argillaceous schist, derived from some neighbouring source, would account for the extraordinary appearances at this junction.

The compactness of the whole, and its apparent identity with the granite, are no further matters of surprise, than what takes place in the cases above quoted, with respect to gneiss, or micaceous schist; since the lowest, their stratum of conglomerate, in these cases, appears to belong to the gneiss, or micaceous schist, not to the superincumbent sandstone.

With respect to the apparent transition between the granite and the conglomerate which so much resembles it, I must now also remark, that it is one of those resemblances which so often deceive us with the appearance of identity. In the work above quoted, I have mentioned instances in the red sandstone of West Sutherland, and Ross-shire, where the finer conglomerates, from the nature of their composition, and their crystalline compactness, were scarcely distinguishable from granite, on a superficial view. The same happens in the case of quartz rock, so often, as to have deceived the observers of this rock, and to have caused them to consider it akin to granite.

Yet, in these instances, the nature of the rock, if it could not be determined by mere inspection of the specimens, is known by its connection and position; no granite being present, and it being connected by a regular gradation, with unquestionable specimens of sandstone. In fact, where such conglomerates consist of the same materials that constitute the neighbouring granite, as they do in this particular case, and where they are, at the same time, compacted by the crystallization of intermediate quartz, while the fragments are small and angular, it is scarcely possible, without great experience, to distinguish them from granite of the same composition. If, as in this case, from being detached, and lying promiscuously on a beach, in company with fragments of granite, no traces of their geological connection remain, while the very circumstance leads to confusion, it is not surprising that they should not easily be distinguished.

We are not, therefore, entitled to conclude that there is a real gradation of mineralogical character, between this granite and the superincumbent sandstone connected with, and dependent on their geological proximity, and capable of proving a sequence or continuity of geological formation. The same resemblances take place where no granite is present, and where, from the quantity and nature of the intermediate rocks, it is certain that a great interval of time has elapsed between the formation of the granite and that of the sandstone.

If these explanations, then, of the probable nature, both of the geological relations, and the mineralogical resemblance of these two rocks, be judged satisfactory, the difficulties arising from the first contemplation of these appearances, vanish, and the whole is reduced to the ordinary laws, by which the relative positions and nature, both of granite and sandstone, have hitherto been supposed, by all rational observers, to be regulated. The coincidence alone is the cause of the apparent difficulties; and that coincidence, as far as their appearances are concerned, is accidental. One useful general lesson is, at any rate, to be deduced from it; namely, that we should never suffer ourselves to be seduced by the first obvious appearances, which, in this science, as in others, are always ready to mislead us, particularly when they coincide with our wishes or our prejudices. The philosopher, "naturæ minister," has one especial duty to perform, that of recording faithfully the facts which fall under his observation; and it is also his business to spare no trouble or thought, in investigating, with an unbiassed mind, all the most minute circumstances, whether direct or analogical, which may tend either to reduce them under the laws already admitted, or to the establishment of a new rule.

It only remains, in concluding this subject, to inquire how far the facts above detailed are irreconcileable to the theory which supposes granite to be of igneous origin.

As far as the apparent geological and mineralogical transitions between the two rocks are concerned, if the explanation above given is satisfactory, they in no way interfere with it. According to that view, the same, or analogous phenomena, might result, had the sandstone been deposited on gneiss; the formation of the granite and that of the sandstone are as independent in point of cause and time, as if they had been separated by intermediate rocks of any extent. The only difficulty, then, that remains, as far as the igneous origin of the granite is concerned, relates to the very oblique or inclined position of the sandstone above it. Now, in the views of those who maintain that granite is not of igneous origin, and who, in general, equally maintain that the superincumbent strata have not been elevated, since they cannot have been elevated by that rock, this question needs no examination. The sandstone is in its natural position. Whether the granite, in this case, has been elevated so as to elevate the sandstone with it, or whether that has been deposited in its present position, it offers no further difficulty, than that which is of daily occurrence, in examining the primary strata. Quartz rock, stratified with similar regularity, is often equally, or still more highly elevated; and the same regularity is often found in other cases of highly inclined strata, whether granite is present or not; leaving no doubt, that if it is impossible that strata should be deposited from water in positions highly inclined, they may at least be elevated to high angles, by posterior causes, without losing their evenness or continuity.

It is to be concluded, that the granite is in this place prior to the sandstone, because no veins are found passing from it unto the latter, and because, as yet, no instance of granite, subjacent to secondary strata, has been found to send veins into them.

But geologists, even where they admit the production of granite from fusion, have very thoughtlessly assumed, that it must always have been elevated in the fluid state, and in this state brought into contact with the several rocks which it may touch. Not only there is no necessity for this supposition, but the fact must often have been otherwise. Every thing in the history of granite proves successive productions of this substance, as I have demonstrated on sundry occasions; and that solid stratified rocks have been elevated by granite, is one of the very bases of geology. There is, consequently, no reason why a previous and consolidated mass of granite also should not have been elevated by a posterior eruption of the same rock; so far, indeed, is the case otherwise, that a large portion of the history of the changes of the globe is implied in this very fact. In such a case, the strata lying on the granite might be disturbed in any manner; elevated to the highest possible angles, and also broken or bent. But they would not contain veins of the granite in immediate contact with them; because they were deposited on its solid surface, and elevated afterwards only in consequence of its elevation. And in reality, while Scotland presents numerous instances of this very fact, the case before us is but one of them. The sandstone, and the lignite formation after it, have been deposited on the consolidated granite of this district; while a subsequent elevation, of which I have not yet discovered the cause, or The Granite, has brought the whole secondary strata into the position which they now display.

I may now proceed to describe the lignite formation, or what is called the coalfield of Sutherland. That I formerly noticed its general nature in the paper on Lignites, does not preclude a proper description of that which the scattered peculiarity of its positions in Scotland renders additionally interesting.

Sutherland, August, 1819.

Observations on the Existence of Chlorine in the Native Peroxide of Manganese, &c. *By* Robert John Kane, Esq.

Having, in some late numbers of your valuable Journal, observed some papers on the existence of chlorine in the native oxide of manganese, by Mr. John Mac Mullen, I was struck by the extraordinary inferences which that gentleman deduced from, in my opinion, very inefficient data. However, owing to numerous other avocations, I was unable at that time to give the subject the attention it deserved.

I have been now induced to present the following observations on that subject, in consequence of some experiments, the results of which tend, at least, to shed some light on the causes of the phenomena observed by Mr Mac Mullen.

Having had occasion since the publication of his paper to execute an analysis of the impure nitrate of potassa, used in the manufacture of sulphuric acid, and finding muriate of soda present to a considerable amount, I was led to suspect the existence of hydro-chloric acid in oil of vitriol, from the probability that as, in the theory of the production of the latter substance, a portion of the sulphur being acidified, decomposed a quantity of nitrate of potassa, and as the nitre contained muriate of soda, we may infer *à priori* that a quantity of muriatic acid would be eliminated at the same time, and would be absorbed by the water.

To verify these ideas, I made some attempts to ascertain the presence of hydrochloric acid in the sulphuric acid of commerce, and after some trials, I found the following process to answer perfectly.

A portion of the sulphuric acid of commerce perfectly clear, being diluted with twice its weight of water*, was filtered through powdered quartz to separate the sulphate of lead precipitated. It was then neutralized by pure bi-carbonate of potassa (prepared by decomposing cream of tartar, and passing carbonic acid through the solution of the carbonate thus obtained)—a large quantity of sulphate of potassa was deposited

* We deem it almost unnecessary to mention that the water used in this and in other experiments was distilled.

in minute crystals. The supernatant fluid was poured off, and the crystals washed with a little cold water, which was added to the rest of the fluid. The whole being now diluted, was mixed with a solution of nitrate of silver, and the precipitate being allowed to subside, was boiled in a large quantity of water to dissolve the sulphate of silver. (The quantity of which produced varied according to the state of concentration of the liquid.) Chloride of that metal remained, which in different specimens indicated a quantity of dry muriatic acid, varying from 0.03 to 0.14 per cent. of the strong sulphuric acid used.

I have in this manner examined the sulphuric acid from Mr. Mac Mullen's, Mr. Jones's, and my father's factories. The specimen which contained 0.03 per cent. was from Mr. Jones, and that which contained 0.14 was from Mr. Mac Mullen's manufactories.

Having thus obtained a key to the numerous otherwise inexplicable deductions of that gentleman, it is totally unnecessary for me to give a detailed explanation of the phenomena which he relates. I shall, therefore, merely take up a few of his most important assertions, with a view of showing their total inaccuracy and insufficiency.

Mr. Mac Mullen sets out by observing, that in his *very extensive* factory he has had many opportunities of observing the peculiar smell which manganese emits when acted on by sulphuric acid, and that the odour depends on the disengaging of chlorine.

He then states that he thought it could not be present in the manganese, as a muriate of iron, copper, or lead, metals which that mineral sometimes contains; because, that before he added the sulphuric acid, he had washed the ore, and the washings had not precipitated nitrate of silver; and that the water would have removed those muriates if they had been present.

As Mr. Mac Mullen complains that Mr. Phillips misinterpreted him, I shall take his own words. Mr. Mac Mullen proceeds—" Having in this manner thoroughly satisfied myself of the existence of chlorine in the black oxide of manganese, I was naturally led to inquire in what chemical state it might be concluded to exist in the combination. That it should be either the muriate of the oxide or chloride of the metal, seems highly improbable, from the single circumstance that oxygen gas is abundantly obtained by heat. A further consideration of this last-mentioned circumstance ultimately suggested to me that view of the subject which, after the most minute and anxious investigation, I have been led to adopt, namely, that chlorine exists in the native oxide of manganese, as chloric acid, and that the mineral is in part a chlorate of manganese."

Mr. Mac Mullen infers that if the muriate of iron, of copper, or of lead had been present, they would have been removed by washing; that if muriate or chloride of manganese (between which substances Mr. Mac Mullen seems to make some distinction) had existed, the ore would not have given out oxygen*. Now I ask Mr. Mac Mullen, whether a degree of washing sufficient to remove muriate of lead, would leave any muriate of manganese behind? It is quite unnecessary to answer this question. Even if Mr. Mac Mullen had not made the experiment, any table of salts would have informed him of their relative solubility. I next would inquire how the presence of a very small quantity of chloride of manganese could prevent the production of oxygen from the peroxide? When chlorate of potash is decomposed by heat, does the chloride of potassium which is formed from the instant the first bubble of oxygen comes over, prevent the further decomposition of the chlorate?

Laying aside the total inaccuracy of Mr. Mac Mullen's statements, and supposing with him that the mineral cannot contain the chlorides of iron, lead, copper, or of manganese, he next says, that since these do not exist in it, the ore must contain chloric acid, because it gives out chlorine on the addition of sulphuric acid. Now does Mr. Mac Mullen say that a chlorate decomposed by sulphuric acid gives out chlorine? As he quotes Dr. Ure occasionally, I shall refer to his work. If Mr. Mac Mullen had attended to the article "Chloric Oxide," he, I am certain, would not have advanced such a statement.

* Even if chlorate of manganese had existed, would not the washing remove it? The chlorate of potash will not precipitate any metallic solution.

Mr. Mac Mullen next travels to the subject of chameleon mineral, and entirely overturning by theory the facts of Chevillot and Edwards, he says, that in their well-known experiments the azote was absorbed, and prevented the formation of chameleon mineral, not by excluding oxygen, but by forming nitrate of manganese. From this explanation, Mr. Mac Mullen infers that chameleon mineral is a chlorate of manganese and potash, and supports his opinion by the inflammation of manganesiate of potassa with combustibles.

Wishing rather to increase the validity of the opinion of the French chemists than to overturn that of Mr. Mac Mullen, I made some experiments, which I will state in full.

Exp. 1. Into a strong tube-retort, a small quantity of oxide of manganese and pure potash was introduced, and rammed tight by a copper wire. The open extremity was now drawn out, like the neck of the receiver figured in Faraday's *Manipulation*, p. 399. The retort was then filled at the mercurial trough with nitrogen previously passed over dry chloride of calcium, the capillary end was then strongly sealed. The body of the retort being now coated with lute, was brought to redness, and the heat increased until it began to soften; it was then removed, and the sealed end broken under mercury; instead of any absorption, a quantity of gaseous matter rushed out, which I could not obtain for examination. In repeating this experiment several times the tubes generally burst, unless they were very thick, well sealed, and removed the instant they began to soften.

Exp. 2. The retort was charged, and the open end drawn into a fine tube, which passed under a jar in the mercurial trough; a considerable quantity of gaseous matter was obtained, and found to consist, the first portion of oxygen and the nitrogen of the apparatus, the latter of pure oxygen.

Exp. 3. The retort being charged with manganese and potassa, was filled with oxygen at the mercurial trough, and sealed; the luted portion was brought to bright redness; when coated, the narrow end was broken under mercury, when a considerable rise into the retort indicated the absorption of oxygen. Mr. Mac Mullen states that the experiment, to insure success, should be performed in vacuo. I had not a good air-pump, or

I should have done so; but I would ask that gentleman, how he will reconcile the above facts with his theories.

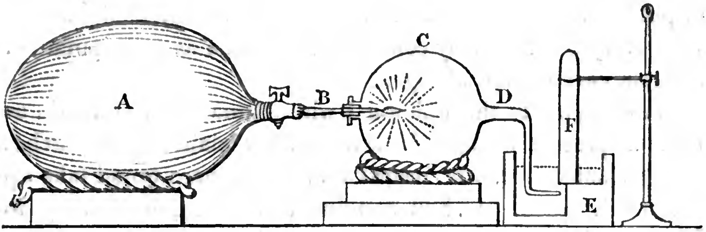
Mr. Mac Mullen next proceeds to state that the same effects take place when red lead or when the peroxide of lead were used, as when manganese. We shall not, therefore, notice them, more particularly as the explanation of one is satisfactory for both.

Reasoning on the thus proved existence of chlorine in red lead; and reflecting on the nature of the circumambient agents in the manufacture of that article, which he estimates as four, *viz.*, oxygen, carbon, nitrogen, and hydrogen, he jumps to the settling, as he himself says, " of a question, on which the highest powers of the first philosophers of the age have been literally exhausted,"—the composition of chlorine.

Omitting the numerical calculations of the composition of chlorine from the atomic weights of its constituents, and which he settles as $\overset{\text{prim. oxygen.}}{2} = 16 + \overset{\text{p. azote.}}{1} = 14 + \overset{\text{p. carbon.}}{1} = 6 = \overset{\text{p. chlor.}}{36}$ or $\overset{\text{p. Nit. Oxide}}{22} + \overset{\text{p. Carb. oxide.}}{14} = \overset{\text{p. chlor.}}{36}$. I shall proceed to the experiments by which he, I believe, intended to support it.

Into a Papin's digester he put salt, manganese, and sulphuric acid diluted with water; he then placed in the digester a glass vessel, containing manganese and hot sulphuric acid. The cover being put on, the oxygen and chlorine passed out through a tobacco-pipe, at the end of which they were inflamed. A bell glass held over the flame was found to contain water, muriatic acid, chloric acid, and euchlorine, but no carbonic acid. Mr. Mac Mullen satisfied himself of the absence of carbonic acid, as the vapour did not precipitate lime water; but he forgets to state how he detected chloric acid and euchlorine. He allows before that he could not prove chloric acid to exist in manganese, yet he avers that he found it as a product of this combustion, but without saying how. In what manner he detected euchlorine in such an apparatus, I am at a loss to conceive.

To investigate the truth of this experiment I made the following: Having obtained a sufficient quantity of chlorine and of oxygen, a bladder was filled with the gases mixed in equal volumes; a piece of thermometer tube was then fitted to the stopcock of the bladder, and thrust through a perforated cork: this cork fitted accurately the mouth of a glass globe, from which a quill passed to the mercurial trough. The cork being pushed up, I applied a taper to the stream of gases issuing from the tube, but they would not inflame. A jar was then filled with the mixture over warm water, and a lighted taper plunged into it; the taper of course, burned brilliantly, but there was no explosion.



A. The bladder and stopcock containing the mixed gases. B. The thermometer tube cemented into the stopcock, and passing through the cork of the globe. C. A globe, to the mouth of which a cork was fitted. The cork being slipped upon the tube, the stream of mixed gases was fired, and the tube and cork then introduced to the globe. D. The quill going from the globe to the mercurial trough. E. The trough. F. A gas jar to receive the elastic fluid evolved.

I then put a mixture of salt, manganese and sulphuric acid into a cast-iron bottle, and collected the gas evolved. When the apparatus above-mentioned was used, the gases burned with a yellow flame, and water, impregnated with muriatic acid, was produced, a great excess of chlorine and of oxygen passing over into the jar, the mercury absorbing the former, and calomel being produced.

I next mixed equal volumes of oxygen, hydrogen, and chlorine, and using the same apparatus, exactly the same results were obtained. I never could detect any chloric acid or euchlorine.

Mr. Mac Mullen says next, that when muriatic acid gas is passed over heated oxide of manganese, the same inflammable gas is produced. On repeating this experiment with a series of tube apparatus, and receiving the elastic fluid produced over water heated to 100°, I got nought but chlorine, and when there was not a sufficient quantity of muriatic acid to decompose the whole of the manganese, a little oxygen.

Mr. Mac Mullen makes some observations on the common fulminating powder, to infer, that when it fuses, the carbonic acid of the salt of tartar and the nitric acid of the nitre combine, and form chloric acid, which then detonates with the sulphur.

The proof he brings of this, is, that there exists a general rule that all detonating compounds contain azote, and as chlorate of potash is considered not to contain nitrogen, he does away with every objection, by considering chlorine as a compound of oxygen, carbon, and nitrogen.

With regard to this opinion I will not make any remark, as I am at present engaged in some rather elaborate researches on that substance, the insertion of which here would take up too much space. I shall conclude, therefore, by hinting to Mr. Mac Mullen, that although "analogy has its use of connecting facts together, and guiding us to new experiments, yet when totally unsupported by exact experiments, it certainly should not be carried so far."

On the Inland Navigation of the United States.

[Part III.]

In the State of New Jersey, the only work of any note is the Morris Canal. The object of this navigation is, to form a communication between the upper waters of the Delaware and the city of New York. The Delaware is navigable, above the Falls at Trenton, for vessels carrying from ten to twelve tons. These descend, by the force of the current, loaded, and are carried back empty, or with small loads, by the aid of poles. The Lehigh, a branch that joins the Delaware at Easton, is also navigable in the same manner. More recently, improvements have been made in these navigations, that will be mentioned in their proper place. By the improvement of the latter river, coal, from the great anthracite formation in Pennsylvania, descends to Easton, and, when the Morris Canal is completed, will be enabled to proceed to New York. This is the shortest practicable route between the coal mines and that city.

The value of a direct communication across the State of New Jersey, first struck an intelligent and public-spirited inhabitant of Morris County, Mr. G. P. Mac Culloch. This gentleman devoted much time and labour, and no inconsiderable amount of his private funds, to the examination of the route, and the exhibition of the value it would be of to the district through which it passed, as well as in a general point of view.

When, however, the ground came to be actually examined, difficulties were discovered that must have caused the scheme to be abandoned, had the investigation been entrusted wholly to engineers acquainted with no other principle in canals than the use of the lock. It so happened, however, that Mr. Mac Culloch had engaged a friend of his to assist him in the preliminary investigation, who, as he afterwards expressed in his Report, was of opinion, that "it would be unreasonable to suppose that, where no physical obstacle exists—where water is found in abundance, to be used as a moving power—where the wants of the country call for navigable communications, and where the prospects of a lucrative traffic hold out the most ample encouragement for the investment of capital, the science of hydrodynamics does not possess resources fully adequate to the removal of every difficulty."

The difficulties consisted in the great deviation of the summit, which is 900 feet above the tide, and 700 feet above the Delaware at Easton. But upon this summit is situated a lake, having a surface of 1500 acres, and emitting a river that, in the driest seasons, is more than sufficient to supply a lock navigation. This river is a branch of the Delaware; but as a cut of no great length, and of small depth, would carry it towards the Hudson, the engineer did not hesitate to pronounce the scheme of the Canal practicable, and undertook to point out practicable means for its execution. As locks were inadmissible, not only from their cost, but still more so from the delay they would have interposed to the passage of boats, he turned his views to the principle of the inclined plane.

In the infancy of artificial navigation, the simple sluice, and the inclined plane, were both used, according to circumstances. In China, they are used even at the present day; but both are rude, and require much labour in their use. The lock (probably an accidental discovery) was an improvement on the sluice; and, as the difference of level in the navigations then in use, or which were for a long period planned, was small, it superseded the inclined plane also. Still partial attempts were made, from time to time (and some of them eminently successful), to make the inclined plane, as well as the lock, a self-acting machine.

All these plans, whether actually carried into effect or merely projected, were in turn examined; and the result showed that no one of them was applicable to the circumstances of the Morris Canal. The engineer was therefore driven to his own resources, and was successful in proposing a plan for overcoming the great altitude of the summit of this canal, which, in the opinion of the persons to whom it was submitted, was entirely adequate to the purpose. It being considered of the greatest importance to convince the public of the certainty of the means proposed for the execution of this canal, the Commissioners appointed by the State of New Jersey took every means of obtaining the opinions of men of the highest knowledge and experience. Among these were the late De Witt Clinton; Benjamin Wright, the most successful of the engineers of the great New York canals; General Swift, formerly at the head of the Engineer Corps of the United States; General Bernard, a French engineer of high reputation and rank in the "Corps de Génie," now in the service of the United States; and Colonel Totten, of the United States Engineers. The opinions of all were favourable. The two last-named officers, being ordered to examine the scheme by the War Department, made it the subject of more detailed investigation than the others, and reported fully, comparing the inclined plane proposed by the engineer of the canal, with all other modes of passing from one level to another that were known to them. Their Report concludes in the following terms:—"To us, however, whether we consider the economy, the utility, or the durability of these inclined planes, ALL IS CERTAIN; and we look confidently forward to the day when their introduction will be regarded as a most important era in the history of canal navigation, and especially in this country, to which they are so peculiarly adapted."

About the time of completing this Report, General Bernard received from Count Réal a plan of an inclined plane proposed for the Canal of Charleroi in France, by De Solages. The principle was the same as the one under investigation, and both engineers had set off from the same point: both had met with the same difficulties; but the means of removing them were so different, and the effect of the American plan so much superior, that it was not considered necessary even to modify the Report. They were rather inclined to consider it as fortunate that it had not been previously known, as it would have probably been adopted, and precluded the search which led to a superior form.

In addition to his Report upon the means of construction, Clinton undertook to exhibit the advantages the State of Jersey would derive from constructing the Morris Canal on its own account. However powerful his reasoning was, it failed in overcoming the apathy, or in neutralising the opposing interests in that State. After an interval of two years of earnest application to the Legislature, the friends of the scheme were compelled to accept a charter for the formation of a private company.

When the books of this company were opened for subscription, the public, fascinated with the boldness, the obvious advantages, the certainty of the scheme, sought for the stock with more avidity than ever occurred in any former instance in the United States. Seven times the required capital were subscribed, and the commissioners actually held in their hands in cash, nearly all the funds necessary for completing the canal. These circumstances led to a desire to obtain an undue advantage in the distribution of the stock, and a majority of the commissioners was found to sanction such a selection of names, as threw the whole controul into the hands of a party, who had no other object in view than stock-jobbing. A Board of Direction was elected, which could be managed in such a way as to carry this object into effect, and care was taken to leave out, or overrule, all persons who had any interest in the execution of the canal, either pecuniary, or as a matter of reputation. Hence, although the original projector of the canal was placed in the Board, he was deprived of all influence, and the engineer who had explored the route and planned the means of execution was not employed. These violent measures defeated the object they were intended to subserve; the public had not confidence in the parties who had thus grasped the whole controul, nor in the means, by which, deviating from the original plan, they proposed to execute it. Failing in their hopes to make a *bubble*, the speculators had the address to withdraw their deposit from the funds of the company, and to leave it with a capital reduced one-half, and the character of an abortive *job*. This, however, was not effected, but at the risk of a criminal prosecution, on which some of the parties were convicted, but were saved from the punishment they so richly deserved, by legal quibbles.

Under such circumstances, it was hardly to be hoped that the Morris Canal would be soon rendered a second time popular. It cannot, however, be doubted, that had there been an immediate return to the original principles, and plans, it must speedily have recovered public confidence. A new subscription was obtained for a part of the withdrawn capital, in the hopes that this course would have been pursued. But while the new officers have no doubt acted with honesty, they have shown great want of knowledge and intelligence, and have continued to entrust the construction of the canal to persons incapable, themselves, of proposing any means of overcoming the difficulties that oppose progress, of appreciating the merit of the original plan, or of judging among the innumerable fancies that a population teeming in *notions* exhibit to them. Several inclined planes have been actually experimented upon at a large expense, but all have proved abortive, and the ridicule is enhanced, from the fact, that more than one of them are upon principles that had been examined and reported upon by the original engineer, and the persons called to inspect his plans, and declared unfit for the purpose. At the present moment, it is difficult to prophesy what will be the result; success is not entirely hopeless, much work having been well done, and much capital been left; but still no great expectation of a favourable result can be entertained, unless more fixed principles and more enlightened views be brought into its direction.

The state of Pennsylvania was settled at a later period than the other middle states. The original population brought with them the useful arts in a state of greater advancement than the settlers of New England, or the Dutch planters of New York. The soil, also, was more fertile, and the climate milder. All these circumstances, together with the religious character of the Quakers, if they impeded the birth of enterprise, and that restless spirit of locomotion, that has extended the New England race so widely, yet led to the construction of public works of more stability and expense. The buildings, the roads, the bridges of Pennsylvania, have a character of solidity and permanence, rarely to be met in other parts of the Union. Penn, the first proprietor of Pennsylvania, was also possessed of the most liberal and extended views, while the rapid and unlooked for progress of the other colonies, authorized him to indulge in anticipations that had never entered into the minds of those who preceded him in the adminstration of the more ancient colonies. The plan of the city of Philadelphia was capable of execution only, in the case of the Schuylkill being rendered navigable; and the delay that has attended the execution of this original project of the first proprietor, has caused the extension of the city to take place in directions not anticipated by him. It is only since the completion of that navigation, that the foresight of this remarkable man has been justified by the formation of a second port on the Schuylkill, extending its arms to meet the main city on the Delaware. Various projects of inland navigation were formed in Pennsylvania, long before the other states turned their attention to the subject; but, committed to private companies, with deficient capital, none were completed, and many, after being nearly finished, were abandoned. The success of the New York canal gave a new impulse, and fresh capital was found to be embarked in these languishing enterprises. The state itself has recently entered, on its own account, into the construction of new canals, or become a partner in those authorized to be constructed by private companies. By recent and very accurate information *, it appears, that within the state of Pennsylvania there are about three hundred miles of canal actually completed, about one hundred and twenty miles of improved river navigation, and eighteen miles of railway; while the legislature has authorized the construction, in all, of one thousand two hundred miles of canal, the improvement of two hundred and twenty miles of river, and the formation of nearly six hundred miles of railway. Of the canals thus authorized, seven hundred and fifty miles more are actually in progress, and reasonable expectations may be entertained, that they will be finished in less than three years from the publication of this paper. The extent, therefore, of the internal navigation of Pennsylvania will probably soon exceed that of any state in the Union.

Among the more important of these canals is one from the tide waters of the Delaware, along its western shore, as far as the boundary of the state of New York. This will be productive of much advantage, for there is a large district of country in that direction, which has lain so far from any accessible route, as to have remained in a state of wilderness, although much of it is capable of being rendered fertile. The Lehigh river, which, as we have before stated, joins the Delaware at Easton, has for several years engaged the attention of a company formed for working the coal mines that lie a few miles from its channel. This river carries a large body of water, but is so rapid as to be dangerous for a descending, and impracticable for an ascending navigation. The first attempt at a navigation was a simple improvement in the channel itself, by weirs, sluices and wing-dams; and the object was limited to obtaining a safe descending navigation for *arks*. Although the transport of coal by means of these rude vessels was not found too expensive, still it furnished a supply far too limited for the demand; and a canal, for the whole distance, with locks, has been undertaken, and will soon be finished. It will communicate by the Morris Canal with the city of New York, and by the Delaware canal with the tide waters of that river. It has, however, been constructed upon a scale much greater than

* Furnished the writer by Mr. G. W. Smith of Philadelphia, a high authority in all questions of internal improvement.

was necessary; for the canal itself, and its locks, are much larger than either of the navigations that give an outlet to its trade.

In the State of Delaware we have no inland communications of importance to mention, besides the Cheseapeake and Delaware canal, described in treating of the great system parallel to the coast.

In Maryland, the principal contemplated improvement is a communication between Yorkville, on the Susquehannah, and the city of Baltimore; this was originally examined with the view of effecting it by a canal, but the elevation of the summit, and the scarcity of water, have merged this plan in that of a railway.

In Virginia, we have no works of mere local importance to speak of, except the improvement of the Apomatox river, for a distance of one hundred and ten miles.

In North Carolina, great attention has been paid to the examination of plans for the improvement of its navigation; but the efforts have been principally directed to the opening of a ship channel through the bars that inclose the sounds extending along its coast. The inlets have become so shallow, that North Carolina, possessing a wide extent of coast, has not a single sea-port for large vessels.

In South Carolina, the Santee was joined as long since as the year 1802, with Cooper river, one of the streams at whose confluence the city of Charleston is situated. This canal, which is twenty miles in length, is remarkable among the public works of the United States for the solidity and beauty of its works of masonry. The Santee river is navigable from the junction of this canal as high as Columbia, the seat of the state government, and hence a mixed navigation has been carried into Abbeville county; the whole distance from Charleston being one hundred and fifty miles.

The State of Georgia has not executed any important canals, but has for some years devoted appropriations to the examination of the resources and means of the state for constructing them.

In Louisiana, the engineers of the general government have reported a plan for a canal between the Mississippi at New

Orleans and Lake Ponchartrain. This canal will not only be valuable as a navigation, but as affording the means of diverting the waters of that great river when swollen beyond its usual size.

In the newer states, great opportunities will no doubt be presented for extending an inland commerce, by the improvement of rivers and the construction of canals. Years, however, must elapse before their resources become adequate to such enterprises. Many have already been spoken of; but in this view of the subject, however interesting to the neighbouring inhabitants, they can possess no claim to our attention. We have, indeed, extended this essay so far beyond the limits within which we at first hoped to comprise it, that a fear of becoming tedious has prevented us from dwelling upon many points that, in the views of those interested, may be considered at least as important as those on which we have dilated. Those, however, who may wish to obtain more minute information than we have given, will find it in a work on "The Internal Navigation of the United States," published in Philadelphia, by Carey and Lea, in 1826. To this we have been indebted for many of the facts we have stated, that had not come immediately under our own view, and have, in consequence, to make our acknowledgments to the laborious and intelligent compiler of that work.

To the Editor of the QUARTERLY JOURNAL *of* SCIENCE.

SIR,

The *Illustrations of Nature* published in your last number, (VI.) extended to the confines of the animal reign. Perhaps it might have been expected that illustrations of general zoography should next have followed; but to illustrate the animal kingdom generally, even in outline, would lead either to a very extended or a very superficial essay; and as the general view could be but faintly seen, and feebly understood, unless particular associations were previously pointed out, I have resolved to confine my present observations to a very well marked series of animals; *viz.* apes and their allies; and make the selection, not only because they long have been, and still may be considered the primates of the brute creation, but also as they afford a good praxis on methodical arrangement.

It may chance, that by some I shall be blamed for introducing so many, and by others for not adopting all, the modern improvements (innovations?) in nomenclature: but as far as they involve no fundamental error, I have preferred old names, because they are familiar; although their definitions occasionally require to be modified. Hence I have introduced new terms reluctantly, and only when the current ones were inconvenient and incorrect; nevertheless when so, I have not scrupled to propose their abolition, however high the authority by which they have been imposed. Utility, rather than novelty on the one hand, or subserviency on the other, having been the object aimed at in the present sketch, it will I trust plead my apology for such alterations as have been needfully, not wilfully, introduced.

I have the honour to remain,

Yours obediently,

Gilbert T. Burnett.

Sept. 1*st*, 1828.

Illustrations of the Manupeda, or Apes and their Allies; being the arrangement of the Quadrumana or Anthropomorphous Beasts indicated in Outline.

Animals of the ape or monkey type approach more nearly to the human structure, than do any others of the brute creation; hence, perhaps, the name monkey, monkin, manikin, *quasi dicat*, somewhat like or akin to man; a term as familiar and equally expressive with the more classical anthropomorpha. Their gestures, also, seem often to have been thought a caricature or mockery of human ways, hence are they called apes.

These beasts and their natural allies (the Pitheci and Cercopitheci of the Greeks, the Simiæ of the Latins, the Monichi of the barbarous ages) form a very natural association. Their approach to the human shape, the original bond of union, has been much strengthened by the notice of other common characters, especially their having thumbs, and all their feet having somewhat the structure, and performing many of the offices of hands, being organs equally of progression and prehension: thus associated, they constitute the Anthropomorphous quadrupeds of Ray and Pennant; the genera Simia and Lemur of Linnæus; the Quadrumana of Blumenbach, Cuvier, and most modern writers.

The apes and their more immediate allies, which by Ray were associated in a distinct group, by Linnæus, on account of their teeth, were blended with man and bats, in his order Primates; which thus forms a very heterogeneous assemblage. Modern zoographers have therefore returned them, in spite of their teeth, to the plan of Ray; and separating the true apes and their natural allies from men and bats, have called them Quadrumana, [Quadrumanes, four-handed beasts,] instead of anthropomorphous digitated quadrupeds. Perhaps, strictly speaking, neither quadrupeds nor quadrumanes are appropriate terms; for in some, as the Chamek [Ateles pentadactylus], the thumb scarcely protrudes externally; and in the four-fingered Chamek [Ateles paniscus], it is completely hidden beneath the skin. Cuvier observes that in Ouistitis [Titi], the thumbs are so little widened from the other digits, "qu'on ne leur donne qu'en hésitant le nom de quadrumanes;" and Carus well remarks, that "the so called hands of apes should rather be termed feet," for hands are organs of touch and of prehension, *not* of progression; but feet are organs of progression rather than of prehension: therefore, as the paws of apes, &c., are chiefly organs of progression, but which progression takes place by prehension, might not their form and use be better designated by the term Manupeda, Manupeds, foot-handed or hand-footed beasts?

The Quadrumana of modern systems were comprised in the two genera Simia and Lemur of Linnæus, from the former of which Cuvier has most judiciously separated a very distinct race, under the name Ouistitis [Titi], the Hapales of Illiger, and Arctopithecus of Geoffroy.

As the species of the Linnæan genus Simia, even in his time, were very numerous and very various, and as since then they have much increased both in number and variety, many schemes of subordinate arrangement have been devised, and subgenera, sections, and subsections have been introduced, often without regularity and without principle: which joined with the numerous synonymes which authors have imposed, and different systematists not only giving different names to the same individual, but also describing different individuals under the same appellation, has introduced much irrelative obscurity; and tended not a little to increase that confusion, in which this interesting department of the animal reign is still partially involved. The tail-less, short-tailed, and long-tailed sections, as enumerated by Ray and others, although in part obvious, are far too vague distinctions; and the anatomical structure of the animals arranged by them, as Apes, Baboons, and Monkeys, by no means justified the distribution. This arrangement, therefore, requires to be carefully remodelled; so that, if possible, without losing sight of the obvious distinctions of our elders, their groups may be corrected, and their plan enlarged and strengthened by the researches of modern times.

On a general view of this (the Manupeda or Quadrumana) as of the other types of animals, after having associated into one group all accidental or slight variations from some normal specimen, thus by the union of *varieties* which are akin to each other, constituting a *species*, it is found that two or more species, although not so near akin to each other as are the varieties which constitute each species, still are more intimately connected than either of them to certain other groups, which, although of the same kind, are not (as our provincial dialect would express it) of the same kith or kin. These associations are called indifferently, genera or subgenera, or subsections of subgenera, &c. Again, these *groups of species*, although distinct from, are more closely allied to, each other, than any of them to certain larger groups, which comprise the groups of species, but which are not of the same *kind*. Sometimes it happens that only a single species or genus may be known of a given kind, but more frequently there are several or many; and often there are several kinds which form associations among themselves, more intimate than with certain other kinds, being what in familiar language is called a *race*;

as, for instance, the anthropomorphous race, which includes the several kinds of the true apes, baboons, monkeys, &c., as contrasted with the lemurs. Each of these successive stages has been called a genus, and the others, sections and subsections, subgenera, &c.

Thus the genus Simia of Linnæus comprehended all the true Pithecatæ [the ape or monkey race], with the Arctopithecatæ [the Ouistitis], most judiciously (on account of its having claws instead of nails, &c.) separated by Cuvier. The genus Simia, even when the Ouistitis is removed, contains not only the true Simiadæ or Apes, but also the Papionidæ or Baboons, the true or lax-tailed Monkeys, the Howlers, or Monkeys with prehensile tails, &c., all amalgamated together; and each of these *kinds*, when separated and respectively analysed, consists of several subsections: *e. g.*, the Simiadæ contains the Mimetes or Chimpanzee, the Simia Satyrus or Orang Ootan, the Gibbon, &c., and so of the others. Each of these subsections, which are the genera of Geoffroy, Illiger, and others, would in fact seem to be the true genera, containing one or several species, and the species consisting of varieties as before explained.

Hence it becomes evident, that if all these successive stages of alliance be called indifferently genera, subgenera, &c., needless intricacy must be the result; therefore, as the group of varieties is acknowledged to form a *species*, would it not be better to call the group of genera *a kind;* the group of kinds *a race*, and so on,—thus precluding that indeterminate phraseology which is the bane of science? Yet, to avoid misconception, I must here advert to what has been already hinted, and at another time must be more fully dwelt on, *viz.*, that all *essential* * characters, as they are called, can be but relative; and as new species and genera are discovered, or as our knowledge of the old becomes more perfect, these characters, which are but conveniences, and invented solely to direct the mind at once to the most important peculiarities, often require to be modified; the natural characters, which are too tedious for general use, being the only true and permanent distinctive signs: examples of these series may be seen in the

* *Indicative* is a better term.

adjoined Table, *q. v.*, one will suffice for illustration. The collared and uncollared mangabeys are instances of two species, each consisting of varieties dependent on trifling and often transitory marks. These two species are much more intimately allied than either of them to the Rolowai or the Malbrouk, these forming the true genus Cercopithecus, as those Cercocebus, would commonly be called subsections of a subgenus, contained in one of the sections of the genus Simia. Again, the Mangabeys [or genus Cercocebus] are more closely connected to the Malbrouks [or genus Cercopithecus] than either of them to the Apes, Baboons, or Howlers; which further alliance, by distinguishing the true monkeys from their associates, has been called a subgenus; in fact, it is the distinction of a kind, and indicated by the termination, *dæ* or *idæ*, affixed to the names of the most important or most familiar, *i. e.*, the normal genus,—in this case Cercopithecus,—and hence the group is called Cercopithecidæ, or Cercopithedæ, *i. e.*, Monkeys, or the monkey kind. But as apes, baboons, monkeys, &c., are more nearly allied than either to the Titidæ (Ouistidæ,) or Lemuridæ, (the genera Ouistitis and Lemur, of Cuvier and Linnæus,) they form together the Pithecatæ [the Ape or Monkey race]: and these three races, *i. e.*, the genera Simia and Lemur of Linnæus, with the Ouistitis of Cuvier, are connected as a district or type, by their all possessing hand-like feet, [their indicative character,] under the name Quadrumana, four-handed; or rather Manupeda, hand-footed beasts: thus entirely avoiding that intricacy of genera and subgenera, sections and subsections, which inevitably occurred when the whole of these animals were considered as belonging to only two genera, *viz.*, Simia and Lemur.

To define the species and genera, or even to enumerate the whole, would be without my plan, (which is to give merely an outline or prodromus,) and for these I must at present refer to the works of Pennant, Shaw, Audebert, Geoffroy, Cuvier, Illiger, and others; my object now is to condense into a practical form, to draw as it were into a focus, those general views which must be sought in several languages, and are spread through many volumes: some few, however, of the characters of the type, races, and kinds, must not be omitted.

Type MANUPEDA, (Quadrumana Anthropomorpha). Four hand-like feet; all the paws fitted equally for prehension and progression; not well suited for walking on the earth, or for maintaining an upright posture. Orbits enclosed from the temporal fossæ. Three sorts of teeth. Pectoral mammæ.

Races, PITHECATÆ, (Simia, Apes, Baboons, Monkeys, Sagoins, and Howlers). Four opposed incisors in each jaw, all the nails flat.

ARCTOPITHECATÆ, (Ouistitis). Teeth as in the former; nails claw-like, except the hinder thumbs; thumbs so little separated as scarcely to be quadrumanous.

ODONTIPITHECATÆ, (Lemur). Teeth irregular, upper incisors not opposed to lower; two middle upper incisors separated; nails all flat, except hind index, which is claw-like; thumbs widely separated from the other digits; nostrils sinuated.

PITHECATÆ, Monichrace, or Monkey-race.

Kinds, Simiadæ, Apes. Facial angle 65°—50°; teeth 32, grinders 20; os hyoides, liver, and cæcum like man; no callosities? no pouches?

Papionidæ, Baboons. F. a. 40°—30°; hence protruded jaws; 32 teeth, 20 grinders, last molars 5—7 tubercles; callosities, pouches; tail generally short, sometimes obsolete.

Cercopithedæ, Monkeys (common), Guenons. F. a. 50°—40°; teeth as baboons and apes, only 4 tubercles on last molars; pouches, callosities; tail generally long, always lax.

Geopithedæ, Sagoins. F. a. about 60°; teeth 36, 24 grinders; no pouches, no callosities; tail lax, or nearly so.

Stentoridæ (Helopithedæ), Howlers. F. a. 60°—30°; teeth as former; no pouches, no callosities; tail prehensile.

ARCTOPITHECATÆ, Titirace.

Titidæ, or Ouistitidæ, the only admitted kind, formed of two genera, &c. vide Table. Cheirogaleus may not impossibly belong to this race; further observations must decide.

ODONTIPITHECATÆ, Lemurace.

Indridæ? Indris. Incisors equal in number in each jaw; tarsus proportionable.

Lemuridæ, Makis. Most incisors in lower jaw; tarsus proportionable.

Tarsidæ, Spectres. Most incisors in upper jaw; tibia longer than femur; tarsus triple the length of metatarsus.

	Species.	*Genus.*	*Kind.*		
Chimpanzee,	Troglodytes,	Mimetes.	SIMIADÆ, APES.	PITHECATÆ, APE OR MONKEY RACE.	
Orang Ootan,	Satyrus,	Simia,			
Black Gibbon,	Lar,	Cheiron,			
White G., or Moloch,	Leuciscus,	——			
Pigmy,	Sylvanus,	Pithes ?			
Barbary Baboon,	Innuus,	Pithecus,	PAPIONIDÆ, BABOONS.		MANUPEDA.
Boggo,	——	Pongo,			
Mandrill,	Maimon,	Papio,			
Common Baboon,	Sphynx,	Cynocephalus,			
Dog-faced,	Hamadryas,	——			
Lion-tailed,	Silenus,	Macacus,			
Collared Mangabey,	Æthiops,	Cercocebus,	CERCOPITHEDÆ, MONKEYS.		
Uncollared,	Fuliginosus,	——			
Rolowai,	Diana,	Cercopithecus,			
Malbrouk, Vaulting,	Petaurista,	——			
Negro Monkey,	Maurus,	Semnopithecus,			
Couxio, or Sakis,	Satana,	Pithecia,	GEOPITHEDÆ, SAGOINS.		
Sakis, Handdrinker,	Chiropotes,	——			
Douroucouli,	Trivirgatus,	Aotus,			
Saimiri,	Sciurus,	Callithrix,			
Royal or Red Howler,	Seniculus,	Stentor,	STENTORIDÆ, HOWLERS.		MANUPEDS.
Black,	Niger,	——			
Preacher,	Belzebub,	Mycetes,			
Grison,	Canus,	Lagothrix,			
Chamek,	Pentadactylus,	Ateles,			
Four-fingered ditto,	Paniscus,	——			
Weeper,	Apella,	Cebus,			
Horned ditto,	Fatuellus,	——			
Ouavapavi,	Albifrons,	——			
Sai,	Capucinus,	——			
Titi,	Iacchus,	Ouistitis,	TITIDÆ, TITIS.	ARCTOPITH.	
Mico,	Argentata,	——			
Marikina,	Rosalia,	Midas,			
Red-pawed,	Rufimanus,	——			
Short-tailed Indris,	Brevicaudatus,	Indris,	INDRIDÆ.	ODONTIPITH.	
Long-tailed,	Longicaudatus,	——			
Ring-tailed Lemur,	Catta,	Lemur,	LEMURIDÆ, LEMURS.		
Slender,	Gracilis,	Loris,			
Slow,	Tardigradus,	Nycticebus,			
Madagascar,	Madagascarensis,	Galago,			
Thick-tailed,	Crassicaudatus,	——			
Spectre,	Spectrum,	Tarsius,	TARSIDÆ.		
Brown-pawed,	Fuscimanus,	——			

On the Organic Remains of the Diluvium in Norfolk. Communicated by C. B. Rose, Esq.

Having, in a former communication, given a description of the diluvial covering of the county, with a list of the materials of which it is composed, I shall, in this division, present to the readers of this Journal an account of the organic remains collected therefrom.

As I possess many fossils from this deposit, at present unfigured by authors, I purpose, in the list of the *testaceæ*, confining myself almost entirely to those specimens which are identified with individuals figured in that splendid work, "The Mineral Conchology of Great Britain," by Messrs. Sowerby, and merely notice some species that remain undescribed by that indefatigable and meritorious family.

The organic remains of the diluvium admit of a division into those of animals inhumated at the period of the great catastrophe, and since mineralized; and those of animals enveloped at various periods, during the formation of the regular strata, and consequently anterior to that grand epoch; the former may be denominated *diluvian* remains, the latter *ante-diluvian.*

DILUVIAN REMAINS.

These consist of teeth, tusks, horns, vertebræ, and various other bones of the mastodon, elephant, hippopotamus, gigantic elk, and the enormous horned bison, the horse, the ox, and two or more species of deer; they occur in great abundance on the eastern coast, exposed by the action of the tidal waters upon the diluvium, and by the agency of springs; immense masses of the cliffs are thus detached from the main land, and left to crumble away upon the beach. I have not had an opportunity of examining a complete series of these interesting relics, therefore cannot enter into further details respecting them. The teeth and vertebræ of some of these animals are also found in the interior of the county; at Whitlingham, near Norwich, a tooth of the mastodon, figured in Smith's "Strata Identified;" it is deposited in the British Museum; a tooth of the Asiatic elephant, with some vertebræ, were discovered a few years since at Narford, near Swaffham; and several bones, supposed, from their coarse texture to belong to a species of whale, have, at various times, been met with in a gravel-pit at Roydon, near Diss.

ANTEDILUVIAN REMAINS.

Pisces.

1. The anterior portion of a fish, with part of its head. From the elongated rhomboidal form of the scales, I presume it to be a specimen of the *Dapedium politum,* a species of a new fossil genus formed by Dr. Leach, for the reception of the fossil fish described and figured by Mr. De la Bèche, in Geol. Trans. vol. i., part first, N. S. page 45, plate 6, fig. 1.

2. A fragment of the armed fin-bone of a species of *balistes.* It is a tuberculated variety; a similar one is noticed by M. De la Bèche, in Geol. Trans. vol. i., N. S. page 43, as occuring in the lias at Lyme.

3. Small vertebræ of a species of *squalus.*

4. Part of the spinal column of a fish with the ribs attached. It consists of nine vertebræ, enveloped in the grey centre of a black flint: it is too imperfect to determine of what fish it is the remain.

5. A small lanceolate tooth of a species of *squalus,* imbedded in a flint.

6. Vertebræ of a species of esox; originally deposited in the crag, or upper marine formation.

Sauri.

1. Caudal vertebra of a *crocodile,* resembling that of Hornfleur.

2. Tooth of a species of *crocodile.*

3. Tooth of an *ichthyosaurus,* agreeing with those of *I. communis.* It forms one of the pebbles of a coarse sandstone breccia.

4. Tooth of a species of *ichthyosaurus;* imbedded in the bituminous shale of the kimmeridge or Oxford clay. This tooth does not resemble those of either of the three species described by the Rev. W. D. Conybeare in the Geol. Transactions.

5. Vertebræ of the *ichthyosaurus,* belonging to the anterior

cervical; anterior, middle, and posterior dorsal; lumbar; and caudal portions of the spine.

6. *Plesiosaurus.* A cervical vertebra, resembling that figured in Geol. Trans. vol. v. part 2, plate XLI. fig. 3.

7. Vertebræ from the middle dorsal, lumbar, and caudal portions of the spine of the *plesiosaurus.*

8. Bones apparently belonging to the paddles of the *plesiosaurus.*

Testaceæ.

Ammonites *peramplus, biplex, decipiens, rotundus, mutabilis, sublævis, binus, excavatus, Birchii, Taylori, dentatus, Strangwaysi, annulatus, serratus, rotiformis,* and some others not yet figured.

Ampullaria, a cast in calcareous sandstone, associated with a Pectunculus, Venus, and Avicula.

Arca, two or three species not yet figured.

Astarte *lineata* and *planata.*

Avicula *costata, inequivalvis,* and *echinata.*

Belemnite. The remains of this fossil are found in all parts of the county: they appear to have been originally derived from the cornbrash, marly sandstone, green sandstone, and chalk strata. I possess one, showing the septa and siphunculus; and another, with some of its iridiscent internal pearly coat attached to it.

Cardium. Mineral Conchology, tab. XIV. *middle figure.*

Cirrus, of an *elliptical* form, not produced by fracture.

Comilaria, a *trigonal* species, in a boulder of calcareous sandstone, associated with trigonia *clavellata.*

Cucullæa (arca *subacuta* of Min. Conch., tab. XLIV. *upper figure*).

Dentalium *incrassatum,* in a septarium, with part of an ammonite.

Gryphæa *incurva, obliquata, dilatata,* and *bullata.*

Inoceramus, several species in chalk boulders, and casts in flint. The latter are very common in the light lands of the chalk district; and they frequently exhibit upon them casts of the workings of animalcular parasites in this shell, described by Parkinson, and also by the Rev. W. Conybeare, in Geol. Trans., vol. ii.

Lima, an elegant little species, not yet figured.

Lingula *ovalis*, with an ammonite and tellina.

Lutraria *ambigua*, in a boulder with

Gervillia *aviculoides*.

Modiola *Hillani*, *cuneata*, *aspera*, and some others, resembling *parallela* and *elegans*.

Mya *literata* or *V-scripta*, and two or more species undetermined.

Nautilus, a cast in flint of an oblique species.

Nucula *trigonia*, *ovum*, and claviformis; the latter imbedded in a septarium, with a rostellaria possessing *two* processes on its outer lip.

Ostrea *deltoidea*, *carinata*, and Marshii. The numerous species and varieties in this genus defy designation. I have, therefore, only noticed those, in my collection, that I can satisfactorily identify with Mr. Sowerby's figures.

Patella *latissima*. Mr. G. B. Sowerby, in his Work on the Genera of Shells, says, "The patella *latissima* and *lævis* of mineral conchology may possibly be the upper valves of orbiculæ."

Pecten *corneus*, *orbicularis*, *arcuatus*, *lens*, *fibrosus*, *nitidus*, *lamellosus*, *cinctus*, *barbatus*, *vimineus*, and *vagans*.

Perna *maxillata*. Sowerby's *Genera of Shells*, *Perna*, *plate* II., *fig*. 1. In a boulder, associated with pecten *lens*, ammonites *excavatus*, and astarte *planata*.

Pinna *tetragona* and *affinis*.

Plagiostoma *gigantea*, *spinosa*, *Hoperi*, and *rigida*.

Rostellaria, scalaria, serpula, and tellina, the species of which have not yet been determined.

Terebratula *crumena*, *tetraedra*, *subrotunda*, *subundata*, *semiglobosa*, *octoplicata*, *plicatilis*, *obliqua*, *lata*, *ovoides*; and there are found, in the diluvium, several species not yet figured by Sowerby. One (in my collection) is the young of a spinous terebratula, figured, by the Rev. Joseph Townsend, in his *Geological Researches*, *plate* XIV. *figs*. 8 and 9.

Trigonia *clavellata*, *alæformis*, *costata*, and a very depressed variety of the latter.

Trochus *punctatus*, and casts of trochi, are found in calcareous sandstone boulders; but the small portion of shell attached is not sufficient to identify the species.

Turbo *ornatus*, and *muricatus* imbedded in a boulder with Turritella *muricata*. A pyritous cast of a turritella in calcareous sandstone, an imperfect shell in an oolitic nodule, and casts of others in sandstone, are all too imperfect to receive a specific name.

Venus. *Casts* of two or three species occur.

Unio *Listeri*, *hybridus*, *crassissimus*, *concinnus*, and a very laterally elongated species, not yet figured.

Echinidæ.

All the fossilized remains of this order, that I have met with in this county, were *originally* derived from the chalk strata. I have not seen a single specimen in the debris of any other stratum. They almost invariably occur as casts in flint—the shell destroyed. There are instances of casts *entirely* surrounded by the flint; fracture liberates the *cast*, and exhibits the *impression* of the exterior of the shell: occasionally the shell is preserved enveloped in flint; more rarely, the mineralized shells, filled with chalk or flint, are found in chalk boulders. I possess the following:—

Cidaris *mammillata*, with several spines in contact, but not attached; in a chalk boulder. Cidaris *papillata*, and *corollaris*. The long cylindrical spines of cidaris *mammillata*, and the fusiform or cucumerine and clavated spines of cidaris *papillata*, are not unfrequently met with in chalk boulders, or their impressions on flint.

Echinocorys scutatus, casts in flint of both gibbous and depressed species or varieties. The same varieties of Conulus *albogalerus;* and echinodiscus, *scutella of Lamark.*

Spatangus *cor marinum*, *ovum marinum* (brissus), and casts of a species of this brissus, occur, with its dorsum singularly elevated at the commencement of the dorsal groove.

Stelleridæ.

Asterias *semilunatus* (pentagonaster semilunatus). Parkinson's Organic Remains, &c., vol. iii., tab. I. fig. 1. I possess a nearly complete impression on flint, of a perfect remain of this asterias.

Asterias *regularis (pentagonaster regularis of Linck).* Vide Parkinson's Org. Rem. vol. iii, tab. I. fig. 3. My specimen is an impression on flint; two *margins* only are preserved.

Crinoidea.

Impressions of the articulating surface of the vertebræ of pentacrinites *basaltiformis* of Miller, are frequently met with on flints, from the light lands and gravel beds.

A nodule of decomposing mountain limestone, almost entirely composed of fragments of vertebral columns of poteriocrinites *crassus* of Miller, was found in Roydon pit, near Diss.

ZOOPHITES.

Ventriculites of Mantel; Mantellia of Parkinson. Flints derived from the disrupted chalk strata, exhibit this zoophite in every degree of contraction and expansion; and also its tubular structure, and membranous or spongoid laminæ. They are usually found pyriform, or having the form of a mushroom. The species most frequently met with is *radiatus.* Mantel's Fossils of the South Downs, tab. X.

Syphonia of Parkinson. Oviform spongoid remains in flint, having longitudinal tubes filled with the matrix, are frequently found on the light lands.

Choanites *Konigi.* Mantel's Fossils, &c., tab. XVI. fig. 19. Is occasionally found in the beds of gravel, and light lands.

Spongoid remains are very numerous, preserved in a matrix of flint. They occur in a variety of forms, as spheroidal, ovoid cylindrical, branched, palmated, and reniform, with and without pedicles; among them are found spongus *labyrinthicus* and *Townsendi.* Mantel's Fossils, &c., tab. XV. figs. 7 and 9.

Madreporite allied to Madrepora *annularis,* of Solander and Ellis.

Madreporite allied to Madrepora *galaxea,* Sol. and El.

Madreporite allied to Madrepora *ramea,* Sol. and El. (the caryophillia ramea of Lamark).

Madreporite allied to Madrepora *siderea,* Sol. and El.

Madrepora centralis, Mantel's Fossils, &c., tab. XVI. figs. 2 and 4, enveloped in a black flint.

A compound porpital madreporite, resembling fig. 4, plate VII. in vol. ii. of Parkinson's Organic Remains.

The vegetable remains consist of fragments of wood, imbedded in septaria, calcareous sandstone, carstone, oolitic limestone, and black flint; it occurs bored by teredines, bituminized, silicified, and impregnated with oxide of iron, and iron pyrites.

A few remarks only remain to be offered in conclusion. On referring to the *diluvian* remains enumerated, it cannot but attract notice, that they should consist of the bones of *land* animals, except in a very few instances of those of the whale. This singular fact naturally leads to an inquiry into the source of the water that inundated the earth at the Noachian deluge, and raises a question as to its *marine* origin. If the deluge were produced by the sea leaving its bed (our present dry land), to be deposited in another basin; or inundated the then inhabited surface, for a certain time, and receded; in either case, surely, some of its testaceous inhabitants would have been deposited with the mud. I am not aware that this subject has been before placed in a similar point of view. I have not time to search early writers and theorists; indeed, I do not intend to argue the point, nor, perhaps, am I competent to the task; I merely state the fact and raise the question. I believe it not to have been a marine irruption. Holy Writ * bears me out in my assertion; the absence of the *diluvian* remains of marine testacea is powerfully corroborative, and (in my opinion) alone warrants the doubt above promulgated.

* Genesis, chap. vii. verses 4, 11, and 12.

In D'Oyly's and Mant's Bible, the following notes are given upon verses 11 and 12. Verse 11—*And the windows of Heaven were opened*—" By this must be understood the causing of the waters, which were suspended in the clouds, to fall upon the earth, not in ordinary showers, but in floods, or, as the Seventy translate it, in *cataracts;* of which travellers may have the truest notion, who have seen those prodigious falls of water, so frequent in the Indies, where the clouds many times do not break into drops, but fall with a terrible violence in a torrent."—*Bishop Patrick*——*Stackhouse.*

Verse 12.—*And the rain was upon the earth forty days, &c.*—" It continued raining so long without any intermission."—*Bishop Patrick.*

Reply to Mr. James Ivory's Answer in No. XXIII. of the Philosophical Magazine and Annals of Philosophy.

In the *Phil. Mag.* and *Annals* for November last, Mr. Ivory has brought forward what he calls an "Answer" to my article, in No. VII. of the Journal of Science, on his doctrines about sound and heat. A prominent, and perhaps unavoidable feature of Mr. Ivory's answer, which cannot fail to strike the reader's attention, is, the total absence of everything bearing immediately on the points in dispute. The whole affair is got conveniently over, by a series of excuses more or less plausible; while every one of my criticisms remains *unanswered* in full force.

Mr. Ivory's first insinuation is, that my strictures are little else than taken from Professor Leslie's article *Acoustics*. He takes good care to offer no evidence of this. I have only to regret, that, so far from its having been the fact, I had entirely forgotten that that valuable article contained any objection to the theory of sound. I now see, that had I looked into it in time, I might have materially improved my paper. I presume, however, that by endeavouring to sift the analytical investigation to the bottom, I have distinctly pointed out several striking inconsistencies, impossibilities, and unwarrantable assumptions, not before noticed by any one; and therefore, "the subject is not left," as Mr. Ivory could wish, "just where I found it."

Mr. Ivory next remarks on my article, that "whatever purpose such discussions may serve, one is at a loss to find out how they can benefit science." A very natural remark to be sure, while the tide of discussion ran against Mr. Ivory. He might just as well say, he was at a loss to see how the destruction of weeds, and other useless or noxious herbs, can benefit the produce of a garden. The removal of spurious productions, especially those wearing the garb of mathematical investigation, being as necessary and beneficial to the progress of science, as the destruction of weeds in the other case. I would rather ask—what benefit can result to science, from an "Answer," which leaves *unanswered* everything it professed to answer? In particular, it "leaves the analytical theory of sound," which I had impugned, "to stand on its own merits," after it had not a foot left to stand upon. This is bringing succour to the distressed, with a witness; and evinces the heroism of the champion who had purposely come forward to answer the charges in my paper.

Mr. Ivory's pretended ignorance of what I had previously written on these subjects, must appear in no small degree paradoxical to those who examine his papers in the *Phil. Mag.* for February, March, and April, 1827. These papers bear in such a way upon the chief points discussed in my previous writings, as can neither be ascribed to blind chance, nor explained by the doctrine of probabilities. Of this, I shall now give an instance or two. In the articles just cited, Mr. Ivory shows an anxiety he had never till then manifested, for impressing his readers with the belief, that the common mode of graduating the air-thermometer forms a true scale of temperature. Now, "how could it benefit science" to press such a doctrine, without at the same time adducing so much as a new shadow of proof in its favour, if no person had been recently calling it in question? For, so far as I know, no one had done so, for a long while, till I took up the subject, a few months before Mr. Ivory appeared with such zeal in its defence. Out of several, I shall add another instance. Neither Mr. Ivory, nor any one else, had questioned and rectified a certain integration in book xii. of the *Mecanique Céleste,* or in Mr. Poisson's Memoirs on the same subject, till I did so; and then, Mr. Ivory was ready to question them too—with this very notable difference, however, in attempting to correct the error, that my view of the matter is quite consistent, whereas Mr. Ivory's is full of contradiction, and directly opposed to some of the most familiar facts: he having precipitated himself into far greater errors than those he was pretending to correct. This difference only strengthens the evidence, because Mr. Ivory would not be beholden to me for any assistance in correcting an error, possessing, as he did, such ample resources of his own.

The greater part of the answer consists of extracts, which Mr. Ivory brings from his own papers. How the repetition or copying of these, and that too in the same Journal, "could benefit science, one is at a loss to find out;" more especially after it had been clearly shown, that the doctrines which they inculcate, are full of extravagant inconsistency, and run counter to universal experience. But having produced these extracts, Mr. Ivory consoles himself that all my objections to his doctrines are derived from *extreme* cases. That this pitiful excuse is groundless, may be seen from some of the examples I formerly adduced; where the *errors* alone are extreme, and scorn all bounds, while the cases are taken within moderate limits. Let us take, for instance, Mr. Ivory's own example, to which, of course, he cannot object, *Phil. Mag.* for February, 1827, page 94, where the density is to be doubted, and the initial temperature of the air is 32° F. By putting, therefore, $\rho'=1$, $\rho=2$ and $\tau=32°$ in the general formula—

$$\frac{3}{8}\left(448° + \tau\right) \times \frac{\rho'-\rho}{\rho}*$$

it becomes equal to 90° for the rise of temperature due to doubling the density; making the resulting temperature 122°. Now, if Mr. Ivory's rule were correct, the same air, by having its acquired density halved, should just have its temperature lowered 90°, or from 122° back again to 32°; being in every respect restored to its original condition, for its quantity of heat is supposed to have all the while undergone no change. But if, in the same formula, we put $\rho'=2$, $\rho=1$, and $\tau=122°$, the depression of temperature, in place of 90°, is no less than 213°.75; furnishing the *extreme* error of 123°.75, in Mr. Ivory's own *mean* case.

But, lest all these instances may not satisfy Mr. Ivory, it may be worth while to prove the *universal* inconsistency of his rule, be the case what it may. For this purpose, I resume the general expression

$$\frac{3}{8}\left(448 + \tau\right) \times \frac{\rho-\rho'}{\rho}.$$

First, let the density suffer a sudden increase, or let ρ exceed ρ', which makes the last expression a rise of temperature. Next suppose the same mass of air, which has acquired this rise of

* This, it will be recollected, is the general and analytical expression for Mr. Ivory's new law. In it, ρ' and τ denote the density and Fahrenheit temperature of the air, at the beginning of any sudden change of its volume, and ρ the density at the end; the formula itself being the corresponding change of temperature.

temperature, to have its volume instantly enlarged or restored to what it was at first. Then it is manifest that, if the quantity of heat in the air have all the while undergone no change, such air will just be restored to the first temperature, τ. An expression for the diminution of temperature caused by this enlargement of volume, may obviously be obtained with the same values of the symbols, by making ρ' and ρ to change places in the above general formula, and likewise using, in it, the augmented temperature $\tau + \frac{3}{8}\left(448 + \tau\right) \times \frac{\rho - \rho'}{\rho}$ in place of τ. By this means we obtain, for the fall of temperature due to restoring the original bulk,

$$\frac{3}{8}\left[448 + \tau + \frac{3}{8}\left(448 + \tau\right) \times \frac{\rho - \rho'}{\rho}\right] \times \frac{\rho' - \rho}{\rho'}$$

which, with its sign changed, must just equal

$$\frac{3}{8}\left(448 + \tau\right) \times \frac{\rho - \rho'}{\rho}$$

the rise of temperature due to the previous condensation. From the equation so formed, we obtain, by reduction, $\rho = \rho'$; that is, there has been no change of density, which is absurd, because directly contrary to the main supposition with which we set out.

Hence, because ρ' and ρ are any densities, and τ any temperature, the *universal* inconsistency of the rule is manifest. It cannot, therefore, be the law of nature, even within a limited range. Indeed, human genius could scarcely have devised a rule which would set reason and experience more completely at defiance. Thus, in place of compression causing an *indefinite* rise of temperature, the rise is confined under a ridiculously-contracted range; while, instead of an impassable limit of cold (which is inseparable from the scale Mr. Ivory defends), we have one which is bottomless and unfathomable!

Whether, then, shall we adopt a rule which is full of extravagant inconsistency, and palpably at variance with the facts best known, merely to accommodate Mr. Ivory, because he has run himself into a labyrinth from which he is unable and even loth to come out, or shall we venture on the more rational alternative of adopting the formula

$$\left(448^\circ + \tau\right)\left[\left(\frac{\rho}{\rho'}\right)^{\frac{1}{8}} - 1\right]$$

where no inconsistency can be detected, and which, for aught that yet appears to the contrary, may hold good throughout the whole range within which air maintains the same form and constitution? This latter formula is equally free from the glaring case of tinder kindling under the boiling point, from a limited rise of temperature, and from an indefinite descent below the impassable limit of 448°.

It is curious that Mr. Ivory's answer should conclude with his old complaint against M. Poisson's integrations. For if we will persist in assuming inconsistent hypotheses, there is no need for wondering at strange and incoherent results. The whole mystery arises from their *assuming* the common scale of the air-thermometer to be a true scale of temperature, which is utterly inconsistent with what they also lay down—that the specific heat of air under a constant pressure has an *invariable* ratio to its specific heat under a constant volume. Such mathematicians ought to know (for it is upwards of two years since I laid it before the public), that, if the invariable ratio just mentioned be made a fundamental principle, the necessary and unavoidable consequence is, *that, when the variations of the quantity of heat in air are uniform, those of its volume, under a constant pressure, form a geometrical progression.* I have more recently touched on this point, in an article which was written and sent off before seeing Mr. Ivory's answer. From it, the reader will be enabled to estimate what confidence is to be put in those " easy deductions from the usual theory of the thermometer," on which Mr. Ivory is incessantly harping*.

I wish it to be distinctly understood, that, in discussing this subject, I do not endeavour so much to establish a particular theory, as to point out some of the consequences which are unavoidable, when we proceed on certain data; and I only insist on these consequences within the range throughout which the data are supposed to hold good.

HENRY MEIKLE.

* This article will appear in our next Number.—ED.

Remarks on the Stowage and Sailing of Ships and Vessels. *By* Commander John Pearse, R. N.

The following remarks have sprung from ideas formed during, and after a practical experience, as a seaman, of upwards of twenty years' active sea service. I commit them to paper as a seaman; and solely with the intention of endeavouring to show, by a plain statement, what appear to me as errors in the system of stowing and sailing our ships. Perhaps I may venture to assert that, generally speaking, there is no regular system followed, or that the subject is not sufficiently considered on mathematical principles.

Chapman, in the preface to his *Treatise on Ship-Building*, observes that " In the construction of ships, people usually make attempts at different times to improve the form, each person according to his own experience; thus after the construction of one ship, which has been tried and found to possess such or such a bad quality, it seems possible to remedy this defect in another. But it often (not to say generally) happens, that the new ship possesses some fault equally as great, and frequently even that the former defect, instead of being removed, is increased. And we are unable to determine whether this fault proceeds from the form of the ship, or from other unknown circumstances.

" It thus appears, that the construction of a ship with more or less good qualities, is a matter of chance and not of previous design. And it hence follows, that as long as we are without a good theory on ship building, and have nothing to trust to beyond bare experiments and trials, this art cannot be expected to acquire any greater perfection than it possesses at present.

" At the same time the construction of ships and their equipment are attended with too great expense, not to endeavour beforehand to ensure their good qualities and their suitableness for what they are intended for. The theory then which elucidates the causes of their different qualities, which determines whether the defects of a ship proceed from its form, or from other causes, is truly important; but as the theory is unlimited, practice must determine its limits. We may consequently further conclude, that the art of ship-building can never be carried to the last degree of perfection, nor all possible good qualities be given to ships, before we at the same time possess in the most perfect degree possible, a knowledge both of the theory and practice.

"Lastly, it is evident from all that has been said, that a ship of the best form will not show its good qualities, except it is at the same time well rigged, well stowed, and well worked by those who command it."

I have quoted the authority of Chapman to show that much depends on the stowage and management of a ship.

Many experiments have of late years been tried, many improvements made, and much, no doubt, yet remains to be done. But although naval architects appear still to differ respecting the formation of the body, it appears to me there is less room for improvement in the structure, than in the system of stowage and sailing, which appears to require considerable alteration, before it can reach perfection.

It has been very generally observed how well our own ships after an action, and those captured from the enemy, have appeared to sail under jurymasts, or, comparatively speaking, they have appeared to sail better in proportion under jurymasts, than with their proper ones,—a strong proof in support of which occurred a few years since:—

The Essex, late American frigate, sailed from Plymouth, under very low jurymasts, attended by the Dwarf cutter, of two hundred and ten tons, for Dublin, there to remain as a depôt. At the entrance of the Bristol Channel, they fell in with a large smuggling lugger, to which they gave chace off the wind; the Essex, after a considerable run, capturing the lugger, and leaving the Dwarf out of sight, or nearly so,—a proof she must have sailed as fast as if properly masted.

I shall mention one more circumstance. It will be recollected, the Vanguard, Lord Nelson's flag-ship, lost her foremast off Toulon, previous to his fleet joining him, and his pursuit of the French fleet to Egypt. His lordship proceeded in the Vanguard, under a jury foremast, to Egypt, back to Sicily, and to Egypt again, where he found the enemy; and I do not recollect the Vanguard causing the least delay or impediment to the fleet. Had his lordship considered it likely, or that the Vanguard would not have been able to keep her station in chace, in the event of falling in with the enemy at sea, he would, no doubt, have shifted his flag to some other ship; and the world knows well his lordship was too anxious to be first in action to risk any thing that might disappoint him.

From these and similar circumstances, an opinion that many of our ships are overmasted may have partly originated.

Ships are supposed to be constructed agreeable to plans mathematically arranged, varying in their formation according to the ideas of different architects, and intended to be immersed to a certain depth in the water, when complete and ready for sea. To which floatation, it appears to me, they should be brought to and kept as near as possible, except there should be any material deviation in a ship from the plan about the load water-line; in that case it is better, and as a general thing it is as well, to observe the formation of the bottom about the load water-line, and be guided accordingly.

If a ship is built strictly agreeable to the plan, and is immersed deeper than the builder intends, his views are frustrated, and the object of constructing a ship adapted for fast sailing lost sight of.

Professor Inman, in his general remarks on the construction of ships of war, observes,—

"It may be observed, generally, that it is advantageous to give the projected ship the requisite stability with as little ballast as possible, by which means a constructor is enabled to reduce the displacement or magnitude of the body under water, a circumstance very favourable to a ship in sailing and working. With a similar view every weight put on board, and reckoned in getting the displacement, should be kept as low as possible. No useless baggage or weight of any kind should be put on board on any account whatever."

I have quoted the above authority as bearing a little on the point, and in corroboration of what I allege.

I must also observe that, by too deep an immersion, the fullest part of the body would be carried below the surface; consequently, the displacement would be considerably increased, and also the resistance of the water on the lower part of the bottom. It therefore appears to me, when a ship, immersed to a certain depth, has not sufficient stability under sail, that the masts should be reduced, in preference to immersing it deeper in the water.

That it is most natural a ship should have masts in proportion to the body below the surface, when immersed to what is considered a proper depth, must appear very evident; and also that, with such masts, she would sail equally well, as with a deeper immersion and proportional addition to the masts. But it must be expected the ship would be more uneasy under the increased masts in bad weather; and as the masts and rigging may be considered as back sail on a wind, the increased masts and rigging must also add to the impediment of her sailing. Consequently, the advantage must evidently appear in favour of the lesser masts and immersion.

The case of the Essex must appear as strong proof in favour of the principle in sailing off the wind; being, no doubt, ballasted in proportion to her masts. At the same time it must be allowed no just conclusion could have been derived from a trial of her on a wind; as on that point, although the sail might be considered in fair proportion to the body immersed, yet, from the body floating high above the surface, the resistance of the wind on it would have been much greater, than with a deeper immersion and masts in proportion. Neither would the immersion have been sufficient to resist the lateral impulse.

Chapman, in his *Treatise on Ship Building*, observes that "to determine the surface proper to be given to the sails from the knowledge which we have of the effect of the wind on planes or sails, with different velocities in different directions, it would be necessary to enter upon long calculations of great difficulty, and yet of little importance. We may compare plans of ships and of their rigging which are tried and known, and nothing will be required further than to be guided by those ships which have the best proportion of canvass, with respect to the centre of effort of the wind on the sails and the stability."

And Professor Inman, in his general remarks on the construction of ships of war, observes that, "After all the pains the constructor may take, from the imperfection of the theory of resistances, or from some other unknown causes, it is possible that a ship, on going to sea, will not be found to have the point of sail exactly adjusted to the mean resistance. In this case nothing can be done except by altering the masting, for effecting which, if possible, every practical facility should in the first place be left in the building, or by bringing the ship more by the head or stern; thus adjusting the seat of the ship in the water to the masting, as it is."

By which it appears there is no rule which can be strictly depended on, to calculate a correct proportion for the masts or their positions.

Many of our ships of the same class and computed tonnage differ in the formation of their bottoms; it also frequently happens that one ship shews greater stability under sail than another; and that, with the same quantity of provisions and stores, one is deeper immersed in the water than another. The only supposition to be drawn from it is, that they have the same proportion of masts and ballast.

If the supposition be admitted, it appears to me very erroneous, and that ships, though of the same computed tonnage, differing in the formation of their bottoms, ought not, as an established rule, to have the same proportion of masts and ballast.

If one has less displacement than another, she will have less capacity in the hold; consequently, will not stow so much, or require so great a weight to immerse her so deep in the water. It therefore appears most natural she should have a less proportion of masts.

As well as what Chapman and Professor Inman observes, respecting the proportions for and positions of the masts, it may be supposed, from the many alterations which have been made in the masts of our experimental ships, there is no rule to be depended on. And as Chapman observes, " we may compare plans of ships and of their rigging, &c.," which I have quoted before.

That such would be the most correct method to ascertain a true proportion there can be little doubt; but it appears to me no just conclusion can be made as to the proper proportion of masts, till the system of stowage has undergone considerable alterations.

I have ventured to assert that, generally speaking, there is no regular system followed in stowing our ships. In proof of which, I think it is only necessary to recollect the many alterations in the stowage of our experimental ships, and the various alterations in the trim of them at sea, and which appear to have made considerable alterations in their sailing qualities.

It is a very common practice to trim ships by making each man carry a shot or two aft; removing the foremost guns aft, and which is frequently done to ease a ship in a head-sea. But it should be recollected when a ship is in chace, the foremost guns are those generally first wanted, and require to be in their places, and the men frequently required in their stations. It must, therefore, appear evident, that but few of our ships are properly stowed; and that it is most essential an attempt should be made to improve the system, to obviate the necessity of such alterations at sea, by a better arrangement of the ballast, stores, and provisions.

I am of opinion that perfection in the stowage should be made a primary object; when that is attained, there will be a sure foundation to work from, in finding a true proportion for the masts. It appears to me perfectly easy, and I hope to see it reduced to a regular and perfect system.

I am also of opinion that, after viewing the formation of a vessel's bottom, very little consideration is required to determine how she ought to be stowed. Naval architects recommend the weight to be kept as near the centre as possible; to me the formation of the bottom points it out, and that the extremities should be kept as light as possible, to ease the pitching and sending motion in a head sea. That all has not been done to effect that object, and which appears to me easily may, I shall endeavour to explain.

Much cannot be said, and very little need be, as to the most proper place for stowing the ballast alone or its arrangement: it is the distribution of the other weights which can most contribute to it; and the great weights, at present in the fore extremity, stand opposed to it.

A fact well worthy of notice may be introduced here :—

It is universally known how well the Kentish and Sussex smuggling vessels have always sailed. The fact is, there is very little but the cargo to stow, and that is placed in the centre of the vessel. A small quantity of provisions, a few sails, and their anchors and cables, is the only weight beside, and which is also kept clear of the extremities. And this method appears to be derived more from a sort of established knowledge or practice, which descends from father to son, than an acquired system, as perhaps there are no men possessed of less scientific knowledge.

I shall therefore only observe, with respect to the ballast, that it should be winged in proportion to the supposed or ascertained inclination of the ship to rolling, and on no account to extend it to the finer parts; as without it, there is sufficient longitudinal space in a large ship to trim the ballast, so as to bring her to a proper draught of water, provided the stores and provisions are also properly arranged, and those of the least weight selected for stowing nearest the extremities. To contribute towards which, I propose removing the cat-heads, and stowing the anchors further aft; which may be effected, in my opinion, with little or no difficulty.

Viewing it on lever principle, the weight is now at the very extreme end, and the lever in the position " horizontal" of its greatest power.

Chapman observes, " the reason of the pitching and sending motion is easily seen. When a wave has passed the fore-part of the ship, and is got near the middle, there is a great void space under the bows, where the ship is not supported. It precipitates itself, therefore, with a certain momentum, which is the product of the weights in the fore-part, multiplied by their distance from the point where the ship is sufficiently supported.

" This kind of motion is greater in ships which are very full near the load-water-line fore and aft, and very lean below; but if the weights in the fore-part are carried nearer the middle, the momentum with which the ship plunges itself in this part will be less; and not only this motion becomes less quick, but, moreover, the following waves which meet the fore-part of the ship have less difficulty in raising it again. The same observation may be made on the aft-part."

The preceding observations ought to be sufficient to convince all but the most prejudiced, that I am justified in what I propose; and naval architects must concur with me in its utility, although they have never put it in practice. I shall therefore endeavour to show that it is practicable.

By removing the cat-heads aft, there would be a few fathoms more of cat-fall to run up, but which, considering the very short time taken to cat the anchor, does not appear to me can be considered an objection: and the anchors being brought abaft the round of the bow, would fish clearer of the side than at present, and stow neater, and similar to the spare and sheet anchors.

From observations which I have made on several frigates, it appears to me, if the cat-heads were removed to the fore part of the channels, which, in the several ships, would be from eight to ten further aft than where now placed, that both stock and fluke would stow perfectly clear of the ports. And when it is considered that a forty-six gun frigate's two bower anchors weigh four tons and a half, which, together with the stocks, cat-heads, knees, fastenings, &c., cannot be estimated at less than six or seven tons, there can be no doubt the removal aft of so much weight would give considerable relief to a ship in a head sea; contribute to a better arrangement in stowing the ship, and in many cases obviate the necessity of stowing ballast too near the after extremity. It would also be removing fifty per cent more weight than the two foremost guns.

There are no vessels which require easing in a sea more than our brigs. The weight of an eighteen gun brig's two bower anchor, stocks, cat-heads, knees, fastenings, &c. cannot be estimated at much less than four tons; and which, by a little alteration in the arrangement of the chain-plates, to admit the fluke of the anchor in between two shrouds, I am of opinion may be removed aft seven or eight feet, and the anchors stow clear of the ports: as the spare anchor stows clear which is placed abaft the channels, I can see no reason why the bowers should not.

In cutters I have had the cat-heads so far aft, that the crown of the anchor has stowed close to the fore shroud; they have stowed perfectly easy, and I observed that the copper about the bows has been less injured, than in vessels which had their cat-heads further forward.

It is in our smaller and sharpest vessels its effect would be greatest, but there can be no doubt of its advantage in all ships; and should the experiment be tried, I have no doubt it will be extended to all.

In many ships, and all our smaller vessels, the coal-hole is also very far forward; and as the coals are a considerable weight, and not speedily consumed, I should recommend stowing them further aft, and water where they are now, and to use this water first.

There is also a very great weight in the fore extremity of a line-of-battle ship; the gunner's, boatswain's, and carpenter's stores, which it appears to me may be stowed nearer the centre the ship, by the following arrangements.

Where those stores are now stowed, to build a room to receive part of the bread from aft, keeping a clear wing passage round the bows, by which means the bread would be as well preserved as if in the after bread-room. The bread forward can be used first, or it can be taken from forward and aft alternately, as the trim of the ship may appear to require. The remaining space to be divided into store-rooms for marine-clothing, slops, beds, and such light weights as are at present stowed in the after cock-pit.

All the mates, midshipmen, and assistant-surgeons to mess in the gun-room, a much more healthy and airy situation than the cock-pit, and to have a store-room in the cock-pit for the mess-utensils and sea-stores.

By the removal of bread forward the after bread-room may be considerably reduced, and the surgeon's and purser's cabins removed further aft. By such an arrangement there would be sufficient space abaft the cable-tiers for the reception of the gunner's, boatswain's, and carpenter's stores; and which would not be much abaft the centre of the ship.

In frigates also, and smaller vessels, similar arrangements should be made as far as possible; and if those were carried into effect, there would be no necessity for extending the stowage of the ballast to the after extremity, as is too often the case.

In the Cruizer, "eighteen gun-brig," we were obliged to stow ballast in the bread-room—the case I believe in most of the brigs; which would not have been requisite had the anchors and coal-holes been removed further aft. Since which I believe some alteration has been made in the store-rooms; but if weight has been reduced in the fore extremity by that means, it has been increased again by the addition of a heavy forecastle, which also considerably increases the top weight. Poops and quarter boats have followed the fore-castles; and to the little ten gun-brigs, all this additional top weight has been added—even a cutter of one hundred and twenty tons follows the rage for carrying quarter boats.

I must repeat an observation of Professor Inman's, as it is most applicable to the point in question:—

"It may be observed, generally, that it is advantageous to give the projected ship the requisite stability with as little ballast as possible, by which means a constructor is enabled to reduce the displacement or magnitude of the body under water, a circumstance very favourable to a ship in sailing and working. With a similar view every weight put on board, and reckoned in getting the displacement, should be kept as low as possible. No useless baggage or weights of any kind should be put on board on any account whatever."

Every one who can agree with Professor Inman, must condemn the system of adding so much top and overhanging weight.

Fore-castles were first fitted by the captains, were very light, and could not have caused much impediment to the vessels sailing; but the heavy way they are now fitted, many with heavy bulwarks above them, and other additions which have succeeded, must be materially felt.

I may be told the quarter-boats are of a light description; but when every common sailor will admit a jacket hung in the rigging to be an impediment to sailing on a wind, I say it is inconsistent to carry quarter-boats in such small vessels. But allowing them to be of a light description, the weight of the iron davits is to be added, and it should be remembered that when a vessel is launching, although its actual weight is not, yet its power is considerably increased by the action of the vessel.

I was four years and a half first lieutenant of an eighteen gun-brig; they were then in their original state, and no vessels sailed better. I afterwards commanded a large cutter four years, and had many opportunities of sailing with brigs, in their original state. Subsequent to which I commanded a large cutter three years, and under the orders of several of the finest brigs in their present state; and I am convinced they neither sail nor work so well as formerly, and that it is attributable to the alterations which have been made.

I am also of opinion, that by taking away the poops and quarter-boats, building the fore-castles as light as possible, with only a low wash-stroke of three-quarter-inch elm or oak board above it, which would be quite sufficient, and removing the anchor and coal-holes further aft, they would be superior in their sailing qualities and as sea-boats, to what they were in their original state.

Masts are frequently raked, by some to ease a ship in a head sea, by others to improve her sailing.

It does not however occur to me that it can give much relief to a ship: for, supposing it to be the overhanging weight of the mast, acting on lever principle, which contributes to the pitching motion, the mast must be considerably beyond a perpendicular before it can have much effect; and admitting the supposition, it may naturally be supposed that a raking mast will act diametrically, and increase the sending motion aft.

But there appears to me a great objection to raking masts, in ships and square-rigged vessels.

If masts are raked, the yards are not at right angles with them, when a ship is on a wind. The consequence is, the sails are put out of their proper form, and drawn, or I may say forced, towards a diamond shape. The mainsail shows it in the diagonal girt across the sail from the tack to the lee-earing; the leeches of the sails are quite slack, and bag to leeward.

It is allowed the wind acts on the same principle when striking the sails obliquely, as if perpendicular or at right angles—its power decreasing as the obliquity increases. If, therefore, the lee leeches of sails are slack and bag to leeward, the obliquity of the wind must be increased, and its power lessened. It therefore appears much preferable to give a ship relief by removing weight from the fore extremity, than by raking the masts.

I have frequently heard of various alterations having been tried in ships, to improve their sailing, and that raking the masts was the only one which proved successful. This is a circumstance which, it appears to me, may be considered as arising from some of those unknown causes alluded to by Professor Inman in the following observation:—

" After all the pains the constructor may take, from the imperfection of the theory of resistances, or from some other unknown causes, it is possible that a ship, on going to sea, will not be found to have the point of sail exactly adjusted to the mean resistance. In this case, nothing can be done except by altering the masting; for effecting which, if possible, every practical facility should, in the first place, be left in the building, or by bringing the ship more by the head or stern, thus adjusting the seat of the ship in the water to the masting as it is."

The circumstance, however, proves an error; but where it is difficult to determine,—and without being in possession of particulars, it were useless to attempt it. The ships might not have had their proper seats in the water; the point of sail might not be correctly adjusted, or it might arise from some other cause.

It is evident, however, that, seated in the water as they were, they required more after-sail—consequently could not have steered well; and it generally happens when a ship steers badly she does not sail well. The conclusion, therefore, may very naturally be, that raking the masts improved both steerage and sailing qualities.

It however appears to me, when a ship is properly seated in the water and requires more after-sail, that it is preferable to remove the foremast a little further aft than to rake the masts, as it would have the same effect, and give considerable relief to the fore extremity.

There is one part of Professor Inman's observation—"or by bringing the ship more by the head or stern, thus adjusting the seat of the ship in the water to the masting, as it is," which it appears to me can only be meant as a temporary expedient, and till an opportunity offers to alter the masting, as it would be sacrificing a very material point—the proper seat of the ship in the water.

J. P.

Plymouth, Nov. 22, 1828.

On the Elevation of Water by the momentive Force of that Fluid in the Suction Pipe of a Pump. Communicated by R. Addams, Esq.

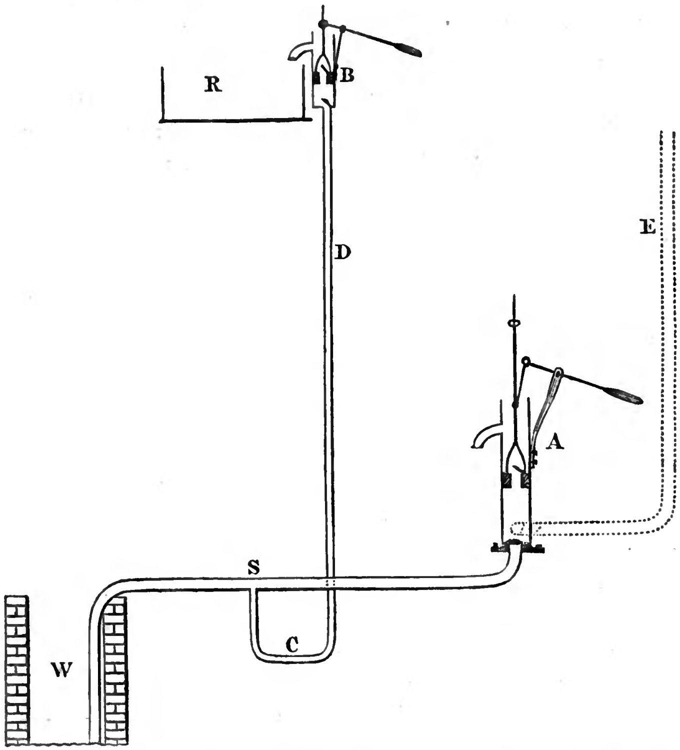
The principle upon which the action of the water-ram (bélier hydraulique) depends, is known to the readers of this Journal*; but I am not aware that any one has, publicly, noticed or observed the same principle producing a similar effect without a fall or head of water. I am, therefore, inclined to suppose the following description of a hydraulic operation, which I have lately witnessed, possesses sufficient novelty to constitute an admissible article in the *Journal of Science and the Arts.*

In the water-ram, the momentum is generated in proportion to the velocity of the water; and the velocity is dependent upon the fall: but it is obvious the same effect would take place from the same momentive force, in whatever manner this force may originate.

Now, when a common pump is worked and drawing water from a well, the air presses the water through the ingress or suction-pipe with a certain velocity: the moving water in that pipe will have a proportionate momentum; and if it be suddenly checked it will exhibit its force as in the water-ram. This I have seen verified in a pump at the house of Mr. Balaam at Clapham. I was requested to examine it, and explain its action.

* *Journal of Science,* Vol. I., O. S.

The accompanying section will serve to abridge the description of the parts.



A is a pump which draws water from the well W, about twelve feet deep. C D is a pipe which branches from the ingress pipe S, and ascends fifteen feet above S to supply (by means of a *second* pump B) the reservoir R. The bend in the pipe at C was made that it may conform to the floor of an underground cellar.

When the pump A is worked, water rises up the pipe C D, passing through the valves of the pump B, and is discharged from thence, in a pulsatory manner, at every descent of the piston A.

The quantity which *thus* flows into R is about a quart in seven minutes. But a contrivance belongs to the pump which can be made to keep its piston-valve shut; then it acts as a forcing pump to impel water up the pipe E, and in this mode of employing it, a larger portion of water ascends into the receptacle R.

It is thus explained. When a current of water is urged along the pipe S, it is *partially* checked when the piston A is depressed, and working as an ordinary pump, but entirely stopped provided the piston-valve be kept closed: in either case the momentive force of the water expands in every direction, and the column of fluid in the rising pipe C D is put in motion, but more considerably in the latter condition, in consequence of the stoppage of the water being more complete.

No doubt the effect of the engine would be increased if the upper pump were removed, and a single valve, contained in an air-chamber, placed near the bottom of C D.

Nov. 27th, 1828.

Remarks on some Remains of Elephants, lately found on the American Shore in Behring's Straits, by John Ranking, Esq.

" Two tusks of the mammoth were brought home by Captain Beechey. they are in fine preservation, and not bent in one direction, but twisted spirally like the horns of some species of cows. The smallest is quite entire, and is nine feet nine inches long; the largest, which wants a small part of the point, must have measured originally twelve feet. Professor Jameson stated to the Wernerian Society, that the mammoth to which the largest belonged, must have been fifteen or sixteen feet high *, and consequently larger than the elephant, which is of the same species. They were found on the west coast of America, near Behring's Straits, at Escholz Bay, latitude 66°, in a bluff or mountain of ice, which has been described by Kotzebue: it is one hundred feet high.

* Neither the height of this animal (which is conjecture only), nor the length of the tusks, can be deemed as marking a difference from modern elephants, which are known of fourteen feet, (Ency. Brit. " Elephas.") Coryate mentions them at Delhi thirteen and a half feet high. Fitch, at Pegu, saw some nine cubits in height, (Purchas, vol. v. p. 503.) A tusk is described of the length of fourteen feet, in the possession of a merchant at Venice; and another at Amsterdam, which weighs three hundred and fifty pounds.—(Rees's Cyclopædia, art. " Ivory.")

"This mass of ice had imbedded in it a vast number of the tusks, teeth, and bones of the mammoth, of which the objects we have described were a part. Some parts of the ice near them had a smell of decayed animal matter; arising, no doubt, from the decomposition of the flesh. The tusks are in their *natural state;* but of two teeth, which accompanied them, one seems to be petrified, having, doubtless, been in contact with stone. The mammoth seems to have been an inhabitant of the whole northern hemisphere; its teeth and bones having been found on both sides of North America, in Siberia, England, Scotland, and Italy, and other parts of Europe. The remains found in Ayrshire and England belong to a smaller species than these. The Edinburgh Museum is indebted for them to Lord Melville."—*Globe* Newspaper, Nov. 22, 1828.

"Les rivages de la baie de Kotzebue, (N. lat. 66° 37′, W. Long. from Greenwich, 164° 42′) étaient composés de sable et de cailloux; en d'autres endroits, ils étaient uniquement formés par des masses de glace, dont une couche d'argile et de terre végétale, épaisse d'un pied et demi et couverte de mousse, revêtait les sommets. La plage consistait en terreau noir, entraîné d'en haut par la fonte des glaces, et en couches de mousse et d'argile que la même cause précipite sur les terrains bas; où, quand elles rencontrent des endroits dégarnis par les chaleurs, de l'été, elles empêchent ensuite la glace de fondre. Montés au sommet, nous creusames la terre; partout on trouva la glace quelquefois à moins d'un pied de profondeur; elle était solide et pure, et avait, depuis sa base le long du rivage, près de soixante pieds de hauteur; cette masse gelée se prolongeait dans l'est jusqu'aux montagnes. On découvrit dans les tas de terre et de mousse sur la plage, plusieurs défenses et une dent molaire de mammouth."—(*Voyage Pittoresque autour du Monde, par* M. Louis Choris. Paris, 1822.)

The above are considered by many to be the remains of *mammoths* which existed before the creation of man: by others they are supposed to have been drowned at the great Deluge; by some, these northern regions are imagined to have been tropical countries, and Siberia, England, &c. to have been the native haunts of elephants, tigers, hippopotami, tapirs, &c. The historical origin of such fossil remains in general has been traced, and in nearly every instance with success. The design of these remarks is to prove that those found in Behring's Straits are the remains of those elephants which have belonged to the Turks, Moguls, or Chinese, whose capitals and residences have been, from the earliest ages, on the banks of the several rivers, some of the largest upon the globe, which discharge their copious streams into the Arctic and Pacific Seas: viz. the Amoor, the Hoang, the Kiang-keou, the Lena, the Jenesai, the Irtish, the Tobol, and the Oby.

The *Amoor* is formed by the Argoon and the Shilka, and discharges itself into the Pacific Ocean in north latitude 53°, east longitude from Greenwich, 142° 14′. The Shilka rises in the Yablonnoy Mountains, east longitude 109° 14′, being formed by the tributary streams Ingoda, Onona, and Nertcha, and passes by Nertshinsk. (Tooke, vol. i. 271. Rees's *Cyc.* "Amur.") Genghis Khan, the first Great Mogul, was born near the city of Nertshinsk, and some of his family continued to reside there after his immense conquests. It was the custom of the imperial family to travel in large carriages drawn by four elephants, and four white dromedaries; the emperor, empress, and children each having a separate one.

Pekin was conquered in 1211, and was the capital of the Mogul emperors to the year 1369. They possessed many thousands of elephants, and used those quadrupeds on their hunting expeditions towards the gulf of Leaotong, annually, when the whole court establishment, consisting of fifty or a hundred thousand persons, attended the emperor. Banks of rivers, all of which, in this quarter, run into the Pacific, must have been, from necessity, the usual residence on these expeditions. In the year 1286, Kublai fought his rebel relation, Nayan, chief of a district in Leaotong, who disputed the empire. There were eight hundred and sixty thousand combatants, and elephants were used; the grand khan being in a castle borne upon the backs of four elephants. The Sira Muren, near this scene of blood and destruction, discharges its waters into the gulf of Leaotong.—(Wars and Sports, ch. ii.)

The capital of China is placed by Ptolemy in latitude 38° 36′, and in ancient times there was a "council of *five thousand*, every one of whom findeth an elephant for the commonwealth." —(*Purchas*, ed. 1525, vol. v. p. 400.) As far back as the year before Christ 1100, Singan, in Shensi, was the capital of China. The emperor who resided here invaded Tartary and joined Afrasiab, the Scythian monarch, against the Persians. They were entirely defeated by Roostum. The Chinese monarch was mounted on a white elephant. (Wars and Sports, p. 87.) Thus elephants were in use at Singan, which is in latitude 35° 14′, on the river Hoei-ho, which joins the Hoang-ho in east longitude 110°, the mouth of which is in the Pacific, in east longitude 120°. Elephants are used for drawing ships upon the river Kiang-keou, which is more than two thousand miles long. (Vincent le Blanc, p. 103.)

From Kinsai, Japan was invaded A. D. 1283, according to the annals of Japan, by the Tartar General Mookoo with four thousand* ships and two hundred and forty thousand troops. At this epoch the emperor Shi-tsu (Kublai) always used elephants in his wars. The expedition was dispersed and supposed to be destroyed by a storm. Moguls and elephants landed in Peru and California, according to all the traditions, and remains of elephants are found at the places which those traditions relate to. Ambassadors were sent upon elephants to the great Mexican lake. Montezuma's ancestors tarried at Culiacan till the year 1324, when they advanced, selected a wild spot of underwood, threw up entrenchments, and *founded* the city of Mexico. They fought the Tlascallans with elephants. A skeleton of an elephant has been found in a tomb in Mexico, which had evidently been constructed on purpose; and wild elephants are now existing at Choco in Colombia, and on the western side of the Missouri.† Chinese ships were found wrecked upon the coast of South America by the earliest Spaniards. We may therefore safely conclude, that many elephants were lost in this tempest. The annals of

* Whatever the real number of ships was, six hundred were built specifically for this expedition, at "Kiang-nan, Fou-kien, Ho-nan and Chan-tong." (Marsden's Marco Polo, p. 574.) These might have the accommodation for the elephants; *two* of which, in each of these ships, would be more effectual against the cavalry of Japan (where there are no elephants) than any number of horses; and be more easily conveyed and fed. They would also land in armour, and in better condition for immediate action than cavalry.

† See Quarterly Journal of Science, January, 1828, p. 356.

Mexico, and also of the Incas, according to Sir Isaac Newton's mode of computation, accord with the date of this invasion of Japan.

The *Lena*. This noble river commences on the west side of Lake Baikal, and flows into the Arctic Sea in the latitude 73°; it is five thousand versts* in length. The islands at the mouths of the Lena have been famous for the *mammoth*† fishery from the earliest ages. The tusks are prized by the Chinese, Turks, and Persians as infinitely preferable to elephants' ivory.‡ The Chinese history, five centuries before Christ, mentions these walrus haunts. The furs, the hawks and falcons of these northern latitudes are highly esteemed. The Turks, who possessed elephants, conquered Yakutsk in the sixth century, and named that country Northern Turquestan. In the thirteenth century the Emperor Kublai sent to the islands at the mouth of the Lena for his hawks and falcons.|| He kept ten thousand falconers. Some elephants have been found in the ice in these parts, and Mr. Adams describes ruins of ancient forts and mutilated remains of grotesque sculpture. The number of walruses slain annually is quite astonishing.§ The mountain scenery at the mouth of the Lena, says Mr. Adams, "exalts the soul, and I was filled with emotions of joy at finding so much happiness amidst the Polar ice among these gay and innocent Tunguse fishermen."¶ In the year 1290, Kublai sent mathematicians to ascertain the

* A verst is three thousand five hundred English feet. Monsieur Lesseps crossed the Lena where it was two leagues in breadth.

† This is the name of the walrus in these regions, which has been transferred to the *elephant*: hence innumerable errors in first-rate books of science and speculation.

‡ It is said that they prefer it even to gold for the hafts of their scimitars and daggers. The ivory of the walrus does not become yellow, like that of the elephant.

|| Marsden's Marco Polo, p. 221.

§ Wars and Sports, chap. xvi.

¶ Ibid., p. 249.

latitude, as far as the 55th degree. The vicinity of Genghis's birth-place to the Lena, when we consider the immense scale of the hunting expeditions, will diminish the wonder that remains of elephants have been frequently found in these regions.

The *Jenesai.* The contents of the tombs at the city of Jenesai, at Krasnoyarsk and several other places in these parts, attest the residence of Mogul sovereigns. Thousands of cast idols in gold, medals of gold, large plates upon which the corpse is laid, diadems and chess-boards and men all of gold, &c., &c. and *remains of elephants*, are found in these tombs. The greatest antiquity of these tombs (when discovered) was eleven hundred years, the latest four hundred.*

The *Irtish.* The fertile region at the sources of the Irtish is the favourite head-quarters of Turks, Moguls, and Calmucs (and Chinese) from early ages; the Greek emperors in the sixth century sent embassies to the Turks at this place. Kaidou, great grandson of Genghis and nephew of Kublai, governed central Siberia, and rebelled in the year 1268. Timur Kaan, Viceroy of Bangalla, Ava and Yunnan, invaded Siberia, and this rebellion was not terminated for *thirty-three years*, during which Kaidou was, in 1297, driven *northward*; many battles were fought by the river Irtish; and the Grand Khans were the whole time obliged to keep numerous armies in these countries. The first invasion was with three hundred thousand troops.

At Tara, Ommostroc, Tomsk, Batsamki, and Isetskoe, the governors permitted the people to ransack the neighbouring tombs, reserving a tenth share for themselves; the treasures were not exhausted after many years digging. Urns, figures in gold of the hippopotamus, tables of silver, and innumerable curiosities were found in the tombs. As Timur Kaan had all the elephant countries east of the Burrampooter under him (he became emperor in 1294, named Ching-tsong), we may safely conclude, many remains having been found in these

* Tooke's Russian Empire, vol. ii., p. 48.

quarters, that he was accompanied by numbers of those animals.

The *Tobol*. Between the sources of the Ischim and the Tobol, Oguz, the Cæsar of the East, resided. Many ruins and stone sculptures are existing in those deserts. This great conqueror subdued Cathay, and the countries called India extra Gangem, about eight centuries before Christ. Wars against China were frequent in those ages; and, in the note on the Amoor, we have seen that the Emperor of China was an *ally* of the Scythians against the Persians, and fought upon an elephant.—(Wars and Sports, ch. iii. and v.)

The *Oby*. The Tobol flows into the Oby at Tobolsk, and the Irtish joins the Oby in lat. 61°. The Oby reaches the Arctic Sea in lat. 66°.

A.D. 1242, Sheibani, grandson of Genghis Khan, founded Genghidin, on the west side of Tobolsk; and the capital was, after some years, established at Siber, eastward of Tobolsk. Siber was the capital of western Siberia, till that immense region was *discovered* by the Russians, long after the death of Columbus; and Siber was conquered, and the Mogul power abolished, in the year 1586. All India beyond the Ganges was conquered by Kublai, cousin of Sheibani, in 1272. Remains of elephants have been found in many places in this neighbourhood—some of them very little decayed.

Although some of the rivers above named may not be of a depth or description to admit of the dead body of an elephant being conveyed to the ocean, it must nevertheless be considered that many of the rivers enumerated are every year much swollen by the melting of the snow, and that heavy weights may float to their mouths upon the ice which breaks up in the spring *. We must take into consideration the number of elephants that may have been lost on the Japanese expedition, and the cer-

* The reader may judge of the commotions in the elements in these regions, by the fact of whales (one of them eighty-four feet long), described also by scientific writers as *mammoths*, being found above eight hundred miles inland from the Arctic Sea.—*Strahlenberg*, p. 404.

tainty that the largest and most valuable animals, from any peculiarity in the tusks, would naturally be sent as presents from the elephant provinces to the family residences, as the most acceptable; and that the same shaped tusks of these supposed *antediluvian* elephants have been found at Newnham, near Rugby, in England (the *Tripontio* of the Romans) *; and that remains of another elephant were found near Gloucester, mingled with bones and horns of oxen, sheep, and hogs; and a square stone with them, supposed to have belonged to a sacrificial altar.—(Hakewill's Apology, p. 228.) One was found near the sign of Sir John Oldcastle, in a gravel pit, near which a battle had been fought between the Britons and Romans, and with it the head of a British spear, made of flint. If we add to these facts that, in almost every place in Italy, Spain, and France, where remains of elephants have been dug up, it is known that the Carthaginians and Romans had fought battles in which elephants were slain, the reader who is in search of the truth will not fail to hesitate in his speculations regarding these tusks from Behring's Straits being of antediluvian origin.†

* Horsley, Brit. Rom., p. 436.

† The following is another proof of the necessity of caution on this interesting subject:—

"The jaw-bone of an enormous unknown animal has been discovered at Epperheim, in the canton of Arrey, on the left bank of the Rhine, by M. Schleïermacher. Several teeth had previously been found, resembling those which this jaw-bone contains; but as they were similar to those of the tapir, credit was given to the antediluvian existence of a gigantic species of that animal. This discovery will undeceive naturalists on that point. This animal belongs to a new genus. Supposing that its body was as small in proportion to the head as in the hippopotamus, its entire length must have been nineteen French feet. The largest quadruped hitherto known was a gigantic sloth, the *megalonix*, which was twelve feet long."—*Literary Gazette, Nov.* 22, 1828. We are not acquainted with the anatomy of the Om-Kergay, described by Burckhardt (*Quarterly Review, Dec.* 1823, p. 521) as quite harmless, and the size of a rhinoceros.

Practical Comparison of different Tables *of* Mortality. *In a Letter to* Sir Edward Hyde East, Bart., M.P., F.R.S.

(By a Correspondent.)

My Dear Sir,

I had the honour of addressing to you, a few years since, an investigation of the value of human life, which was published in the Philosophical Transactions for 1826: and I had then occasion to employ a formula for expressing the annual decrements of life at all ages, in such a manner, as to serve sufficiently well for the intended purpose, of *harmonizing* the mean standard table, of which I had obtained the basis from a comparison of various documents. This formula would have been much too complicated for any thing like a direct introduction into the detail of calculation: but I have lately had the good fortune to discover some simpler expressions, which are capable of being extensively applied, with great convenience, to different cases occurring in the practice of Insurance, and which may also be readily adapted to a variety of tables of mortality, so as to afford a far nearer approach to the results belonging to each, than could be obtained from calculations derived from any other tables; and will frequently indeed be more likely to represent the true law of nature at each place of observation, than the actual records of a limited experience for each particular year throughout life.

2. The great computer Demoivre employed, on different occasions, two different hypotheses respecting the mean value of life: and each of these has its advantages in particular cases. The first was the arithmetical hypothesis, supposing, for instance, that out of 100 or of 86 persons born together, 1 shall die annually till the whole number be exhausted. The second was the *geometrical* hypothesis, as, supposing that 1 in 50, or in 100, of the living at any age shall die within a year: a law which seems somewhat to approach to that of nature in extreme old age.

3. I have lately added to these, from examining a report of the experience of the Equitable Assurance Office, a third hypothesis, which may be called the *exponential;* the proportional mortality ap-

pearing to be represented by a geometrical progression of divisors; so that we may suppose the divisor to be doubled once in every ten years that the age falls short of 115; while, in the Northampton table, which approaches very near to the law of the arithmetical hypothesis, the divisor requires to be doubled more nearly once in 22 years.

4. The exponential hypothesis affords us, as I have shown by an example, a ready mode of computing the number of survivors at a given age, as required by the supposed law of the divisors; but if we proceed to compute by it the expectation of life, or the value of an annuity, it leads, in the simplest cases, to a transcendental quantity, which has long served for the amusement or for the torment of the most refined mathematicians, under the name of a logologarithmic integral, without having been rendered the more manageable by all their elaborate investigations.

5. Still less would it be practicable to make any use of an additional exponential term, which might be made to express with great accuracy the decreasing mortality of early infancy and childhood. A difficulty nearly similar occurs also in computing from an expression which I had deduced from the equable variation of the value of an annuity under certain circumstances; a property which I have lately employed, as you will recollect, for facilitating the valuation of outstanding policies for insurance. This formula for the decrement was $\frac{a+x}{b-x}$, which leads to the same hyperlogarithmic series as the exponential hypothesis.

6. We may form a correct conception of the character of the exponential hypothesis by laying down, in the diagram of my paper in the Transactions, the numbers of the table that I have published in my letter to Mr. Morgan, taking $\frac{2}{3}$ of the quinquennial differences for the comparative annual mortality: and it will be found that the curve thus obtained, approaches, in its general appearance, surprisingly near to that of the Carlisle table, and considerably resembles the curves of Deparcieux and of Finlaison, especially between the ages of 40 and 80.

7. But something much more simple than this is required for practical purposes; that is, if we attempt to apply a formula to the detail of our computations; and we may exhibit the basis of such a formula to the eye by drawing a straight line from the Age 0 to the highest point of the Carlisle curve, and continuing it to the age 85 or 90; and it will be obvious, from inspection, that a triangle like this approaches much nearer, between 10 and 80, to the character of all the rapidly ascending lines of Carlisle, Finlaison, and Deparcieux, than either Demoivre's horizontal line, or the slightly irregular curve of Northampton; and, from the employment of the area of the triangle, the law derived from it may be called the *quadratic* hypothesis.

8. In other words, we find that many of the modern tables appear to indicate, instead of a uniform decrement of life throughout the full period of vitality, a decrement nearly proportional to the age itself, and the quadratic hypothesis carries to its greatest possible extent the exaggeration of the *climacteric age*, as I have before denominated the age of the greatest mortality, which seems to have been actually creeping upwards for the last century, though less rapidly than has sometimes been supposed. Deparcieux made it 73, the Carlisle table 74, Mr. Finlaison 78, and Mr. Babbage's reduction of the alleged mortality of the Equitable Office 82, though my late computation upon the exponential hypothesis, derived from a corrected report, makes it only about 75. Now, the triangle of the quadratic hypothesis rises highest at its termination, and makes the supposed climacteric the year of unavoidable death to those who attain it. This is a peculiarity not very credible as a correct statement of a matter of fact, though it requires little or no correction when applied to the generality of results like those of the Carlisle tables; and, in other cases, its imperfections may probably be remedied without difficulty. On the other hand, the true climacteric of nature, as well as that of the geometrical hypothesis, is the year of birth, while in the arithmetical hypothesis there is no climacteric at any age. The mortality of London in 1815, and the Northampton table, approach to the arithmetical hypothesis as having no strongly marked climacteric after the year of birth, though they have

each a maximum about the middle of the whole range of life. The abridged formula, which I suggested in my former letter to you, was $368 + 10x$ for the decrement of life, which is a combination of the arithmetical and quadratic hypotheses in equal proportions at the age of about 37, and expresses, as it was intended to do, a mean mortality between the old and the new observations; but it is more convenient to keep them separate in computation.

9. I shall now proceed to compare, with the different tables of Morgan, Milne, and others, the results of the arithmetical hypothesis, as expressed by $s = 1 - \frac{x}{c}$; and those of the formula $s = 1 - \frac{xx}{cc}$, which is the quadratic hypothesis; s being the comparative number of survivors at the age x, and c a constant quantity, which may be varied at pleasure from 80 to 100.

10. The first point of comparison is the annual mortality of the *arithmetical* hypothesis with that of the tables of Northampton, and the bills of mortality of London.

A. *Annual* mortality approaching to $\frac{1}{c} = .0115$.

Age.	Northampton $\times$ .015.	London, 1815, $\times \frac{1}{8}$.
10	(.0078)	(.0048)
20	.0108	(.0046)
30	.0112	.0114
40	.0114	.0122
50	.0121	.0128
60	.0123	.0106
70	.0120	.0102
80	(.0094)	(.0054)
90	(.0013)	(.0025)

11. With the decrements of the *quadratic* hypothesis, we may compare those of the tables of Deparcieux and of Carlisle, and those which I have lately computed from the supposed experience of the Equitable Office.

B. *Annual* mortality approaching to $\frac{2x}{cc} = -\Delta s$, or $\frac{-\Delta s}{x} = \frac{2}{cc}$.

Age x.	$\frac{\Delta s}{x}$ Deparcieux.	$\frac{\Delta s}{x}$ Carlisle.	$\frac{\Delta s}{x}$ Equitable Ph. M.
10	(5.8)	(2.9)	
20	(2.6)	2.2	
30	(1.8)	1.9	
40	1.2	1.7	1.3
50	1.2	1.2	1.9
60	1.6	2.0*	2.7
70	1.6*	1.8	3.3*
80	1.4	1.4	3.0
90	(.3)	(.4)	1.8

The precise value of c is here disregarded, but it may be observed that it is nearly constant in each column towards the middle of life, and that it must be perfectly so for some time about the maximum, which is 60 or 70. The agreement is, however, less clearly seen in this comparison than by means of the diagram; the effect of the discordances and irregularities of observation being here most strongly marked, and disappearing as we pursue the computations further. The Northampton table, treated in this manner, gives a series of numbers always diminishing.

12. The whole number of the living at each age, exhibited in any tables, is computed from the annual decrements; and this number is next to be compared with the two hypotheses, omitting the years of infancy.

C. Number *living*, compared with $s = 1 - \frac{x}{87}$.

Age x.	s.	North. $\times$ .015
10	.885	.851
20	.770	.770
30	.655	.648
40	.540	.545
50	.425	.429
60	.310	.306
70	.195	.185
80	.080	.070
90	.000	.007

D. *Living* compared with $s = 1 - \frac{xx}{cc}$.

Age x.	$c=87$	$c=90$	$c=93$	Deparc. $\times .17$.	Carlisle. $\times .155$	Eq. Offi. $\times \frac{1}{12}$.
10	.987	.988	.988	(1.021)	(1.001)	
20	.947	.951	.954	.961	.944	(.820)
30	.881	.889	.896	.858	.874	(.803)
40	.789	.802	.815	.763	.787	.770
50	.670	.691	.711	.674	.682	.713
60	.524	.556	.584	.542	.565	.610
70	.343	.395	.433	.359	.372	.442
80	.155	.210	.260	.138	.148	.234
90	.000	.000	.064	.014	.022	.065

It is obvious that the formulas approach, in both these comparisons, much nearer to the tables than in A and B. The column of Deparcieux is best represented by the divisor 87, at least from 25 to 80, and the same is true of the Carlisle table, except just about 60; while the supposed experience of the Equitable Office, after 40, agrees best with the divisor 90, or even 93.

13. The *expectation* of life, or the value of a life annuity without interest, is next to be determined for each hypothesis. The fluxion of the expectation is evidently equal to the fluxion of the age, multiplied by the chance of surviving to that age, which is expressed by the quotient of the survivors, $\frac{s}{k}$, supposing k to be the initial number of the living at the given age, and the fluxion of the expectation is $\frac{s}{k} \mathrm{d}x$, that is, $\left(1 - \frac{x}{c}\right) \frac{\mathrm{d}x}{k}$ or $\left(1 - \frac{xx}{cc}\right) \frac{\mathrm{d}x}{k}$, according to the hypothesis to be employed, and the fluents are $\frac{x}{k} - \frac{xx}{2ck}$ and $\frac{x}{k} - \frac{x^3}{3cck}$ respectively, taking the values from $s = k$ or $x = q$, the given age, to $x = c$, the extreme period of life assumed in the hypothesis.

14. Now, in the arithmetical hypothesis for $\frac{x}{k} - \frac{xx}{2ck}$, we have $\frac{q}{k} - \frac{qq}{2ck}$ and $\frac{c}{k} - \frac{cc}{2ck}$, the difference being $\frac{1}{k}\left(c - \frac{c}{2} - q + \frac{qq}{2c}\right) = \frac{cc - 2cq + qq}{2ck} = \frac{(c-q)^2}{2ck}$. But $k = 1 - \frac{q}{c} = \frac{c-q}{c}$, and the expectation e becomes $= \frac{c-q}{2}$, as is well known.

15. In the quadratic hypothesis, the two values of the fluent are $\frac{q}{k} - \frac{q^3}{3cck}$, and $\frac{c}{k} - \frac{c^3}{3cck}$, the difference being $\frac{3c^3 - c^3 - 3c^2q + q^3}{3cck}$ $= \epsilon$: but $k = 1 - \frac{qq}{cc}$, $cck = cc - qq$, and $cckq = ccq - q^3$; whence $\epsilon = \frac{2c^3 - 2c^2q - c^2kq}{3cck} = \frac{2cc}{3cck}(c-q) - \frac{q}{3} = \frac{2cc}{3} \cdot \frac{c-q}{cc-qq} - \frac{q}{3} = \frac{2cc}{3(c+q)} - \frac{q}{3} = \frac{c-q}{3} \cdot \frac{2c+q}{c+q}$, which varies from $\frac{2}{3}(c-q)$ to $\frac{1}{2}(c-q)$.

E. *Expectations*, compared with $\epsilon = 43.5 - \frac{1}{2}q$.

Age q.	ϵ.	Northampton.
10	38.5	39.8
20	33.5	33.4
30	28.5	28.3
40	23.5	23.1
50	18.5	18.0
60	13.5	13.2
70	8.5	8.6
80	3.5	(4.7)
90	.0	2.4

F. *Expectations*, compared with $\epsilon = \frac{2cc}{(3c+q)} - \frac{q}{3}$.

Age q.	ϵ, $c = 87$.	Deparcieux.	ϵ', $c = 90$.	$\frac{\epsilon + \epsilon'}{2}$.	Carlisle.
10	48.7	46.9	50.7	49.7	48.8
20	37.5	40.3	42.4	40.0	41.5
30	33.2	34.2	35.0	34.1	34.3
40	26.5	27.8	28.2	27.3	27.6
50	20.2	20.5	21.9	21.0	21.1
60	14.3	14.2	16.0	15.1	14.3
70	8.8	8.8	10.4	9.6	9.2
80	3.6	4.7	5.1	4.4	(5.5)
90	0.0	1.8	0	0	(3.3)

It appears, from this comparison, that we approach very near to the expectation of life at Carlisle, by taking the mean of ϵ and ϵ', or by making $c = 88.5$: and from 10 to 70 the formula appears to represent the mortality more correctly than the tables, which are extremely irregular in their differences, probably on account of the very small population on which the observations were made.

16. The only remaining determination to be considered, that is exempt from the effect of interest, is that of the *probability of survivorship* between two lives; a probability which is made up of the sum of the probabilities of survivorship for every year, or every portion of a year, throughout the full range of the life of the eldest; that is, the probability that the one will die within the element of time considered, while the other survives: so that the fluxion of the probability is $\frac{s}{k} \cdot \frac{\mathrm{d}s'}{k'}$: k being the number surviving at the age of the eldest, q, and k' at that of the youngest, while s and s' represent the variable number of survivors.

17. In the arithmetical hypothesis we have constantly $\mathrm{d}s = -\frac{\mathrm{d}x}{c}$, and the fluxion of this probability is $\frac{s}{k} \cdot \frac{\mathrm{d}x}{ck'}$; which is equal to the fluxion of the expectation ϵ, divided by ck', and the fluent being taken between the same limits $x = q$ and $x = c$ in both cases, it follows that $\frac{\epsilon}{ck'}$ is, in this hypothesis, the value of the probability that the younger life will fail first; and, since $\epsilon' = \frac{c - q'}{2}$, (14) and $k' = 1 - \frac{q}{c}$, we have $\frac{\epsilon}{ck'} = \frac{\epsilon}{2\epsilon'}$: a very simple consequence of this hypothesis, which appears hitherto to have escaped observation. The PROBABILITY, therefore, THAT THE YOUNGER OF TWO LIVES WILL FAIL BEFORE THE ELDER, IS EXPRESSED BY THE EXPECTATION OF THE ELDER DIVIDED BY TWICE THAT OF THE YOUNGER. And it is obvious that by taking the several expectations as directly computed from the tables, this determination may be extended, as a good approximation, to the utmost limits of the observations.

G. *Probabilities* of survivorship, compared with $\pi = \frac{\epsilon}{87k'}$.

Ages.	π.	Northampton.	$\frac{\epsilon}{2\epsilon'}$ N.
10, 20	.435	.415	.420
40, 50	.394	.394	.390
70, 80	(.206)	.300	.276
10, 50	.239	.206	.226
40, 80	.075	.102	.103
10, 80	.044	.044	.060

18. In the quadratic hypothesis, s being $1 - \frac{xx}{cc}$, we have d$s =$ $-\frac{2x}{cc}$ dx, and $\frac{2x}{kcc}$ dx is the fluxion of the probability that a person of the supposed age will die at a certain time, which, for the age of the younger $x - p$, taking k' for k, becomes $2\frac{x-p}{cck'}$ dx, to be multiplied by $\frac{s}{k}$, the probability that the elder will survive, that is, by $\frac{1}{k}\left(1 - \frac{xx}{cc}\right)$: the product is $\frac{2}{cckk'}$ $\left(x\text{d}x - p\text{d}x - \frac{x^3}{cc}\text{d}x + p\frac{xx}{cc}\text{d}x\right) = \text{d}\,\frac{2}{kk'}\left(\frac{xx}{2cc} - \frac{px}{cc} - \right.$ $\left.\frac{x^3}{4c4} + \frac{px^3}{3c4}\right.$, which taken from $x =$ to $x = q$, becomes $\frac{2}{kk'}\left(\frac{1}{2} - \frac{p}{c} - \frac{1}{4} + \frac{p}{3c} - \frac{qq}{2cc} + \frac{pq}{cc} + \frac{q^4}{4c^4} - \frac{pq^3}{3c^4}\right)$; that is, putting $\frac{p}{c} = p,$ and $\frac{q}{c} = q,$ $\pi = \frac{2}{kk'}\left(\frac{1}{4} - \frac{2}{3}p,\right.$ $\left. - \frac{1}{2}q,^2 + p,q, + \frac{1}{4}q,^4 - \frac{1}{3}p,q,^3\right)$.

H. *Probabilities* of survivorship, compared with the quadratic hypothesis.

Ages.	π.	Carlisle.	$\frac{\epsilon}{2\epsilon'}$ C.	North.
30, 60	.172	.158	.209	(.230)
40, 80	.060	.074	.100	(.102)

The ages are here assumed very distant, in order to compare the extreme cases; otherwise the agreement would have been much more accurate: but it is obvious that the formula comes far nearer to the direct computation from the Carlisle tables than the value derived from the Northampton tables.

19. We are now to examine the consequences of the two hypotheses in cases which require the consideration of interest or discount, to be combined with that of the contingency of survivorship at each step. In order to represent such cases, we must multiply, as is well known, the fluxion of the contingency of payment by the power r^x, or rather r^{x-q}, for the value as referred to the age q, r being the present value of a unit payable at the end of a year: for instance $\frac{100}{104}$, if we reckon at 4 per cent. compound interest. But it must be remembered that in this mathematical sense of compound interest, the interest of £100 for a quarter of a year is no more £1, at 4 per cent., than it is £16 for 4 years; and if we wish to reckon at the rate of £$\frac{4}{365.25}$ for a day, we must necessarily make the interest something more than £4 for a year. In almost all cases occurring in practice, the difference of the two modes of considering the interest is half a year's purchase of an annuity, payable annually: but sometimes, for an annuity of a very short duration, a further correction may be required: the correction is, however, in all cases, very easily computed, and generally by taking the fluent half a period later, both at the beginning and at the end of the term.

20. The present value of an annuity on a single life may, therefore, be represented by $-\int r^{x-q}\frac{s}{k}\,\mathrm{d}x$, since $\mathrm{d}x$ is negative; that is, in the arithmetical hypothesis $-\int\frac{r^{x-q}}{k}\left(1-\frac{x}{c}\right)\mathrm{d}x = -\int\frac{r^{x-q}}{k}\mathrm{d}x+\int\frac{r^{x-q}}{ck}x\mathrm{d}x$; and the fluent must be taken as usual from $x=c$ to $x=q$.

21. The general theorem for fluents of this form is $\int a^x x^n \mathrm{d}x = \frac{a^x}{h\mathrm{l}a}\left(x^n - \frac{nx^{n-1}}{h\mathrm{l}a} + \frac{n(m-1)x^{n-2}}{h\mathrm{l}^2a} - \frac{n(n-1)\ (n-2)\ x^{n-3}}{h\mathrm{l}^3a}\right.$

$+ \ldots \Big)$, or putting $\frac{-1}{hla} = \lambda$, $= -\lambda a^x (x^n + \lambda n x^{n-1} + \lambda^2 n(n-1) x^{n-2} + \ldots)$ whence the present fluent becomes $+\frac{\lambda}{k} r^{x-q} - \frac{\lambda r^{x-q}}{ck}$, $(x+\lambda)$, λ being here positive, because a is less than unity, and putting q and c successively for x, we have $\frac{\lambda}{k} - \frac{\lambda}{ck}(q+\lambda) = \frac{\lambda}{k}\left(1 - \frac{q}{c} - \frac{\lambda}{c}\right) = \lambda - \frac{\lambda\lambda}{ck}$ and $\frac{\lambda}{k} r^{c-q} - \frac{\lambda}{ck} r^{c-q}(c+\lambda) = -\frac{\lambda\lambda}{ck} r^{c-q}$, the difference being $\lambda - \frac{\lambda\lambda}{ck}(1 - r^{c-q}) = \Lambda$, the present value of the annuity: and at 4 per cent. we have $\lambda = 25.497$, $\lambda\lambda = 650$, and if $\frac{1}{c} = .0115$, the formula becomes $\Lambda = 25.497 - \frac{7.475}{k}(1 - r^{c-q})$; the results of which agree sufficiently well with the Northampton table.

I. *Annuities* at 4 per cent. upon the arithmetical hypothesis.

Age.	Daily payments.	Annual payments.	North. tables.	Difference.
10	17.49	16.99	17.52	
20	16.56	16.01	16.03	+.02
30	15.31	14.81	14.78	—.03
40	13.20	12.70	13.20	+.50
50	12.02	11.52	11.26	—.26
60	9.75	9.25	9.04	—.21
70	6.99	6.49	6.36	—.13
80	2.56	2.06	(3.64)	

22. In the quadratic hypothesis, the fluxion is $-\int r^{x-q} \frac{s}{k} \, dx = -\int r^{x-q} \frac{dx}{k} + \int r^{x-q} \frac{xx}{cck} dx$ and the fluent $\frac{\lambda}{k} r^{x-q} - \frac{\lambda}{cck} r^{x-q} (x^2 + 2\lambda x + 2\lambda^2)$ which from $x = q$ to $x = c$ affords us $\frac{\lambda}{k} - \frac{\lambda}{cck}(q^2 + 2\lambda q + 2\lambda^2) - \frac{\lambda}{k} r^{c-q} + \frac{\lambda}{cck} r^{c-q}(cc + 2\lambda c + 2\lambda\lambda) = \frac{\lambda}{k}\left(1 - \frac{qq}{cc} - \frac{2\lambda q}{cc} - \frac{2\lambda\lambda}{cc} + r^{c-q}\left[\frac{2\lambda}{c} + \frac{2\lambda\lambda}{cc}\right]\right)$,

or, since $k = 1 - \frac{qq}{a}$, $\Lambda = \lambda - \frac{\lambda}{k}\left(\frac{2\lambda q}{cc}. + \frac{2\lambda\lambda}{cc} - r^{c-q}\left[\frac{2\lambda}{c} + \frac{2\lambda\lambda}{cc}\right]\right)$: that is, at 4 per cent. and taking $c = 88.5$, and $\frac{1}{c} = .0113$, λ being $= 25.497$, $\lambda\lambda = 650$, and $\frac{\lambda}{c} = .28812$,

$$\Lambda = 25.497 - \frac{1}{k}(4.233 + .166q - 18.923\, r^{c-q}).$$

K. *Annuities* at 4 per cent. from the quadratic hypothesis.

Age.	Daily payments.	Annual payments.	Carlisle tables.	Difference.
10	20.43	19.93	19.58	+.35
20	18.93	18.43	18.36	+.07
30	17.25	16.75	16.85	−.10
40	15.37	14.87	15.07	−.20
50	13.17	12.67	12.87	−.20
60	10.67	10.17	9.66	+.51
70	7.40	6.90	6.71	+.19
80	3.72	3.22	4.18	−.96
			Mean	− .04

It is obvious that a mean error so small and so subdivided is as likely to belong, in great measure, to the observations as to the computations.

23. It was my intention to proceed, in a similar manner, through the computations of annuities on two joint lives, and of the contingent reversions of survivorships: but the accuracy of the proposed formulas appears to be already abundantly demonstrated by the two last comparisons, and I shall confine myself, for the present, to the great remaining problem of *three joint lives*, the facilitation of which would be really a step of practical importance, even if we allowed the accuracy of the existing tables, which have been the most extensively employed for calculations of this kind.

24. The age of the eldest of three lives being x, and the ages of the two younger $x - p'$ and $x - p''$, the initial values of s, s', and s'' being k, k', and k'' respectively, when $x = q$; the probability of the survivorship of the whole three, for any other values of x, will be $\frac{ss's''}{kk'k''}$, and the fluxion of the present value of the annuity

will be $\frac{ss's''}{kk'k''} r^{x-q}\,\mathrm{d}x$. Now, $s = 1 - \frac{xx}{cc}$, $s' = 1 - \frac{(x-p')}{cc}$, and $s'' = 1 - \frac{(x-p'')^2}{cc}$, or neglecting c, which always accompanies x and p, till the end of the computation, $s' = s + 2p'x - p'p'$, and $s'' = s + 2p''x - p''p''$; whence $ss' = ss + 2p'xs - p'p's$, and $ss's'' = s^3 + 2p'xs^2 - p'p's^2 + 2p''xs^2 + 4p'p''x^2s - 2p'p'p''xs - p''p''s^2 - 2p'p''p''xs + p'p'p''p''s = s^3 - (p'p' + p''p'')\,s^2 + p'p'p''p''s + 2\,(p' + p'')\,s^2x + 4p'p''sx^2 - 2p'p''\,(p' + p'')\,sx$.

25. Hence it appears that this contingency comprehends that which belongs to the value of three equal joint lives, as well as those which relate to one life and to two, these latter being also complicated with the expression of the age and of its square: and it is obvious that the result may be reduced to the form $kk'k''\Lambda = Q - (p'p' + p''p'')\,Q' + p'p'p''p''Q'' + 2\,(p' + p'')\,Q''' + 4p'p''Q'''' - 2p'p''\,(p' + p'')\,Q'''''$: all the quantities Q... being dependent on the oldest life only, and capable of being expressed in a table by as many single numbers for each age, to be afterwards combined according to the variations of the younger lives, as here expressed by the differences. The first three numbers might be readily obtained according to any given tables of observations from the tables of the values of *equal* joint lives already in existence, or they may be computed with the rest, from the formulas: the last of all is also subservient to the calculation of the value of survivorships.

26. In the first place, for the quantity Q, belonging to three equal lives, we have $-\int s^3 r^{x-q}\mathrm{d}x = -\int r^{x-q}\mathrm{d}x\,(1 - 3x^2 + 3x^4 - x^6) = \lambda r^{x-q}(1 - 3\,(x^2 + 2\lambda x + 2\lambda\lambda) + 3\,(x^4 + 4\lambda x^3 + 12\lambda^2x^2 + 24\lambda^3x^2 + 24\lambda^4) - (x^6 + 6\lambda x^5 + 30\lambda^2x^4 + 120\lambda^3x^3 + 360\lambda^4x^2 + 720\lambda^5x + 720\lambda^6)$; which, taken from $x = q$ to $x = c$, becomes $\lambda\,(1 - 3\,(q^2 + 2\lambda q + 2\lambda\lambda) + 3\,(q^4 + 4\lambda q^3 + 12\lambda^2q^2 + 24\lambda^3q + 24\lambda^4) - (q^6 + 6\lambda q^5 + 30\lambda^2q^4 + 120\lambda^3q^3 + 360\lambda^4q^2 + 720\lambda^5q + 720\lambda^6) - r^{c-q}\,[1 - 3\,(1 + 2\lambda + 2\lambda\lambda) + 3\,(1 + 4\lambda + 12\lambda^2 + 24\lambda^3 + 24\lambda^4) - (1 + 6\lambda + 30\lambda^2 + 120\lambda^3 + 360\lambda^4 + 720\lambda^5 + 720\lambda^6)]) = Q$: the numbers λ and q, wherever they occur after the first λ, being understood as divided by c.

27. The second quantity is $Q' = -\int r^{x-q}\, ssdx$, ss being $= 1 - 2\frac{xx}{cc} + \frac{x^4}{c^4}$, or $1 - 2xx + x^4$; which gives the fluent λr^{x-q} $(1 - 2(x^2 + 2\lambda x + 2\lambda\lambda) + x^4 + 4\lambda x^3 + 12\lambda^2 x^2 + 24\lambda^3 x + 24\lambda^4)$: and this from $x = q$ to $x = c$ is $\lambda\,(1 - 2q^2 - 4\lambda q - 4\lambda\lambda + q^4 + 4\lambda q^3 + 12\lambda^2 q^2 + 24\lambda^3 q + 24\lambda^4 - r^{c-q}\,[1 - 2 - 4\lambda - 4\lambda\lambda + 1 + 4\lambda + 12\lambda^2 + 24\lambda^3 + 24\lambda^4]) = Q'$: restoring c in its place.

28. For Q'' we have $-\int r^{x-q}\, sdx$, as in the case of a simple annuity (22), $= \lambda\, r^{x-q}\,(1 - x^2 - 2\lambda x - 2\lambda^2)$ giving $\lambda\,(1 - q^2 - 2\lambda q - 2\lambda\lambda + r^{c-q}\,(2\lambda + 2\lambda^2) = \lambda\,(k - 2\lambda q - 2\lambda\lambda + 2\lambda r^{c-q}\,[1 + \lambda])$.

29. In the next place $Q''' = -\int r^{x-q}\, ssxdx = -\int r^{x-q}\,(xdx - 2x^3 dx + x^5 dx) = \lambda r^{x-q}\,(x + \lambda - 2\,(x^3 + 3\lambda x^2 + 6\lambda^2 x + 6\lambda^3) + x^5 + 5\lambda x^4 + 20\lambda^2 x^3 + 60\lambda^3 x^2 + 120\lambda^4 x + 120\lambda^5)$; which, from $x = q$ to $x = c$, gives $\lambda\,(q + \lambda - 2\,(q^3 + 3\lambda q^2 + 6\lambda^2 q + 6\lambda^3) + q^5 + 5\lambda q^4 + 20\lambda^2 q^3 + 60\lambda^3 q^2 + 120\lambda^4 q + 120\lambda^5 - r^{c-q}\,[1 + \lambda - 2\,(1 + 3\lambda + 6\lambda^2 + 6\lambda^3) + 1 + 5\lambda + 20\lambda^2 + 60\lambda^3 + 120\lambda^4 + 120\lambda^5])$.

30. For Q'''', derived from sx^2, we have $-\int r^{x-q}\,(x^2 dx - x^4 dx) = \lambda r^{x-q}\,(x^2 + 2\lambda x + 2\lambda\lambda - [x^4 + 4\lambda x^3 + 12\lambda^2 x^2 + 24\lambda^3 x + 24\lambda^4])$; and this, when corrected, becomes $\lambda\,(q^2 + 2\lambda q + 2\lambda\lambda - q^4 - 4\lambda q^3 - 12\lambda^2 q^2 - 24\lambda^3 q - 24\lambda^4 - r^{c-q}\,[1 + 2\lambda + 2\lambda\lambda - 1 - 4\lambda - 12\lambda^2 - 24^3\lambda - 24\lambda^4]$.

31. Lastly, for Q''''', from sx, we have $-\int r^{x-q}\,(x - x^3)\,dx = \lambda r^{x-q}\,(x + \lambda - (x^3 + 3\lambda x^2 + 6\lambda^2 x + 6\lambda^3)$, which becomes $\lambda\,(q + \lambda - (q^3 + 3\lambda q^2 + 6\lambda^2 q + 6\lambda^3) - r^{c-q}\,(1 + \lambda - [1 + 3\lambda + 6\lambda^2 + 6\lambda^3])$.

32. Taking, for a single example, the value of three joint lives of 30, at five per cent., we have $q = 30$, $\frac{1}{c} = .0113$, $\frac{q}{c} = .339$, $\lambda = 20.5$, $\frac{\lambda}{c} = .23165$, and $r^{c-q} = .0576$. By substituting these quantities in the expression $\frac{Q}{k^3} = \Lambda$, we have the value 11.37.

L. *Three joint* lives, at 5 per cent., compared with the Carlisle tables.

Common age.	Daily payments.	Annual payments.	Carlisle tables.	North. tables.
30	11.37	10.87	10.82	(8.50)

33. For the arithmetical hypothesis, the computation becomes still more simple, and requires no auxiliary tables beyond those which are universally known. The contingency for the three joint lives here becomes $\frac{s^3 + (p' + p'')\, s^2 + p'p''s:}{kk'k''}$ and the value, taking Λ', Λ'', Λ''' for the existing tabular values of 1, 2, and 3 lives at the age of the eldest, $\frac{k^2\Lambda''' + (p' + p'')\, k\Lambda'' + p'p''\Lambda'}{k'k''}$, p' and p'' being the excess of the elder above the two younger respectively, divided by c: we might also add half a year to the tabular numbers, and deduct it from the final result, if necessary.

34. The same simplification is applicable to two joint lives, the contingency becoming $\frac{s}{k} \cdot \frac{s'}{k'} = \frac{1}{kk'}\, s\,(s + \frac{p'}{c}) = ss + \frac{p's}{kk'}$ and the values $\frac{k\Lambda''}{k'} + \frac{p'\Lambda'}{ck'}$.

M. *Two joint* lives, at 4 per cent., from the equal lives.

Ages.	Approximation, N. T.	Particular tables. (Morgan.)
40, 20	10.929	10.924
60, 40	7.396	7.490
80, 70	2.951	2.757

N. *Three joint* lives, at 4 per cent., from the equal lives.

Ages.	Approximation, N. T.	Morgan, Table VI.
30, 20, 10	10.13	10.438
50, 40, 30	7.42	7.571
70, 60, 50	4.26	4.219

35. You will perceive, my dear Sir, that these examples sufficiently establish the accuracy of my method of computing, without immediate reference to tables, during the most important portions of life; and if it be found sufficient, it must be allowed to possess a decided superiority, in the facility with which any imaginable change in the value of life is introduced into the computation. This modification is very readily effected by changing the constant quantity c in either mode of computation, or by combining the results obtained for any particular case from both methods, in such a manner and in such proportions as may be thought most desirable. But it is obvious that, in making these combinations, there must still be ample scope for the exercise of sound judgment and discretion, aided always by personal experience and cautious reflection.

I am, my dear Sir,

Yours most sincerely,

* * * *

Waterloo Place, 17 *Nov.* 1828.

Postscript. The *quadratic hypothesis* may easily be accommodated to any table like my own, of which the decrements are nearly expressed by the mixed formula $368 + 10x$, considering the fluents between the given age and the time of total extinction only: and the same formulas will comprehend the *arithmetical hypothesis* as a particular case.

A General Description of Lake Erie. Communicated by John J. Bigsby, M.D., &c. &c.

GENERAL REMARKS.

The following pages will present a rapid view of the position and dimensions of Lake Erie, and of the leading features of its vicinity. The course of the heights surrounding its tributary streams are next described; and then the shores, islands, and a few of the rivers of this body of water. For topographical and statistical details, not necessary to geological description, the reader may consult with advantage the writings of Bouchette*, Howison†, Gourlay‡, Darby§, and Kilbourne‖.

Lake Erie has few of the fascinations of scenery to boast of, apart from the large mass of waters it exhibits—in tranquillity, or in motion, sometimes most vehement. It is only at its west end that it is adorned by islands. The morasses, earthy scaurs, or gentle uplands of its coasts, are only remarkable for their large walnut and buttonwood trees, which, in a dense umbrageous belt, shut out all view of the interior from the traveller on the lake, except at the partial clearances.

Neither is the vicinity of this lake agreeable as a residence, in the western half, at least in the summer. The heat then, although not thermometrically extreme, is peculiarly oppressive, relaxing, and long continued. The steaming swamps¶, which are almost universal, are full of putrifying substances, occasioning the bilious remittents there so prevalent. The

* Topography of the Canadas. † Sketches of Upper Canada.

‡ Statistical Account of Upper Canada.

§ Tour from New York to Detroit. ‖ Gazetteer of Ohio.

¶ Clearances, by affording a free access of the air to swamps, greatly diminish their size. It has been found also, in Ohio, that the progress of cultivation tends, at the same time, to increase the dimensions of the rivers. Thus, Todd's Fork of the Little Miami River was formerly often dry in the summer, but is never so now; and the same has taken place with Kinnickinnick, in the County of Pickaway. Attention has been drawn to these subjects by the frequent want of water for mills and navigation.—Kilbourne.

water in common use is heated, and ill tasted. Moskitoes, sand, and black flies abound, and, extending their attacks to the domestic animals, aided by a fly nearly an inch long, almost drive them distracted. There are circumstances also, in social life, which render this region a disagreeable residence, but which are gradually disappearing. Its extreme fertility, the moderate sum of its annual heat, and its facilities of communication with other countries, will, in progress of time, render it the seat of a dense population, and a principal granary of the western continent. Wheat, maize, and tobacco, are cultivated with equal success. The returns of the agriculturist are large, secure, and of excellent quality. The last-named article has been grown in considerable quantity about the river Detroit, near the head of the lake, and favoured, in a small remission of duty, by the British Government, is sent to England, after having undergone an inland carriage, to Quebec, of 814 miles. Salt springs exist in almost every township, accompanied, in one or two cases, by large beds of gypsum. Bog iron ore is common on the north-east side of the lake, and is worked. The water communications of these countries are astonishingly easy. Canoes can go from Quebec to the Rocky Mountains, to the Arctic Circle, or to the Mexican Gulf, without a portage longer than four miles; and the traveller shall arrive at his journey's end as fresh and as safely as from an English tour of pleasure. It is common for the Erie steam-boat to take goods and passengers from Buffaloe, to Green Bay and Chicago, in Lake Michigan, a distance of nearly 900 miles, touching, at the same time, at many intermediate ports. In about three years, in addition to the canal connecting Lake Erie with tide-water in the Hudson, another will be excavated across the southern dividing ridge, to communicate with the Ohio. Near its place of junction with this river, a canal from the Atlantic, across the Alleghanies, will enter the Ohio. Lake Erie will then also have a steady line of water transport to Baltimore, on the Chesapeake, and New Orleans, on the Mississippi. The surveys, preparatory to these projects, have been in execution for two years; there is no doubt of their practicability.

I cannot even hazard a conjecture as to the number of inhabitants around Lake Erie. They are numerous, and daily augmenting; but with incomparably greater rapidity on the south side of the lake, distributed between the States of New York, Pennsylvania, and Ohio. Ohio, which occupies the largest portion, in 1800, had 45,000 inhabitants; in 1810, 250,760, and, in 1820, 581,434. At present, it cannot have less than 750,000 inhabitants, and there is ample room for more. There are few or no Indians on the north borders of the lake. The Mohawks are placed high up the river Ouse, and the Hurons, from four to ten miles up the river Detroit.

The winds are generally either up or down the lake, and in summer they are in the former direction for two-thirds of the time. In the middle of this season they are commonly mild, but occasionally in perfect tornadoes, accompanied with tremendous lightning and heavy rain. The gales begin in October, and are both violent and dangerous. Many lives are lost annually. The winters are mild and short. The inhabitants do not reckon on the ground being covered by snow more than three or four months. They turn their cattle into the woods in March and April, but the lake remains full of floating ice until May. On the 12th of May, 1821, the steam-boat could not proceed on account of the ice. From an adjacent eminence, the lake was seen to be covered with it in one compact mass, as far as the eye could range. As might be expected, remittent and intermittent fevers are very prevalent in the autumn. The febrile action rises high, and there is usually a topical affection conjoined: to this the stimulating diet and frequent use of spirituous liquors, and exposure to heat, mainly conduce. In the year 1819, these diseases raged with particular violence. The British and American Boundary Commissioners, consisting of thirty-five individuals, were then encamped among the small islands at the west end of the lake. Scarcely one escaped an attack of remittent fever. In three instances it proved fatal, one of which was the British commissioner, Mr. Ogilvy, a man of great worth, activity, and talent. Most of the others recovered with difficulty, and remained, during the whole of the following winter, in a sickly state. The disease made its appearance in September. During the years 1820 and 1821, the united commissions pursued their surveys at the west end of the lake, and along the water communication between Lakes Erie and Huron. Their officers and men were then attacked by mild intermittents, and, in one case, a severe remittent.

GEOGRAPHY.

Lake Erie is placed on the north side of the hilly country giving rise to the principal tributaries of the Ohio, and at the south edge of the fertile peninsula included by the waters of Lakes Huron, St. Clair, Erie, Ontario, and Simcoe.

The part of Lake Erie nearest to Lake Huron is Port Talbot (about the middle), and it is 57 miles distant. It is 21 miles from Lake Ontario, at Sugar-loaf Hill, and 15 miles from Lake St. Clair, at the township of Tilbury East.

This lake extends (in a narrow, oblong form, and much contracted at its north-east end) 231 miles from S.W. to N.E., according to Bouchette, and from long. 78° 16′ to long. 82°. This estimate agrees closely with data I received from my friend, Lieutenant J. Grant, R.N.* Mr. Bouchette adds, that Lake Erie has its greatest breadth of 63½ miles (lat. 41° 10′—42.3) at Port Talbot, and that it is commonly 30 to 40 miles broad, but is little more than 20 at Long Point, on the north shore, 70 miles from the lower end.

The distance between Point Pelé and its opposite headland on the south shore, the peninsula of Sandusky, is very incorrectly represented in the usual maps, which are, indeed, inaccurate in every thing respecting the head of the lake.

Purdy's Map of Cabotia makes it an oblong cul de sac, 17 miles broad, at the mouth of the Detroit river; whereas it is a short oval, 30 miles in breadth at that spot, and 42⅔ miles long, measuring W. by N., from midway between the two headlands. He gives 13 miles as the interval between these promontories, while it is 25½ miles. Mr. Tanner, a skilful geographer of Philadelphia, assigns to this last space 36 miles in his Map of the State of Ohio; and Mr. Carey, of the same place, makes it 50 miles, in his Maps of the Michigan Terri-

* According to Mr. G., who was stationed on this lake in 1820, Middle Island is 190 miles, by ship's course (nearly direct), from the lower end of Lake Erie; and it is 38½ miles direct from the upper end, by the maps of the Commission: thus making a total of 228½ miles.

tory*. Lieutenant J. Grant, R.N., found its general depth to be 15, 18, and 25 fathoms, and in one place only three fathoms—bottom sandy. The Canadian shore is bolder than the American, which, in some parts, runs out shoal for two or three miles.

Compared with the other lakes, this is shallow. In a gale of wind it is rendered turbid, by the sand and mud washed from the bottom, as I have myself witnessed. The sounding-lead frequently brings up clayey mud, into which it sometimes sinks entirely. Horizontal rocks now and then form its floor—most frequently at the S.W. end of the lake, where reefs and shallows are common. The water is always good, some distance into the lake; but in summer, near shore, it is much contaminated with animal and vegetable matters in a state of putrefaction. In that season, in the middle of the day, the shoal water is heated to 90—95° Fahrenheit.

The height of Lake Erie above the Atlantic Ocean has been ascertained to be 565 feet. The barrier which contains it is so low, that, were it to rise only six feet, it would inundate, on its northern and western borders, several millions of acres, now partly occupied by towns, villages, and farms; and it is estimated that a further rise of six or eight feet would precipitate a vast flood of waters over the state of Illinois from the south end of Michigan; the great Canadian lakes then discharging also into the Mexican Gulf. This last idea originated, I believe, with Mr. Stickney, a very intelligent resident on the river Maumee.

This barrier, the height of land surrounding the basin of Lake Erie and its rivers, by no means follows the shores of that body of water with fidelity—the great departure taking place about its S.W. and N.E. extremities. I shall now trace its course in a general, but sufficiently accurate, manner; and, in so doing, shall present a rapid sketch of the region in which this lake is placed.

The peninsula which it traverses on the north is an east arm of the levels, bordering the south shores of Superior, Michigan, and Huron, which are themselves parts of the vast plains of

* Atlas of the United States.

the Rocky Mountains, and of the valley of the Mississippi. It is an undulating tract, declining insensibly from north to south, and abounding in low, rich lands, intersected by numerous prolonged elevations, too broad to be called ridges, which give off on each side, occasionally, large streams belonging to the same or to different lakes. These rivers, and their branches, frequently pass over raised beds, which subside laterally, with more or less rapidity, into morasses, natural meadows, and moist woods. There is an eminence on the River Thames, in the township of Westminster, 150 feet high (Gourlay). The soil of this region varies, but is chiefly black and often marly loam, with patches of sand, and resting on gray, blue, or red clay; as is finely displayed in the deep banks of the river Thames, and on the north shore of the lake itself. Its timber is of large size, and consists of hickory, maple, oak, button-wood, walnut, ash, elm, &c. &c.

In this sort of country, eastward from the River Detroit as far as Port Talbot, this line of heights (so almost nominally) is seldom more than ten miles from Lake Erie; the River Thames, a large tributary of the St. Clair, during great part of this space being only 13-20 miles from the former lake. The creeks, entering this portion of Erie, issue from extensive marshes, and in many cases flow sluggishly through prairie lands, scarcely raised above the level of the reservoir they seek. From Port Talbot, while the north shore of the lake runs east for nearly 70 miles, the line of head-waters runs N.E., or N.N.E., to near Oxford (35 miles from the lake direct), at the upper part of the Thames, and from thence passes north for 60 miles. In the unexamined country thereabouts, the sources of the Ouse, or Grand River, approach streams which discharge, severally, into Huron, Simcoe, and Ontario.

Having swept to the north and east round these, the dividing line turns abruptly south, and skirts that river at the distance of 5-7 miles to near the mouth of the Ouse—from thence bending eastward to the end of the lake: the interval is still 5-7 miles, and is very marshy. Its elevation here is only sufficient to determine the course of its streams.

On the north shore, the eye can discover from the lake only a few scrubby heights about Point Abino; but on the south shore, there is visible, for 110 miles from its east end, a bold slope of woods, of smooth and uniform aspect, and divided into an upper and lower bank in many places; the latter being separated from the lake by an alluvial level, considerable only along its western half and at its extremities. This elevation is, perhaps, understated at 600 feet; but at the middle of the lake it becomes broken and variable: it, or its immediate vicinity, is the true height of land. At the east end of the lake it is 25 miles distant, but from thence westward it approaches the lake shore obliquely, and at Portland, 58 miles from the River Niagara, it comes within three miles of it, and then follows its shores to the village of Erie. In this distance, the narrow body of water called Lake Chatanghque, one of the sources of the Alleghany (a branch of the Ohio) is only 9½ miles from Lake Erie.

A few miles westward from the disappearance of this eminence from observers on the lake, " it enters the state of Ohio, near the dividing line of Ashtabula, and Trumbull, and Portage counties diagonally. From the S.W. angle of the latter, it passes along the north border of Stark and Wayne, and more than half that of Richland county. From hence the ridge turns S.W. as far as the Maumee River."—(Darby, p. 181, Tour.) On the south and west of Lake Erie it does not often appear in the actual form of hills, with intervening vales, although they are common, as before said, about its middle, but spreads into an extensive table land, with torpid streams contained in beds elevated above the general surface of the country; much of which is prairie covered with an exuberant herbage.—(Darby.)

Of the height of land so far traced, Mr. Shriver * has ascertained the exact elevation in several places—his measurements, however, referring to water-courses, and not to the hills which may be around them. They have been made most probably at the lowest points of the dividing ridge. Four miles south of Warren (Trumball county) a swamp on the River Mahoning, discharging its waters into Lake Erie and the Ohio, is 342 feet above the former, and 35 miles distant by a direct course.—(Survey, p. 71.)

* Surveys for the Chesapeake, Ohio, and Lake Erie Canals, 1824.

About forty miles S.W. from this, an extensive morass, (35—40 miles from the lake) pours its waters into the Cuyahoga of Cleaveland (L. E.) and the Tuscawara of Ohio. It is 404 feet above Lake Erie, and has steep hills on its north (*Survey*, p. 76). But the greatest depression is in a cranberry marsh about 64 miles to the S.W. of Warren, south of, and near, the village of Mecca. It is 337 feet above Lake Erie, and about 30 miles direct from it (p. 80.) These marshes are not always occasioned by their situation, but often arise from the impervious nature of their substratum, or subsoil, to their being on a dead level, or confined by embankments.

The point of separation of the rivers Maumee and Miami (of Ohio) three miles north of Fort Loramie, 98 miles direct from Lake Erie, is 399 feet above that lake; and another depression, 11 feet lower, has been found, not far from this place.

From Fort Loramie northward and westward, I have no information respecting the course of the dividing ridge. It must necessarily be distant from the west end of the lake, as the St. Joseph, a large branch of the Maumee, overspreads that region for 60 miles toward Lake Michigan. The back country between the Maumee and River Detroit, is very low; it is a dense forest interspersed with morass. All the streams which are north of, and near, the mouth of the Detroit discharge into it, except the River Raisin, whose source is remote in the west. Between the streams tributary to the lake and those of the Detroit, there is of course an elevation, but it is imperceptible to the eye, the land being almost all a swamp.

I now proceed to describe the shores and islands of Lake Erie, with a few very general notices of its rivers.

The margin of the lake is composed of various materials, attaining various but moderate elevation.

From the Detroit River, near its head, to Long Point, (about 215 miles) the north shore consists wholly of clay, gravel, and sand-banks; the first being lowest, where I have seen them together. Sometimes, as at 16 miles east from the above river, and between Points Pelé and Landguard, they are in scarps 100 and 200 feet high, sinking, however, rapidly in the rear. In most cases the height is by no means so great, and at the most prominent points, such as Point Pelé, is usually but little above the surface of the lake. From Long Point (wholly sand) to the River Niagara, the shore (5—30 feet above the water-level, but still swampy) is defended at the outer angles of the indents, by large ledges or floors of limestone, while beaches of sand, or shingle very much rolled, line the inner portions; backed here and there with red clay, which, about five miles east of Grand River, is for a mile and a half 40 feet high, and supports a great morass, called the "Cranberry Marsh." This is replaced on the east to the end of the lake, by mounds and banks of pure sand, which in the western half of this interval often extend a mile or more into the interior, and sometimes rise 100—250 feet high, in confused groups, round backed and conical. The "Sugar-Loaf-Hill," 20 miles from Fort Erie, is one of these, and overlooks a great extent of ponds and swamps; with here and there a patch of ground fit for cultivation. Near the River Niagara the lake for great spaces is floored with rock.

For the first ten miles along the south shore, from the north-east end of the lake, shingle and sand prevail in low ridges, which advance 50 or 100 yards into the woods, and are based on red clay. From thence to the village of Erie, rocky cliffs, often 40 feet high, with ledges and shingle beaches interspersed, abound, and support the beds of clay, pebbles, and sand, mixed with branches of trees, fresh-water crustacea and shells, which constitute the level space under the dividing ridge. From the above village to Cleaveland (90 miles) low shores and sand banks are often met with, but accompanied by occasional ledges of rock. Cleaveland itself stands on a lofty scarp of clay and sand at the mouth of the River Cuyahoga, which enters the lake from among an assemblage of picturesque eminences of those materials. Two miles or so west of this town, high calcareous precipices, dipping vertically into deep waters, continue alongshore for many miles, and are succeeded westward by swamps and shingle beaches, from about the Black River to Sandusky; the east face of whose peninsula consists of rocky ledges; as also do most of the shores of the neighbouring islands. The island named from Point Pelé, not very remote, is a mere swamp, with a belt of sand girding its circumference. From Sandusky (including its bay) to the mouth of the Detroit River, (by Maumee River, &c.) the margin of the lake is almost altogether a marsh, with here and there a sandy beach and a few boulders.

In the form of its shores, Lake Erie obeys the law common to all the lakes of this chain (or parts of lakes) resting on secondary or diluvial materials. They describe large and regular, but shallow curvatures, unbroken by the multitude of land-locked coves and inlets, which beset a coast formed of a more ancient order of rocks. There are, in fact, almost no harbours in the lake. The headlands, where they do not terminate in long spits of sand, are often so obtuse as to disappear on near approach. The west shores of this lake are distributed into these extensive curves;—on the south side, so great as to be only two in number, from Maumee River to Hat Point of Sandusky Peninsula, while almost exactly two-thirds of the distance between Point Pelé and the Detroit (33 miles) is taken up by the unvarying concavity of Pigeon Bay, which is, indeed, much larger than is represented on the maps at present in use. The river Detroit enters Lake Erie by a very wide mouth, but without occasioning a distinct bay. That at the mouth of the Maumee River, (24¾ miles S.S.W. of the Detroit,) the only harbour on the Main between it and Sandusky, is 3¾ miles wide at the mouth, and has two small islands off its southern angle, which is called Cedar Point. There are seven feet of water at the bar (*Darby*, p. 203). It is very swampy within, and contracts gradually as it meets the river.

The north shore east of Point Pelé, independently of a great number of shallow indents, quite open to storms, is divided into three chief flexures, by Landguard and Long Points. Point Pelé is a narrow stripe of marsh and sand, ten miles long, in a southerly direction. Close to it, on the main, is a swamp, with a large pond at its centre. The gentle curvature which, together with Landguard Point, it contains, is 40 miles across. (*Purdy's Cabotia.*) The next curve, bounded by Landguard and Long Points, is 94 miles across. (*Purdy.*) It exhibits no features of note. Long Point projects eastwards into the lake for nearly 20 miles, making an arm that embays a large body of water. It is always narrow; and so much so near its junction with the main, as to allow batteaux to be hauled over, and, in fact, when the lake is high, its water flows across in the form of a creek. The curve (with a chord of 57 miles) of which it is the west angle, is only well marked in this its west end, the remainder toward the fort of the lake being almost straight, but subdivided as usual. A scrubby height of rock and sand jutting into the lake ten miles from Fort Erie, called Point Abino, is sometimes used as shelter against a south-west gale.

Such is the form of the north, or Canadian coast. On the south side, about 42 miles easterly of the west end of the lake, Sandusky Bay is a great deviation from the usual outline of its shores. Its entrance is nearly S.E. from the mouth of the River Detroit. I have several times visited it; but all I know of it is best summed up in the following words of Mr. Darby (*Tour*, p. 180, 181):—"The narrow strip forming Point Peninsula of Sandusky Bay is twenty miles long and two to three broad, and ends east by an additional low narrow point two miles long, meeting a similar long, low, narrow bar, projecting from the main. The last is called Sandy Point; the other, Point Prospect; the entrance being close to Point Sandy. The bay is about four miles wide, and so continues almost to its head, except at the Narrows, about five miles above the village of Sandusky. Its shores are but little raised above the water, and are, in some places, flat and marshy." The village of Sandusky is on the south-east side of the bay, and near the entrance. It is a miserable cluster of houses facing the water. The river Sandusky enters at the west end of the village. Another collection of deserted dwellings is about three miles below, called Venice. The whole vicinity is peculiarly subject to remittent and intermittent fevers of dangerous character.

From Sandusky Bay eastwards, with the exception of two slight inflexions, in which are placed the mouths of the rivers Huron and Cuyahoga, the south shore is remarkably straight. Ninety miles, however, from the east end, there is a long narrow tongue of land, called Presqu'isle, two miles south of the village of Erie, which, near its termination, inclines a little towards the main shore, and so forms a fine harbour, assisted by a broad bar, which connects the outer end of this low headland to the main: it has seven feet water. The village is built on a rocky cliff thirty or forty feet high, and is an improving and pretty place. There is another much smaller, but useful indention, fifty miles to the east, with a bar across its mouth drawing seven feet water. The village which it has created is called Dunkirk.

The islands of this lake, excepting two, are at its southern end. They amount to twenty-six including the cluster of five, called the Hen and Chickens, three of which are mere rocks. Point Pelé Island is the largest, being 8¾ miles long by 4½ miles in greatest breadth. It is compact, swampy within, and surrounded by a belt of trees. It is 7½ miles S.W. of Point Pelé, and has Middle Island 1¾ miles on its south; the latter being oval and two-thirds of a mile long. It is just within the British territory, and is 16 miles from the north main, and 10 from the south. A mile and two-thirds south of Middle Island is Ship Island, a mere rock, just within the jurisdiction of the United States. Cunningham Island 3⅕ miles N.E. from Sandusky Peninsula, is 3¾ miles long, by 1¾ in greatest breadth. North of Hat Point (the most northerly part of the Peninsula) is a groupe of nine islands, called the "Bass," "George," or "Put-in-Bay Islands"—valuable for the excellent anchorage and shelter they afford. Three of them are much larger than the others, and lie south and north of each other, and are therefore named in the Boundary Commission, "south, middle, and north" Bass Islands. The first is 2¾ miles from Hat Point, and is 4 miles long, by 1⅓, where broadest; but it is often very narrow. Distant from this, only half a mile, the Middle Bass, is 2¾ miles long, by a mile in greatest breadth, but its shape is also very irregular. Not quite a mile from the Middle Bass, the northern island of the three is a mile and three-quarters by one mile broad.

Moss Island, of this groupe, is to be noticed for its large deposit of strontian. It is 1000 yards long and a mile west of the South Bass.

Of the detached islets, the "Eastern," "Middle," and "Western Sisters," are useful in stress of weather; the Eastern Sisters are two in number, each 500 yards long or thereabouts, and 6½ miles N. by W. from the North Bass. The Middle Sister is half a mile long, and 12½ miles N.W. from the same island; and the Western Sister is 14 miles west from it, and two-thirds of a mile long*.

Excepting Point Pelé Island, and the Eastern Sisters, these islands are high and rocky. They are uninhabited, except by runaway slaves, or outlaws for a season.

Of the two islands mentioned as being in the eastern part of the lake, one is in the bay formed by Long and Turkey Point, near Point Pottohawk: it is small. The other is, as far as I can learn, unnoticed by any chart or by any writer. It is close to the north shore two and a half to three miles east of the Ouse or Grand River, and is called "Gull Island." It is low, floored on limestone, indifferently wooded, and about one-third of a mile long.

Of the rivers entering Lake Erie, the Detroit (to be described elsewhere) is the largest. On the north shore, from this river to Orfordness, the vicinity of the Thames and of St. Clair renders them few and of insignificant size. From Orfordness they continue to be small as far as Catfish and other creeks, east of Port Talbot; the interval between the Thames and Lake Erie being there much increased. Otter Creek is of considerable magnitude, and navigable for 40 miles by boats of 20 tons burthen (*Gourlay's Reports*); but its utility is diminished by the extreme shallowness of its mouth. The largest river on the north shore, next to the Detroit, is the Ouse, 35 miles from the north-east end of the lake. The British have a naval establishment at its mouth, but now consisting of log houses for a few soldiers and sailors, and the rotting hulls of one or two schooners. It discharges a reddish water, and is about 200 yards wide at its mouth, with a sand-bar of five feet water. It rises 80 or 90 miles (direct) in the north, in unsurveyed woods, and in the remote township of Garafraxa, where it is not far from the streams belonging to Lake Simcoe. The country it passes through is low, but fertile in the extreme, and possesses, sixty miles from the lake, some flourishing settlements.

The principal rivers of the south shore are the Maumee, Sandusky, Cuyahoga, Grand River, and Buffaloe Creek. As they are not involved in geological details, and as they have been

* These admeasurements are taken from the maps of the British and American Boundary Commission.

described repeatedly, I refer to the map for their position and length, and for further details to Kilbourne's Gazetteer. I may here mention that all the rivers of Lake Erie have sand-bars at their mouths, preventing their being used as harbours for vessels of considerable burthen.

GEOLOGY OF LAKE ERIE.

Extensive deposits of loose transported materials almost altogether cover the fixed rocks of Lake Erie. The suggestions thence arising have been already noticed in a general view of the geology of the Canadas, transmitted to the Geological Society. I shall now, in a very imperfect manner, state their position and nature.

On the south shore, Professor Dewey* reports the highest of the two banks or ridges, visible from the lake, to be composed, in many parts, of the same substances as the present margin—that is, of sand and gravel. The flat country at the foot of the slope is every where (as at Dunkirk, Presqu'isle, Cleaveland, &c.) composed of rich loam, with occasional and large patches of sand, clay, and gravel. Professor Dewey saw a well in the township of Weightsburgh, exhibiting the following appearances. "From the top of the ground, the first three feet is a sandy loam, then a coarse gravel, and then a layer of small stones of the same kind, which we find on the present lake shore: these three layers make about five feet. Beneath these are successive strata of the same kind, to the bottom of the well, which is about 20 feet from the surface. At the bottom of the well, in the coarse gravel, and in a spring or rather subterranean brook, there was found a piece of (apparently) bass-wood, between two and three feet long, and two or three inches in diameter. It was evidently a limb of a trunk, which is now buried in the pebbles and gravel below: its direction was perpendicular, and its texture so little impaired that it was with difficulty broken off. *Lobster-shells*, *cockle-shells*, and *clam-shells*, of the same appearance, are found this depth from the surface, as are now found on the lake shore."

The deep channels of the rivers entering Lake Erie from the

* Mitchell's edition of Cuvier's Theory of the Earth, p. 417.

south, are usually cut through beds of water-worn matters, which are particularly large about the Huron. Mr. Caleb Atwater (*Silliman's Journal*, vol. ii., p. 245) mentions the discovery of a mammoth's tooth on the beach of Lake Erie, in the neighbourhood of this river—of another at Dayton, in the Great Miami, and a third on the Scioto rivers. Mr. Keating* represents the country west of this lake to be covered with granitic boulders, and the soil, likewise, to be studded with pebbles. In Michigan territory and Ohio, and especially on the shores of the Detroit river, and of Lake Erie near the Vermilion river, cinnabar occurs in the form of a red and black sand, but it is usually more abundant in banks of fine ferruginous clay. Near the mouth of Vermilion River, it is in the form of a very fine red powder, or in grains and small masses, disseminated in clay. It yields, by distillation, about 60 per cent. of mercury†. I have collected sand, on the north-west shores of Lake Erie, and near the Grand River, which I conceived to be similar to that of Mr. Stickney; but the packages containing it are lost.

I have already observed that the north coast of this lake, from the river Detroit to Long Point, according to Colonel Hawkins (68th Light Infantry) and others, is wholly in banks and beaches of sand, gravel, and clay. I have seen them 16 miles east of that river: there the under portions are greyish blue, and are both amorphous and in horizontal flakes; their upper parts are of sand and primitive pebbles, capped with loam, bearing oak, sumach, and elm. At the west end of these cliffs, the pebbles become very large, while on the east they decline into low mounds of fine sand. On the beach in their front are rolled masses of greenstone, porphyry, and other rocks belonging to Lake Huron. The sand on the north-east shores of the lake, in such high mounds, is coarse, but nearly pure. The beds of red clay in that neighbourhood, however, contain many angular masses of black limestone and primitive blocks; and the beaches, here and there, have large fragments of Labrador feldspar and white crystalline marble.

* Expedition to the Source of St. Peter's River, vol. i. p. 141.
† Stickney. Silliman's Journal, vol. i. and ii.

The situation in the geological series of the fixed rocks of Lake Erie (limestones and sandstone) is doubtful at the south-west end, and unexamined in the centre—between Presqu'isle and Sandusky; but at the north-east end of the lake, it has been well ascertained. The chasm of the river Niagara lays bare their connection with the saliferous sandstones below; and the State of New York continues the development down to the primitive rocks at the east end of Lake Ontario. These strata are in succession from above, as follows:—

Calcareous Shale (pyritous)	Lakes Erie and Cayuga.
Cornitiferous Limestone	Erie and River Niagara.
Geodiferous Limestone	River Niagara and Lake Ontario.
Argillo-calcareous Slate ("Calciferous" of Eaton)	Niagara, Genesee, Lockport.
Ferriferous and Saliferous Sandstone . .	Niagara, Genesee, &c.
Millstone Grit (Eaton)	LakeOntario, near Utica,&c.
Metalliferous Greywacke (Eaton) . . .	Ditto.
Carboniferous Limestone (Metalliferous of Eaton)	East end of Lake Ontario.
WhiteSandstone and Conglomerate Quartz ——— and Calcareous. . . .	North-east end of Lake Ontario, and its outlet.

While the gneiss supporting these rocks, wherever I have seen it, is at an high angle, they are very nearly horizontal; inclining from the nearest primitive ranges, so as to allow them to appear in succession, at intervals, in the lakes and their vicinity at the same level, the oldest being on the east and north, and the newest in the opposite quarters. Their extent south and west are at present quite unknown. They are spread out in close contact (many, if not all, passing into each other) in very slightly concave layers of vast superficial dimensions, but of comparatively small thickness. It is probable that on the north of Lake Erie the strata dip south-easterly from the apparently greater elevation of the salt-rocks of that lake above those of Ontario. It is almost certain that all these strata underlay both these lakes, and their intervening isthmus; for although the salt rocks have not yet been seen on the north, numerous brine-springs sufficiently attest their presence.

The newest rock in Lake Erie is the shale placed at the head of the above list. It has been noticed by Mr. Amos Eaton, and described by him thus*:—It is a siliceous or calcareous grey rock, with aluminous cement, either slaty or in blocks, and abounding in iron pyrites and petrifactions; the latter sometimes being composed of the former. He has named it "secondary pyritiferous rock." In Cayuga Lake, according to Mr. Eaton, these strata contain numerous thin horizontal seams of bituminous and sulphureous coal, embraced by narrow layers of iron pyrites, (which last alone is found in Lake Erie;) where-ever these are exposed, the adjoining rock was more or less covered with Epsom salts, copperas, and alum.

"This rock occurs on the north shore only of Lake Erie, from its south-east corner in Hamburgh, eight miles from Buffalo, with very little interruption, to Sturgeon Point. It appears from the "Travels of Engineer D. Thomas, that the same rock borders the lake to Cattaranghus, and probably much farther."—(*Eaton*, p. 143.) In fact, according to Dr. Mitchell, of New York†, it exists at Presqu'isle, 90 miles from the foot of the lake. Mr. Eaton continues, "We see but little of the shelly rock, rarely exceeding twelve feet in height, until we come to within about three miles of Eighteen-mile Creek. On both sides of this creek the rock is very similar. The loose scaly slate occupies about 25 feet of the lower part, and the ranges of square-faced blocks of limestone occupy about the same number of feet above, frequently interrupted by the slate." (*Eaton*, p. 143.) The slaty part is remarkable for its finely preserved organic remains: both kinds, however, contain a great and equal number. They are terebratulæ, favasites, turbinolie, milleporites, trilobites encrinites, orthocenæ. (*Eaton.*) Mr. Leseur, of Philadelphia, found in the compact portion of this limestone, near Eighteen-mile Creek, a univalve, which he has erected into a new genus, with two species. He describes them thus:—Genus Maclurite. Shell discoidal, much depressed, unilocular; spire not elevated, flat; umbilicus very large, with a groove formed by the projection of the proceeding whorls; not crenulated.

Species I. M. magna. Shell obtusely carinated on the exterior upper edge, whorls rapidly increasing in size; aperture

* Geological Survey of the District adjoining the Erie Canal, p. 38.
† Minerva of New Work, 1824.

on the left, irregularly oval, horizontally depressed above, lips not reflected.

II. M. bicarinata. Whorls acutely carinated on the middle above, and obsoletely carinated beneath.—*Journal of Nat. Science*, Philad. vol. i. p. 311.

The shale " embraces numerous globular concretions, consisting chiefly of a kind of wacke.

" On breaking a great number of them, which were from an inch to a foot in diameter, I found that they all contained some sort of nucleus different from the inclosing matter. The nucleus is frequently the irregular fragment of a petrifaction; sometimes of limestone, very unlike that variety which is connected with this stratum in layers." (*Eaton*, p. 145.) The iron pyrites " is mostly attached to the under surface of the layers, in a stalactitic or mammillary form. It is a bright golden yellow coating, and forms immense quantities of petrifactions." (P. 144.)

This well characterized rock is seen at Black Rock, on the River Niagara, to rest upon the chertzy limestone, next to be described, which prevails on the north shore of Lake Erie for at least 55 miles from that river; but only in platforms and a few ledges. Whether the transition be sudden or gradual, I am unable to say, never having seen the pyritous shale. This limestone, denominated by Mr. Eaton " cornitiferous," is finely displayed at Black Rock, near the head of the River Niagara. At this village the river bank is a low cliff, surmounted by a rough, steep, grassy slope, cut through, near Major Fraser's house, by the channel of a creek. Just above the ferry here, there are, at water-mark, or a little above, six feet of fine grained blue and pale brown, very conchoidal limestone, without chertz or other foreign matters; then gradually horizontal streaks, of a pale grey colour, appear in the limestone, which, rapidly increasing in number and size, produce, for nine feet higher, a very pale brown rock, filled with small geodes of copper pyrites, calcspar, and a large quantity of foliated strontian, imbedded in thin angular masses of a white colour. I observed, in this part of the cliff, several small producti; but the most numerous traces of animal life are the cavities impressed by the casts of turbinoliæ, which have subsequently disappeared. These impressions are small, and are distributed in thin horizontal layers. This pale stratum ceases suddenly upwards, and, in a few parts, is replaced at once by a calcareous pudding-stone, coated and penetrated by a coppery-green substance. The nodules are small, and too much altered for me to ascertain the stratum to which they might belong. I saw them in 1824. The extensive works now carried on at Black Rock may have, by this time, laid open a larger and less disintegrated portion of this pudding-stone. The most common covering of the pale limestone is a black shale, usually six inches thick, horizontal, with gentle undulations; succeeded by a dark-brown limestone, with dark chertz; at first in small kidneys, which soon coalesce and form lumps, strings, and finally flakes and layers of irregular thickness, and with a waving horizontality. The chertz, sometimes pale, increases in quantity upwards, until (near the level of the village) it becomes the greatest part of the rock, in thick shining seams, of almost pure flint; the calcareous intervals also containing small masses of chertz. Some of the larger knots of chertz shew a distinct madreporic structure, with small cells. Interspersed in the calcareous portions are producti, terebratulæ, corallines, various retepores, turbinoiæ, all composed of chertzy. I have seen a calymene (Blumenb.?) from this rock. Some hundred yards above the spot where I made the above observations, the chertzy limestone is at the water-mark; a fact accounted for by the difference of level at the two places, assisted, perhaps, by a slight dip in the rock itself.

This kind of limestone I carefully traced from Black Rock, along the north shore of Lake Erie for 55 miles, and from the nature of the country was enabled to see that, for several miles westward, no remarkable change took place. About, and for a few miles to the west of Fort Erie, the rock is excessively charged with flint, great portions of which being interspersed with the fossils occurring at Black Rock. Captain Bolton, R.E., shewed me some fine trilobites, and a trochus from this place. On the way to Grand River it becomes paler occasionally, and varies much in the amount of its chertz. From Forsyth's Point to Steel's Tavern, (seven miles) the organic remains are not so plentiful; but are of the same kind. West of Grand River, the limestone only seemed to differ in the diminished proportion of chertz, this substance, however, being still disposed in the same manner as before. The fossils here are both of chertz and granular carbonate of lime. The turbinoliæ differ much in their size. They are all fragments, and they are of a tapering form. Two feet are here a common, but I believe hitherto an unexampled, length. They are often only as many inches. The greatest diameter that I met with was four inches. They are of very various shapes; as straight, gently or very much curved, and they frequently are bent suddenly at right angles, which I have never seen anywhere else. The coral often occupies many square feet of the rock, and are either placed vertically, or radiate from a centre. Common cellular madrepore, in round balls of radiating cells, 2 to 12 inches in diameter, are frequent. Millepore coral, of large size and length, with triangular foramina, is very common, as are incrinites. I met with a few producti similar to the P. lobatus of Sowerby. Pl. 418.

This chertzy limestone is sufficiently described in the above paragraphs *. According to Mr. Eaton, it is repeatedly seen in the state of New York. Its connexion with the inferior rocks in Lake Erie has not been investigated, and, I believe, is completely concealed; but at Black Rock, close at hand, we have seen it to cease suddenly, and to rest in some places on pudding-stone. I consider it and the next rock below to be deposited nearly at the same time; from their conformableness, the similarity of their contents, and from the small quantity and partial distribution of the pudding-stone.

Of the brown limestone in the middle of the south shore of this lake, I know very little. I have seen it frequently from a short distance, bounding the lake in pretty thick horizontal layers. Mr. Maclure visited it many years ago; and found caryophyllites and large terebratulæ in it. Eight miles east of

* Sir Peregrine Maitland mentioned to me, that a considerable vein of galena is found in the banks of the Grand River, 50 to 60 miles up. It is possibly in this limestone, as the river, to its head, passes over calcareous rocks.

Cleaveland, there is, a little way from the lake, and some feet above its level, some strata of greyish-white sandstone, rather fine-grained, and moderately hard. It is used for grindstones. I saw no organic remains in the fragments shewn to me. I had no opportunity of ascertaining its relations with the calcareous rocks around.

The limestone of the head of the lake, including Sandusky Bay and Peninsula, Put-in-bay islands, the mouth of the River Detroit, and its contiguous islands, Celeron and Grosse Isle, is nearly the same everywhere. Its relative age is rendered doubtful by its not having yet been seen in juxtaposition with other rocks, and by the little aid afforded by its mineral and fossil contents. The following circumstances, however, seem to place it just above the sandstones connected with the salt formation. These are, its mineralogical resemblance to a rock on the River Niagara, of known relations, its containing considerable quantities of gypsum, the proximity of salt springs, and its great remoteness from primitive formations, 280 to 300 miles.

It is of a pale grey, or of a greyish straw colour, homogeneous, granular, and rather soft. It is in thick, apparently horizontal layers, and is seldom slaty. It is occasionally cavernous. In the immediate vicinity of masses of strontian, the specific gravity is sometimes 3 and 3.3, and then it is of a white colour. In other places, it is, as usual, about 2.6. Mr. Bird, Astronomer to the Boundary Commission, met with a gray limestone here, which was perfectly dry, and nearly inodorous, when cold, but which, when warmed, became covered with a slight exudation of petroleum, and had the smell peculiar to that substance.

The Bass islands contain a cave, which is entered by a round hole a yard wide, gradually widening for 50 feet, when it opens into a circular space 100 feet in diameter and 7 feet high. The roof is studded with brown stalactites, frequently hollow, and seldom more than three-fourths of an inch thick, or longer than three inches. The floor is covered in a similar manner*.

* This account I received when near the place, in 1819, from Lieut. Dix, aide-de-camp to the American General, Brown.

Persons at Moy (opposite to Detroit) have shewn me conical stalactites from the cavern, 10 inches long by 7 inches at the base.

For considerable spaces, this limestone is free from foreign matters of any kind. At Sandusky village, and Peninsula, and, I doubt not, at many other places, it contains a profusion of organic remains, consisting of terebratulæ, terbinoliæ, fragments of trilobites, probably asaphs, and cellular madrepores, exactly the same as those found in the blocks of pale limestone in the large diluvial mounds of Corlaer's Hook, near New York, and accompanied by producti. They are in balls, composed wholly of cells radiating from a point in their circumference. The cells have sides, and are filled with white calcspar.

Near Hat Point, on Sandusky Peninsula, I met with a very fine cast of a Cardium Hibernicum.

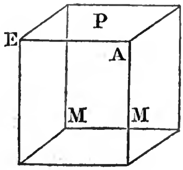
My friend, Dr. Lyons, staff surgeon, favoured me with another from this part of Lake Erie, but the exact spot I do not know.

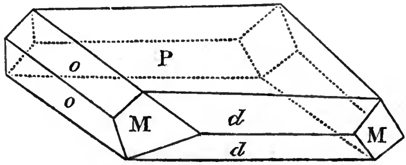
About a mile above Amherstburgh, on the river Detroit, and about half a mile from the river bank, there is, in the woods, a quarry of this limestone abounding in fossils; but I only observed some turbos, and the smaller valves of producti, in addition to those mentioned above. I have to thank Assistant Commissary-General Hare for a small but valuable collection from this quarry.

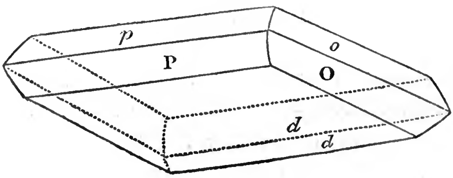
Of accidental minerals, I have only observed strontian: it is sometimes associated with calcspar in trihedral pyramids, and in rhombs. Major Delafield has shewn me fibrous gypsum from Sandusky Bay, but I am ignorant of the circumstances under which it exists.

The chief localities for strontian in this limestone are, Moss Island, the Miami River (Schoolcraft), and Celeron and Grosse Islands. It may occur elsewhere, but probably not in great quantities, or, as in the case of the deposits now known, it would have been discovered by the American surveying party of the Boundary Commission—it is the foliated variety.

In a cliff 50 feet high, on the east side of Moss Island, and at mid-height there is a vein (or rather ramifying mass) of it four feet * in greatest thickness, and extending nearly horizontally for 18 or 20 yards, terminates a foot thick. It consists of promiscuously aggregated bundles of crystals united laterally, closely compacted together at the large end of the vein, more loosely in other parts, and having interstices lined with free crystals. The mass is of a white and bluish white colour, imperfectly translucent. The crystals composing it are from one to four inches long, and from one-fifth to half an inch in breadth. Major Delafield states the cavity in the massive end of the vein to be circular, and three feet in diameter. He found its arch, sides, and floor, to be beautifully studded with pendent crystals, from six ounces to six pounds in weight, and covered with a considerable quantity of brown clay and loam. These crystals are usually flattened hexahedral prisms. In the Journal of the Philadelphia Academy of Natural Sciences (vol. ii. p. 300) Dr. Gerard Troost has described some of these. He finds their primitive form to be a straight prism, with a rhomboidal base, of which the angles are 104°, 48′, and 75°, 12′ (fig. I.) He names a variety of this—" Sulphat of strontian trapézienne " $\underset{d\,o\,P}{\overset{21}{AEP}}$ (fig. II.) The inclination upon the faces are, of







* I was at this spot in 1821 for a few minutes in company with Major Delafield; but, being unexpectedly called away, was prevented from making accurate observations. The dimensions in the text are from Major D. *Silliman's Journal*, vol. iv. p. 279.

O upon P, 128° 31′—of O upon o, 77° 2′—of O upon the returning face, 102° 58′—of d upon d, 101° 32′.

Another is, "Sulphate of strontian épointée $\overset{21}{\underset{\mathrm{M\,d\,o\,P}}{\mathrm{MAEP}}}$" (fig. III). It is "the former, having the solid angles deeply truncated, forming faces parallel to the sides of the primitive rhomboidal prism. The inclination of M upon M is 104° 48′;—that of the other faces coincides with the inclinations of the 'trapezienne.' The crystals are translucent in a great degree, approaching to transparent, and of a bluish-white colour. The size of the crystals is large. I have seen fragments belonging to crystals which must have been four or five inches long, belonging to the sub-variety 'trapézienne élargie.' The surfaces of the faces o o are usually dull, of a more opaque milky white than the remainder of the faces, which have a remarkably fine lustre; the faces corresponding with those of the primitive rhomboidal prism, as P and M, display a fine iridescent colour."

The strontian of the Miami river occurs at Fort Meigs, and was first made known by Mr. Schoolcraft. I am not aware of any account of this locality having yet been published. The crystals Mr. Schoolcraft showed me are precisely the same as the smaller of those in the cavity at Moss Island in Lake Erie.

In Celeron and Grosse Islands it is found in small balls and geodes; at the former in confused crystalline masses—at the latter, it lines cavities in crystals seldom exceeding an inch and a half in length, and terminating by a lanciform opaque white point, more or less disintegrated. Their colour is a fine sky-blue, and their lustre and transparency are considerable. The strontian is only sparingly scattered through this limestone. It is best characterised and most plentiful in a quarry at the lower end of Grosse Island. The reefs of rock, crossing the river Detroit above the adjacent island of Boisblanc, show faint traces of strontian. It is remarkable that wherever there are organic remains there is no strontian, and *vice versâ*; but this only applies to Lake Erie and its vicinity—for the limestones of Lakes Huron and Simcoe exhibit both together in the same hand-specimen.

All that is known of the fixed rocks of Lake Erie, I believe I have now stated. It remains to be mentioned that on its north coast, in Orford and Camden, there are brine springs.—(*Gourlay's Reports*, vol. i. p. 295.) These townships are on the river Thames, and not far from Lake St. Clair. Salt springs are also met with, accompanied by gypsum, in Norwich (*Gourl. Rep.*, v. i. p, 333), at the head of Otter Creek near Long Point, and in Canboro, Haldimand, Dumfries, and Waterloo, on the river Ouse, together with very large beds of granular gypsum in Dumfries, which have been for some time quarried for agricultural purposes.—(*Gourl. Rep.* v. i., p. 382, 383, 386, 453.)

On the south shore of this lake, Mr. Kilbourne, in his Gazetteer of Ohio, says " that salt springs have been discovered and worked to a very considerable extent on the waters of the Killbuck in the county of Wayne*—on Yellow Creek, in Jefferson county†—on Alum Creek, in Delaware county—on Muskingham river, a few miles below Zanesville, and in various other places."

No steady inquiries have been made for indications of salt in these remote countries, which are still principally wood and morass. Those which chance has brought to light have never been recorded. We need not, therefore, be surprised to find our enumeration of localities so scanty.

Bog iron ore is abundant in the wet grounds over the whole north shore of Lake Erie, and probably on the south also. The chief townships in which it is found, in the Canadian territories, are Westminster, Dorchester, Norwich, Burford, Middleton, Charlotteville, Woodhouse, Bayham, and Bertie. —(*Gourlay's Reports.*)

Petroleum springs are reported by the Moravian missionary Denkè in Orford and Camden.—(*Gourl. R.*, v. i., p. 295.) Springs of sulphuretted hydrogen are numerous everywhere.

* Fifty-five to sixty miles from Sandusky.

† Seventy to seventy-five miles from Lake Erie at Presqu'isle.

Account of a Luminous Animalcule. *By* Captain Home, R.N. F.R.S., &c. in a Letter to the Editor.

Ham, Dec. 6, 1828.

My dear Sir,

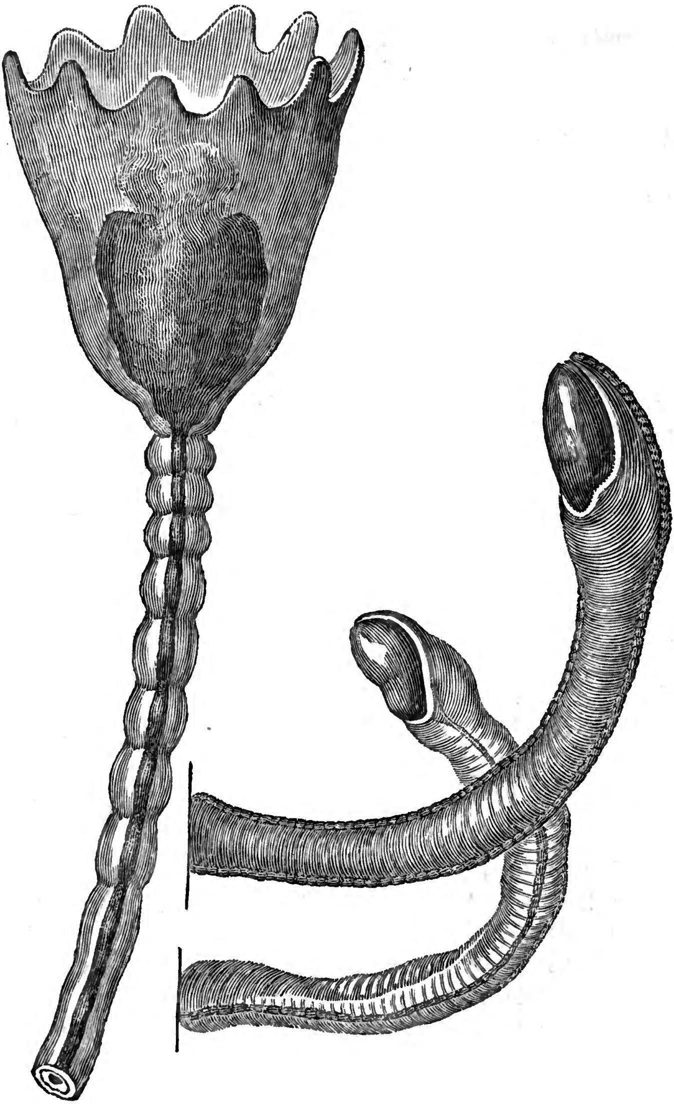
According to your desire I send you the following account: Being at Lancing on the coast of Sussex in October 1827, I considered it a good opportunity to examine into the cause of the light which I had often before observed so brilliant in the sea-weed thrown upon the beach.

It had blown hard from the south-west on the 8th and three following days, and so great a quantity of weed had been thrown up, that the beach was covered with it to more than two feet deep in many places; it appeared for the most part to be of the same species. After it was dark, I collected a small quantity of the most brilliant, and found it always to be that which had been left at the first of the ebb, and was only moist, rather than that just washed up. This portion was taken into a dark room, where I picked out a single spark, the brightest that could be found, and carried it to the next room where there was a candle. After seeing the shape of the weed in my hand, I returned and picked off every thing, leaving that only on which the light appeared, keeping my eye on it through a common lens, till I was so near the candle as to lose the light upon the weed. In this manner I ascertained that it was produced by a minute body adhering to the sea-weed, which I am informed by Mr. Brown is *Sertularia volubilis* of Ellis (*Clytia volubilis* of Lamouroux.)

The light would remain steady sometimes for about five seconds, often less, and when it ceased, was renewed by touching it with the finger. I immediately made a sketch of it as it appeared in a small microscope I had carried with me.

I had still my doubts that what I had seen was the cause of the light, but was convinced of it after repeating the same observations for the five evenings I remained at Lancing.

Being anxious to examine these bodies in daylight, I entirely darkened a room, but was surprised to find they gave no light whatever; yet the same weed, kept till the evening, was as brilliant as any I had seen.



On my return to town, I brought with me a basket full of weed fresh that morning from the beach, and forwarded it the same evening to Mr. Bauer, who employed that and the three following evenings examining its contents. He has been so kind as to make a drawing of it, which will explain their appearance far more clearly than any thing that I can say*. They are engraved and described by Ellis in the *Philosophical Transactions*, vol. lvii. and in his *Corallines and Zoophytes;* but as I have no where seen them described as luminous, I have ventured to give you this account, as it is interesting to you, and I am, my dear Sir,

Very truly yours,

EVERARD HOME.

The Newly Discovered Temple at Cadachio, in the Island of Corfu, illustrated by William Railton, Architect. *Folio,* London. Priestley and Weale. 1828.

AT Cadachio, situate about a mile and a half to the south-east of the city of Corfu, in the Ionian island of that name, are springs which rise in a ravine of Mount Ascension, at the foot of which, on the edge of the sea, is a fountain or reservoir into which the water they supply is received, and which is resorted to by the ships which frequent the adjacent port, in order to fill their casks.

In the autumn of 1822, the springs, upon which, among other vessels, those of the British navy depend for water, were unusually low; and the government, in consequence, directed that a party of engineers, under the orders of Colonel Whitmore, should endeavour to free the mouth of the ravine from that accumulation of soil which appears to be annually brought down, and of which the quantity appeared to have impeded or, perhaps, diverted the descent or course of the waters. In the process of excavation, a Doric column was discovered *in sitú;* and that event induced Colonel Whitmore to make a search, by which was brought to light the ground plan of a temple. The columns of the west, or land-side, were in their places; as were also five on the south, and two on the north, but in a very mutilated state. "The walls of the cella have been re-

* We have taken our two wood cuts from this beautiful drawing, and regret our inability to give the other figure.

moved; in the interior there are some curious remains of an altar;" but "the remainder of the building, together with the cliff upon which that part of it stood, has fallen into the sea. The front of the building faced the sea, with an east-south-east aspect; and the platform at the top of the cliff, upon which the temple was erected, stands at the level of about a hundred feet above the sea.

Colonel Whitmore, to whose classical taste and erudition Mr. Railton and the public are indebted for some account both of the discovery, and of his own conjectures concerning the history of the temple, relates, that "the excavation has further brought to light several female heads, and a small leg, in terra cotta, which," the Colonel observes, "might have been either votive offerings, or portions of the jointed toys not unfrequent in the tombs of children: there have been also found earthen cones, the foot of a statue, unguentaries and libatories, brazen pateræ, scarabæi, glass beads, ivory, copper, iron and lead; a bronze four-spoked wheel (which was the emblem of Nemesis), weights, the heads of arrows, pieces of ear-rings, and a number of coins of Epirus, Apollonia, Corinth, Syracuse, and Coracya. The cones are supposed to have been attached to the necks of cattle, and the scarabæi to have been worn by the soldiers as amulets." The temple was roofed, and covered with tiles, many of which have proper names impressed on them; and probably they were those of the chief magistrates during its construction or renovation. Among them are the following:—Aristomenes, Thersia, Damon, &c.

From the forms of the letters composing these names, as well as from the architecture, its proportions, &c. &c. a very remote construction is attributed to this temple; and Colonel Whitmore, from a comparison of these latter peculiarities with those which are remarked in the Parthenon, and in the Temple of Theseus, ventures to assign it to a similar date; that is, to about the middle of the fifth century before Christ. As further evidence, however, of the antiquity of this temple, Colonel W. appeals to an inscription extant in the Museum at Verona, and already edited by Maffei, in his Museum Veronense; which inscription appears to refer to this identical temple at Cadachio.

The inscription has been translated from the Doric into Latin by Maffei, and into Italian from the Latin, by Mustoxidi; it is here presented to us by Mr. Railton, in the classical Greek, with the English translation of Colonel Whitmore:

. . ΟΙΔΙΚΑΣΤΑΙΚΑΙΚΟΙΝΟΙΕΥΔΟΚΟΥ
. . ΚΑΙΥΠΕΡΤΑΝΠΟΛΙΝΤΩΝΣΥΝ
. . ΝΑΙΚΑΝΕΙΜΕΝΑΠΟΔΙΚΟΝ
. , ΑΤΟΣΤΑΣΣΤΕΓΑΣΤΟΥΝΑΟΥ
. . . ΑΑΠΤΗΤΟΡΥΜΑΤΟΝΤΟΙΧΟΝ
. . . ΟΣΕΠΙΣΚΕΑ ΞΕΙΝΤΑΝΠΟΛΙΝ
. . . ΑΝΑΛΩΜΑΤΩΝΑΝΥΠΟΔΙΚΟΝ
. . . ΤΗΡΙΩΝΑΠΕΡΙΤΑΝΚΟΡΧΥΡΕ
. . . ΑΝΕΚΤΑΣΟΙΚΙΑΣΕΙΣΤΟΝΑ
. . . ΡΥΜΑΤΟΣΤΟΥΡΕΟΝΤΟΣΑΠΟ
. . . ΕΠΙΤΟΝΑΩΡΙΟΝΣΤΡΕΨΑΙΔΕ
. . . ΑΣΣΚΕΟΘΗΚΑΣΤΑΜΠΟΛΙΝ
. . , ΕΜΒΑΛΕΙΝΔΕΚΑΙΕΙΣΕ . .
. . . ΡΑΟΒΕΛΙΣΚΟΝΟΡΘΟΝΟΠΩΣ
. . . ΗΑΝΑΓΡΑΦΗΤΩΕΙΣΣΤΑ
. . . . ΤΟΝΤΟΙΧΟΝΕΝΤΩΙΔΑ
. . . . . ΕΘΗΕΙΣΤΟΙΕΡΟΝΤΟΥΑ
. . . . . . ΟΣΤΑΝΕΠΙΜΕΛΕΙΑΝ
. . . . . . ΑΝΑΓΡΑΦΗΑΥΤΑΑΕΠΙ
. . . . . ΤΙΕΣΤΙΕΠΙΤΙΜΙ
. . . . ΕΡΟΙ

"This inscription commemorates the sanction of the Corcyrean republic of the construction of certain public works. It details the prices of or costs of tin, lead, brass, cartage, excavation, and workmanship; the expense of a brazen serpent, of nitre for the altar, the erection of an obelisk, and *a retaining wall* built by Metrodorus. By it the magistrates approve of what has been executed. They state also the removal of the roof of the temple; *the abduction of the water-courses, lest the force of the springs should injure the retaining wall;* and although much is obliterated, intimate that the impetus of the flowing waters was to be diverted from the temple towards the docks and store-houses."

Many other particulars are here given in the publication of Mr. Railton, inclusive of Colonel Whitmore's reasons for supposing the temple to have belonged to Apollo, rather than to Æsculapius, as imagined by Maffei. In either of these cases, however, it appears certain, that the very *springs* which draw modern attention to the spot, were the occasion, from an imputed sanctity, of the building this temple in the remarkable situation which it occupies; the mouth of a ravine, where anciently the water and now the soil brought down, were the subjects of continual difficulty and repair. Col. W. supposes the "nitre" of the inscription to mean natron; and observes, that it is "remarkable that the altar still exhibits, after the lapse of twenty-two centuries, fragments of a coating that seems to contain soda."

Mr. Railton being at Corfu in the spring of 1825, waiting for an opportunity to proceed on a professional tour through Greece and Egypt, found the engineers at work, for the second time, in clearing away the soil, which, coming down the ravine, had again buried the temple; and, upon that occasion, made his measurements and drawings. Returning to England at the close of last year, and finding that no detailed drawings had hitherto been given of it, he has been induced to offer the plates which compose the elegant fasciculus before us.

The plates are five in number, and reflect the highest credit upon Mr. Railton's professional taste, talents, and studies. The first discovers the ground-plan of the building; the second, a restoration of the part toward the sea; and the third, fourth, and fifth, details of the order, with the addition of a "Doric capital found in a ruined church within the French lines; and which," as observed by Mr. Railton, "has very much the character of the columns of the pseudodipteral and hexastyle temples at Pæstum." It appears certain that in this publication, Mr. Railton has afforded the materials of much pleasure and information; not only to his professional brethren, but to every lover of the fine arts, and to every classical student.

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

i. *Elementary View of the* UNDULATORY *Theory of Light. By* Mr. FRESNEL.

[Continued from the Number for September.]

Colours of Crystallized Plates.

WHEN a pencil of polarised light passes through a rhomboid of calcarious spar, the principal section of which is parallel to the plane of polarisation, it is well known that the extraordinary image disappears: but it is reproduced when we place before the rhomboid a crystallized plate, possessed of the property of double refraction, and of which the principal section is neither parallel nor perpendicular to the primitive plane of polarisation: its intensity becomes even equal to that of the ordinary image, when this principal section makes an angle of 45° with the primitive plane. In this case, as well as in the others, both the images are white, if the interposed plate is thick enough, that is, if its thickness is not less than one fiftieth of an inch, when it is of rock crystal or sulfate of lime; but when it is thinner, the two images are tinted with complementary colours, the nature of which varies with the thickness of the plate, though their brightness only is changed when it is turned round in its plane, keeping it always perpendicular to the incident light.

This brilliant discovery, for which we are indebted to Mr. ARAGO, has since occupied much of the attention of all the natural philosophers of Europe, and particularly of Mr. BIOT, Dr. YOUNG, and Dr. BREWSTER, who have done the most in discovering the laws of the phenomena. Mr. BIOT first observed that the colours of the crystallized plates followed, with respect to their thicknesses, the same laws as the colours of the Newtonian rings; that is to say, that the thicknesses of two crystallized plates of the same nature, which exhibited any two tints, were to each other as the thicknesses of the plates of air which reflect similar tints in the coloured rings. A short time after the publication of the valuable memoirs of Mr. Biot on this subject, Dr. Young remarked, that the difference of the paths of the ordinary and extraordinary pencil, transmitted by a crystallized plate, was precisely equal to that of the paths of the rays reflected at the first and second surface of the plate of air which affords the same colour; and that this numerical identity remained unchanged in every possible inclination of the rays to the axis of the crystal. This very important theoretical observation, which excited but little attention when it was first published, served to give a new proof of the fecundity of the principle of interference, as it established the most intimate numerical relation between two classes of phenomena, greatly differing from each other, as well in the great disproportion of the thickness of the crystallized plates, and the plates of air which exhibit the same colours, as in the diversity of the circumstances necessary for their production.

Dr. Young demonstrated only by his calculations that the colours of the crystallized plates were to be attributed to the interference of the ordinary with the extraordinary undulations: he did not attempt to explain in what circumstances this interference was possible, or why it was necessary that the light should be polarised before its entrance into the crystal, and should receive a new polarisation after its emersion; or how the intensity of the tints varied with the relative direction of the primitive plane of the principal section of the plate and that of the rhomboid. The principal object of the memoir which I submitted to the Academy of Sciences, the 7th of October, 1816, and of the supplement added to it in the month of January, 1818, was to explain the influence of these different circumstances, and to represent the laws of the phenomenon by general formulas, which should give for each image the intensity of the different kinds of coloured rays. I shall now explain this theory, continuing to refer to experiment for the bases on which it is founded; and for the sake of greater simplicity, I shall suppose the light to be homogeneous.

If we polarise by reflection at the surface of a plate of glass, blackened at its second surface, the diverging rays proceeding from a luminous point, and then cause them to pass through two rhomboids of equal thickness, placed one before the other, and having their principal sections perpendicular to each other, and at the same time inclined in an angle of 45° to the plane of reflection, it is known that the two pencils, produced by this pair of rhomboids, can only exhibit fringes when they are brought back to common planes of polarisation, by the assistance of a third rhomboid, or a pile of glass placed before or behind the lens. The most advantageous direction of the principal section of the third rhomboid is that which makes an angle of 45° with the principal sections of the two others; because then each of the two pencils which are emitted by these is divided equally into the ordinary and extraordinary images produced by the third rhomboid; and this equality of the two systems of undulations, which interfere in each image, renders the points of complete discordance as dark as possible: and if the light were perfectly homogeneous, they would in this case be completely black. The apparatus being thus arranged, if we consider any one point in the group of fringes, that, for example, which occupies the middle, and answers to equal paths described by the two constituent pencils of each image, it may be remarked that there is a maximum of light in the ordinary image, when the principal section of the rhomboid is parallel to the primitive plane of polarisation, which we may call horizontal, and that the same point, on the contrary, is black in the extraordinary image, that is, the light is reduced to 0. It is restored by degrees as the rhomboid is turned, and its intensity augments in proportion as the principal section is removed from the horizontal direction: when it is inclined in an angle of 45°, the light of this point is as great in the ordinary as in the extraordinary image: at last it disappears entirely in the ordinary image, and becomes a maximum in the extraordinary, when the principal section is vertical. We see then that the whole light, collected in this point, presents all the characters of a complete polarisation in a horizontal plane. If we now consider the point which answers to the difference of a quarter of an undulation in the progress of the two pencils, we shall find that it always preserves equal intensities in the two images, as we turn the rhomboid, and that its light acts as if it had been completely depolarised. Passing now to the point which answers to the difference of half an undulation between the two systems of waves, we shall find it perfectly dark in the ordinary image, and at its maximum of brightness in the extraordinary, when the principal section of the rhomboid is horizontal, and when it is vertical wholly dark in the extraordinary image, and at its maximum of brightness in the other; so that all the light collected in this point is polarised vertically. Continuing to go through the different points of interference of the two luminous pencils, we find, in general, that *their union* produces a light completely polarised in a horizontal plane; that is to say, in the primitive plane of polarisation, when the difference of their paths is either nothing, or an even number of semiundulations: that the whole light is polarised vertically, or in the azimuth $2\,i$, when the difference of the paths is an uneven number of undulations; and that the whole light, on the contrary, is entirely depolarised when this difference is a whole uneven number of quarters of an undulation; and that in all intermediate cases there is only a partial polarisation. The perfect polarisation is only observable in the fringes of the first three orders, but it is clear that if the middle points of the dark and bright stripes of the other orders appear only partially polarised, this circumstance depends on the want of homogeneity of the light, which cannot be more simplified without weakening it too much.

Mr. Arago has invented a valuable method of considerably augmenting the intensity of the light in the experiments on diffraction, which may be applied to experiments of this kind. It consists in substituting for the lens employed, a portion of a cylindrical surface, which forms a linear focus instead of a point, and when this line is turned into the direction of the fringes, they become extremely distinct, the situation being easily found by turning the cylindrical lens while the fringes are viewed with a common magnifier.

In order to study with convenience the kind of polarisation of the different lines of agreement or discordance, we must fix our attention on the point we wish to observe, by bringing to it the wire placed in the focus of the lens of the micrometer; or, what is still better, by substituting for this wire a screen furnished with a narrow slit, which only allows the light of this part of the fringe to pass through it. The horizontal or vertical polarisation of the points of complete agreement or disagreement ceases to take place when we intercept one of the pencils, and receive in the slit the light of the other only: it is then polarised like this latter alone; that is to say, in a direction inclined in an angle of 45° to the horizon. Hence it follows, that the polarisation in the primitive plane, or at the azimuth $2i$, results from the union of the two pencils, and does not take place in either of the pencils taken separately, which are always polarised in the direction parallel or perpendicular to the principal section, whether we observe them separately by the lens, or together without the lens, so as to see both luminous points at once, and observe both their polarisations. The lens preventing the distinct vision of the two points by the expansion of their images, which mixes their rays at the bottom of the eye, reproduces there the interferences which took place in the focus; and for this reason it is necessary for viewing the phenomena of interference, when the two images of the luminous point are not mixed together, or, in other words, when the two systems of undulations, which interfere, form a sensible angle with each other. It is easy to be convinced that the lens has no other effect here, and that it does not exercise any sensible polarising influence, by looking at a luminous pencil polarised in a known direction, for it will be seen that the interposition of the lens makes no change in it. The polarisation, therefore, that we have observed, in the primitive plane, and at the azimuth $2i$, depends wholly on the combination of the two pencils emerging from the rhomboids which cross each other.

If, leaving the principal sections always perpendicular to each other, we turn the two rhomboids round, we may remark, in every position of the system, that the lines of the fringes, which answer to a difference in the paths of an even number of semiundulations, are polarised in directions parallel to the primitive plane, and those which answer to an uneven number are polarised at the azimuth $2i$, and that the rest exhibit only a partial polarisation.

The experiment with the two rhomboids affords us a singular example of rays polarised in two orthogonal planes, which produce, by their union, light completely polarised in an intermediate direction: and this circumstance supports the hypothesis which has been mentioned in speaking of the law of Malus, when it was hinted that the vibration of light might be executed in planes parallel or perpendicular to that of the polarisation.

Phenomena of the same kind are afforded, in the same circumstances, by thin crystallized plates; when the rays have been polarised in a common plane, before their entrance into the plates, and when the difference of the paths of the two systems of undulations, at their emersion, is equal to a whole number of semiundulations: when it is an even number, the whole light that emerges is polarised in the primitive plane, and when an odd number, it is polarised at the azimuth $2i$. For example, if the angle i is 45°, that is, if the axis of the plate makes an angle of 45° with the primitive plane, the whole light will be polarised in the first case in the direction of the primitive plane, at 45° from the axis, and in the second case, at the azimuth of 90°, or perpendicularly to the primitive plane. But it does not follow, because the whole compound light is thus polarised, that the ordinary and extraordinary rays composing it are polarised in the same direction, as we have seen by the experiment with the two rhomboids. In fact, the circumstances of the experiment are the same, the only difference is that the two systems of undulations transmitted by the crystallized plate are parallel to each other, while those which pass through the rhomboids form a sensible angle, so that they are not visible without the employment of a luminous point, and a lens, which are required to make the effects of their interference perceptible. But, in consequence of this inclination, they exhibit at once all the differences of routes in the different points of the groups of fringes which they produce, and collect in this manner, into a single picture, all the cases afforded by crystallized plates of different thicknesses.

Mr. Biot, guided by the theory of emanation, could not suspect that the light which was polarised in a certain plane, could be composed of rays polarised in different directions, and naturally judged of the direction of the polarisation of the ordinary and extraordinary rays transmitted by the crystallized plate, from that of the whole light. Hence he was led to conclude, that the rays did not undergo in crystallized plates the same kind of polarisation as in crystals thick enough to divide the light into two separate pencils. But this is not a necessary consequence of the phenomenon; since the experiment with the two rhomboids demonstrates that the same appearances are produced by the combination of two distinct pencils, polarised in directions parallel and perpendicular to the principal section of the crystal: and besides, the hypothesis would be contradictory to other facts, since we have always found the ordinary and extraordinary rays actually polarised in directions parallel and perpendicular to the principal sections of the crystals that transmit them. It is, therefore, not to the separate pencils of ordinary or extraordinary rays that we must apply what Mr. Biot has said of the mode of polarisation of the light which has passed through a crystal, but to the light collectively: and it is still necessary to modify the proposition of this distinguished observer, in order to render it perfectly correct; for his words imply that each kind of homogeneous rays is always *completely* polarised either in the primitive plane, or at the azimuth $2i$. Now we have seen, in the experiment with the two rhomboids, that this *complete* polarisation is only acquired in particular cases; and the direct experiment on crystallized plates leads us to the same result.

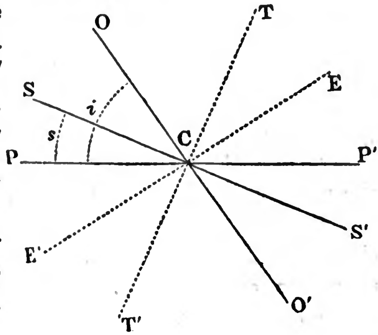
In short, all the phenomena exhibited by crystallized plates are easily explained, and even foretold, by the ordinary rules of the laws of interference; and the small number of particular modifications of these laws, as they relate to the mutual influence of polarised rays, determined by experiment.

Rays which are polarised at right angles to each other have no mutual influence; and it is for this reason that the two systems of undulations transmitted by crystallized bodies do not immediately exhibit any effects of this kind, even when the difference of their paths is so small, that we should expect them to produce very bright colours in white light. It must be recollected that a small difference in the routes is necessary, in order that the different effects that it occasions in the different kinds of undulations may produce sensible colours, as in the case of fringes produced by mirrors, and of reflected rings.

It is not sufficient that the rays which have been polarised at right angles should be brought back to a common plane of polarisation, in order that they should be able to influence each other; they must have been originally polarised on the same plane: hence arises the necessity of employing light previously polarised, when we wish to exhibit colours in crystallized plates.

We have seen by the experiment with the combined rhomboids, that when two luminous pencils, originally polarised in the same plane, are then polarised at right angles, they produce two complementary images in passing through a new rhomboid, which brings them back to a common plane of polarisation; for when the central stripe, for example, was black in the extraordinary image, it was at its greatest brightness in the ordinary image, and the same opposition was observable in all the bright and dark stripes of the two images. The two images exhibited by the polarised light, transmitted by a thin crystallized plate, must also be complementary to each other. It follows of necessity, that if one of them corresponds to the difference of the routes of two systems of transmitted undulations, the other must correspond to the same difference, augmented or diminished by half an undulation, because there is a perfect agreement in the one and discordance in the other.

In order to know which of the two differences must be so augmented or diminished, we must observe the following general rule. *The image, of which the tint agrees precisely with the difference of the paths described, is that for which the planes of polarisation of the constituent pencil, after having been separated from each other, are brought back by a contrary motion to be reunited; while the planes of polarisation of the two pencils, constituting the complementary image, continue to separate from each other, as considered* *on one side of their intersection, and to become continuations of each other beyond that intersection.*



Thus, let PP′ represent the primitive plane of polarisation of the incident rays, OO′ the principal section of the crystallized plate, and SS′ that of the rhomboid through which it is viewed. Now, the incident light, at first polarised in CP, is divided, as it passes through the plate, into two parts, one which undergoes the ordinary refraction, and receives a new polarisation in CO, the other the extraordinary, being polarised in CE, perpendicular to CO. Let us represent the former by F_o, and the latter by F_e. The passage through the rhomboid divides F_o, which is polarised in CO, into two other systems of undulations, one polarised in the direction of CS, the principal section, which may be called $F_{o+o'}$, the other in the perpendicular direction CT, and this may be called $F_{o+e'}$. In the same manner F_e, polarised in CE, is divided by the rhomboid into two systems, the first $F_{e+o'}$, polarised in CS, the other $F_{e+e'}$, polarised in CT. If we examine the motions of the planes of polarisation of the two pencils $F_{o+o'}$ and $F_{e+o'}$, which contribute to the formation of the ordinary image, considering them on one side of C, we see that, beginning from CP, they separate, and take the directions CO and CE′, and then meet in CS. Now, in this case, the ordinary image will answer precisely to the difference of the paths described at the same moment by the ordinary and extraordinary rays transmitted by the plate. And following, in the same manner, the progress of the planes of polarisation of the two pencils constituting the extraordinary image, $F_{o+e'}$ and $F_{e+e'}$, we see that they both set out from CP, and having assumed, in the plate, the directions CO and CE′, instead of approaching each other, they continue to separate, until they form continuations of each other in the directions CT and CT′; so that, according to the rule given, we must add half an undulation to the difference of the paths described by the two systems of undulations, or, what is the same in effect, change the signs of the motions of oscillation in one of them, if we wish to calculate the colours which they will exhibit, according to the laws of interference. It is evident that the effects are precisely such as would arise from the combination of forces in the plane of the figure, that is to say, in a direction perpendicular to that of the rays, in the planes of polarisation or in the perpendicular planes; for if two forces, represented by CO and CE′, should unite in CS, they would have the same sign; as the two pencils $F_{o+o'}$ and $F_{e+o'}$, which are united in that line of polarisation, have the same sign; and the two other constituent forces, CT and CT′, acting in opposite directions, would be affected by contrary signs.

The principle of the preservation of living force made it easy to foretel that the two images must be complementary to each other, but it did not indicate which of the two should agree with the simple difference of the paths, and which should require the addition of the half undulation. I have, therefore, referred to the facts, and have deduced the rule here given from the experiments of Mr. Biot. It may also be inferred from the experiment with the two rhomboids.

This rule explains why two pencils of direct light, which are polarised at right angles, do not exhibit any appearance of mutual influence, when they are brought back to a common plane of polarisation by the action of a pile of glass or a rhomboid of calcarious spar. It is not that they have no power of influencing each other under such circumstances, for, independently of general considerations of a mechanical nature, this supposition would be unsupported by analogy; but the fact is, that the effects of different systems of undulations of direct light compensate each other, and disappear. In fact, we may imagine direct light to be an assemblage, or rather a rapid succession, of an infinity of systems of undulations, polarised in all possible azimuths, in such a manner, that there is always as much light polarised in any given plane as in the plane of the perpendicular to it: now it results from the rule which has been given, that if, for example, we are obliged to add a half undulation to the difference of the paths described, in order to calculate the proportion of the extraordinary image produced by polarised light in the first plane, it must not be added for the extraordinary image from the light polarised in the second plane; so that the two tints which they exhibit, either at once or in rapid succession, in the extraordinary image, will be complementary to each other. Hence, this kind of compensation, which occurs at every possible azimuth, prevents our perceiving the effects of interference.

Returning to the case represented by the figure, when the incident light has undergone a previous polarisation in the plane PP′, before it passes through the crystallized plate, of which the principal section OO′ makes an angle i with this plane; if we inquire, for a particular kind of homogeneous light, having its undulations of the length λ, what must be the intensities of the ordinary and extraordinary images given by the rhomboid of calcarious spar, of which the principal section SS′ makes an angle s with the primitive plane PP′: we may neglect in the calculation the loss of light occasioned by the partial reflections of the two surfaces of the plate and of the rhomboid, because it only affects the total brightness of the images, without any alteration of their comparative intensities, which alone we are considering. If the actual velocity of the oscillating particles be represented by F, for the incident polarised pencil, the brightness of the light will be expressed by F^2, which is the living force, in the common sense of the term; this being the true mode of estimating the intensity of light in all optical experiments: since it is the sum of the living forces, and not that of the simple velocities, which remains constant, as the total intensity, in all the subdivisions to which the light is subjected. This being granted, the incident pencil, in passing through the crystallized plate, is divided into two others, of which the luminous intensities must be equal, according to the law of Malus, to $F^2 \cos^2 i$, for the ordinary refraction, and $F^2 \sin^2 i$ for the extraordinary: consequently the actual velocities, in the former, will be expressed by $F \cos i$, and in the latter by $F \sin i$; hence the incident light, in passing through the plate, is divided into two systems of undulations, which may be thus represented—

$\cos i \; F_o$ $\sin i \; F_e$

P . O.   P . E′.

The small letters, placed below F, are not intended to alter its magnitude, they merely indicate the length of the paths which have been described at the same instant by the ordinary and extraordinary rays, after their emersion from the plate, and determine by their difference, $o-e$, the interval separating the corresponding points of the two systems of undulations. The capital letters P.O. and P.E′. show the successive situations of the planes of polarisation of each pencil, to facilitate the application of the rule which has been laid down.

Each of the two systems of undulations will be divided into two others, by the action of the rhomboid of calcarious spar, which will afford us in the whole the four following pencils, of which the first two are produced by the first system of undulations, and the latter two by the second.

$\cos i \cos (i-s) \; F_{o+o'}$, $\cos i \sin (i-s) \; F_{o+e'}$,

P. O. S.   P. O. T.

$\sin i \sin (i-s) \; F_{e+o'}$, $\sin i \cos (i-s) \; F_{e+e'}$,

P. E′. S.   P. E′. T′.

The first and third of these compose the ordinary image, the second and fourth the extraordinary.

In the case of the latter, we see, from the progress of the planes of polarisation indicated by the capital letters, that the paths of the rays, reduced to a common plane, must be supposed to differ half an undulation, independently of the difference $o-e$ of the paths described; we must, therefore, add half an undulation to $o-e$, or change the signs of one of the expressions representing the intensity of the common factor of the velocities. The problem is thus reduced to that of finding the result of the combination of two systems of undulations, for which the difference of the paths is $o-e$, and the velocities of oscillation are respectively expressed by $F \cos i \sin (i-s)$ and $-F \sin i \cos (i-s)$.

Applying to this case, the general formula given in the abstract of my Memoir on Diffraction, (*Ann. Ch.*, vol. xi., p. 258,) we have $A^2 = a^2 + a'^2 + 2\,a\,a', \; \cos 2\,\pi \left(\frac{c}{\lambda}\right)$; a and

u' representing the velocities belonging to the two systems of undulations, 2π the circumference of the radius unity, c the difference of the paths described, and λ the length of the undulation: and for the intensity of the homogeneous light in the extraordinary image, we find

$$F^2 [\cos^2 i \sin^2 (i-s) + \sin^2 i \cos^2 (i-s) - 2 \sin i \cos . \sin (i-s) \cos (i-s) \sin 2\pi \frac{o-e}{\lambda}] = F^2 [\{-\cos i \sin (i-s) + \sin i \cos (i-s)\}^2 + 2 \sin i \cos i \sin (i-s) \cos (i-s) (1- \cos 2\pi \frac{o-e}{\lambda})] = F^2 (\sin^2 s + \sin 2i \sin 2(i-s) \sin^2 \pi \frac{o-e}{\lambda}).$$

Making a similar calculation for the two constituent pencils of the ordinary image, and observing that the two expressions, $F \cos i \cos (i-s)$, and $F \sin i \sin (i-s)$, must have the same sign, in consequence of the direction of the motion of their planes of polarisation, we find, for the intensity of the light in the ordinary image:

$$F^2 (\cos^2 s - \sin 2i \sin 2(i-s) \sin^2 \pi \frac{o-e}{\lambda}).$$

These are the general formulas which give the intensity of each kind of homogeneous light in the ordinary and extraordinary images, in terms of the length of the undulation and the difference $o-e$ of the paths described by the light in its passage. Knowing the thickness of the plate, and the velocity of the ordinary and extraordinary rays in the crystal, it will be easy to determine $o-e$. For the sulfate of lime, for rock crystal, and for the greater number of the other crystals possessing the property of double refraction, $o-e$ undergoes but very slight variations for the different kinds of luminous rays, so that it may be considered as a constant quantity, at least for the crystals here considered, in which the dispersion belonging to the double refraction is very small in comparison with the general separation of the two kinds of refraction. If, after having calculated the difference of the paths $o-e$, we divided each successively by the mean length of the undulation of each of the "seven" principal kinds of coloured rays, and if we substitute successively these different quotients in the above expressions, we shall have the intensities of each kind of coloured rays in the ordinary and extraordinary image, and we shall then be able to determine the tints of these images by means of the empirical formula which Newton has given for finding the colour resulting from any mixture of different rays, of which we know the relative intensities. We may, therefore, consider the general formulas which give the intensity of each species of homogeneous light, in terms of the length of the undulation, as the direct expression for the tint produced by white light; this, at least, is all that can at present be deduced from theory, and for the rest, we must have recourse to the empirical construction of Newton, which agrees pretty well with experience, at least with regard to the principal divisions of colours.

These formulas may be considered independently of the common factor F^2, which may be called unity, and we have

$$\text{Ordinary image} \;..\; \cos^2 s - \sin 2i \; \sin 2(i-s) \; \sin^2 \pi \frac{o-e}{\lambda}.$$

$$\text{Extraord. image} \;..\; \sin^2 s + \sin 2i \; \sin 2(i-s) \; \sin^2 \pi \frac{o-e}{\lambda}.$$

We see by inspection that the two images must both be white when the term containing $\frac{o-e}{\lambda}$ vanishes, since this is the only part which varies with the length of the undulation, and which causes the intensity to differ for the different coloured rays. Thus the images will become white when $\sin 2i \; \sin 2(i-s) = 0$, which will happen when either of the angles i or $i-s$ vanishes, or becomes equal to 90°, 180°, or 360°.

We have, therefore, eight cases in which the images must become white; that is to say, the principal section of the crystallized plate must be parallel or perpendicular to the primitive plane of polarisation, or to the principal section of the rhomboid, which might easily be inferred from the theory without this formula: for when the principal section of the plate is parallel or perpendicular to the primitive plane, the incident light undergoes but one kind of refraction within the crystal; and when the principal section is parallel or perpendicular to that of the rhomboid, each image contains such rays only as have undergone the same refraction within the plate: thus in the one case, as well as in the other, each image will contain one system of undulations only, and consequently no colours, because there are no interferences.

Both images, on the contrary, are distinguished by the brightest colours, when the coefficient of the variable term is equal to unity, which happens when $s = o$, and $i = 45°$; and then the two expressions become

$$\text{Ord. im.} \ldots \; 1 - \sin^2 \pi \frac{o-e}{\lambda}, \text{ or } \cos^2 \pi \frac{o-e}{\lambda}.$$

$$\text{Extr. im.} \ldots \qquad \sin^2 \pi \frac{o-e}{\lambda}.$$

It must be remarked, that the second expression is similar to that which indicates, for the common coloured rings, the result of two systems of undulations reflected perpendicularly at the first and second surface of a plate of air, when its thickness is equal to $\frac{1}{2}$ $(o-e)$, which makes the difference of the paths described equal to $o-e$. In fact, if we represent the velocity of oscillation for each system of undulations by $\frac{1}{2}$, and remark that these velocities must be taken with contrary signs, because one of them is reflected within a denser medium, and the other without it, which occasions an opposition of signs, as we have seen in the explanation of the phenomena of coloured rings, we may proceed to find for the intensity of the resulting light, by the formula already employed, $\frac{1}{4} + \frac{1}{4} - 2 \cdot \frac{1}{2} \cdot \frac{1}{2} \cos 2\pi \frac{o-e}{\lambda}$, or $\frac{1}{2} - \frac{1}{2} \cos 2\pi \frac{o-e}{\lambda}$; or lastly, $\sin^2 \pi \frac{o-e}{\lambda}$.

Thus the tints of the extraordinary image, produced by crystallized plates, must resemble those of the reflected rings, as the observations of Mr. Biot had demonstrated; at least while the difference $o-e$ produced by the crystal does not vary sensibly with the nature of the rays: for, in the coloured rings, this difference being twice the distance of the thickness of the plate of air, in a perpendicular direction, is rigorously the same for all kinds of rays.

The formulas which Mr. Biot has derived from this resemblance represent, with great fidelity, the colour produced by a single plate. Instead of giving immediately the intensities of each species of coloured rays, like those which are here calculated, they refer to the table of Newton, which contains the tints of the reflected rays, and they show, at the same time, the quantity of white light to be added to these tints, in consequence of the relative directions of the primitive plane, of the primitive section of the plate, and of that of the rhomboid of calcarious spar.

The above expressions, $\cos^2 \pi \frac{o-e}{\lambda}$, and $\sin^2 \pi \frac{o-e}{\lambda}$, which show the respective intensities of the ordinary and extraordinary image in a homogeneous light, of which the length of the undulation is λ, when the axis of the crystallized plate makes an angle of 45° with the primitive plane of polarisation, and when the principal section of the rhomboid is parallel to this plane, show us that the combination of the systems of waves which emerge from the crystallized plate, must be polarised in the primitive plane of polarisation, when $o-e$ is either 0 or equal to a whole number of undulations, because then $\sin^2 \pi \frac{o-e}{\lambda}$ becoming $= 0$, the extraordinary image vanishes. On the contrary, when $o-e$ is equal to an even number of semiundulations, it is $\cos^2 \pi \frac{o-e}{\lambda}$ that becomes $= 0$, and consequently the ordinary image vanishes; whence we may infer that the whole of the light is polarised in the plane perpendicular to the principal section, which is here precisely at the azimuth $2\,i$. But for all the intermediate values of λ, the *combination* of the two systems of undulations can only exhibit a partial polarisation: and it must even appear completely depolarised when $o-e$ is equal to an odd number of quarters of an undulation, because then $\cos^2 \pi \frac{o-e}{\lambda}$ and $\sin^2 \pi \frac{o-e}{\lambda}$ becoming each equal to $\frac{1}{2}$, the two images are of the same intensity; and this is the case whatever may be the azimuth in which the principal section of the rhomboid is placed, as we may find from the general formulas given above; putting $i = 45°$, and $\sin^2 \pi \frac{o-e}{\lambda} = \frac{1}{2}$; for they then give,

Extr. image .... $\sin^2 s + \frac{1}{2} \cos 2\,s = \frac{1}{2}$.
Ord. image ..... $\cos^2 s - \frac{1}{2} \cos 2\,s = \frac{1}{2}$.

It is easy to see in the same formulas, whatever may be the value of i, that when $o-e$ is equal to 0, or to an even number of semiundulations, the extraordinary image vanishes in the case $s=0$, and when $o-e$ is equal to an odd number of semiundulations the same expression becomes $=0$ when $s=2i$, and consequently the whole light is polarised in the primitive plane, in the first case; and in the second, at the azimuth $2i$; while, for all intermediate values of $o-e$, neither image can wholly disappear, whatever may be the direction of the principal section of the rhomboid. And all these consequences of the theory are confirmed by experiment.

When we cause polarised light to pass through several cystallized plates, of which the principal sections cross each other in any manner, the phenomena become greatly complicated, but may always be computed by the same theory. The incident light is first divided, in the first plate, into two systems of undulations, of which we may determine the intensities of oscillation by the law of Malus, and the relative positions by the difference of their paths, as we have done for a single plate: then each of these systems of undulations is subdivided into two others in the second plate; each of these four new systems of undulations is again divided into two in the third plate, and so forth. It is plain that when the azimuths of all the principal sections are known, as well as that of the rhomboid which affords the double image, we can determine the comparative intensities of all the systems of undulations which enter into each image, and that it is equally easy to determine the difference of their paths, having regard to the different species of refractions which they successively undergo, when the thicknesses of the plates are known, as well as the proportions of the velocities of the ordinary and extraordinary rays which pass through them. We shall, therefore, have for each image, the intensities and the relative situations of all the systems of waves which contribute to its formation, and the result of the whole may be determined by the general method pointed out in my memoir on Diffraction, p. 256. In these calculations, every thing is theoretically determined from the fundamental principles deduced from facts, and nothing has been borrowed from experiment, even in the most complicated cases. It is in this respect that the system here explained is greatly superior to that of moveable polarisation, which becomes so embarrassing when we wish to inquire how the *oscillations of the axes of the luminous particles* are continued in their passage from one plate to another, in which the principal section makes any angle with that of the first. Thus the hypothesis of Mr. Biot has not enabled him to determine all the coefficients of his formulas for two plates placed on each other, except in very particular cases; and there is even one case in which his formulas do not accurately agree with the phenomena, as I found by comparison with my own: it is that in which two plates of the same kind have their axes crossed at an angle of 45°. The discussion of this particular case, and the general formulas for the tints given by two plates, will be found in the second note added to the report of Mr. Arago on my Memoir, page 267 of the seventeenth volume of the *Annales de Chimie et de Physique.*

I have shown, in the same note, that we may explain in the simplest manner the principal properties of polarised light, the law of Malus, and the singular character of double refraction, if we suppose that, in the luminous undulations, the oscillations of the particles are executed in directions perpendicular to the rays, and to that which we have called the plane of polarisation. Adopting this hypothesis, it would have been more natural to have called the plane of polarisation that in which the oscillations are supposed to be made: but I wished to avoid making any change in the received appellations. This hypothesis, particularly pointed out by the laws observed by Mr. Arago and myself, in the interferences of polarised rays, shows how these laws must necessarily result from the nature of the undulations; so that the formulas which I have just given for crystallized plates, as well as those which represent the phenomena of diffraction, reflection, refraction, and the coloured rings, are thus reducible to a single supposition: for it agrees, as well as that which we at first adopted, with the calculations of the interferences which served to explain these phenomena: since it is indifferent in these calculations, as was observed at the beginning of this essay, whether the oscillating motions to be combined, were parallel or perpendicular to the rays, provided that they had always the same directions in the two interfering portions of the undulations. According to this new hypothesis, common light must be a combination, or rather a rapid succession of an infinite number of undulations, polarised in all manner of directions: and the act of polarisation must be considered, not as creating transverse motions, which already exist in common light, but in decomposing them according to two invariable rectangular planes, and in separating from each other the systems of undulations polarised in these two directions, either by altering their general direction, or simply by means of the difference of their velocities.

Experiments, as well as the principle of interference, have taught us, that when a pencil of polarised light is divided into two systems of undulations of equal intensities, polarised in rectangular directions, and separated by the interval of a quarter of an undulation, it exhibits upon the reunion of the two systems of undulations, the appearance of complete depolarisation, that is to say, that the whole light, when analysed with a rhomboid of calcarious spar, gives always images of equal intensities, in whatever direction we may turn the principal section. The light, thus modified, resembles in this respect direct light; but it differs from it by some very curious optical properties which are the principal subject of a memoir that I communicated to the Academy of Sciences, the 24th of November, 1817.

[To be continued in our next Number.]

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ii. *Remarks on* Mr. HENDERSON's *Improvement on* Dr. YOUNG's *method of computing the Longitude from the observed occultation of a fixed Star by the Moon: with* Mr. HENDERSON's *Answer.*

"WHEN neither of the altitudes has been observed, the computation of that of the moon is liable to considerable uncertainty, as depending upon the supposed longitude by account." This is very true; and, in fact, it ought never to be attempted. Neither, in strictness, ought it to be observed, unless with a very good sextant, and under very favourable circumstances with respect to the horizon; and, if observed, the star's altitude ought also to be observed;

for, if the one be observed and the other computed, their difference may err not only seconds but several minutes, which will render any method quite impracticable.

Since the true time at ship is always required to be accurately known in order to take the observation, and since the sun's right ascension does not depend much on the supposed longitude by account, the star's horary angle and true altitude may be found to great perfection. And if the difference of apparent altitudes can be observed, without measuring either of the altitudes themselves, the method by Dr. Young will be the most simple, as well as the most accurate that can be devised for the purpose.

Mr. Henderson's method is proposed with much ingenuity and mathematical talent, and may, in many cases, give pretty accurate results. However, it may be shown to be only an approximation, and which diverges from the truth as the observer approaches the perpendicular to the moon's apparent path, the moon and star being nearly in the same vertical circle.

The error is occasioned by the effects of parallax being computed from the star's altitude, instead of the apparent altitude of the moon; and its quantity may vary from 0 to a whole degree of longitude, and perhaps more.

As an example, suppose the immersion of ϑ♒ was observed, Jan. 5th, 1824, in latitude 28° 17′ N. at $1^h\ 37^m\ 49''$.

Then star's right ascension	22^h	7^m	32″
Sun's	19	2	19
Diff. of R. A.	3	5	13
Time of observation	1	37	49
✱'s horary angle, east of meridian . .	1	27	24

Hence with these, the star's altitude may be computed thus:

Log. vers. $1^h\ 27^m\ 24''$	= 21° 51′	=	8.856358
Log. cos. declination . .	8° 39′ 17″	=	9.995027
Log. cos. latitude . . .	28 17 0	=	9.944786
Natural numb. . 062542		Log.	8.796171
N. S. 799285		Mer. alt.	53° 3′ 43″
N. S. 736743		✱'s alt.	47 27 17

Consequently, by Mr. Henderson's method, we have,

Log. sec. alt. 47° 27′ 17″ =	0.169942	Orb. A.	65° 29′ N.E. ☽ N.
Log. cos. lat. 28 17 0 =	9.944768	Compl.	−24 31
Log. sin. H.A. 21 51 0 =	9.570751	Par. A.	+29 0
Log. sin. P. A. 29 0 0 =	9.685461	C.P.O.A.	+4 29

(χ)

P. L. red. H. P. . .	54′ 9	. . .	0.52167
Log. sec. Alt. . . .	47 27 17	. . .	0.16994
Log. cosec. compl. A.	4 29 0	. . .	1.10696
Orbit. Parallax . .	+ 2 52	P. L.	1.79857
Log. tang. compl. A. . . .		. . .	8.89437
Perp. Parallax . .	− 36 30	P. L.	0.69294

(ψ)

Nearest distance		+50′ 31″	
Perp. Parallax		−36 30	
Sum		+14 1	
Semidiameter		14 46	
Sum		28 47	P.L. 0.79613
Difference		0 45	P.L. 2.38020
			2)3.17633
Side . . .	+4′ 39″	. . . . . .	P.L. 1.58816
Orbit. Parallax	+2 52		
	+7 31		

The hourly motion being 29′ 42″, this gives 15^m 11″, which subtracted from 3^h 17^m 1″, the time of nearest approach, leaves the

True time at Greenwich	3^h 1^m 50″	
True time at ship . .	1 37 49	
Difference of time . .	1 24 1	= 21° 0′ 15″ W. Long.

To find the difference of the apparent altitudes, and from thence the error of the foregoing solution:—

Semidiameter	14′ 46″	P. L. 1.08599
Sum before found	14 1	P. L. 1.10863
Angle A	71° 40 0	Sin. 9.97736 diff.
90°−4° 29′ = ...	85 31 0	
Difference	13 51 0	= Angle B.

Angle B	13°	51′	0″	Sec.	0.01281
Semidiameter		14	46	P. L.	1,08599
Diff. of apparent altitudes .		14	20	P. L.	1.09880
✳'s apparent altitude .	47	28	9		
☽'s apparent altitude .	47	42	49	Sec.	0.17204
Reduced horizontal parallax		54	9	P. L.	0.52167
Log. cosec. compl. A.					1.10696
Orbit. parallax	+	2	51	P. L.	1.80067
Log. tan. compl. A.					8.89437
Perp. parallax	−	36	20	P. L.	0.69504
Nearest distance		50	31		
Perp. parallax	−	36	20		
Sum		14	11		
Semidiameter		14	46		
Sum		28	57	P. L.	0.79362
Difference		0	35	P. L.	2.48900
					2)3.28262
Side	+	4′	7″	P. L.	1.64131
Orbit. parallax	+	2	51		
	+	6	58		

Whence 6′ 58″ gives 14^m 4″.4 of time, which subtracted from 3^h 17^m 1″ leaves the

True time at Greenwich .	3^h	2^m	56″.6	
True time at ship . .	1	37	49	
Difference of time . .	1	25	7.6	= 21° 16′ 54″ W. long.
Longitude found by Mr. Henderson's method				= 21 0 15 W.
Error of his method, in the present example				0 16 39

Now by referring to a diagram, it may be easily discovered, that the operation marked χ ought to comprehend the solution of a rightangled plane triangle, whose hypotenuse is the moon's parallax in altitude, the perpendicular being the perpendicular parallax, and the base the orbital parallax. This would be precisely the case, if, in the second term, the log. secant of the moon's apparent altitude were substituted instead of that of the star. Hence, on this account, since the difference of apparent altitudes may amount to 14′ or 15′, the error in the orbital parallax, as also in the perpendicular parallax, may vary from 0 to 16″. And the sum of the perpendicular parallax, and the nearest distance, may therefore err from 0 to 16″, according to Axiom 4, Book 1, Euclid.

Consequently, putting D = the moon's semidiameter, $s=$ the sum of the nearest distance and perpendicular parallax, and $x=$the side to be found, we shall have, in the operation marked ⫛, the hypotenuse (D) and the perpendicular (s) given to find the base $x=\sqrt{D^2-s^2}$ $\therefore \delta x = \frac{-s}{\sqrt{D^2-s^2}} \times \delta s = \frac{-s}{x} \times \delta s$; that is, $x : s :: \delta s : \delta x$. Whence, should, in any case, the base equal 1′, and the perp. s = 15′, which may sometimes happen, we shall have 1′ : 15′ :: 16″ : 240″ = 4′. And supposing the moon's hourly motion from the star to be 30′, which it is very nearly when at its mean quantity, this will give 8 minutes of time, or 2 degrees of longitude on the equator!

Mar. 20, 1828. T. B.

Answer.

Mr. B. has objected to the method for computing an occultation, published in the *Nautical Almanac* under my name, that "it is only an approximation, which diverges from the truth, as the observer approaches the perpendicular to the moon's apparent path, the moon and star being nearly in the same vertical circle; that the error is occasioned by the effects of parallax being computed from the star's altitude, instead of the apparent altitude of the moon, and that its quantity may vary from 0 to a whole degree of longitude, and perhaps more."

On the other hand, I maintain that the effect of parallax is computed upon principles which are mathematically correct; and consequently that the method in question is not an approximation, for the cause assigned by Mr. B.

As he justly remarks, the effect of parallax is, in this method, computed from the altitude of the star, and not from the apparent altitude of the moon. But it is a proposition, which is capable of strict demonstration, that if, in an eclipse or occultation, the parallaxes are computed from the sun's or star's place, the apparent differences of longitude, latitude, right ascension, declination, azimuth, altitude, and the apparent distance thence resulting will be obtained as correct, as if the parallaxes had been computed from the moon's apparent place, provided each of these quantities be increased by its proportional part of the augmentation of the moon's diameter, on account of her altitude. In an occultation, the apparent distance, at the immersion or emersion, is equal to the moon's semidiameter, and therefore, in the computations of these phenomena, it is unnecessary to apply the augmentation either to the distance or semidiameter; and this is the reason why, in Precept III., the moon's semidiameter is directed to be used "without augmentation."

In place of attempting any demonstration of the proposition, I believe that it will be more satisfactory to refer to the writings of those eminent mathematicians and astronomers, who have considered the analytical calculation of eclipses, where it will be found demonstrated with the utmost rigour. For it should be observed, that neither is the method in question, nor the principle of computing the parallaxes there adopted, a new invention, as both have been known and practised for a considerable time. What is published in the *Nautical Almanac* ought to be considered as a set of practical rules, accommodating the method to the elements published in that work.

In a memoir first published in the *Ephemeris* of Berlin, for 1782, and afterwards in the *Connaissance des Tems*, for 1817, (Additions, p. 237), under the title, "Sur le calcul des Eclipses sujettes aux parallaxes," the celebrated La Grange has discussed the problem of eclipses and occultations in its utmost generality; and has given formulæ for its solution, in which the position of the plane of projection, or the point of the celestial sphere for which the parallaxes are to be computed, is completely arbitrary. He has adapted the general formulæ to the case, in which the plane of projection is supposed to be a tangent to the sphere at the place of the sun or star, or in which the parallaxes are computed for that point, as being the most convenient in practice. The method in the *Nautical Almanac* follows in the path pointed out and shown to be correct by the illustrious author.

In the *Connaissance des Tems* for 1818, (Additions, p. 311) M. Puissant has applied La Grange's method to the calculation of an example taken from De Lambre's Astronomy, and the difference between the result of this method, and that of computing the parallaxes from the moon's place, is the insensible quantity of one tenth of a second.

La Grange published another memoir upon Eclipses in the Berlin *Ephemeris* for 1781, and it has been reprinted in the *Connaissance des Tems* for 1819, (Additions, p. 344.) Here also the plane of projection is supposed to be a tangent to the sphere at the centre of the sun or star; and the apparent distance found from the projection is directed to be increased by a correction, " semblable à celle du diamètre apparent de la lune;" and he adds, that the " réduction, que nous avons proposé ci dessus de faire aux distances des centres mesurées sur la projection, suffit pour donner à la méthode des projections toute la rigueur qu'on peut désirer, et une rigueur égale à celle qui résulte de la méthode des parallaxes." The method in the *Nautical Almanac* is nothing else than La Grange's projection reduced to calculation.

The next author to whom I refer is Du Sejour, in his *Traité Analytique des Mouvemens apparens des Corps Célestes;* in which eclipses and occultations are treated of at very great length. His method, which is fully demonstrated, is the same as that in the *Nautical Almanac*, the parallaxes being computed from the place of the sun or star, and, together with the moon's motion, being reckoned in the direction of her orbit and the perpendicular to it. A different formula is employed for computing the parallaxes, but it is the same at bottom as that in the *Nautical Almanac*. Of the similarity of the two methods I was not at first aware, but it was acknowledged in the *Quarterly Journal*, p. 328.

In the *Œuvres de Goudin*, Paris, an VIII, there is demonstrated a similar method for solar eclipses, which are the same in principle as occultations of the stars.

In the *Connaissance des Tems* for 1806-7, (Additions, p. 487,) a paper by M. Chabrol is published upon the analytical method for the calculation of eclipses, in which the principle of computing the parallaxes from the place of the sun or star is followed. In this paper, (which has been translated in Rees's *Cyclopædia*, art. Eclipse,) the method is very clearly demonstrated.

The same method is also followed by Monteiro da Rocha, in *Mémoires sur l'Astronomie pratique*, Paris, 1808; by Carlini, in the Milan *Ephemeris* for 1810, and in Santini's *Elementi di Astronomia*, Padua, 1819; and by Dr. Maskelyne, in Vince's *Astronomy*, vol. i., p. 527, second edition.

The last two authors rest the method upon this principle, that at the immersion or emersion of a star, its true place is the same as the apparent place of the moon's limb, where the phenomenon happens, and consequently the parallaxes being computed for this place, the true position of the point of the limb is obtained, the true distance of which from the centre is the horizontal semidiameter without augmentation. The case of the beginning or end of a solar eclipse may be considered to be the same as the occultation of a star, the moon being supposed to have an apparent or augmented semidiameter, equal to the sum of her real augmented semidiameter and the semidiameter of the sun; and the true distance of the point of the fictitious limb from the centre will be equal to the fictitious semidiameter divested of augmentation; that is to say, to the sum of the moon's horizontal semidiameter and of the sun's semidiameter, diminished by its proportional part of the augmentation. Accordingly, several of the authors referred to direct the sun's semidiameter to be diminished by a proportional quantity of the augmentation of the moon's semidiameter; while the computed apparent distance and the moon's semidiameter remain without augmentation. This was noticed in the *Quarterly Journal*, vol. xx., p. 94.

When the example which Mr. B. has considered is computed according to the principles now laid down, no discordance will be found between the results of the two methods. (See the annexed calculation.) Mr. B. introduces refraction into his computations, but this is anunnecessary complication of the problem; for, as refraction is supposed to operate equally upon two celestial bodies in apparent contact, their relative position is not thereby affected.

Upon the whole, it is hoped that the objection stated to the method in question has been shown not to be well founded. I believe that it will further be admitted, that the method of computing the parallaxes for the star's place is more convenient than the other.

Du Sejour, in the work already referred to, (vol. i., p. 270,) has shown that the supposition of the portion of the moon's orbit described during an eclipse or occultation being a straight line causes only an insensible error. An error of greater magnitude may arise from the horary motion being supposed to be uniform, while [it is generally variable; but this may be corrected by making an allowance for the variation of the horary motion.—*Quarterly Journal*, vol. xx., p. 328.

Mr. B.'s remarks on Dr. Young's method seem to be correct. It is to be regretted that the difficulty of obtaining the difference of the apparent altitudes, with the requisite accuracy, should prevent the success of a method which is so very simple. The small correction which he proposes was under consideration when the method was first published, and it was remarked that it "may be altogether omitted without inconvenience." *Quarterly Journal*, vol. xv., p. 360. Besides, if the utmost exactness were attempted, it would be necessary to take into account the spheroidal figure of the earth, which besides occasioning an alteration in the parallax in altitude, gives to the moon a parallax in azimuth.

Leopold-Place, Edinburgh, THOS. HENDERSON.
April 11, 1828.

CALCULATION.

Mr. B. has rightly performed the first part of the calculation; the other part should be as follows:—

Difference of apparent altitudes by Mr. B. .	14′ 20″
Add for augmentation	10
Correct difference of apparent altitudes . .	14 30
Star's altitude	47° 27 17
Moon's apparent altitude	47 41 47

The complementary orbital angle must be found for the moon's apparent place. By the differential analogies of spherical triangles, we have

Cot. ✳ alt. : sin. comp. orbit. ang. :: +14′ 1″	:	+1′ 12″
Rad. : cos. comp. orbit. ang. tang. alt. :: − 4 39	:	−5 3
		−3 51
		4 29
Comp. orbital angle for ☽'s apparent place . .		4 25

Log. secant ☽'s apparent altitude		0·17195
Prop. log. reduced hor. parallax		0·52167
Log. cosecant comp. angle		1·11346
Prop. log. orbital parallax . .	+ 2′ 48″	1·80708
Log. tang. comp. angle . . .		8·88783
Prop. log. perpendicular parallax	−36 20	0·69491
Nearest distance .	+50 31	
Sum	+14 11	
☽'s hor. semidiameter 14′ 46″		
Augmentation $10\frac{1}{2}$		
☽'s augmented semidiameter .	14 $56\frac{1}{2}$	
Sum	29 $7\frac{1}{2}$	P. L. 0·79100
Differ.	0 $45\frac{1}{2}$	P. L. 2·37541
		2)3·16641
Side	+4 42	P. L. 1·58320
Orbital parallax . .	+2 48	
	+7 30	

in place of 7′ 31″, as by the other method. The difference is occasioned by the minute quantities omitted in the arithmetical operations, which, in this instance, have a sensible influence upon the result.

iii. APPROXIMATE TABLE OF EQUINOXES. H. J.

Mean Equinox.									Apparent Equinox.				
Centuries.			Odd Years.						Centuries.				
Date.	Vernal Equinox.	Autumnal Equinox.	Yrs.	Add.	Yrs.	Add.	Yrs.	Add.	Date.	Vernal Equinox.	Autumn. Equinox.	Sun's Declination. North.	South.
	March d.	Sept. d.		d.		d.		d.		d.	d.	d.	d.
−800	29,16	27,78	0	1,00	35	1,48	70	0,96	−800	−1,71	+1,68	186,01	179,23
700	28,40	27,03	1	1,24	36	0,72	71	1,21	700	−1,74	+1,71	186,07	179,17
600	27,64	26,27	2	1,48	37	0,96	72	0,44	600	−1,77	+1,74	186,13	179,11
500	26,89	25,51	3	1,73	38	1,21	73	0,69	500	−1,79	+1,77	186,19	179,05
400	26,13	24,75	4	0,97	39	1,45	74	0,93	400	−1,82	+1,80	180,24	179,00
300	25,37	23,99	5	1,21	40	0,69	75	1,17	300	−1,84	+1,82	186,29	178,96
200	24,61	23,23	6	1,45	41	0,93	76	0,41	200	−1,87	+1,85	186,33	178,91
−100	23,85	22,46	7	1,70	42	1,18	77	0,65	−100	−1,89	+1,87	186,37	178,87
0	23,09	21,71	8	0,94	43	1,42	78	0,90	0	−1,90	+1,89	186,41	178,83
+100	22,32	20,94	9	1,18	44	0,66	79	1,14	+100	−1,92	+1,90	186,45	178,80
200	21,56	20,18	10	1,42	45	0,90	80	0,38	200	−1,94	+1,92	186,48	178,77
300	20,80	19,42	11	1,66	46	1,14	81	0,62	300	−1,95	+1,93	186,50	178,74
400	20,03	18,65	12	0,91	47	1,39	82	0,87	400	−1,96	+1,95	186,53	178,72
500	19,27	17,89	13	1,15	48	0,63	83	1,11	500	−1,97	+1,96	186,55	178,70
600	18,50	17,12	14	1,39	49	0,87	84	0,35	600	−1,98	+1,97	186,56	178,68
700	17,74	16,36	15	1,63	50	1,11	85	0,59	700	−1,98	+1,97	186,57	178,67
800	16,97	15,59	16	0,88	51	1,36	86	0,83	800	−1,98	+1,98	186,58	178,66
900	16,20	14,82	17	1,12	52	0,60	87	1,08	900	−1,99	+1,98	186,59	178,65
1000	15,43	14,05	18	1,35	53	0,84	88	0,32	1000	−1,99	+1,98	186,59	178,65
1100	14,66	13,28	19	1,59	54	1,08	89	0,56	1100	−1,98	+1,98	186,59	178,65
1200	13,89	12,51	20	0,84	55	1,32	90	0,80	1200	−1,98	+1,98	186,58	178,66
1300	13,12	11,74	21	1,09	56	0,57	91	1,05	1300	−1,98	+1,98	186,57	178,67
G.1400	12,35	10,97	22	1,33	57	0,81	92	0,29	1400	−1,97	+1,97	186,56	178,68
1500	11,58	10,20	23	1,57	58	1,05	93	0,53	J. 1500	−1,96	+1,96	186,54	178,70
J. 1500	21,58	20,20	24	0,81	59	1,29	94	0,78	G.1500	−1,96	+1,96	186,54	178,70
1600	20,81	19,43	25	1,06	60	0,54	95	1,02	1600	−1,95	+1,95	186,52	178,72
1700	21,03	19,66	26	1,30	61	0,78	96	0,26	1700	−1,94	+1,94	186,50	178,74
1800	21,26	19,88	27	1,54	62	1,02	97	0,50	1800	−1,92	+1,93	186,47	178,77
1900	21,49	20,11	28	0,78	63	1,26	98	0,74	1900	−1,91	+1,92	186,44	178,80
2000	20,71	19,33	29	1,03	64	0,50	99	0,98	2000	−1,89	+1,90	186,41	178,83
2100	20,93	19,56	30	1,27	65	0,75			2100	−1,87	+1,88	186,37	178,87
2200	21,16	19,78	31	1,51	66	0,99			2200	−1,85	+1,86	186,33	178,91
2300	21,38	20,00	32	0,75	67	1,23			2300	−1,83	+1,84	186,29	178,96
2400	20,60	19,22	33	0,99	68	0,47			2400	−1,80	+1,82	186,24	179,00
2500	20,82	19,44	34	1,24	69	0,72			2500	−1,78	+1,79	186,19	179,05
2600	21,04	19,66							2600	−1,75	+1,77	186,13	179,11

Example.——V. E. 1828..1800	. .	Ma. 21,26
28	. .	0,78
M. E. .	.	22,04
App. Eq.	. .	1,92
		20,12

March $20^d\ 2^a\ 53^m$. N. A. $20^d\ 2^h\ 47^m$.

Note.—This Table is calculated for the Meridian of Greenwich.

iv. *Principal* LUNAR OCCULTATIONS *of the Fixed Stars in the months of February and March,* 1829, *calculated for the Royal Observatory at Greenwich. By* THOMAS HENDERSON, *Esq.*

Date.	Names of Stars.	Magnitude.	Immersion.		Emersion.	
			Mean Time.	Point.	Mean Time.	Point.
			H. M.	°	H. M.	°
Feb. 14	λ Geminorum	4.5	16 37	7 R.	16 55	42 R.
16	α^2 Cancri	5.	17 51	56 L.	Under H	orizon.
Mar. 17	π Leonis	4.5	5 36	141 L.	6 49	35 R.

The calculations having been performed in an approximate manner, observers are advised to be prepared two or three minutes before the times above stated. The fifth and seventh columns show the point of the Moon's Limb where the immersion and emersion will take place, reckoning from the vertex or highest point, the letters L. and R. signifying towards the observer's left and right hand.

[To be continued.]

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v. *Computation of* LONGITUDES *on a* SPHEROID.

A geodetical line may be considered as belonging, throughout each element of its length, to the conical surface which is a tangent to the curved surface on which it is described; and the surface of every cone being capable of being unrolled on a plane, without any alteration of its elementary form, the corresponding elements of the curve must form straight lines by this developement.

We may therefore consider, in the first place, the state of a plane surface rolled round a cone, and inquire into the form assumed by a straight line on that surface. Calling the line a tangent to a circle, of which the centre is applied to the apex of the cone, it is obvious that the secants will be made to correspond with the superficial radii of the cone, the point of contact becoming the vertex of the geodetic line, and the diameter of the circle constituting an asymptote which the curve can never reach at any finite distance; as will be obvious on inspection, if we wind a sheet of paper round a funnel, or twist it into a *cornet*. The angle of the secant with the tangent is obviously equal to the angular distance from the diameter which becomes the asymptote, to which it becomes the alternate angle. Hence as we roll the surface equably round the cone, it is obvious that the inclination of the geodetic line to the superficial radius must also vary equably, the change amounting for instance to a quadrant between the vertex and the asymptote; a distance which will correspond to the same curvilinear length, on the base of the cone, whether referred to the apex of the cone or to the centre of the base, and will subtend an angle in the base as much greater than from the apex as the oblique side of the cone is longer than the axis, or as the sine of the angle formed by the side with the axis is less than the radius. Hence it follows that the angular change of azimuth of this geodetical line is always as much smaller than the angular increment of the base, as the sine of the inclination of the side to the axis is less than the radius.

We have, therefore, the general equation $d\mu = s dz$, μ being the azimuth of the geodetical line, z the longitude, and s the sine of the inclination of the surface of the cone touching the given solid to the axis; or, in other words, the sine of the latitude.

Hence for any short distance, the difference of longitudes may readily be deduced from the difference of azimuths, by taking the sine of the mean latitude, or that of the latitude of the middle of the arc, for a divisor.

It may, however, often be easier, and it will be perfectly accurate, to consider only the plane triangle formed by the rectilinear distance in a tangent plane with the two meridians; for if the tangent of the difference of the two azimuths thus measured, be divided by the sine of the true latitude at the point of contact, where the lines are horizontal, the result will be accurately the tangent of the difference of longitude, whatever the form of the solid of revolution may be.

This proposition will be readily admitted on considering that the axis of the solid is the intersection of the two meridians, and that the distances of the points of the axis in the plane of the given horizon and in the plane of a parallel circle, either from the tangent point or from the perpendicular falling from the remoter station on the meridian, are in the ratio of the radius to the sine of the latitude, and that this perpendicular is the tangent of each of the angles to be compared, with respect to these two distances considered as radii of the respective angles.

In the practice of surveying there can be no possible difficulty in taking stations near enough to employ this method with the utmost accuracy, and in ascertaining separately the true latitude of each station, with moderate care, as subservient to the computation of the *difference* of longitudes.

The observed difference of latitude between the two oblique stations, compared with the difference of longitude and the azimuth, will give the relation of the two radii of curvature, and consequently the eccentricity of the spheroid, without any linear measurement whatever; unless there should be any error in these hasty remarks.

T. Y.

MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *M. Brard's Test of the Action of Frost and Weather on Building Materials.*—Some years since M. Brard devised a process by which a degree of knowledge was supplied relative to the property of building stone to resist the action of frost.* This process has been minutely investigated in France, and, with certain precautions, found highly useful. The following is a set of practical directions, which has been drawn up for the purpose of being placed in the hands of architects and workmen.

i. Specimens of the stone are to be taken from the various doubtful places in the quarry, *i. e.* from places where the colour, grain, and aspect vary.

ii. These are to be cut or sawn into cubes of two inches in the side, with sharp angles: pieces broken off are not fit for trial, as they may be injured by the force of the blows applied, and so give indications of weakness not belonging to the stone.

iii. Each specimen is to be numbered, or marked, with China ink, or by a steel point; and notes relative to the place from which each cube came are to be taken.

iv. A quantity of water, sufficient for all the specimens, is to be saturated at common temperatures, with sulphate of soda, for which purpose rather more than the water will dissolve should be left in the vessel an hour or two after it has been thrown in.

v. This solution is to be heated in a convenient vessel until it boils freely; when, without removing the vessel from the fire, all the specimens are to be introduced, and entirely submerged.

vi. The boiling is then to be continued for half an hour, and on no account for a longer time; or effects too strong in their kind will be obtained.

vii. The specimens are then to be taken out, and suspended by thread or string, so as to touch nothing else. Beneath each is to be placed a clean vessel, containing some of the clear liquor in which the cubes have been boiled.

viii. If the weather be not too damp or cold, the cubes will be found twenty-four hours afterwards covered with small white saline needles, similar in appearance to the crystals of saltpetre, and other salts occurring upon walls. The cubes are then to be dipped in the portions of solution respectively between each, so as to destroy these crystalline appearances. This is to be repeated each time the crystals are well formed; they will usually be found larger and more abundant after a night than in the course of a day: the trial may be conducted with advantage in a close apartment or a cellar.

ix. If the stone tried can resist frost, this operation will remove nothing from it, and there will be found neither grains, nor scales,

* See Quarterly Journal, xvii. 148.

nor fragments in the vessel beneath corresponding to it. If, on the contrary, the stone yields to frost, then the salt, even in its first appearance, will carry off with it fragments of the stone, and the cube will lose its angles and edges. The trial should finish on the fifth day from the first appearance of the fine crystals upon the stone; at which time there will be found at the bottom of each vessel the quantity of matter which has been detached from the respective cube.

The crystallization of the salt may be facilitated by dipping the stone into the solution beneath so soon as the crystals have appeared upon a few points, and repeating this slight operation five or six times in one day. The observation already made that the solution is to be saturated when cold, is highly necessary to be attended to, for experiments have shewn, that stone which completely resisted the action of frost, and of a cold solution, was entirely disintegrated by the use of a hot solution; and the same effect takes place, though in a less degree, if the trial be continued longer than the fifth day.

x. To judge between two stones of the degree in which they resist the action of frost, it is only necessary to collect the portions of matter which have been detached in the above trials, from the faces of the cubes, to dry and weigh them; the greater the weight of the matter separated, the more has the stone yielded. If a cube 2 inches in the side, or having 24 square inches of surface, loses 180 grains in this way, a cubical toise of the same stone would lose 3 lb. 6 oz. in the same space of time.—*Annales de Chimie*, xxxviii. 160.

2. *On the Compression of Water in different Vessels.*—A series of very interesting researches have been made lately by different philosophers on the compressibility of fluids; but a serious difference of opinion has arisen relative to the effect of the pressure used upon the vessel containing the fluid experimented with. The fluid is put into a vessel, as a globe with a very narrow neck, and then this vessel being immersed in water, or some other fluid, pressure is applied to the latter in such a manner that it not only presses upon the fluid in the first vessel, in consequence of the neck being open, but also with an exactly equal force on the outside of that vessel; so that, in reality, the pressure on the inside and outside of the measuring vessel is exactly equal. Then arises the question of what alteration takes place in the form and capacity of this vessel in consequence of the pressure. MM. Colladon and Sturm, whose memoire has been rewarded by the Academy of Sciences, think that the bulk and capacity of the vessel has been *diminished* in the same proportion that would have occurred if it had been the outside layer of a solid body of the same form and material; whilst M. Oersted, and others, reasoning upon the circumstance, that as the pressure increases on the outside an equal increase on the inside is opposed to it, think that, in reality, the capacity is *increased*, but by a quantity so small as not to be appreciable. The only

effect of the pressure is in this case supposed to be that of making the vessel a little thinner.

M. Oersted, to prove his opinion, compressed water in vessels formed from very different materials, and having very different compressibilities in themselves. Thus he compared the results obtained in lead, with those obtained in glass; the former according to him being 18 times more compressible than the latter. After making the necessary corrections, he found that the apparent compressibility of water in lead was a very little more than in glass—the difference being only two millionths of the whole volume of water under the pressure of one atmosphere. Instead of this result, he states that the water ought to seem to expand, if, as his opponents say, the vessel contracts under the pressure as if it were the exterior of a solid mass of lead.

Similar results were obtained with bottles of brass and tin, and the details of all the experiments will shortly be published. M. Oersted cautions experimenters against the errors occasioned by bubbles of air, often found when water remains long in contact with metals, and also against an apparent effect of great compressibility when water has been for a short time only in contact with the surface of glass or metal.

In a note, appended to the remarks of M. Oersted, is a mathematical investigation of the question by M. Poisson, in which he arrives at the directly contrary result, *i. e.* the opinion adopted by MM. Colladon and Sturm, and states that a hollow sphere equally pressed within and without, suffers, when all other things are equal, the same diminution in radius as if it were a perfectly solid globe. From the same train of reasoning he also draws the conclusion, that calculating, from the same experiment, on the compression of a bar of lead, as that referred to by M. Oersted, the diminution in capacity of a leaden bottle is only one half of that stated by the latter philosopher in his remarks.—*Annales de Chimie*, xxxviii. 326.

3. *Maximum Density of Salt Water.*—A highly interesting set of experiments upon the maximum density of solutions of common salt has been made by Dr. Erman, fils, the results of which are as follows.

i. Salt water of specific gravity 1.027 has no *maximum* of condensation whilst it remains liquid; even when ice is forming in it, the portion remaining increases in density as the temperature falls.

ii. Salt water of specific gravity 1.020 has no maximum of condensation, or at least its maximum is not sensibly distant from 1°. 25 R. (34°.8 F.) which is the point of congelation.

iii. Salt water of specific gravity 1.01 has a maximum, but very much lower than in pure water, for it is attained at the temperature of 1°. 5 R. (35°.4 F.).

It appears, therefore, that the addition of common salt makes the point of maximum density of water descend, and, ultimately, quite disappear; or, what appears more probable, puts it, as it were, below the point of congelation. This is a circumstance shewn to exist, with regard to the alloy of Rose, and which might perhaps be met with in many other bodies, if the changes in their volume occurring near the point of congelation were examined.—*Annales de Chimie*, xxxviii. 287.

4. *Chladni on the Propagation of Sound.*—MM. Chladni and Soemmering, whilst experimenting on the polarization of sound, as observed and described by Weber, have remarked certain effects which they think throw great light upon the subject, and upon the propagation of sound in air. Applying the theory of liquid undulations to those which occur in the air, M. Chladni made sonorous rods vibrate in water, on the surface of which he had spread a very thin layer of lycopodium for the purpose of rendering the undulations visible. When a sonorous metal or glass rod is passed through a liquid surface, four currents are observed about it, of which two are in the direction of the vibrations of the rod, and the other two perpendicular to this direction. The currents in the direction of the vibrations are outward or excentric, the two other currents are entering or concentric. In all the outward currents, the lateral parts are curved outwards and then converge with the entering currents, so that there is found between each excentric and concentric current a circular movement representing an oval, the most acute end of which is turned inwards. When a part of the excentric current is thus in contact with the opposed current, it becomes diverging close to the sounding rod, then reunites again with the excentric current, and so on in succession: from this it results that the centre of the oval seen between two currents is near to the sonorous body, and that it revolves round itself.

The excentric currents are generally longer and narrower than the concentric currents. When a tuning fork is used, or any other instrument with a double vibrating stem, each stem indicates its own particular set of appearances. M. Chladni says, "that as that which passes in aërial undulations may be understood from these appearances, it becomes easy to explain the interruption to the progress of sound in certain directions where the waves take transversal courses," *i. e.* where they pass from a centrifugal to a centripetal state.—*Kastner's Archives*, vii. 233.—*Bull. Univ.* A. x. 136.

5. *Experimental Velocity of Sound.*—Experiments were made on this point in Acoustical Philosophy, by MM. Myrbach and Stampfer, between Untersberg and Moënchstein near Saltzbourg, from August 15, to September 30, 1822. The distance was 30,601 French feet. The difference of level 4198 feet. The mean of 88 observations gave 1025.9 feet as the velocity of sound per second, at the temperature of 32° F.

6. *Syphon Hydrometer.*—A description of Mr. Meikle's syphon

hydrometer is given at page 371, of vol. xxii. of the former series of this Journal. Since then he has simplified the application of the principle. "What I would suggest," he says, "as likely to render a simple glass syphon very convenient as a hydrometer is, merely to put a small hole in its upper or bent part. On immersing each leg of such a syphon in a separate liquor a portion of the air escapes through the hole, and allows the liquids to rise in the tubes to the level of their cisterns. If we now apply the finger to the hole, and raise the instrument, I need scarcely say, not wholly out of the fluids, the liquors will be raised in the tubes by the pressure of the atmosphere so as to form columns elevated to heights above their respective cisterns, inversely proportional to their specific gravities. For the weights of the two columns must obviously be equal, each being the difference between the pressure of the atmosphere and that of the included air.

"If the tube be very wide, the effect of capillary action may in most cases be neglected: so that the one column being water, we divide its length by that of the other liquid, and obtain a quotient which is the specific gravity of the latter. But the effect of capillary action may be easily obviated altogether by holding the syphon at two different heights, and noting the corresponding columns. We obtain the specific gravity free from capillary action by dividing the *difference* of the columns of water by the *difference* of those of the other fluid. The greater these differences can be made so much the better; even using, perhaps, for the shorter columns the mere capillary elevations."

In transparent liquors we may make the one pair of columns to be depressions under the surfaces of the liquids contained in glass cisterns; which may be effected by immersing the instrument, with the hole previously stopped, to confine air. In this case, we obtain the specific gravity by dividing the *sum* of the elevation and depression of the water by the *sum* of those of the other fluid.

As a hole may be apt to weaken a glass tube, especially at the curved part, where it should be strongest, two straight pieces of glass tube may be joined by means of a bit of bent tin tube. The hole may then be more easily made, and will be less apt to weaken the instrument. The legs of the syphon should be graduated or divided into small equal parts: this may be very easily done by merely transferring to the tubes, with the assistance of a square, the divisions which are already made on any scale of small equal parts. It is obvious that the legs ought to be parallel.—*Phil. Mag.* N. S. iv. 258.

7. *Alteration of weight in Rock Crystal, by pulverisation between Agates.*—The following experiments are by M. Pajot Descharmes. A quantity of rock crystal was several times heated red hot and plunged suddenly into cold water; it was then dried; pulverized in an agate mortar, and after being sifted, weighed one ounce or 576 grains.

i. This portion was pulverised still further on an agate plate, with an agate muller, for twenty minutes. Being then collected, it was found more bulky in volume than before, and increased in weight 120 grains; the whole quantity now being 696 grains.

ii. 288 grains of this were pulverized anew, in the same manner and for the same time: the volume increased a little; the weight increased from 288 to 342 grains.

iii. Two gros (118 grains) of the latter portion, pulverized again for the same time, increased in weight 31 grains.

iv. A third rubbing on the agate for nine minutes, caused an increase in weight of 6 grains.

v. A fourth rubbing for fourteen minutes increased the weight 10 grains, so that by successive pulverization, the two gros had increased in weight 47 grains.

vi. Another rubbing was given to the powder, but the matter seemed incapable of further division under the muller, and the increase was only 2 grains.

For the purpose of ascertaining to what the increase of weight was due, half an ounce of that mentioned in the second experiment was heated and stirred in a porcelain basin; being then weighed, whilst warm, the excess of 54 grains was found diminished to 43 grains. The weight of the plate and muller were not sensibly changed in these experiments, and the surface was scarcely altered. —*Recueil Industriel*, vii. 64.

8. *Friction of Screws and Screw-presses.*—An examination of the friction in screws having their threads of various forms, has led M. Poncelet to this very important conclusion, namely, that the friction in screws with square threads is to that of equal screws with triangular threads, as 2.90 to 4.78, proving a very important advantage of the former over the latter, relative to the loss of power incurred in both by friction.

9. *Paper Linen.*—According to the Paris papers a new invention, called *papier linge*, has lately attracted much attention. It consists of a paper made closely to resemble damask and other linen, not only to the eye but even to the touch. The articles are used for every purpose to which linen is applicable, except those requiring much strength and durability. The price is low, a napkin costs only five or six centimes (about a halfpenny), and when dirty, they are taken back at half price. A good sized table-cloth sells for a franc, and a roll of paper with one or two colours for papering rooms or for bed curtains, may be had for the same price.—*New M. Mag.* xxiv. 545.

10. *Hardening of Steel.*—According to M. Altmutter, when steel is hardened by being quenched in any other liquid than mercury, it is

always more or less oxidized. According to him, immersion when hot in mercury is the only method of obtaining steel hard and uninjured by oxidation.—*Jahrb. der Wien*, 1828.

11. *On the dyeing of Wool with Prussian Blue.*—A very favourable report has been made to the Academy of Sciences at Paris by MM. Thenard, D'Arcet, and Chevreul, on a process devised for the above purpose, by M. Raymond, fils. A reward of 25,000 francs was offered by the French government in 1811, and has not yet been claimed.

One great difficulty in the way of dyeing wool blue by means of this substance was the charging the wool with a sufficient quantity of peroxide of iron previous to its immersion in the ferrocyanic acid. After numerous trials, M. Raymond found that this object was best attained by the use of a solution prepared by mixing water, sulphuric acid, nitric acid and proto-sulphate of iron, so as to convert the latter into per-sulphate, and then adding a mixture of sulphuric acid and bitartrate of potash, which, according to him, is equivalent to tartaric acid and sulphate of potash. This solution, which he calls the tartro-sulphate of peroxide of iron, should have a specific gravity of 1.333, and is then to be used as a bath either for woollen cloth or for the unspun wool.

Four operations are then mentioned as necessary to produce the colour in the wool.

i. *Iron bath.* This should be of specific gravity 1007. It should be heated by steam, and when at from 86° to 104°, the cloth is to be immersed and retained in motion on a wheel, whilst the heat is to be raised to the boiling point. Two sets of test specimens are required to guide the dyer; one a series of pieces of cloth with different quantities of oxide of iron, the other a corresponding series dyed blue. From these it may be judged when the cloth has received the quantity of iron required for a particular tint. It is then to be removed from the bath, allowed to drain a short time, and then washed in river water with great care. Cloths to be dyed of a clear blue are to be immersed in the bath when cold, and an addition of sulphuric and cream of tartar is required. A bath which has been once or twice used may be refreshed by the addition of tartro-sulphate of iron; but a period is at last attained when, containing an injurious quantity of acid and greasy matter, it should be thrown away.

ii. *Blue bath.* This is a solution in water of ferro-prussiate of potash; the latter being in the proportion of 0.085 of the weight of the cloth. The cloth is to be immersed for a quarter of an hour, and then taken out. A quantity of sulphuric acid, equal in weight to the ferro-prussiate, is then to be diluted with 5.5 times its weight of water and divided into three equal portions; one of these is to be added to the bath, and the cloth immersed for a quarter of an hour: being taken out, the second portion is to be added, and then the cloth replaced for half an hour without agitation: the cloth is then to be placed on the wheel, the bath heated, the cloth passed through, and then taken away and passed through cold water.

iii. *Milling.* This is to be done at common temperatures, with a solution of 1 part of soap in 20 parts of water for 20 parts of cloth by weight.

iv. *Brightening.* The brightening of deep blues is done by plunging the cloth for twenty-five or thirty minutes in water containing one three-hundredth of its volume of ammonia. As this gives too grey a hue to clear blues, an acid water consisting of 5 parts of cream of tartar dissolved in 10 parts of water and added to 1000 parts of water, is prepared for them. This is heated by steam, and the cloth passed through it for twelve or fifteen minutes, after which it is washed in running water.

A kilogramme (about $2\frac{1}{4}$ lbs.) of cloth dyed blue in this manner costs $1\frac{1}{2}$ francs, whilst dyed with indigo it would cost more than double. Prussian blue thus applied on wool resists the action of cold water, of air, the sun, friction; has the characters of a good solid colour, and more lustre than indigo.

The commissioners proved by their own experiments that the pertartro-sulphate of iron is a very proper agent for the transference of oxide of iron to wool, and may be in this respect employed with great advantage in many dyeing operations.

M. Raymond also remarks a peculiar change in the mechanical properties of wool occasioned by chlorine. When passed through a bath of that substance in solution, the wool becomes silky and loses its property of felting; a change which, though it might be advantageous sometimes, would occasionally be very inconvenient and injurious.—*Rèvue Ency.* xxxix. 779.

§ II. Chemical Science.

1. *Electro-chemical Theory of Combination.*—One part of this theory, according to certain persons, supposes that elements capable of combining are in opposite states of electricity; that when they combine, the electrical attractions draw them together, and that in the act of combination these electricities mutually neutralize each other. The discharge of the electricity has been considered as the source of the heat and sometimes light evolved, as in combustion. This view involves the difficulty, that *after* the electricities are neutralized the attraction supposed to depend upon them still continues. Upon this, M. Fechner remarks, that the difficulty no longer exists if it be supposed that there is a separation of electricity equivalent to a discharge before the particles come into actual contact. On this view chemical light and heat are not the consequence of the union of different electricities, but of their separation. When two combining particles are found within the distance of molecular attraction they will always remain in opposite states of electricity,

even after the separation of the two electric fluids has happened, just as occurs with two voltaic piles. The two particles would approximate and even come into perfect contact were it not for the repulsive power of heat which keeps them separate, and prevents the neutralization of their electricities.—*Jahrb. der Phys.*—*Bull. Univ.* A. x. 150.

2. *Chemical Powers of Magnetism.*—The following experiment is by the Abbé Rendu. If a bent glass tube be filled with the tincture of red cabbage and two iron wires suspended to the poles of a magnet be immersed in the liquid in the two branches, the tincture will, in a quarter of an hour, become blue or of a deep green * in both branches of the tube, although the magnetism of the two wires must be of different kinds. The same result is produced if well tempered and polished steel needles be used in place of the wires. If one wire be removed, the effect takes place only in the other branch of the tube where the wire remains. The same results occur if the wires are not in contact with a magnet; but being then cleaned they are found to have become magnetic. Tincture of litmus undergoes similar changes, but far more slowly, and the colour becomes green only in the leg containing the north wire.

M. Biot considered that the oxidation of the wires might in these cases produce the ordinary effects of a voltaic current, but that as magnetism exerted its influence notwithstanding the presence of interposed bodies, he advised M. Rendu to separate his wires from the tincture by small glass tubes closed at their lower extremities. In this case even, according to M. Rendu, the *same* phenomena were produced, but much more slowly. The tincture of red cabbage, however, became perfectly green in two days.—*Mem. de Savoie.*—*Bull. Univ.* A. x. 196.

3. *Effect of Magnetism on the Precipitation of Silver.*—The following very singular results (if correctly reported, and there seems to be no reason to doubt them in that respect) strikingly illustrate the influence exerted by magnetism over the precipitation of silver. All of them were made by Professor Muschman, and some so early as 1817. A glass syphon, half an inch in diameter and fixed with its angle downwards, had mercury poured in so as to cover the lower part, but not prevent free communication between the two legs, and then a solution of nitrate of silver of specific gravity 1.109 was poured in so as partly to fill both legs. The syphon was placed accidentally in the plane of the magnetic meridian; in a few minutes the silver was precipitated, and, to the surprise of the observer, more abundantly and with more brilliancy in the leg towards the north than in the other.

* The resemblance of salts of iron to alkaline solutions in their effects on vegetable colours has been noticed long ago, and must not be forgotten here.—See Quart. Journal, vol. xiii. p. 315.

As no other cause than terrestrial magnetism could be perceived likely to produce this result, the experiment was repeated with syphons of the same diameter, but having legs twelve inches long. Two simultaneous experiments were made, in one of which the syphon was made to coincide with the plane of the magnetic meridian, as before; in the other, the syphon was perpendicular to this direction. The two were placed in the same chamber on the same table, and in the same relation to light, air, &c. The silver began to precipitate first in the former of these syphons; the metal rose in the north branch with greater brilliancy, in larger quantity, and in larger needles than in the southern branch. The silver in the latter seemed as if fused, was scarcely at all radiated in form, and was mixed with much mercurial salt that collected on this side. In the other syphon, there was no change in less than twelve hours, after which the silver began to precipitate slowly, but equally in both limbs of the instrument. Next morning the first syphon had deposited all its silver; in the other a deposition still went on; the latter was placed therefore on another table, with the south pole of a magnet under one of its legs. A day after it was clearly seen that the silver had passed in the direction of the magnet, and rose higher in that limb than the other. These experiments, repeated many times with different sized syphons, always give the same result.

For the purpose of giving the silver more freedom of choice during its precipitation, circles were drawn with tallow upon small plates of glass, solution of nitrate of silver placed on the glass within the circles, and a rounded piece of zinc placed in the centre of each solution. The silver immediately began to precipitate in circular zones, but so that the circles always extended much more towards the north than in the other directions; the zinc and its oxide passed towards the south. These experiments, repeated, always gave the same results.

Plates of glass, similarly charged with solution and zinc, were then placed about two metres from the poles of strong artificial magnets, whilst similar plates were preserved at a distance. The effect was striking on the plate placed near the south pole of the magnet, for the silver passed towards the pole with great rapidity, and was entirely precipitated in one-fourth of the time necessary for the solution on the distant plates.

In the experiments with magnets it may be supposed that the conducting power of the iron may have some effect analogous to that observed by Zimmerman, when metallic precipitations took place in the vicinity of conducting bodies; but in the experiments without magnets, the influence of terrestrial magnetism appears no longer doubtful.

M. Muschman then refers to a disposition of metals in the bowels of the earth, perhaps referable to a similar cause. A vein of metallic silver is found at Königsberg, in the *Fall-band*, which extends from north to south. The presence of the silver is always indicated by the occurrence of a certain quantity of pyrites and blend. May one not be led to conclude, that the silver was originally united with the sulphur, but that, by the effect of terrestrial magnetism, it has been carried towards the copper and zinc, or, in other words, that the vein occurring in the direction of the magnetic meridian, the magnetic force has produced a similar effect in it to that occurring with a platina wire in the voltaic circuit?—*Ann. de Chimie*, xxxviii. 201.

4. *On the Magnetism of Galvanometer Wires, and of Metals generally.*—Every electrician is acquainted with the beautifully delicate galvanometer, contrived by M. Nobili, in which two magnetic needles nearly neutralize each other's tendency to point in obedience to the earth's magnetism, and are conjointly acted upon by a wire conducting voltaic electricity, so as by their deflection to indicate the presence and force of the current in the wire. M. Nobili has often observed that the needle of his instrument, when out of use, instead of pointing to zero, and taking up a position parallel to the conducting wires, deviated to the right or the left, sometimes by as much as 15° or 20°. This deviation only occurred in those instruments, which were so thoroughly neutralized, that the earth possessed scarcely any directive influence over them. On examining the wires of the galvanometer no iron could be detected in them, and, after much research, M. Nobili concluded that the effect was due to an attraction exerted by the copper wires of the instrument, analogous in its nature to that observed by M. Arago between the magnetic needle and various metals*. Six or seven copper wires, about one hundredth of an inch in diameter, being united and placed about the twenty-fifth of an inch from the needles in the plane of their motion, drew them from 15° to 20° from their place of rest, when uninfluenced. The same effect took place with platina wires, but more weakly; silver wires were found to be almost without action on the galvanometer needles. By using silver wires M. Nobili was able to construct a galvanometer, far more sensible than with wires of any other metals, in consequence of its freedom from interfering action. This instrument he intends shortly to describe.—*Bib. Univ.* 1818, p. 79.

5. *Early History of Electro-Magnetism.*—In the year 1801 Gautherot brought two fine pianoforte strings in contact, one with the upper and the other with the lower, end of the pile, keeping the extremities fluttering in the air. When these ends touched each other, he says (*Ann. de Chimie*, xxxix. 209,) "A very decisive adhension took place; they seemed united as by a magnetic power, which was so strong, that he could move the united wires in every direction to a distance of some centimetres." Thus it may be seen that, in the investigation of nature, a few detached observations are

* According to M. Arago, this is rather a repulsion than an attraction.—Ed.

insufficient; but that they must be pursued and combined, by which means it was that Oersted became the discoverer of electro-magnetism.—*Schweigger's Jahrbuch*, 1828.

We can see nothing in the above effect at all resembling, or dependent upon, electro-magnetism. The attraction at first, if there were any, was common electrical attraction, well known to be exhibited by the voltaic poles. The after-adhesion was, most probably, the result of slight fusion at the place of contact—certainly, not of electro-magnetic attraction.—Ed.

6. *Intense Light.*—The intense light produced by igniting lime in the oxy-alcohol flame, is well known, and has been beautifully applied in the construction of geodesical signals by Lieutenant Drummond. It is said, that a very ready mode of exhibiting it on a small scale is, to place a small piece of lime on charcoal, lighted, at the spot by a little piece of tinder, and throw a jet of oxygen from an ordinary blow-pipe aperture upon it.

7. *Table of atomic weight of Bodies, by* Berzelius.—The table by Berzelius is stated to be according to the most recent and exact analyses. We do not think it necessary to give any more of it than that part which relates to hydrogen as unity. The atomic numbers adopted by Berzelius are often double, and sometimes triple and quadruple those of other chemists; but these cases generally explain themselves, and, for the simple substances in the following list, are marked by preceding figures:—

2	Oxygen	16.026	2	Uranium	434.527
	Hydrogen	1.000	2	Bismuth	213.208
	Nitrogen	14.186	2	Tin	117.839
2	Sulphur	32.239	2	Lead	207.458
2	Phosphorus	31.436	2	Cadmium	111.665
	Chlorine	35.470	2	Zinc	64.621
2	Bromine	150.821	2	Nickel	59.245
	Iodine	123.206	2	Cobalt	59.135
2	Fluorine	18.734	2	Iron	54.363
2	Carbon	12.250	2	Manganese	57.019
2	Boron	21.793	2	Cerium	92.105
6	Silicium	44.469	2	Zirconium	67.348
2	Selenium	79.263	2	Ittrium	64.395
2	Arsenic	75.329	3	Glucinium	53.123
2	Chromium	56.383	2	Aluminium	27.431
2	Molybdenium	95.920	2	Magnesium	25.378
2	Tungsten	189.621	2	Calcium	41.030
3	Antimony	129.243	2	Strontium	87.709
4	Tellurium	129.243	2	Barium	137.325
	Columbium	184.896	2	Lithium	20.474
2	Titanium	62.356	2	Sodium	46.620
	Gold	199.207	2	Potassium	78.518
2	Platina	194.753		Ammonia	34.372
2	Rhodium	120,305		Cyanogen	52.872
2	Palladium	114.526		Sulphuretted hydrogen	34.239
2	Silver	216.611		Muriatic A.	72.940
	Mercury	202.863		Hydrocyanic A.	54.872
	Copper	63.415		Water	18.026

Nitrous oxide	44.398
Nitric oxide	30.212
Nitrous A.	76.449
Nitric A.	108.503
Hyposulphurous A.	48.265
Sulphurous A.	64.291
Hyposulphuric A.	144.609
Sulphuric A.	80.317
Phosphoric A.	143.003
Chloric A.	151.071
Perchloric A.	167.097
Iodic A.	326.543
Carbonic A.	44.302
Oxalic A.	72.578
Boracic A.	139.743
Silica	92.548
Selenious A.	111.315
Selenic A.	127.341
Arsenious A.	230.790
Oxide of Chromium	160.845
Chromic A.	104.462
Molybdic A.	143.999
Tungstic A.	237.700
Protoxide Antimony	306.565
Antimonious A.	161.296
Antimonic A.	338.617
Oxide Tellurium	161.296
Columbic A.	417.878
Titanic A.	94.409
Protoxide Gold	414.441
Peroxide Gold	446.493
Oxide Platina	226.806
Rhodium	228.689
Palladium	130.552
Silver	232.637
Protoxide Mercury	421.752
Peroxide Mercury	218.889
Protoxide Copper	142.856
Peroxide Copper	79.441
Protoxide Uranium	450.553
Peroxide Uranium	917.132
Oxide Bismuth	474.495
Protoxide Tin	133.806
Peroxide Tin	149.892
Protoxide Lead	223.484
Minium	462.995
Peroxide Lead	239.511
Oxide Cadmium	127.691
Zinc	80.649
Nickel	75.271
Cobalt	75.161
Peroxide cobalt	166.349
Protoxide Iron	70.389
Peroxide Iron	156.804
Protoxide Manganese	73.045
Deutoxide Manganese	162.117
Peroxide Manganese	89.071
Manganesic acid	194.161
Protoxide Cerium	108.132
Peroxide Cerium	232.289
Zirconia	182.775
Yttria	80.425
Glucina	154.325
Alumina	102.942
Magnesia	41.404
Lime	57.056
Strontia	103.735
Baryta	153.351
Lithia	36.501
Soda	62.646
Peroxide Sodium	141.318
Potassa	94.541
Peroxide Potassium	126.593
Sulphate Potassa	174.859
Protosul. Iron	150.706
Persul. Iron	397.754
Protochloride Iron	125.303
Perchloride iron	321.545
Protochloride Mercury	476.666
Perchloride of Mercury	273.803
Ferrocyanate of potash	370.008
Alum	951.378
Feldspar	567.673

Ann. de Chim., xxxviii. 426.

8. *Test of the presence of Oxygen.*—This test, which is recommended by M. Kastner, consists of a protoxide of iron. A stoppered flask is to be filled with hot water, and the water then boiled by a spirit lamp; 5 parts of recent protosulphate of iron for every 100 of water is to be added, the ebullition continued for a minute, and then ammonia added until in excess; the flask is then to be closed, the precipitate allowed to fall, when the liquid must be removed by means of a glass syphon; the precipitate is to be washed with boiled water, and finally hot alcohol poured into the flask until it is full.

When this test-oxide is to be used, a small quantity is to be taken out rapidly in a little spoon and put into a jar filled with water, previously deprived of air by ebullition. The gaseous body to be tested is then to be passed into this jar; if there be only 1000dth of oxygen present, it will be indicated by the ocraceous colour communicated to the oxide of iron.—*Kastner's Archiv. Bull. Univ.*, A. x. 157.

9. *Combinations of the Nitrous Oxide with Salifiable Bases.*—The substances left by the partial decomposition of certain nitrates by heat are considered by M. Hess as compounds of the nitrous oxide or oxide of azote with the bases of the salts used. The compound with *potash*, for instance, may be obtained by heating nitrate of potash to redness in a silver crucible, so long as it disengages oxygen: when no more smoke rises from the crucible, and an inflamed body is extinguished upon immersion in its atmosphere, then the decomposition has been carried far enough; and the fused salt is to be poured out upon a plate of iron. When cold, its fracture is radiated; it is unaltered in the air, soluble in cold water, more so in boiling water, and crystallizes on cooling. It so much resembles nitrate of potash as not to be distinguishable in appearance. It is insoluble in alcohol; fuses with the readiness of nitre. It includes no water of crystallization, but contains per cent. 61.14 potash, and 38.80 oxide of azote.

The *soda* compound is prepared in a similar manner from the nitrate of soda, but more readily. It crystallizes in rhomboids, insoluble in alcohol. The water included in the crystals cannot be dissipated by heat. It contains 44.52 of soda, 42.67 of oxide of azote, and 12.81 of water per cent.

The *baryta* compound is to be obtained in the same manner, but the heat must not be intense, nor of long continuance; the mass is then to be dissolved in water, evaporated and crystallized, after which it should be purified by a second crystallization from the carbonate of baryta mixed with it. It is constituted of 61.47 baryta; 24.07 oxide of azote; 14.46 of water. The water of crystallization cannot be separated by heat.

The combination with *lime*, obtained in the same way, gave per cent. lime 27.58; oxide of azote 28.94, and water 43.48.

The compound with *silver* was procured by decomposing the baryta compound by sulphate of silver. During evaporation the liquid deposited long straw-coloured needles, which became black by sun-light: being slightly heated in a glass tube, they were decomposed, yielding metallic silver and nitrous acid. This compound was difficult to obtain.—*Ann. der Phys. und Chimie*, 1828, p. 257.

10. *Decomposition of Boracic Acid by Hydrogen.*—The opinion that boracic acid is not decomposed by hydrogen is not quite correct. M. Varvinski passed hydrogen through a porcelain tube heated to redness and containing boracic acid in scales. The result was a brown vitrified boracic acid, which being acted upon by

boiling distilled water, left a flocculent olive coloured residue undissolved. This substance being separated, washed and dried, was examined. Being heated on platina, it burnt into a vitreous substance. Another portion, heated in nitric acid, caused the evolution of nitrous acid, and by evaporation gave a vitreous substance, which, dissolved in water, produced a flocculent precipitate with baryta water. The olive substance, in fact, was found to be boron, and the product of its combustion boracic acid.—*Bull. Univ.*

11. *Preparation of Hydriodic Acid.*—Dissolve sixty grains of iodine in a sufficient quantity of alcohol; diffuse one ounce of finely divided starch through four ounces of water, and add this, drop by drop, to the former solution; allow the iodide of starch to settle, and pour off the clear liquid. Pass a current of sulphuretted hydrogen through the deposit, the colour will at first change to orange yellow from the formation of an iodide of sulphur, then it will become yellow, and ultimately white. The whole is to be filtered, the insoluble part washed with small quantities of water, and the solution slightly heated to dissipate the sulphuretted hydrogen. The solution may be obtained of specific gravity 1.5, and is pure hydriodic acid.—*Brande's Archives*, xxii. 45.

12. *Formation of Cyanide of Potassium.*—When nitrogen gas is passed over a mixture of potash and charcoal, heated to redness, a considerable quantity of cyanide of potassium is obtained. When ammoniacal gas is passed over a heated mixture of carbonate of potash and charcoal, the same result is produced.—*Ann. de Chimie*, xxxviii. 158.

13. *Phosphoric Acid in Potash.*—According to M. Kobell, phosphoric acid is found in nearly all potash, in crude tartar, and in the ashes of most plants. It may usually be found in potash by saturating the alkali with muriatic acid, evaporating and crystallizing, redissolving the crystals, adding ammonia to the solution, and then muriate of lime. A precipitate forms more or less slowly, which has the characters of phosphate of lime before the blow-pipe, and moistened by sulphuric acid, communicates a green colour to the flame of a spirit lamp.—*Kastner's Archives*, viii. 323.

14. *New Compound of Silica and Potassa.*—This compound, which has been prepared and described by M. Fuchs, is intermediate between glass and the oil of flints. It may be prepared by saturating a boiling solution of potash with recently precipitated silica; but better by the following process. Fuse a mixture of ten parts carbonate of potash, fifteen of quartz, and one of charcoal; pulverise the product, and dissolve it in four or five parts of boiling water, which will slowly take up nearly the whole. The solution evaporated until of a specific gravity of 1.24 will be a viscid, opalescent liquid, which, whether evaporated further, quickly or spontaneously,

will become a solid, vitreous, transparent mass, fixed in the air, and resembling ordinary glass, except that it is less hard.

This substance has an alkaline action; it dissolves with difficulty in cold water, more easily in boiling water. It is somewhat hygrometric, and in many weeks will attract moisture from the air, which penetrating it does not however destroy its aggregation, but causes the surface to become covered with scales or powder. Alcohol precipitates the aqueous solution; acids decompose the substance; many salts form insoluble precipitates with it. This new silicate of potash is composed of sixty-two parts silica, twenty-six of potash, and twelve of water. It may be employed as a coating for wood and other objects to preserve them from fire, and also as a lute in the laboratory.—*Kastner's Archives*, v. 385.

15. *Fulminating Powder*.—According to M. Landgerbe, a mixture of two parts nitre, two parts neutral carbonate of potash, one part of sulphur, and six parts of common salt, all finely pulverised, makes a very powerful fulminating powder. M. Landgerbe adopts the extraordinary error of supposing that these preparations act with more force downwards than in any other direction.—*Bull. Univ.* A. x. 151.

16. *Crystallization of Sulphate of Potash*.—If it be desired to procure very large and regular crystals of neutral sulphate of potash, it is only necessary to add a little sub-carbonate of potash to the solution of this salt, and then leave it to evaporate spontaneously in a vessel rather greater in width than depth; only one, two, or at most three crystals will be obtained, but they will be of extreme size and beauty.—Van Mons, *Kastner's Archives*, v. 462.

17. *On Double Saline Compounds obtained by Heat and Fusion, by* M. Berthier.—M. Berthier has described an extensive series of experiments on the production of double salts by igneous fusion: the results are highly interesting, and we shall give most of them, but in as condensed a form as possible.

One atom of native carbonate of baryta 24.64, with one atom of anhydrous carbonate of soda 13.32, fused at a red heat, becoming as clear and transparent as water, and by cooling, forming a compact mass, penetrated by numerous crystalline plates.

Single atoms or proportionals of the carbonates of strontia and soda, readily fused, producing, when cold, a stony compound of unequal fracture, and but slightly crystalline.

One proportional of carbonate of soda, with one of carbonate of lime, and with two; these mixtures readily fused, and if cooled suddenly, were compact, white, and translucent like enamel, and with a very crystalline fracture. They may be readily re-fused; but if the temperature be raised too high, carbonic acid is evolved, and the fused mass becomes solid.

Four proportions of carbonate of soda and one of dolomite became very liquid at a red heat: when cold, the compound was homogeneous, slightly translucid, and with a crystalline lamellar fracture.

Single proportionals of sulphate of soda and carbonate of baryta, or sulphate of baryta and carbonate of soda; or sulphate of soda and carbonate of strontia; or sulphate of strontia and carbonate of soda, gave very fusible compounds, liquid at a white heat; which, when cold, were compact, stony, nacreous and opaque, with an irregular fracture, and slightly crystalline.

Single proportionals of sulphate of soda and carbonate of lime, or sulphate of lime and carbonate of soda, fused quietly at a red heat, and yielded compact granular substances slightly crystalline, white, and translucent. A higher heat evolved carbonic acid.

Single proportionals of chloride of sodium and carbonate of baryta, or of chloride of barium and carbonate of soda, readily fused into transparent liquids, yielding compact translucent solids, very white, and with a scaly, unequal, fracture like quartz.

Single proportionals of chloride of sodium and carbonate of lime, or chloride of calcium and carbonate of soda, gave similar results, but would not bear a white heat without evolution of carbonic acid.

Single proportionals of chloride of barium and carbonate of baryta very readily liquefied; the solid matter was compact, of a fine blue colour, translucid, of a scaly fracture, and presented appearances of crystallization.

A similar mixture of chloride of calcium and carbonate of lime fused with equal facility at a red heat, but became solid at a white heat.

One proportional of carbonate of potash, with one and with two proportionals of fluorspar. Both mixtures readily fused; the solid compounds were compact, stony, slightly translucid, and presented appearances of crystallization. A white heat solidified them.

Sulphate of soda and sulphate of lime easily fuse together; the *glauberite* is such a natural compound. Single proportionals of sulphate of soda and sulphate of magnesia become fluid at a red heat, and give a compact double salt, semi-transparent, having a waxy fracture and surface like chalcedony, and no appearance of crystallization.

Single proportionals of the sulphates of soda and baryta fused completely at a white heat, and produced a compact, white, opaque solid, of a granular fracture, crystalline, and with small cross crystals here and there.

Single proportionals of the sulphates of lead and soda became as liquid as water at a red heat. When cold, the matter was compact, opaque, and gave no appearance of crystallization.

All these compounds are very feeble; water alone destroys them; but that they are true compounds, and not mixtures, is shown by their perfect fluidity and homogeneity, and especially by the circumstance that, in all those containing carbonic acid and lime, although perfectly fluid at one temperature, a higher evolves the carbonic acid, and then the mass becomes solid from the presence of lime, incapable of combining. They may probably be obtained in the crystalline form by slow cooling, and the abstraction of the fluid part when one portion only has become solid.—*Ann. de Chimie*, xxxviii. 246.

18. *Two Sulphates of Manganese.*—If black oxide of manganese be digested with sulphuric acid and the solution be evaporated, two proto-sulphates of manganese are obtained, distinct in their physical properties and chemical characters. One crystallizes in quadrilateral prisms, colourless, transparent, and terminated obliquely at the extremities. It is composed of 28 parts of water, 28.06 sulphuric acid, and 43.34 protoxide of manganese. Carbonate of potash throws down a carbonate of manganese, which becomes brown by exposure to air. The other salt is in the form of rhomboids of a rose-colour, and consists of water, 44 parts; sulphuric acid, 32 parts, and protoxide of manganese, 24 parts. This salt is not precipitated by subcarbonate of potash.—*Pfaff.*—*Jahrbuch der Chimie*, 1828, p. 121.

19. *Carbon in pig iron.*—According to M. Karsten, white pig iron contains more carbon than grey pig. The following are the proportions of carbon per cent. in pig iron, according to several of his experiments.—

White pig iron.					
Combined carbon	0.60	0.81	1.00		
Uncombined carbon	4.62	4.29	4.05		
	5.22	5.10	5.05		
Grey pig iron.					
Combined carbon	0.89	1.03	0.75	0.58	0.95
Uncombined carbon	3.71	3.62	3.15	2.57	2.70
	4.60	4.65	3.90	3.15	3.65

Mem. de Berlin, Bull. Univ. A. x. 222.

20. *New Metal, Pluranium.*—This new metal is described by M. Osann, as one of those he has discovered in Russian platina. The part of this native platina, insoluble in nitromuriatic acid, is to be operated upon for osmium; that is, treated with potash, evaporated to dryness, the dry mass mixed with a few crystals of nitre fused, and when cold, digested in water: the insoluble portion is to be again acted upon in the same way, until the part left has no metallic appearance, and then the various solutions so obtained with the undissolved residue, are to be mixed together and nitric acid added in slight excess; a black precipitate is deposited, and the odour of osmium disengaged. The liquid is then to be distilled for the purpose of separating the osmium, and when reduced to one-half its

bulk, is to be left for twenty-four hours at rest, in which time long prismatic crystals will be produced, white, with a tinge of rose-colour, and very brilliant. These are perfectly soluble in water, and are reproduced by evaporation. Being heated on charcoal, they are decomposed, one part subliming and the other being converted into a metallic globule. On adding a little muriatic acid and a piece of zinc to the aqueous solution, the zinc becomes covered with a dark grey reduced metal. When the crystals are heated in a tube, the substance sublimes entirely and unaltered, recrystallizing above.

That this oxide in crystals is not a compound of osmium is proved by its want of odour, and its distilling over in the solid instead of the liquid form, which belongs to the latter. It is not an oxide of bismuth, tellurium or antimony, because it is entirely soluble in water. A portion being sent to M. Berzelius, he confirmed the results of M. Osann, and says, " according to all the trials I have made to convince myself, you have really found a new substance; for the small crystals which sublime, cannot be confounded with any other known body. At first, I thought them to be oxide of tellurium or antimony, but they do not sensibly dissolve in bi-hydro-sulphuret of ammonia, but are converted into a grey metallic sulphuret. This sulphuret easily fuses, becoming transparent and reddish yellow, but, on cooling, acquires an opaque, metallic, grey appearance. It may be easily roasted, and the oxide so produced sublimes at a dull, red heat. This easy sublimation, and the properties of the sulphuret, prove that the crystals are not a compound of bismuth."—*Annalen der Physik*, 1828, p. 287.

21. *Preparation of Titanic Acid*, by M. Henri Rose.—Pulverise and wash menachanite, heat it highly in a porcelain tube, and pass dry sulphuretted hydrogen gas over it: the oxide of iron will become sulphuret, the titanic acid will remain unchanged. When cold, digest the product in strong muriatic acid, sulphuretted hydrogen will be evolved, and the titanic acid, rendered insoluble by the heat, and coloured grey by the sulphur, will remain. It is to be washed, and heated red-hot, to drive off the sulphur. One operation is not sufficient to free the titanic acid perfectly from iron; the product is, therefore, again to be heated in a tube, through which sulphuretted hydrogen is passing, and then treated as before; when, again washed and heated red-hot, it becomes perfectly white and pure.

Any titanic acid containing iron may be purified in this way. The heat given to the substance, when in the tube, should be high, or the titanic acid will render the washing water milky, and pass through the filters. The operation may be shortened by heating the titaniferous iron with sulphur in a crucible, and then acting by concentrated muriatic acid; but, in this first operation, as much iron remains with the titanic acid as exists in rutilite: then, an operation with sulphuretted hydrogen, renders the substance perfectly pure.—*Annalen der Phys.*

22. *Volatilization of Silver.*—The following case of the volatilization of silver is noticed by Mr. Princep, in a paper on the measurement of high temperatures. He had long used a pyrometric bar, carrying at one extremity an index, made on the compensation principle, of silver and gold. "The heat communicated to this compound index can never have much exceeded the melting point of lead, or about 700° F.; and yet the surface of the gold has gradually become perfectly discoloured, and apparently penetrated by the silver in the same manner as would have been produced by mercury at a common temperature. This effect commenced on the edges of the slip of metal, and has now advanced nearly over the whole surface of the slip of gold, giving it the appearance, under the microscope, of being studded over with hard tubercles of a leaden colour. The golden yellow, where it is not yet thoroughly changed, has become green, like that of an alloy of gold and silver. The impregnation has extended to a considerable depth in the gold, and consequently the index has become less and less sensible to changes of temperature. But I should remark, that at the fixed end of the plate, where a piece of platina foil had been joined, to strengthen and support the index, no discolouration has taken place, the platina covering seeming to shelter the gold from the argentine vapours. I should also remark, that the two metals were originally quite pure, and were united without any alloy by simply laying an ingot of silver over one of gold, and heating the two until the former just began to melt; the compound ingot was then laminated." The bar had been used constantly for nearly five years. Mr. Princep omitted to keep any note of the original weight of the bar, by which any positive loss might have been ascertained.

Query. Might not the silver in this case have entered and permeated the gold by a process analogous to that which takes place with carbon in cementation and the formation of steel?—Ed.

23. *Crystallization of Argentiferous Salts.*—As all the salts of silver are soluble in ammonia, and only the chloride, the nitrate, and the sulphate, form triple compounds with ammonia, M. Fischer proposes this method for the production of crystals of what are called the insoluble salts, considered only in relation to water. A great number of these may be thus obtained in definite forms.—*Kastner's Archives.*

24. *Argentiferous Precipitate analogous to the Purple of Cassius.*—If dilute solutions of nitrate of silver, and nitrate of tin, be mixed, the fluid gradually becomes yellow, then brown, reddish brown, and ultimately dark purple. On the addition of dilute sulphuric acid, a brownish purple precipitate is deposited, consisting of the oxides of tin and silver, and analogous in its nature to the purple of Cassius. If no acid be added, the fluid becomes colourless by degrees, and ultimately furnishes very little precipitate.—*Ann. der Phys.* 1828, p. 285.

25. *Colouration of Gold.*—The following mixtures are recommended by M. Castellani, for this purpose:—Water, 150 parts; muriatic acid, of specific gravity, 1.18, 10 parts; oil of vitriol, 4 parts; crystallized boracic acid, 2 parts.

A second mixture consists of 150 parts of water; 13 parts of a solution of acid muriate of alumina; 4 parts of crystallized sulphate of soda; and 3 parts of crystallized boracic acid. To each of these mixtures should be added 20 grains of a neutral solution of muriate of gold.

26. *On the Absorbent Powers of Salts for Water.*—The following results have been obtained by M. Brandes. The substances after being dried, even by a red heat, when they would bear it, were then placed in the atmosphere of a close vessel containing a little water.

Carbonate of potash—100 parts absorbed 360.3 of water in 42 days; the solution itself is hygrometric; but the power varies with the state of the atmosphere.

Sulphate of potash—100 parts, heated for half an hour, lost 1.25 parts, and regained them immediately in a moist atmosphere.

Bitartrate of potash—100 became 96 by drying, and then 100.2 in the moist atmosphere.

Tartrate of potash—100 absorbed 82.3 of water in 53 days, and became liquid.

Acetate of potash—100 absorbed 91.1 in 4 days, and became liquid.

Sulphate of soda—100 became 41 by drying; then absorbed 54.6 in 36 days.

Phosphate of soda—100 lost 63 by drying; the rest absorbed 65 in 21 days.

Nitrate of soda—100 absorbed 80 in 220 days, and became liquid.

Borax—50 parts lost 18 by calcination; the rest absorbed 41 in 130 days.

Acetate of soda—100 lost 39.2 by heat; the rest absorbed 64.2 in 38 days.

Rochelle salt—100 dried became 82.5; these absorbed 26.75 in 8 days.

Chloride of Calcium—100 absorbed 124 in 96 days; the whole was liquid on the 4th day.

Sulphate of Magnesia—100 gave 57 by heat; these absorbed 44.68 in 83 days.

Alum—100 lost 42.9 by heat; the rest absorbed 46.5 in 25 days.

Sulphate of Copper—50 parts absorbed 17.35 in 62 days.

Verdigris—100 became 74.5 by drying; these absorbed 30 in 12 days.

Crystallized Chloride of Antimony—50 parts absorbed 55 in 70 days.

Emetic Tartar—50 lost 1 by heat; the 49 absorbed 1.5 of water in 18 hours.

Protosulphate of Iron—100 gave, when heated, 55.25 not quite anhydrous; these absorbed 52.75 in 14 days.

Sulphate of Cadmium—25 p. absorbed 35 in 220 days.

Sulphate of Zinc—100 gave 64 by heat; these absorbed 39.8 in 21 days.

Acetate of Zinc—100 lost 23 by heat; the 77 parts absorbed 23.7 in 19 days.—*Jahrb. der Phys.* 1827, p. 420.

27. *On the Production and Nature of Alcoates.*—Mr. Graham has formed, analyzed, and described a class of bodies which he has named *alcoates*, from their analogy to hydrates. They perfectly resemble the latter bodies, except that they contain alcohol, instead of water. The alcoates obtained were not numerous, and were formed simply by dissolving the salts constituting their base, and previously rendered *anhydrous* in *absolute alcohol*, with the assistance of heat. On cooling, the *alcoates* were deposited in the solid state. The crystallization was generally confused, but in some cases crystalline forms appeared of a singular description. The crystals are transparent, decidedly soft, and easily fusible by heat in their alcohol of crystallization, which is generally considerable.

Alcoate of Chloride of Calcium.—Chloride of calcium, as is well known, dissolves in abundance in alcohol; it furnishes one mode, and perhaps the best, of separating alcohol and ether in analytical investigations. To obtain the alcoate, 1 part of the chloride is to be dissolved in 5 parts of alcohol; the solution filtered whilst hot, and then concentrated by heat until it is thick and viscid: being then left to cool, crystalline plates appear on the surface of the solution, and sides of the vessel; they are always small, often delicately striated, and of the form of an isosceles triangle. These are frequently grouped together. They cannot be removed uninjured, because of their softness; they deliquesce in the air, and melt by the heat of the hand. The whole of the alcohol is expelled by a heat of 250°.

A portion being dried by pressure between linen, and then between bibulous paper, resembled bleached wax. Being decomposed by heat it appeared to consist of

2 proportionals chloride of calcium,	14.
7 proportionals of alcohol,	20.125.

Mr. Graham found that when water was present, chloride of calcium would retain alcohol even at temperatures of 400° or 500°, for several hours.

Alcoated Nitrate of Magnesia.—Nitrate of magnesia is to be rendered anhydrous in a glass tube by the heat of a spirit lamp, and then dissolved in boiling absolute alcohol. 1 part of the nitrate dissolves in 4 parts of alcohol, at 60°; in two parts when boiling: the magnesia from that part of the nitrate decomposed during the desiccation is left, and must be separated by filtration or decantation. The most convenient proportions are 1 to 3. The great proportion of crystalline matter is obtained as a mass of scales, of a pearly lustre and whiteness, apparently made up of small crystals. Being dried as before, it resembled the former alcoate, melting by heat, boiling and giving off alcohol. When cautiously decomposed in this manner, the proportions of alcohol and salt obtained were as follows:—

1 atom nitrate of magnesia, 9.25.
9 atoms of alcohol, 25.875.

Alcoated Nitrate of Lime.—Boiling alcohol, saturated with anhydrous nitrate of lime, gave a solution, viscid on cooling, which, during a frosty night, was resolved into an amorphous solid, slightly moist, but without any appearance of crystallization. Being dried and analyzed in the usual way, it gave the following products:—

2 atoms nitrate of lime, 20.5.
5 atoms alcohol . 14.375.

In another strong alcoholic solution, a few irregular crystals were deposited, proving the power of this *alcoate* to crystallize.

Alcoated Proto-Chloride of Manganese.—A hot saturated solution of this compound in absolute alcohol readily afforded crystals upon cooling, which were plates with ragged edges. These, collected and decomposed as before, gave the following as their composition:—

1 atom proto-chloride of manganese, 8.
3 atoms of alcohol 8.625.

Alcoated Chloride of Zinc.—The solution of chloride of zinc in absolute alcohol may be concentrated to such an extent, that when cold, it is so viscid as not sensibly to flow. It then begins to deposit crystals, which are small and irregular; such a solution was found to contain 20 parts chloride of zinc, and only 7 of alcohol. The crystalline matter, being dried with difficulty, was of a yellowish colour, and soft; by heat it entered into semifusion, and evolved its alcohol. 9 grains gave 7.65 of chloride, so that the composition is probably

2 atoms chloride of zinc, 17.5.
1 atom alcohol . 2.875.

Besides these compounds, similar combinations of chloride of magnesium and proto-chloride of iron were found.—*Trans. Soc. Edinburgh.*

28. *New Vegeto-Alkalies.*—The list of these bodies is exceedingly fluctuating. The highly interesting nature of a few of them confers great importance on the class, whilst the large number which are unimportant prevents that quick and specific verification of the announcements given to the world from various quarters which is desirable. It is only, therefore, occasionally, that some are struck out of the list, and that a great number remain of doubtful character.

M. Wackenroder describes one of these substances, which he calls *corydalia*, in consequence of its having been procured from the tubercles of the *Corydalis tuberosis.* The tubercles in coarse powder are to be macerated for several days in water. A deep brown, slightly acid liquid is obtained, which, being precipitated by an excess of sub-carbonate of soda, yields a clear grey precipitate. This digested in alcohol forms a greenish-yellow solution, which, filtered and allowed to stand, produces small crystals of *corydalia.* The largest part remains, however, in solution; the liquid, therefore, must be evaporated, the residue dissolved in dilute sulphuric acid, and the filtered solution precipitated by an alkaline carbonate. The precipitate is also *corydalia.*

This principle forms colourless, prismatic, or scaly crystals, without taste or smell; very little soluble in water; soluble in alcohol, and then acting on agents as an alkali; neutralizing acids, and forming saline compound of extreme bitterness.—*Kastner's Archives*, viii. 417.

Guarania is another of those substances described by M. Martius. He obtained it from the fruit of the *Paullinia Sorbilis.* The fruit is to be digested in hot alcohol; as the alcoholic solution cools, it deposits a fat oil, which is to be separated: it (query? the alcoholic solution or the fatty matter?) is again to be heated, and the *Guarania* will sublime. To obtain it quite pure, the sublimation must be repeated. This substance is white, crystalline, and has a penetrating odour when heated: it readily dissolves in alcohol; with more difficulty in water; the solutions have a bitter taste, and act like an alkali on alcoholic tincture of roses, and litmus paper. The aqueous solution precipitates the solutions of nitrate of silver, proto-nitrate of mercury, and the acetate and sub-acetate of lead.—*Kastner's Archives*, viii. 266.—*Bull. Univ.* A. x. 170.

29. *Amylic Acid, a New Compound of Carbon and Oxygen.*—This acid has been discovered and described by M. Tünnermann. Equal parts of starch and black oxide of manganese are to be well mixed, and put into a retort, so as to fill one fourth of it, and then a third equal part of water added, and made to moisten the mass equally. A receiver, with a safety tube, is to be connected with the retort, and then heat applied until the mixture is nearly at the boiling point; 3 parts of muriatic acid being at the same time added by degrees through a feeding tube. Much effervescence is occasioned; and when the substance in the body of the retort is nearly dry, the distillation is to be stopped, that no impure matter may distil over. The product is impure *amylic acid,* scarcely coloured, and, though containing no hydrocyanic acid, having a strong odour of bitter almonds. To free it from mixed muriatic acid, the liquid is to be saturated with carbonate of lime, filtered, evaporated till a pellicle forms, then allowed to cool and crystallize, and when the crystals of amylate of lime have been separated, the mother liquor is to be further concentrated. The crude amylate obtained is to be purified by further crystallization, until it does not precipitate nitrate of silver. Then mixing 100 parts of these crystals with 73 of sulphuric acid diluted with twice its weight of water, and distilling nearly to dryness, an aqueous solution of amylic acid is obtained.

This acid is sour, reddens vegetable blues, readily evaporates by heat, produces a sharp odour resembling hydrocyanic acid; and combines with bases to form neutral salts, most of which are deliquescent, *all* of which are readily soluble. Some of its salts contain water of crystallization, others none. The dry salts are decomposed by heat into *carbonates and charcoal.* The sulphuric, nitric, and muriatic acids decompose these salts, producing carbonaceous precipitates. The neutral salts reduce nitrate of silver and muriate of gold.

Amylic acid dissolves carbonate of lime, with effervescence. The solution evaporated yields octangular crystals, mingled with plates. The salt is soluble in 4 parts of water, and scarcely in alcohol. Its solution is decomposed by oxalate of potash. It consists of 42.16 of lime, and 57.84 of amylic acid.

The amylate of baryta, obtained in a similar way, exists in quadrilateral prisms, and consists of 57.29 baryta, 29.24 amylic acid, and 13.47 water. The salts of potash, soda, and ammonia, are deliquescent.

Amylic acid consists of 2.5 carbon, and 3 oxygen.—*Trommsdorff. Neues Journal.*—*Bull. Univ.* A. x. 171.

30. *Pinic Acid, a constituent of Venice Turpentine.*—In a long and interesting memoir on resins, contained in the *Annalen der Physik und Chemie*, for 1827, M. Unverdorben states, that Venice turpentine is composed of, i. A large quantity of volatile oil; ii. Of an oil less volatile and strongly retained by the resins present; iii. Of much resin, or *pinic acid;* iv. Of another resin dissolving in all proportions in alcohol, ether, oils, and even naphtha, and not capable of combining with alkalis or metallic oxides; v. Of a little succinic acid; vi. A small quantity of bitter extract; and vii. Of traces of a resin insoluble in petroleum.

When Venice turpentine has been distilled with 20 parts of water, until half the water has passed over, and this operation has been repeated several times, a semi-viscid mixture of resin with oils is left in the retort. This dissolved in alcohol of 65 per cent. gives a green precipitate with an alcohol solution of acetate of copper. This precipitate is pinate of copper, which being washed on a filter with alcohol, and then dissolved in alcohol with a little muriatic acid, may have the *pinic acid* precipitated by water as a white resinous and transparent substance. Being then washed with boiling water, the alcohol is removed, and it becomes a solid, inodorous and almost insipid body.

The *pinic acid*, in alcoholic solution, is gradually altered by exposure to air; it is also affected by heat; it forms only *neutral* combinations; these are most soluble in pinic acid, and also in alcohol. The *pinates of potash and soda* are obtained by slowly boiling an etherial solution of pinic acid, for a few minutes, with the alkaline carbonates, filtering and evaporating the solutions: the residuum is the alkaline pinate, a resinous, colourless mass, which dissolves completely in boiling water. The pinate of potash is precipitated from its concentrated solution, not only by an excess of potash or soda, but also by neutral salts, as sulphate of soda, muriate of soda, acetate of potash, &c.

Earthy and metallic pinates may be procured pure, by dissolving the precipitates from double decomposition in ether, and precipitating them by alcohol of 60 per cent.: or by boiling the earthy or metallic neutral salts with an excess of pinate of potash; or precipitating such salts as are soluble in alcohol by an alcoholic solution of pinic acid.

The *pinate of magnesia* is white, pulverulent, agglomerating in boiling water, dissolving readily in ether, acting with alcohol as pinate of copper. The *pinates of baryta, alumina, manganese, and zinc* are insoluble in alcohol, very soluble in ether, and resemble earthy bodies. *Pinate of lead* is very little soluble in ether; is a white powder, which may be fused without decomposition, and is then resinous and transparent. *Pinate of copper* is of a transparent green colour, very little soluble in absolute alcohol, readily soluble in ether and oils; at a moderate heat it softens, at a higher temperature becomes a proto-pinate. The other metallic pinates have been formed and have similar properties. According to M. Unverdorben, the pinic acid should be placed immediately after the benzoic acid.

The *Silvic acid* is another substance described in this memoir. It occurs in the resins of the *pinus sylvestris* and fir tree, and is probably identical with the crystalline substance found by M. Riess in white pitch. It is separated by acting upon the resin several times with alcohol, which takes up every thing but the sylvic acid. The latter crystallizes almost entirely upon cooling, is colourless, and requires a higher temperature than 212° for its fusion; it does not then crystallize upon cooling, but forms a clear transparent mass. Once crystallized, the sylvic acid dissolves with difficulty in cold alcohol of 65 per cent. If boiling, one third of its weight is taken up. Absolute alcohol and ether dissolve larger proportions; though, when cold, not more than one third of their weight. By this feeble solubility the acid may be distinguished from most resins. The crystals appear as quadrangular prisms. The acid dissolves, in all proportions, in volatile oils. The alcoholic solution strongly reddens tincture of litmus.

The *sylvates* are formed in the same manner as the *pinates*, and have very nearly the same properties. Besides the neutral compounds, a few acid sylvates may be formed; as for instance, with potash and soda. The sylvate of copper is soluble in absolute alcohol, and may in that way be separated from the pinate of the same base.—*Bull. Univ.* A. x. 161.

§ III. Natural History, &c.

1. *Mineral or Native Naphthaline.*—Many specimens of a mineral substance, first found in 1822, have been presented to the Helvetic Society of Sciences by M. Koenlein. It was discovered in the coal formation of Uznach, and has a great analogy to Naphthaline. Its primitive form is an irregular octoedron, not yet measured, but

cleaving in some directions, and breaking in other with a conchoidal fracture. The horizontal surfaces have the lustre of diamond, the others only a greasy appearance. Its colour is white, green, or yellow. It is transparent, brittle, and has the appearance of talc. It is rather heavier than water; fuses at a low temperature, the solid part then floating, and crystallizes upon cooling. It readily burns with a bright flame and much smoke. It is found in the fissures and fractures of bituminous wood, which it sometimes traverses, and where it appears to have been introduced by sublimation. The layer of lignite is from two to six feet thick, belongs to a very recent formation, and contains fossil vegetables analogous to those now existing. M. Koenlein calls it *Resinous Naphthaline.*—*Bull. Univ.* B. xiv. 421.

2. *Aërolites contained in Hail.*—It is said by M. Nelioubin, that hailstones fell in the month of January, 1825, in the circle of Sterletamak, in the government of Orenbourg, which contained small stones; these being collected and analyzed gave per cent.,

Red Oxide of Iron	70.00
Oxide of Manganese	7.50
Magnesia	6.25
Alumina	3.75
Silica	7.50
Sulphur and loss	5.00
	100.00

Kastner's Archives—*Bull. Univ.* A. x. 219.

3. *On Amber.*—M. Berzelius adopts the opinion that amber is of vegetable origin; that, like ordinary resins, it has flowed from vegetables in the state of a balm, and has afterwards acquired hardness gradually. "Amber," he says, "contains *five* substances, i. An odoriferous oil, in small quantity; ii. A yellow resin intimately combined with this oil, dissolving freely in alcohol, ether and alkalies; very fusible; and resembling ordinary vegetable resins; iii. A resin soluble with difficulty in cold alcohol, more freely in hot alcohol, from which it separates on cooling as a white powder soluble in ether and alkalies. These two resins and the volatile oil, if removed from amber by ether, and then obtained by evaporation of the latter on water, form a natural viscid balm, very odorous, of a clear yellow colour, and which gradually becomes hard, but retaining some odour. There is every reason for supposing this to be precisely the substance from which amber originates; but at the same time rather poorer in essential oil than at first, and that the insoluble substances in amber have been gradually formed by a spontaneous alteration of this balm, but at the same time have enveloped one part of it, and so preserved it from entire decomposition or change; iv. Succinic acid dissolved with the preceding bodies by ether, alcohol, and alkalies; v. A body insoluble in alcohol, ether, and alkalies, and analogous in some points to the substance found by

John in gum-lac, and called by him the principle of lac. This is formed in large quantity when a solution of gum-lac in alkali is precipitated by chlorine.—*Annalen der Physik.*

4. *A Lightning Stroke at Sea.*—On Friday, September 7th, at half-past one o'clock, the Dart steam-boat, passing through the water at about thirteen miles per hour on the five fathom channel, opposite Whitstable, running for Margate, was overtaken by a stiff squall from the west, with heavy rain. Several claps of thunder had been previously heard. After the squall had lasted a few minutes, and curled up the sea in a curious manner in patches, the denser part of the cloud seemed to settle down towards the vessel. Whilst noticing its proximity, *first* a very faint illuminating light waved over the starboard paddle-box, and immediately a strong flash and burst took place about nine or ten feet from the deck, directly between, although a little higher than the paddle-boxes. The *noise* of the explosion somewhat resembled the discharge of a large howitzer when close to the hearer, having in addition a hissing noise like a Congreve rocket, yet of shorter duration. The form and appearance of the fire was that of a flash from a twelve or fourteen inch mortar seen at night, accompanied by some thirty or forty red sparks,* like those from red hot iron when struck on the anvil. The flash, sparks, and hissing, seemed to go over the larboard paddle-box towards the sea. One of the seamen on the look out near the head of the vessel was thrown forward, bent, as he expressed it, to the deck. Two others near him received violent blows on the legs, all which, I have no doubt, arose from the sudden expansion of the air,† indeed the expansion sensibly shook the whole vessel, and the passengers on the aft part of the deck felt the heat in their faces* from the flash. Luckily the rain had driven the passengers from the fore deck either into the cabins or under the awning of the after deck.—*Maidstone Journal.*

5. *Height of the Aurora Borealis.*—From a comparison and consideration of numerous observations relative to several zones of light similar to that which appeared in so striking a manner on the evening of the 29th of September, Mr. Dalton concludes that these luminous arches of the aurora which occasionally appear stretching from east to west are all of the same height, and that height about one hundred miles. As to the height of beams, there are no observations from which that may be determined.—*Phil. Mag.* N. S. iv. 428.

6. *Aurora Borealis.*—An aurora borealis was seen from North End, Hampstead, near London, from about seven o'clock until

* These two facts would seem to imply that a part of the iron of the chimney or some other substance had been momentarily inflamed by the lightning. It is not probable that either of the effects depended directly upon the pure electricity.—Ed.

† More probably from what is usually designated as the returning stroke by electricians.—Ed.

eleven, on the evening of Dec. 1. It generally appeared as a light resembling twilight, but shifting about both to the east and the west of north, and occasionally forming streams which continued for several minutes and extended from 30 to 40 degrees high. The light on the horizon was not more than 12 or 15 degrees in height.

7. *Composition of the Mud of the Nile.*—The composition of the deposit from the Nile waters, according to the analysis of M. John, is sand, water, and clay coloured with a little oxide of iron, with a few grains of quartz, and mica, 76 parts; carbonate of lime, 10 parts; carbonate of magnesia, 1 part; oxide of iron, 3 parts; sulphate of lime, 3 parts; extractive soluble in carbonate of potash, 5 parts, with a little extractive soluble in water. The latter substances explain the fertilizing property of this deposit. The specimen analysed was taken from off a wall disinterred at Thebes.

According to M. Regnault a portion of a deposit from Nile water, taken out of a canal, five hundred toises from the river, and dried in the air, contained 11 parts of water, 6 of carbon, 4 of silica, 4 of carbonate of magnesia, 18 of carbonate of lime, 48 of alumina.—*Journal du Bas Rhin.*

8. *Method of preserving Seeds fit for Vegetation.*—Fill an old cask half full of earth, put the seeds as near as possible to the middle of the cask, then fill the latter entirely with moist earth, pressing it down, and finally closing the cask so that neither air nor water may enter it. Keep it from contact of sea water. In this manner seeds may be brought from the East Indies or New Holland in a state of perfect preservation and fit to vegetate.—*Gardener's Mag.*

9. *Preparation of Grain and Seeds by Chlorine.*—M. Remond has been convinced, by numerous trials, that grain of all kinds, maize, the seeds of cruciform plants, potatoes, &c., by treatment with chlorine, are very much increased in vegetative power, are sooner ripe, and produce a crop three or four times as great as that obtained under ordinary circumstances. His process is, to steep the seed for 12 hours in river water, (never in well water,) then 14 or 15 drops of a strong solution of chlorine is to be added for each litre (two pints) of water, the whole well mixed, and the maceration of the seed continued for six hours longer in the sun light, and under a bell-glass, or, for want of a bell-glass, under a cover made with oiled paper. The seed is then to be separated from the liquid on a cloth, and, for the convenience of sowing, mixed with a sufficient quantity of cinders, sand, or dry earth. When sown, the water of maceration is to be poured over the ground.

When possible, it is advantageous to water the ground once or twice at long intervals with water acidulated by muriatic acid, and in the same proportions as those mentioned. In addition to this process the ground must be cultivated in the ordinary way.—*Courier de l'Ain.—Bull. Univ.* D. x. 192.

10. *Employment of Slates for hastening the maturation of Fruits.*—A vine branch had been trained above the window of a house, facing the south, according to custom, in certain parts of France. Beneath this branch was a small slate roof, about three feet wide, serving to shelter a door. It was remarked, that the grapes on this roof were ripe and black, whilst those on the rest of the branch were yet green. This effect, evidently due to the heat accumulated in the slates from the rays of the sun, has been advantageously applied in assisting the ripening of wall-fruit.—*M. Bauchard—Bull. Univ.* D. x. 230.

11. *Exhalation of Chlorine by Maritime Plants.*—M. Sprengel says, that the plants which grow on the sea shore, or in soils containing common salt, exhale chlorine, principally in the night time; that which is evolved when the sun is above the horizon, is immediately converted into muriatic acid. The same plants, he says, secrete chloride of sodium, which is deposited on their surfaces in crystals. He believes, that all plants yielding soda in their ashes when burnt, naturally exhale chlorine; and that the muriatic acid with which the atmosphere near the sea is charged, is not the result of decomposed muriate of magnesia, but produced by maritime plants, and particularly by different species of fucus.—*Kastner's Archives*, vii. 161.

12. *Change of Colour in Leaves.*—M. Macaire Princep has published the account of a series of experiments on the autumnal colouration of leaves, and draws the following conclusions from them: 1st. All the coloured parts of vegetables appear to contain a particular substance (*chromule*,) susceptible of being changed in colour by very slight modifications. 2d. The autumnal change in the colour of leaves is due to the fixation of oxygen, or a kind of acidification of the chromule in them.—*Ann. de Chimie.* xxxviii. 415.

13. *On a new kind of Salad.*—M. Bosc states, that three or four years since some grains of Indian cress (*Sisymbrium Indicum*, Linn.) were sent from the Isle of France to the Jardin du Roi, and having multiplied exceedingly were tried by him as salad for the table, and have been judged of very favourably in consequence of their power of yielding salad during the winter.

Indian cress forms small patches on the ground about three inches in diameter; its leaves are very numerous, are irregularly pinnated, have nearly round folioles, and about three lines in width; the flowers are small, white, and disposed in axillary and terminal pannicles; they begin to fade about March.

The qualities which render this cress desirable for cultivation in our gardens, as a salad, are—1st. That it is eminently antiscorbutic and depurative; 2d. That its leaves are more tender and less acrid than those of other cresses, used as salads; 3d. That it does not suffer from the hardest winters; does not require watering to ensure or favour its growth; and will supply leaves during the winter, and especially in spring.

It is necessary that the seeds should be sown in ground in which none have been grown for some years preceding; its culture does not differ essentially from that of the corn-salad.—*Ann. de l'Agriculture Franc.* xli.

14. *New kind of Coffee.*—Endeavours have often been made in France to discover a substitute for foreign coffee. According to M. Pajot Descharmes, the seeds of the broom answer this purpose exceedingly well, according to the opinion of a person who has taken it for twelve years. Being moderately roasted, ground, and prepared in the manner of ordinary coffee, this person finds no difference between it and coffee. It is not the garden but the forest broom, the seeds of which are to be taken for this use. It appears that in that part of Holland bordering upon Germany, this substance has been used instead of coffee for many years.—*Recueil Industriel.* vii. 85.

15. *Maturation of Wine.*—M. de St. Vincent, of Havre, states, from his own experience of long continuance, that when bottles containing wine are closed by tying a piece of parchment or bladder over their mouths, instead of using corks in the ordinary manner, the wine acquires, in a few weeks only, those qualities which is only given by age in the ordinary way after many years.—*Nouveau Jour. de Paris.*

16. *Indications of Wholesomeness in Mushrooms.*—Whenever a fungus is pleasant in flavour and odour, it may be considered wholesome; if, on the contrary, it have an offensive smell, a bitter, astringent, or styptic taste, or even if it leave an unpleasant flavour in the mouth, it should not be considered fit for food. The colour, figure, and texture of these vegetables do not afford any characters on which we can safely rely; yet it may be remarked, that in colour, the pure yellow, gold colour, bluish pale, dark or lustre brown, wine red, or the violet, belong to many that are esculent; whilst the pale or sulphur yellow, bright or blood red, and the greenish, belong to few but the poisonous. The safe kinds have most frequently a compact, brittle texture; the flesh is white; they grow more readily in open places, such as dry pastures and waste lands, than in places humid or shaded by wood. In general, those should be suspected which grow in caverns and subterraneous passages, on animal matter undergoing putrefaction, as well as those whose flesh is soft or watery.

17. *On Artificial Incubation, by means of Hot Mineral Waters.*—This curious process is described very briefly in a letter by M. D'Arcet. The following are extracts from this letter:—

"In June, 1825, I obtained chickens and pigeons at Vichy, by artificial incubation, effected through the means of the thermal waters of that place. In 1827, I went to the baths of Chaudes-Aigues, principally for the purpose of doing the same thing there. Finding the proprietor a zealous man, I succeeded in making a useful application of this source of heat to artificial incubation.

"The advantage of this process may be comprehended, when it is known that the invalids who arrive at Vichy, for instance in the month of May, find chickens only the size of quails, whereas, by this means, they may be readily supplied six months old.

"The good which may be done by establishing artificial incubation in places where hot springs exist, is *incalculable;* it may be introduced into these establishments without at all interfering with the medical treatment of patients, since the hatching would proceed in winter, at a time when the baths for other purposes are out of use.

"There is no other trouble required in raising chickens, by means of hot baths, than to break the eggs at the proper time; for, when the places are closed, the whole of the interior will readily acquire a sufficiently elevated and very constant temperature."—*Ann. de l'Industrie,* i. 388.

In addition to these details by M. D'Arcet, a letter has been received from M. Felgeris, the proprietor of the baths at Chaudes-Aigues (Cantal), in which he describes the success he has had in following M. D'Arcet's process. This consists in putting the eggs into a small basket, suspending the latter in one of the stoves heated by the hot mineral water, and turning the eggs every day. The very first trial was attended with success, and no failure was experienced in four repetitions of it.—*Jour. des Conn. Usuelles,* vii. 128.

18. *Prevention of Death from Poison.*—A memoir on very simple and effectual processes in some cases of poisoning has been read by Dr. Vernière, to the Academy of Sciences at Paris. His views are founded on the experiment in which M. Magendie entirely prevented absorption in a dog, by throwing warm water into the veins, and thus forming an artificial plethora.

Three grains of the alcoholic extract of nux vomica were put on a wound in the foot of a young dog, and then a ligature placed above the articulation of the humerus. Warm water was then slowly injected by the jugular vein, until the animal could bear no more without great suffering: after which the vein of the poisoned limb was opened beneath the ligature, some ounces of blood withdrawn, and these introduced into the jugular vein of another dog. The latter dog instantly died in convulsions; the wound in the former dog was well cleaned, a little blood drawn, and the animal set at liberty. There was no appearance of poisoning, and eight days after it was perfectly well.

Knowing that plethora prevents absorption, the explanation of the effects described is very easy: only that blood flowing from the vein could have been poisoned, for that vein and its neighbours were the only ones free from the general plethora. Another circumstance in this case opposed to poisoning was, that the current being only from the arteries to the vein opened, the poison introduced was forced with the blood out of the system.

As the production of plethora by the infusion of water is a serious inconvenience, M. Vernière was induced to ascertain whether a local plethora in the poisoned member would not be sufficient, an effect readily produced by a moderate ligature, and thinks that it is. After applying the ligature, all that is required is to open the principal veins in the expanded part of the system, and allow the poisoned blood to flow out.

In an experiment of this kind, three grains of the extract of nux vomica were put over a wound in the right cheek of a small dog. The experimenter immediately compressed the two jugular veins with his thumbs for *six* minutes, after which that on the poisoned side was widely opened by a stroke of the lancet; the blood flowed freely, after which the animal, put upon its feet, experienced only a little weakness.

In another experiment, three grains of the extract were put under the skin, covering the tarsal surface of the right fore-foot of a young dog. A strong ligature was applied at the same time, and after five minutes, the poison was removed by repeated washing: the wound being cleaned, the ligature was removed, and the animal put upon its feet; it walked quietly at first, but was soon seized with violent convulsions, upon which an abundant bleeding of the jugular vein was effected, and in half a minute the convulsions ceased. The animal left at liberty walked quietly as before, with occasional soft inspirations, which, however, soon ceased. M. Vernière thinks, that in this experiment the ligature had been too tight, compressing the artery as well as the vein, and so preventing the plethora from taking place, which should have prevented the absorption. Hence, these two important conclusions—1st. The ligature should not be too tight—2d. Even when the poison has penetrated considerably into the system, large and abundant general bleedings may reach it, and cause its expulsion. It is easy to conceive, indeed, that whilst the poisoned blood is contained only in the large vessels, if these be opened, it will flow out rather than pass into the small vessels, which afford a greater resistance to its passage than the opening made by the lancet.—*Le Globe*, August 13.

19. *Hydrophobia in Foxes as observed near Ulm.*—The following is the substance of remarks on this subject by the Duke Henry of Wurtemberg. It is well known that foxes become mad occasionally, and there have also been examples of dogs which, having been bitten by mad foxes, have not caught the disease. In these cases anatomical investigation has shown that the stomachs of the foxes were filled with wood, earth, stones, leaves, hair, and other substances improper for nourishment. On the contrary, when the madness has been communicated, the stomach and intestines have

been found completely empty. From this difference the author concludes that hunger is the cause of madness in foxes; and this agrees with the results which occurred during and after the rigorous winter of 1826-7, when these animals, with many others, suffered from want of nourishment. It is supposed also that the madness of foxes is not contagious until it has arrived at its utmost degree, and that perhaps the disease may depend upon the corruption of the contents of the gall bladder, which is known to become more vitiated the more the disease augments, and which is at the same time thrown into the stomach.—*Bull. Univ.*, D. x. 160.

20. *Preservation of Eggs.*—Cadet indicated the preserving power of lime-water over eggs immersed in it, and suggested that a solution of muriate of lime would probably answer the same end. Dr. Hopff has verified this conjecture. He finds that eggs fully immersed in a solution of thirty grains of muriate of lime in one pound of water, and preserved in a cool place, were as good at the end of a twelvemonth as those preserved in lime-water.—*Repertoire de Pharmacie*, xxvii. 427.

Relative to the preservation of eggs by immersion in lime-water, M. Peschier has given most satisfactory evidence of the efficacy of the process. Eggs which he had preserved for six years in this way, being boiled and tried, were found perfectly fresh and good; and a confectioner of Geneva has used a whole cask of eggs preserved by the same means. In the small way eggs may be thus preserved in bottles or other vessels. They are to be introduced when quite fresh, the bottle then filled with lime-water, a little powdered lime sprinkled in at last, and then the bottle closed. To prepare the lime-water, twenty or thirty pints of water are to be mixed up with five or six pounds of slaked quick lime put into a covered vessel allowed to clear by standing, and the lime-water immediately used. —*Révue Ency.* xxxix. 237.

21. *Egyptian Manuscript relative to the History of Sesostris.*—At the sitting of the Aix Academy, on the 3d of August, M. Sallier read a report of some very important discoveries in Egyptian history, made at his house, and amongst his Egyptian papyri, by M. Champollion, jeune. The latter gentleman was on his way to Egypt with M. Rosellini, and stopped two days with M. Sallier previous to proceeding to Toulon for the purpose of embarking. During this short period he examined ten or twelve Egyptian papyri, which had been purchased some years ago, with other antiquities, from an Egyptian sailor. They were principally prayers or rituals which had been deposited with mummies; but there was also the contract of the sale of a house in the reign of one of the Ptolemies; and finally three rolls united together and written over with fine demotic characters, reserved, as is well known, for civil purposes.

The first of these rolls was of considerable size, and to M. Champollion's astonishment contained a *History of the Campaigns of Sesostris Rhamses*, called also *Sethos* or *Sethosis*, and *Sesoosis*, giving accounts the most circumstantial of his conquests, the countries which he traversed, his forces, and details of his army. The manuscript is finished with a declaration of the historian, who, after stating his names and titles, says he wrote in the ninth year of the reign of Sesostris Rhamses, king of kings, a lion in combats, &c.

M. Champollion has promised, that, on his return from Egypt, he will fix the manuscript on cloth for its future preservation, and give a complete translation. The period of the history is close to the time of Moses; and apparently the great Sesostris was the son of the king who pursued the Israelites to the borders of the Red Sea; so that a most important period in ancient history will be elucidated.

On the same MS. commences another composition, called *Praises of the great King Amemnengon*. There are only a few leaves of it, and they form the beginning of the history contained in the second roll. This Amemnengon is supposed to have reigned before Sesostris, because the author wrote in the ninth year of the reign of the latter. M. Champollion had not time to enter into a particular examination of these rolls.

The third roll relates to astronomy or astrology, or more likely to both these subjects. It has not been far opened; but will probably prove of the utmost interest, if, as is expected, it contains any account of the system of the heavens as known to or acknowledged by the Egyptians and Chaldeeans, the authors of astronomical science.

A small basaltic figure was purchased with the MSS., and it is supposed found with them. On the shoulders of the figure is written in hieroglyphic characters the name, with the addition of *clerk and friend of Sesostris.* It did not occur to ascertain, until M. Champollion was gone, whether the name on the figure was the same with any of those mentioned in the rolls as belonging to the historian, or to others.—*Bull. Univ.*, G. x. 200.

METEOROLOGICAL DIARY for the Months of September, October, and November, 1828, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For SEPTEMBER, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Monday....	1	55	67½	29.90	29.87	NE	ENE
Tuesday ...	2	49	65½	29.87	29.90	E	NW
Wednesday.	3	49	64	29.90	29.90	NW	NE
Thursday ..	4	53	66	29.90	29.90	NE	NE
Friday	5	50	69	29.84	29.87	E	E
Saturday...	6	43	65½	29.90	29.90	E	E
Sunday	7	49	71½	29.86	29.86	EbS	EbS
Monday....	8	57	73½	29.84	29.72	EbS	SE
Tuesday ..	9	60	72	29.64	29.70	SW	SW
Wednesday.	10	49	68	29.70	29.42	SW	SW
Thursday ..	11	52	72	29.40	29.40	SW	SW
Friday.....	12	55	69	29.36	29.27	SW	SW
Saturday...	13	54	66	29.33	29.50	W	NE
Sunday	14	48	54	29.80	29.90	NE	NE
Monday....	15	42	60	30.11	30.29	N	N
Tuesday ...	16	34	57	30.38	30.38	NE	NE
Wednesday.	17	34	61	30.27	30.08	E	E
Thursday ..	18	40	61	29.97	29.90	EbN	E
Friday	19	39	65	29.90	29.96	E	E
Saturday...	20	42	65	30.00	30.03	E	EbS
Sunday	21	38	63	30.00	29.90	SE	SE
Monday ...	22	42	67	29.82	29.82	SE	W
Tuesday ...	23	46	67	29.91	29.97	W	WSW
Wednesday.	24	51	68	29.90	29.88	WSW	SW
Thursday ..	25	53	70	29.77	29.71	SW	S
Friday.....	26	52	71	29.62	29.70	S	W
Saturday...	27	52	62	29.74	29.70	W	W
Sunday	28	42	64	29.66	29.52	SW	SSW
Monday ...	29	54½	67	29.40	29.37	SW	WSW
Tuesday ...	30	42½	63	29.49	29.50	W	WSW

For OCTOBER, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Wednesday.	1	46	58	29.45	29.50	WbS	WSW
Thursday ..	2	43	60	29.62	29.78	W	W
Friday	3	32½	58½	29.80	29.62	W	SE
Saturday...	4	45	61	29.42	29.48	EbS	NE
Sunday	5	45	62	29.47	29.33	SE	SbE
Monday ...	6	48	60	29.10	29.33	W	WSW
Tuesday ...	7	44	58	29.40	29.30	S	S
Wednesday.	8	47	60	29.30	29.50	W	W
Thursday ..	9	44½	58	29.78	29.90	W	W
Friday.....	10	45	60	29.96	29.87	WSW	WSW
Saturday...	11	45	60	30.06	30.21	W	W
Sunday	12	48	64½	30.26	30.26	W	W
Monday...	13	44	63	30.26	30.21	W	W
Tuesday ...	14	47	58	30.21	30.23	WNW	NNW
Wednesday.	15	43	56	30.23	30.20	NW	W
Thursday ..	16	47½	55½	30.19	30.12	W	W
Friday	17	48	59	30.02	30.00	W	NW
Saturday...	18	36	53	30.17	30.17	NE	ESE
Sunday	19	33	54	30.10	30.02	SE	SE
Monday ...	20	27	54	30.00	30.00	SSE	SE
Tuesday ...	21	39	57	30.00	29.93	SE	S
Wednesday.	22	47	64	29.78	29.67	SbW	SSE
Thursday ..	23	50	54	29.62	29.78	NW	W
Friday	24	31	52½	29.93	30.00	W	SSE
Saturday...	25	37	57	30.00	30.13	SE	SE
Sunday	26	29	55	30.18	30.10	SE	E
Monday....	27	44	53½	30.10	30.18	NE	NE
Tuesday ...	28	40	52	30.33	30.33	NE	NE
Wednesday.	29	36	57	30.36	30.28	NE	E
Thursday ..	30	44	47	30.21	30.20	NE	NE
Friday	31	32	50	30.18	30.16	W	W

For NOVEMBER, 1828.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Saturday...	1	42	53	30.16	30.10	W	W
Sunday	2	38	54	30.10	30.10	W	NW
Monday....	3	32	52	30.14	30.18	NW	NW
Tuesday ...	4	34	50	30.12	30.04	E	E
Wednesday.	5	36	48	30.04	30.00	E	E
Thursday ..	6	33	48	29.98	29.92	E	SE
Friday.....	7	38	49	29.90	29.80	SE	E
Saturday...	8	31½	37	29.80	29.80	E	E
Sunday	9	26	46	29.76	29.53	NE	N
Monday....	10	25	41	29.46	29.50	WbN	NbE
Tuesday ...	11	21	37	29.39	29.41	W	W
Wednesday.	12	17	41	29.46	29.48	W	SE
Thursday ..	13	30	50	29.48	29.48	SW	SbE
Friday.....	14	39	48	29.43	29.30	E	E
Saturday...	15	41	51	29.20	29.20	E	SE
Sunday	16	43	53	29.10	29.10	ESE	W
Monday....	17	44	50	29.43	29.53	W	W
Tuesday ...	18	35½	46	29.70	29.80	W	W
Wednesday.	19	35	50	29.76	29.82	W	NW
Thursday ..	20	39	53	29.90	29.87	W	SW
Friday	21	47	55	29.75	29.70	SW	SW
Saturday...	22	46	54½	29.61	29.60	S	SbW
Sunday	23	31	51	29.76	29.66	SW	S
Monday....	24	36	52	29.52	29.52	SW	SbW
Tuesday ...	25	42	54	29.67	29.70	SbW	SW
Wednesday.	26	44	54	29.64	29.64	SW	SW
Thursday ..	27	42	54	29.70	29.88	SW	SW
Friday.....	28	41	57	29.86	29.90	SW	SSW
Saturday ..	29	49	56½	29.90	29.81	SW	W
Sunday ...	30	46	55	29.90	29.80	W	SW

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