QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART

THE

QUARTERLY JOURNAL

SCIENCE,

LITERATURE, AND ART.

THE ROYAL INSTITUTION OF GREAT BRITAIN

JANUARY TO JUNE, 1830.

LONDON:

HENRY COLBURN AND RICHARD BENTLEY,

NEW BURLINGTON-STREET.

MDCCCXXX.

LONDON:
PRINTED BY WILLIAM CLOWES,
Stamford-street.

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I.—Mechanical Science.

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TO OUR READERS AND CORRESPONDENTS.

THE Managers of the Royal Institution having determined to publish a Quarterly Scientific Journal, *more immediately under their own direction and superintendence, the labours and responsibility of the Editor of the present Journal cease and conclude with this Number.*

We are sorry we cannot make the arrangement in regard to incomplete Papers, which a Correspondent suggests.

TO OUR READERS AND CORRESPONDENTS.

Several papers have been received, but are necessarily postponed to our next Number.

We regret that the Appendix to Dr. Ure's paper on Opium reached us too late for insertion in its proper place; it shall appear (in the present volume) in our July Number.

Want of time has prevented our compliance with Mr. Herapath's request.

F. R. S. is informed, that an excellent abstract of the papers read to the Royal Society is given weekly in the Literary Gazette.

The letter of Aquarius remains for consideration.

MR. BRANDE'S MANUAL OF CHEMISTRY WILL BE PUBLISHED EARLY THIS MONTH.

ERRATA IN THE JOURNAL OF SCIENCE FOR JUNE, 1829.

Page 346, line 1, *for* Henton *read* Hinton.
" 346, " 3, *for* Theynsham *read* Heynsham.
" 346, " 12, *for* Kenig *read* Konig.
" 346, " 14, *for* Perbee *read* Purbec.

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An Experimental Inquiry into the Physiological Effects of Oxygen and other Gases upon the Animal System. By S. D. Broughton, F.R.S., F.G.S., Member of the Royal College of Surgeons, &c. &c.

Physiologists have long known that the compound of oxygen and nitrogen, constituting the atmosphere of our globe, is the only gaseous matter capable of supporting animal life, or of imparting health and vigour to the constitution. It appears, also, from the experimental researches instituted at different periods, that oxygen, in its pure state, is unfit for any lengthened degree of respiration; although it be so essentially necessary to the vital functions, that it should always form a certain portion of the air breathed by animals.

The stream of blood, transmitted from the lungs to every part of the body, derives its scarlet colour from the absorption of oxygen during the pulmonic circulation; and it is only in this state of the blood that the animal functions can be maintained and life preserved. A few waves of this fluid, which have passed round the brain without oxygenated particles, are sufficient to destroy the proper influence of the nervous system; while a deficient supply of natural arterial blood is attended with a proportionate degree of functional disorder, or vitiated vital action.

In reference to these known circumstances, it seems to be an object of interest and importance to inquire into the causes leading to the destruction of animal life, during the respiration of an air which, in its duly mixed state, possesses such a direct property of maintaining the vital principle—a property not belonging to any other substance.

Various opinions have been offered upon this subject; and experimental inquiries, both of a chemical and physiological kind, have frequently been instituted at different periods. But the results of these do not appear to be sufficiently satisfactory to set the question at rest. I am, therefore, disposed to draw attention to some experiments which I have made myself, and which appear to disclose facts leading to an explanation somewhat different from those hitherto received, as far as relates to the *physiology* of this subject, the *chemical* ground being satisfactorily occupied.

These experiments were commenced in the year 1827; and, appearing to afford conclusions opposite to those usually understood, they were renewed in the course of 1828, and farther prosecuted at the beginning of the last year, with the assistance of Mr. George Wood, Mr. Miles, Mr. Ferguson, Mr. Murray, and others, well accustomed to the management of pneumatic chemistry, and conversant with experimental physiology. The gas itself was generally made by exposing black oxide of manganese to the red heat of an iron crucible, and tested with a taper previous to every experiment. Glass jars were inverted upon the shelf of a water-bath, and on this a raised platform supported the animal above the level of the surrounding water. During the severe frosty weather, the bath was kept close to a large fire, and the water was otherwise preserved at an elevated temperature. The animals were passed quickly and carefully through the water into the gas, which was previously collected by a metallic worm, communicating between the bath and the heated crucible; and these, and all the other delicate operations necessary, were very adroitly performed by my intelligent assistants. As a preliminary step, some kittens, mice, and sparrows, were placed under glass jars of atmospheric air, and the duration of their lives was compared with that of others immersed in like quantities of oxygen: the result of which comparative experiment was this—*that the animals died much sooner in the jars of common air than in those of unmixed oxygen.* And, when the gas was tested, after the removal of the animals from the atmospheric air, it evinced the presence of carbonic acid sufficient to extinguish a lighted paper instantly, and to destroy animal life in a few seconds.

But, when the contents of the oxygen jars were tested after the continued respirations of animals in them, *an extinguished taper was uniformly re-illuminated;* and other animals, then placed under the jars, continued to live some time, and very much as those did which were originally immersed. Small collections of the gas were made, for the purpose of testing the contents of the jars with a lighted taper; and, when the gas was no longer required, the main bulk of the oxygen, also, was similarly tested, and with the same results, as to the proportion remaining being sufficient for the support of life and combustion; although, when portions were washed with lime-water, they exhibited the presence of some carbon, by a white turbid appearance.

Having premised these circumstances, I shall now proceed to detail some of the principal experiments which I made at different periods, and which were undertaken entirely independent of any previous anticipation of the results, and unconnected with a wish to support any theory whatever upon the subject; and rather, indeed, as a mere matter of curiosity, to endeavour to place upon a more satisfactory footing certain points which have appeared to myself and others to be imperfectly understood hitherto.

EXPERIMENT I.

A kitten, of about ten or twelve days old, was immersed in pure oxygen, and suffered no apparent inconvenience during the first hour, but afterwards its respirations were quickened, and the sanguiferous system was much accelerated. To this succeeded a state of debility, and gradually a total insensibility, with depression of the voluntary powers, and ultimately the entire loss of them. The eyes became glazed; and, after long and slow inspirations, the diaphragm alone was seen to contract slowly and feebly at distant intervals. Having continued some time in this state, the animal was then removed, and very shortly recovered in the open air—having been immersed *three hours,* the quantity of gas being about one gallon. Some hours elapsed before its strength was regained, but it ultimately recovered altogether.

EXPERIMENT II.

A kitten of the same brood was immersed in the same quantity of pure oxygen, and exhibited similar phenomena. It was not removed until the motion of the diaphragm had ceased some few minutes, and it did not become reanimated. On opening the chest, the heart was found beating strongly; and, after its removal, forcibly contracted upon the knife when cut across. Throughout the brain, and every part of the body, *no trace of venous blood was discoverable,* but everywhere the arteries and veins universally carried *scarlet* blood, as well as both divisions of the heart, which exhibited the internal structure to be entirely of a bright florid colour; and the surface of the lungs appeared as if highly injected with vermillion.

EXPERIMENT III.

A full-grown sparrow was immersed in the remaining gas of the last experiment. During the first hour it appeared to be unaffected, but afterwards began to pant and gasp in a hurried manner. In two hours and a quarter it showed no sign of animation. It was then removed, the heart was found to be in full action, and the vessels universally carried scarlet blood. Several other sparrows and mice were successively immersed in the same oxygen, and the phenomena which occurred evinced no important deviation from the former symptoms.

EXPERIMENT IV.

A rabbit, of about three weeks old, was immersed in a glass jar of oxygen, to the amount of about one gallon. In about an hour its breathing was hurried and laboured, and a quick action of the heart was evinced. Subsequently the respirations became weaker and slower, and a state of insensibility ensued; the nostrils ceased acting, and the animal was on its side, with no sign of motion but that of the diaphragm at distant intervals, which continued a long time; and, at the end of five hours, was still acting, though almost imperceptibly. It was now removed from the jar, but exhibited no sign of sensibility. On opening the chest, the heart was found in full action; and, on puncturing the aorta, the blood jetted out to a considerable height. The diaphragm contracted slightly for a few moments, and the peristaltic motion of the viscera was going on. The diaphragm, however, ceased to act some time before the heart discontinued to contract. The body was of a bright scarlet hue throughout. A sparrow and another rabbit were successively immersed in the same gas from which this rabbit was removed, and they each breathed freely during about an hour.

EXPERIMENT V.

A guinea-pig was immersed in a gallon of oxygen at two o'clock in the afternoon. It was near four o'clock before any inconvenience was manifested in its state, when its breathing became hurried. In somewhat less than three hours it was very weak, and gasped; and, in less than half an hour more, it was altogether apparently insensible. Its eyes were glazed, and no sign of motion appeared, but the slight contractions of the diaphragm, at long intervals. After having been in the gas about three hours and a half, it was removed in this state, and was soon reanimated by inflating the lungs with atmospheric air, from an elastic gum bottle, through the nostrils. The glaziness of the eyes was removed, and it began to breathe regularly, and was completely restored, but remained very weak all the evening, and in the morning was found dead.

EXPERIMENT VI.

A guinea-pig was placed in the same jar of oxygen from which the last was removed—about two pints of the gas being added, to make up for losses in testing and the rising of the water during the experiment. It was seized with hurried breathing, in about the same time as the last guinea-pig, and was removed, after three hours had elapsed, in a state of absolute insensibility; the diaphragm acting slowly, at long intervals, before its removal. On opening the chest, the diaphragm was, however, quite still, but the heart was acting forcibly: the blood was universally arterial.

EXPERIMENT VII.

A rabbit of about three weeks old was immersed in about two gallons of oxygen, at half-past eleven in the forenoon. At three o'clock the animal was still apparently lively and unaffected, and it ate some oats and cabbage, introduced through the water under the glass. At seven, the animal's breathing was quickened, but it showed no signs of insensibility; nor, until nearly eleven o'clock, was it apparently much affected, when some degree of stupor and weakness was evident. At twelve o'clock at night, twelve hours and a half after its immersion, it was in a sitting posture, breathing quick, and somewhat dull in appearance. In this state it was left, the fire allowed to go out, and the bath to cool down to the temperature of the room on a frosty night; so that in the morning it was found dead. On opening the body, the heart and blood vessels universally contained scarlet blood. During this experiment, as the water rose in the jar, about two or three pints of oxygen were added: a flame was excited in a blown-out taper, introduced into the jar, and a mouse breathed some time in it.

EXPERIMENT VIII.

A rabbit of about three weeks old was immersed in a gallon of oxygen at a quarter before twelve. About one o'clock the respiration was evidently hurried. At three o'clock the animal was apparently insensible, its eyes glazed, and breathing feebly and at long intervals. At the end of four hours and three-quarters no sign of motion existed, but a slight movement of the diaphragm occasionally, and almost imperceptibly. It was then removed, and in about a quarter of an hour it was reanimated by means of artificial inflation. It continued very weak during the evening, but was quite well next day, and so continued. The gas in this case was nearly half of it breathed by the guinea-pigs of the sixth and seventh experiments, and the other half consisted of fresh oxygen.

EXPERIMENT IX.

At five o'clock in the evening a rabbit of about three weeks old was immersed in the same oxygen from which the last was taken, with the addition of a quart of fresh oxygen to make up for losses. In about an hour the animal breathed very quick, and in four hours and a quarter it was apparently insensible, and breathing slowly and slightly. At the end of four hours and three-quarters no movement was at all perceptible. It was then removed into the air, and it began to gasp, upon artificial inflation being applied. But the air was unfortunately sent in too strongly by an assistant, and it escaped into the abdomen, which put an end to the means of restoration. On opening the chest, the heart was in full action, and no venous blood to be seen in any part of the body.

EXPERIMENT X.

At a quarter past two P. M. two rabbits of three or four weeks old were immersed in about two gallons of oxygen, a portion of which was fresh, and the remainder had been breathed by the rabbit of the last experiment. A thermometer applied to the groin indicated a temperature of about 90 degrees in each rabbit, previous to the experiment. One was a black and the other a yellow rabbit, the latter being much the largest. In an hour they appeared to breathe quick, but were both lively. The lesser rabbit was apparently distressed long prior to the other. At eight o'clock, five hours and three-quarters after immersion, it was in a prostrate and insensible state, breathing only by the diaphragm, and slowly. The larger rabbit was on its side, weak, and gasping occasionally, but lively when roused. At eleven this rabbit was in the same state, but its head falling under the rising water around, accidentally put a stop to its feeble respirations, and it was therefore removed prematurely. A thermometer was immediately introduced into the abdomen, and indicated 87 degrees. On opening the chest the heart was in full action, and no venous blood was perceptible, the animal having been immersed nine hours altogether. The lesser rabbit was then removed, a very feeble action of the diaphragm, and now and then a gasp appearing, but wholly insensible. In the open air, however, its renewed gasps restored animation in some degree, the glaziness of the eyes went off, it uttered cries, and attempted motion. It was then pithed, and the thermometer being inserted into the abdomen rose to 88 degrees. The heart was in full and strong action, and the blood scarlet, the circulation evidently continuing throughout the body. The peristaltic motion of the bowels had not ceased. The contents of this jar, as usual, rekindled a blown-out taper.

EXPERIMENT XI.

A rabbit of about three or four weeks old was immersed in a gallon of fresh oxygen at one o'clock P. M. In about an hour its respiration was quickened, and in two hours it was very weak, and apparently losing its sensibility. Nearly a quart of oxygen was added during the experiment, to make up for the rising of the water. About seven, having been in the gas nearly six hours, it was convulsed and expired, and was removed in five minutes without any sign of motion. On opening the chest, the heart was in full action, and the diaphragm still. No venous blood was perceptible. The gas remaining after the experiment rekindled a blown-out taper.

N. B. In all these experiments the surface of the lungs appeared much injected. The blood, also, was observed to be very transparent, and to coagulate remarkably quick. The right side of the heart was always much more filled than the left. I am not aware of any other circumstances omitted to be noticed as belonging to the experiments detailed.

Some of the principal experiments undertaken for the purpose of ascertaining the nature of the influence exercised by oxygen, in its unmixed state, over the animal functions, having been thus described, it will be perceived that, in some respects, my results confirm those of others, but are yet opposed to many, and afford also rather a novel view of the subject. At least, the authorities into which I have looked do not appear to have anticipated some of my conclusions. I may here observe, that experiments, of the nature which I have cited, should not be confined to small animals, such as mice, and birds of the size of sparrows; for they are not so well calculated as larger animals to afford satisfactory and clear demonstrations when their internal organs are to be examined. Therefore, feeling it constantly necessary to appeal to the internal state of the animal, I have selected accordingly rabbits, kittens, and guinea-pigs, as the best adapted to my purposes, these being at once sufficiently large and manageable. Many of the older experimenters, as Dr. Priestley and others, deduce their conclusions from *mice*. Dr. Priestley found, that if the temperature of the bath was kept up, the mice lived longer than when surrounded by a medium of a low degree; and hence he drew this inference—*that the oxygen itself is not destructive*. This, however, appears to me to be an unwarranted inference, for I did not find (although latterly the weather was extremely severe) that the preservation of an elevated temperature made such a difference in the experiment as was sufficient to account either for the final result, or the general phenomena presented to my notice. Both in the experiments conducted at a distance from the fire, and without heating the bath, during a severe frost, and in those wherein the temperature was kept up, the *action of the sanguiferous system continued some time after the motion of the diaphragm had ceased*, and a similar progressive insensibility occurred. Possibly the final termination of life may be quickened somewhat by extreme cold in and around the bath, as the means of increasing the force of the debilitating cause.

Another inference in Dr. Priestley's experiments is, that the *carbonic acid* generated by the animal respiring oxygen, has less effect upon a second animal placed under the glass than on the first, from the greater vigour of the former coming fresh out of the atmospheric air. But upon reference to my experiments, this does not appear, no appreciable difference being noticed.

Lavoisier observed, in his early experiments, some indications of increased vascular action, but none in his latter investigations, nor indeed any change whatever. His first conclusions, however, are most probably correct.

Messrs. Dumas and Richerand observed a great degree of pulmonary excitement when animals breathed oxygen during a long time; and M. Richerand remarks, *that they consume no more oxygen than when immersed in so much common air.* From the experiments of Dr. Beddoes, however, this conclusion is controverted. And I am disposed to coincide with this last author in the opinion he entertains, that the fact is not as M. Richerand supposed, and that animals confined in the gas become completely, as it were, *oxygenated.* Dr. Beddoes observed the very florid injection of the lungs and pleura, the long retention of contractility in the heart, the rapid coagulation of the blood, and *the very slight deterioration of the gas,*—all of which facts were so obvious in my experiments. I do not, however, feel disposed to refer the state of the lungs and pleura to *inflammation,* as this author does; but rather to *congestion,* from inefficient respiratory action.

Sir Humphry Davy also employed *mice,* and found that they ultimately died when immersed in oxygen; but he offers no explanation of this result. He imagined that less oxygen was consumed than when common air is breathed. Can the difference of opinion upon this point arise from the animals, being saturated with oxygen, returning it back again unaltered? The state of the gas, after every experiment, seems to favour this question. Whether it be so or not, my experience tends to subvert the conclusion of this author, which is—*that the fatal effects are independent of excess of oxygen.*

Messrs. Allen and Pepys found—*that more of oxygen was consumed, than is sufficient for the production of carbon.* They also observed *that the blood gave off a corresponding quantity of nitrogen; and that the diminution of the volume of air was found to be greater in pure oxygen than in ordinary respiration.* My experiments, in general, appeared to corroborate these observations.

The experience of M. Magendie shows the respiration of pure oxygen to be fatal to animal life, and to exhibit a similar tendency when the gas is mixed in proportions differing from those of the atmosphere. This opinion seems to prevail in France; and it is expressed, also, by the valued testimony of Dr. Prout. All the facts which we possess are decidedly in favour of this opinion, as far as I can judge of them.

Without quoting particular authorities any farther, I find it is generally observable that some circumstances of a *physiological* nature have been apparently overlooked in trying the effects of oxygen upon the animal functions; while the *chemical* phenomena have been more fully investigated, and especially by the recent researches of Messrs. Allen and Pepys, leaving no points, perhaps, upon this ground incompletely treated. From the constancy of the most important facts in my experiments, I am inclined to think that I am justified in believing many of the results, arrived at by others, to be unsatisfactory hitherto; and their occasional apparent contradiction of each other strengthens this notion.

We may refer to the invariable manner in which animals, after remaining some time unaffected by breathing pure oxygen, begin to be excited, and their respiration and sanguineous circulation become greatly increased; and to the gradual state of debility and subsequent insensibility following, with loss of voluntary motion, and the cessation of the diaphragm's contracting long before the heart ceases to act with vigour, and to urge the blood through the vessels. When we refer to these phenomena, and find, also, that the renewal of an atmospheric circulation of air through the lungs is capable of completely restoring animation, *we appear to be presented with a train of circumstances strikingly analogous to those which accompany the absorption of certain poisons into the blood.*

The facts, of which I do not find mention made elsewhere, and which induce this analogous assumption, are the phenomena described as arising during the respiration of oxygen :—such as—*the universal appearance of arterial blood; the gradual cessation of sensibility and voluntary motion; the long-continued breathing only by a slow and feeble action of the diaphragm; the full continuance of the heart's pulsations, circulating nothing but arterial blood, after the diaphragm has become still; the restoration of sensibility and voluntary power by atmospheric inflation; and the maintenance of animal heat within the body during the immersion in oxygen.*

These appear to me to be circumstances well worthy of consideration, and necessary to be taken into account, when the influence of pure oxygen upon the animal functions is the object of our inquiry. And, in reference to these circumstances detailed, perhaps the following positions may be deemed satisfactorily established—some of which are corroborated by the experience of others.

1. Animals immersed in equal quantities of atmospheric air and of pure oxygen, separately, live during different periods; those in the former dying sooner than those in the latter air.

2. The gas, remaining after atmospheric respiration, contains carbonic acid in excess, sufficient instantly to extinguish a lighted taper, and to destroy animal life in a few seconds.

3. The gas remaining, after the respiration of pure oxygen, re-illuminates a blown-out taper, and sustains animal life, during variable periods, as in the first instance of immersion.

4. The gas of pure oxygen is not much deteriorated by the respiration of animals; while that of the atmospheric compound is rendered wholly unfit to sustain life and flame.

5. The tendency of an excess of oxygen is to increase the action of the pulmonic and aortic circulation, in the first in-

stance, and to produce direct debility, insensibility, and loss of voluntary power in the second, involuntary action continuing indefinitely.

6. The invasion of the symptoms from breathing oxygen does not generally occur in less time than about an hour; and the sensibility of the animal is not uniformly affected at the same period.

7. The invasion of the symptoms seems to depend much upon the size, strength, and age of the animal employed.

8. Death is ultimately the constant result of breathing oxygen, pure or in excess.

9. If the motion of the diaphragm has not entirely ceased more than about two or three minutes, animation may be restored by atmospheric inflation of the lungs; and, as the blood acquires free access to common air, the functions of the brain are renewed.

10. The contractility of the heart and intestinal canal is retained long after the functions of the brain have ceased, or when sensibility, voluntary motion, and the action of the diaphragm, no longer exist.

11. Animals, having breathed oxygen during a certain time, circulate no venous blood in any part of the body; the whole mass throughout being of the brightest, transparent, arterial colour.

12. Animal heat is kept up, during the whole period of immersion in oxygen, above the ordinary temperature of the surrounding media, though apparently a few degrees lower than the usual degree of the animal.

13. Quick coagulation of the blood takes place, after death, from the respiration of oxygen.

Such are the facts indicated by the experiments and observations which I have made upon oxygen, at different periods; and which appear to suggest some important reflections relative to the physiological and pathological relations of animal life to the principal component of the atmosphere surrounding our globe; while they tend, also, to place the respiration of pure oxygen in a point of view that may, perhaps, be considered, in some respects, novel and interesting to science.

Encouraged by the novelty and constancy of my results, and directing my experiments exclusively to their physiological tendency, I instituted similar researches into the effects of other gases, as I considered that such a comparative inquiry was wanting to complete the ultimate objects to which my attention was directed; although I am aware that this latter ground is already so well cultivated by others as to render it almost unnecessary to go over it myself.

I. EXPERIMENTS WITH NITROUS OXIDE GAS.

Having carefully prepared some nitrous-oxide over night, in order that it might become purified by standing upon water, the following morning I placed a stout healthy kitten under the glass vessel containing the gas, the animal being raised above the water, and with other precautions, as practised in the experiments with oxygen. The effects of the gas were soon apparent, and in a little more than a quarter of an hour the kitten fell on its side quite motionless, having breathed quick and staggered to and fro at the least efforts to move. Being taken from the vessel, it gradually recovered in the open air, and regained its strength in the course of the evening. Another kitten of the same brood, about a fortnight old, was similarly affected in the gas, and having remained half an hour under the glass, did not recover in the open air. The temperature of the atmosphere was that of an ordinary summer's day. The animal was immediately opened, when the blood was found to be universally florid and more transparent than commonly,—the vessels of the brain, pleura, and lungs, being highly injected.

Some sparrows lived only four or five minutes in nitrous oxide, breathing rapidly; and on being opened they exhibited the same florid appearance and highly injected membranes. The heart in each was found palpitating.

A frog being placed under a glass jar of this gas, did not seem to be affected in any way; but in the morning it was found dead, and the blood vessels were found injected throughout. Some mice were immersed in vessels of this gas, and were almost immediately affected with a staggering and hurried breathing. They lived about seven or eight minutes; and on being examined, the heart was found palpitating in each. A rabbit of about three weeks old was similarly immersed, and in little more than a minute its heart appeared to act strongly, and its breathing was hurried and laborious. It then reeled and staggered, and with difficulty poised itself, finally resting against the side of the glass in a state of stupor. The respirations became weaker and slower, and the nostrils ceased to move. In about two hours and a quarter the diaphragm was perfectly inactive. The animal was then removed and opened, when the pleura, lungs, and brain, were found highly injected with thin florid blood. Another rabbit of the same brood was immersed in the same gas from which the first was taken, and it fell apparently dead in about two minutes. It was removed and resuscitated in the open air. After these experiments a lighted taper was extinguished in this gas.

When the experiments with the nitrous-oxide are compared with those conducted with oxygen gas, it is observable that the results are ultimately very similar, but that the effects are much sooner apparent, more urgent, and resemble rather those of intoxication from alcohol. The combination of azote with the oxygen in the nitrous-oxide seems to account for this difference, and the absorption of the oxygen may explain the similarity of appearance upon dissection when the state of the organs is compared with that derived from breathing pure oxygen. The nitrous-oxide, like the oxygen, seems to bear an affinity to poisonous substances in its effects, and is destructive to animal life when undiluted with atmospheric air.

II. EXPERIMENTS WITH NITROGEN.

Several sparrows were immersed in nitrogen gas, and fell dead in about thirty seconds, having gasped and struggled immediately on immersion. Some mice exhibited similar phenomena. In all these animals the right ventricle of the heart was found full and distended with dark blood, and the vessels of the brain, pleura, and lungs, were collapsed.

A frog was placed under a glass vessel of nitrogen, and remained more than two hours unaffected, at the termination of which period it began to gasp a little, and then appeared dull and lethargic. Next morning it was found dead. The blood was universally dark coloured.

A rabbit of two or three weeks old was immersed in nitrogen, and fell dead, after gasping and struggling, in about thirty seconds. Two or three minutes having elapsed, it was removed, and the blood vessels of the lungs and brain were collapsed, the right ventricle of the heart was full of dark blood, and its irritability was not extinct. Coagulation in all these cases occurred, but not immediately. Another rabbit of the same brood fell dead also in about thirty seconds, and, being removed, was resuscitated and lived.

From the phenomena exhibited in these experiments, it appears, that undiluted nitrogen is quickly fatal to animal life, suspending the functions of the brain almost instantaneously, as soon as a few waves of blood have passed through that organ; but, that the lungs are perfectly capable of receiving and circulating the gas during a few seconds, until the sensibility of the nervous system becomes destroyed. It is nevertheless known, that nitrogen is largely separated from the atmospheric air absorbed in ordinary respiration, and that its supply is necessary to the animal economy.

III. EXPERIMENTS WITH CHLORINE.

Some glass vessels being charged with chlorine, several mice were successively immersed in this gas, and they fell dead in less than thirty seconds. On opening these animals, the heart was found palpitating in each, and the peristaltic motion of the intestinal canal continued, and was kept up by irritating it with a probe. The vessels of the brain were collapsed. *The lungs were tinged with the yellow colour of the gas, and the peculiar odour of chlorine was perceptible throughout their structure.* Several sparrows were similarly immersed and exhibited the same phenomena. Coagulation took place as usual under ordinary circumstances.

A rabbit of two or three weeks old was immersed in chlorine, and it died in less than half a minute. On opening the thorax the heart was found acting freely, and on puncturing the aorta the blood jetted forcibly out to a considerable distance. The peristaltic motion of the bowels was also going on. The vessels of the brain were in a collapsed state. *The lungs were very much distended, and they were tinged with yellow; and when removed from the chest to a distance they emitted the odour of* *chlorine.* The right ventricle of the heart was distended with dark blood. The eyes were much glazed in each experiment.

It has generally been thought that chlorine is incapable of passing the epiglottis; but, from the above observations it is evident that this gas enters the bronchial tubes in the act of inspiration. A portion of it probably circulates through the brain, suspending the cerebral functions without directly destroying the action of the involuntary organs, contractility remaining long after the destruction of animal life, as is evinced by the activity of the heart and the intestinal canal.

IV. EXPERIMENTS WITH SULPHURETTED HYDROGEN.

My attention was next directed to sulphuretted hydrogen, the common gas of privies, which proves so destructive to life, for the purpose of ascertaining the condition of the animal organs after immersion in this gas, and of observing whether it actually enters the bronchial tubes.

It is generally stated that sulphuretted hydrogen destroys life by producing what is erroneously termed asphyxia; or, in other words, that the animal functions cease from the want of the vivifying influence of oxygen, although in fact the heart continues to act.

A rabbit of two or three weeks old was placed under a glass vessel filled with this gas. It gasped and died in somewhat less than half a minute. I removed it after allowing it to remain about two minutes and a half, and on opening the thorax found the heart palpitating freely, and the peristaltic motion of the bowels continuing, but the diaphragm was still. *The blood was universally of a thick and very dark brown tint,* no arterial blood being discoverable. The lungs were collapsed. *The brain seemed to be tinged with a dark-brown colour, and portions of it being removed to some distance off, afforded the intolerable odour of this gas.* The surface of the liver and intestines generally was suffused with a dark-brown tint. This experiment was repeated, and mice and sparrows also were employed with similar results.

It appears evident from these experiments with sulphuretted hydrogen, that the gas enters into the circulation by the lungs, and that passing through the brain it suspends the cerebral functions without directly destroying the spontaneous action of involuntary muscles, the heart continuing to act after the suspension of animal life and the cessation of the diaphragm and lungs, and the left ventricle distributing dark-coloured blood through the body. The absorption of the gas itself thus appears to act like a subtle poison; and as in cases of mere exclusion of common oxygenated air, when respiration is suspended for a longer period, the speedy restoration of the action of the diaphragm and lungs, and the introduction of atmospheric air, appear to offer the surest method of recovery; and the substitution of warmth and friction for the destructive use of tobacco injections and bleeding is essential. Bichat and Chaussure having found that the application of galvanism in cases of immersion in the gas of privies produces a deposition of sulphur, confirms the result of my experiment as to the entrance of sulphuretted hydrogen into the circulation.

V. EXPERIMENTS WITH HYDROGEN GAS.

A kitten of about a fortnight old was placed under a glass vessel of hydrogen gas. It fell dead in less than half a minute, after gasping and struggling, and on being removed into the air it recovered. Another kitten of the same brood, being similarly immersed, was affected in the same manner, but having lain about three minutes in the gas, after falling insensible, it did not recover in the open air. It was then opened, and the circulation was found to be stopped, the right ventricle being distended with dark blood, and the brain and lungs collapsed. A frog was immersed in hydrogen, but it exhibited no signs of inconvenience. In the morning it was found dead, and the blood was uniformly dark coloured.

Several sparrows were immersed in hydrogen, and they fell dead in two or three seconds. On opening them the circulation was still, the right ventricle distended with dark blood, and the brain and lungs collapsed.

The hydrogen gas appears in these instances to be admitted through the bronchial tubes, and, like the preceding gas, to be destructive in the same manner as certain poisonous substances act upon the centre of the nervous system. It is remarkable in the experiments with hydrogen, that no contractility could be excited by mechanical irritation in the heart and bowels. The renewal of atmospheric air in the lungs within the space of three minutes induces restoration of their action and recovery of animation.

VI. EXPERIMENTS WITH CARBURETTED HYDROGEN.

In order to ascertain the effects of this gas, I placed a kitten of about a fortnight old under a glass vessel carefully charged. It made two or three rapid gasps, and fell dead in a few seconds. Having removed it, artificial respiration was employed, and it recovered. Another kitten of the same brood was similarly affected; but, being left about three minutes in the gas, it was not resuscitated. On opening the thorax the heart was still, the blood dark, the right ventricle full, the vessels of the brain nearly empty, and the lungs collapsed. Several sparrows immersed exhibited similar phenomena. This gas also appears to enter into the circulation, darkening the blood, and destroying sensibility after a few waves of the deteriorated blood have passed through the brain.

VII. EXPERIMENTS WITH NITROUS GAS.

Several sparrows were immersed in nitrous gas, and after a few rapid gasps they fell dead. The heart was still, and did not contract on being irritated. Mice placed in the gas exhibited the same phenomena. A young rabbit lived only a few seconds in this gas. The blood appeared to have lost much of its arterial character, the right ventricle was distended, and the vessels of the brain and lungs were collapsed.

The nitrous gas, as might be expected, seems to act directly upon the centre of the nervous system, while it appears also to suspend the contractility of the involuntary organs of motion.

VIII. EXPERIMENTS WITH CARBONIC ACID GAS.

Several sparrows being immersed in this gas, they died in somewhat less than three minutes, having gasped and struggled violently. Upon dissection the brain and lungs appeared to be collapsed, the right ventricle was distended with dark blood, and the circulation was still. Some kittens of about a fortnight old were next made the subjects of experiment. None of them shewed any signs of life after three minutes' exposure to the influence of the gas. At first they gasped and breathed in a hurried manner, and then fell insensible and motionless, after long and slow inspirations. Having removed one of these kittens instantly as it fell, it was restored to life by warmth and artificial respiration. The other, being kept about four minutes under the glass, did not recover by similar means. The blood was uniformly dark, and the circulation still. The lungs were collapsed, and the vessels of the brain contracted.

Animals immersed in carbonic acid gas appear to be destroyed much slower than in other gases uncombined with oxygen, the general difference being between about half a minute and three minutes, the latter being about the average duration of suspended animation from drowning and hanging, in cases of recovery. The cause of death may probably be the same therefore, namely, the want of oxygenated air for the circulation of the brain, for without the red particles of the blood, the cerebral functions cannot go on, and insensibility is directly brought about in animals of hot blood. It is remarkable, that in the experiments with carbonic acid gas, the bodies of the animals were very sensibly elevated in their temperature throughout the whole inside, as if they had been exposed to the influence of a fire.

In reviewing generally the facts developed in the experiments here detailed, it is observable, that all the gases employed are, in fact, perfectly capable of passing the epiglottis, and do, more or less, enter into the circulation through the air-passages of the lungs. And, excepting the carbonic acid gas, each seems to destroy life much in the same manner, and in far less time, than from the mere exclusion of common air. The phenomena attending the respiration of these gases apparently lead to this supposition—that they act upon the principle of certain poisonous substances, which are known to suspend the functions of the brain, and quickly destroy sensibility, while the organic property of vital contractility survives the animal life in most instances some time after all consciousness has ceased.

The comparison which may be drawn from the experiments upon the oxgenated gases and those without oxygen, while it shows the first to be ultimately destructive to sensibility, though tending to prolong animal life to a degree far beyond any other gases, displays the relations of oxygen to animal life in a very striking point of view. This comparison suggests also some valuable considerations; and these, however curious the researches of chemists have been, and of whatever practical importance, principally belong to the department of the physiologist.

If the poisonous principle, upon which the gases now enumerated act, be inquired into, it may probably be referred to the *sedative* class of poisons, which operate so quickly and so decidedly on the centre of the nervous system to the suspension of sensibility, and frequently without interfering directly with vital contractility. It does not appear that they act until they absolutely reach the brain, and then with variable intensity, according to circumstances casually influencing their effects. But, in my experiments, the constancy of effect was very marked; and the smaller the animal the quicker was the operation upon the brain, and the larger, the slower was the effect. So that, when the gases are pure and carefully preserved, no variations occur to render the results doubtful and precarious, and the modifications of the results may be usually anticipated. Although it is not my object at present to indulge in any speculative ideas which may arise out of these experiments—which, in some instances, are confirmed by modern physiologists, who have themselves corrected former errors—yet I cannot avoid the opportunity afforded me, from my facts, of referring to the unscientific method, even now not quite extinct, of attempting reanimation, in cases of suspended sensibility from exposure to certain gases. The inutility and mischief of the treatment alluded to cannot be too strongly and frequently pointed out; nor can the plain and simple indications of nature, as developed by experiments, be too fully made manifest, from reference to the principle on which the noxious gases operate upon animal life, and which it is the province of the physiologist to investigate and make known.

Notice of a Submarine Forest in Largo Bay, in the Frith of Forth. By the Rev. Dr. Fleming, Flisk.

Nearly eight years have elapsed since I transmitted, to the Royal Society of Edinburgh, the description of a submarine forest, which could be traced for several miles along the southern margin of the estuary of the Tay, and on the north side of the county of Fife. The paper referred to occupies a place in the ninth volume of the Transactions of that association. Last autumn, I was successful in detecting a second example of a submarine forest, at the opposite side of the same county, and on the northern margin of the Frith of Forth. It may be readily met with in walking along the sands during ebb-tide, from the village of Lower Largo, to Corn-cockle-burn, on the west side of Kincraig or Ely-ness.

The rocks on which the strata, connected with the submarine forest, rest, belong to the carboniferous epoch; and, though occupying a high place in the series, and abounding with coal, they exhibit, in many of their beds, that reddish-brown colour which has procured, for the lower portions of the formation, the denomination "old red sandstone." They are intimately connected with the several varieties of trap. The soft bed, on which the forest more immediately rests, consists of firmly-laminated clay, of a brown colour, similar to the hue of many of the rocks in the neighbourhood. With the exception of the roots of the trees, to be mentioned afterwards, I was not successful in detecting in it any traces of organic remains; but, judging from the thinness and continuity of its laminæ, and the absence of marine exuviæ, it might probably be referred, with considerable propriety, to *lacustrine silt*. Over the surface of this silt, there is a thin covering of sand and fine gravel, irregularly distributed, and not continuous. This, too, is probably of fresh-water origin. Over these the bed of peat reposes, which serves in a more definite manner to indicate the changes which have taken place on this part of the coast.

The peat is composed exclusively of the remains of land and fresh-water plants, such as commonly occur in such deposits. Along with these, however, appear, and rather frequently interspersed, the remains of trees, particularly of the birch, the hazel, and the alder. The nuts of the hazel, were likewise observable. The roots of some of the trees still occupied their original position, having grown on the surface of the clay, and spread their branches among its layers. I was able to trace the divisions of one of these roots, belonging apparently to an alder, from the trunk, or rather stump, to an extent of more than six feet; and in three directions, into the clay which had thus originally served as a soil. I may add, that the clay at present affords a dwelling to the *Pholas candida*, which forms therein its vertical burrows, confined, however, to those places from which the covering of peat has been removed. The peat itself is penetrated by innumerable vertical cells, containing a spio, which is probably undescribed, and may, for the present, be denominated *Spio emarginatus*. So numerous are these small worms in some places, that the peat, when broken across, seems to be composed of living threads. Perhaps the clay and the peat may have other inmates, but at the period of my visit, they happened to be greatly covered with sand, which prevented so minute an inquiry as was desired.

The phenomena presented by the series of strata now under consideration, seem to render it probable that the subsoil, or laminated brown clay, was derived from the neighbouring rocks of the coal formation; here remarkable, as already noticed, for their peculiar brown colour. The matter thus obtained, seems to have been conveyed into a lake, and there deposited, not hurriedly, like *diluvium*, but at successive, though irregular intervals. To this mode of formation may be referred the firmness of its ingredients, and their laminated, or stratified arrangement. It appears equally probable, that the waters of this lake were, to a certain extent, suddenly withdrawn, so as to enable what may be denominated land vegetation to commence. The change, indeed, appears to have been accompanied by some *disturbance*, as the film of gravel distributed over the clay testifies. During the lapse of a considerable period, the clay served as a soil, and supported a forest of birch, alder, hazel, and perhaps other trees. These at last shared the fate of many other ancient forests of Northern Europe. Decay commenced, and the harbingers and promoters thereof, the lichens and mosses, multiplied, until the whole was transformed into a bed of peat. Subsequent to this period, that remarkable change took place, by which this laminated clay, with its ancient forest, and more recent peat, became subject to be covered at every tide with the waters of the sea.

The existence of these remarkable strata, and even in some degree their value, were found, upon inquiry, to be known in the neighbourhood. The peat had been carted off many years ago, in considerable quantity, to serve as a compost, or manure, to the corn lands in the neighbourhood. The clay, however, has hitherto been permitted to retain undisturbed possession of its bed, though apparently well adapted for the purpose of brick or tile-making, and certainly most suitable for fertilizing the neighbouring sandy plains or downs, at present consigned to the purposes of a rabbit-warren, but which might soon be rendered fit for supporting better stock, if the resources at hand were suitably employed.

The occurrence of a bed of peat, with the stumps of trees in the clay, and their prostrate stems in the mass of vegetable matter above, had not failed to give rise to speculations on the subject, so that even the voice of tradition has not been silent. The late Rev. Spence Oliphant, minister of the parish of Largo, has recorded the leading features of the tradition in the history of the parish, published in the "Statistical Account of Scotland," vol. iv., p. 537. "Largo Bay extends from Kincraig Point, to that of Methul, making a diameter of nearly seven miles in length, and marked by a ridge of sand. The included bay forms a semicircle of about ten miles of sea coast. The above ridge is called by fishermen, the Dyke. Of this there is a tradition, although probably not well founded, among the oldest inhabitants of Largo, that there was formerly a wall or mound, running from Kincraig Point to that of Methul, containing within it a vast forest, called the *Wood of Forth*." In spite, however, of the doubt expressed by the author just quoted, concerning the value of the voice of tradition, regarding the "Wood of Forth," the phenomena still visible on the shore attest its authenticity. But how can we account for the existence of a tradition on the subject? Has the record been handed down through a succession of ages from the period when the forest existed, or was destroyed by being covered with the tide? The existence of the peat excludes any such supposition, for it demonstrates the destruction of the forest by ordinary causes, and the substitution, probably for ages, of a peat-moss, no uncommon occurrence before the submergence began to take place *. It is probable that the tradition on the subject arose from an opinion expressed by some early and meritorious observer, whose very name is now unknown, and by whose influence it became enrolled in the legends of the neighbourhood. The laborious, and generally accurate Sir Robert Sibbald, appears, from no notice being taken of the report in his " History of Fife and Kinross," to have been unacquainted with the phenomena on this part of the coast, and likewise with that tradition, which, though recorded at a more recent period, was accompanied with suspicions of its accuracy.

The natural history of submarine forests does not appear to have attracted that degree of attention from geologists which their importance might have secured, when they are viewed as indications of the changes which have taken place on our shores. Even the variety of situations in which they have been detected on our coast, from Orkney to Cornwall, might have excited the speculative observer to inquire if similar causes had operated in the different localities simultaneously, or in succession,—and led him at the same time to determine whether the phenomena of submerged forests was confined to what may be denominated the " Modern Epoch" of the earth's history, or had occurred during any of the antecedent periods.

That deservedly celebrated observer, Dr. Borlase, in reference to the submarine forests of Mount's Bay in Cornwall, considered that the ground had sunk or subsided, in consequence of earthquakes, and became liable to be covered at full tide with twelve feet of water.

Dr. Correa de Serra, taking into account the soft matter

* The existence of the fruit of the hazel, in submarine forests, has been considered by some as indicating the destructive change to have taken place in autumn. Such a conclusion requires us to believe, that the hazels in the same year produced their first fruit, and suffered death. For if they had produced fruit during a succession of years, the nuts must have been lying in the soil below, independent of the period of the year in which the trees were destroyed.

on which the Lincolnshire submarine forest reposes, considered its present depressed line, as the effect of subsidence suddenly acting by means of an earthquake, and this subsidence he defined to be the natural consequence of gravity, slowly, though perpetually, operating in soft ground.

Professor Playfair, in his invaluable illustrations of the "Huttonian Theory," regards the subsidence, which brought the forest within the reach of the tide, as constituting a part of that alternate depression and elevation of the surface, which, in his opinion, probably extends to the whole mineral kingdom.

In the paper in the Transactions of the Royal Society of Edinburgh, already referred to, I endeavoured to explain the present depressed state of submarine forests, by supposing that their present site had formerly been a lake, which in succession had passed into a marsh and wood; that the barrier having been removed by the encroachments of the sea, a partial drainage took place, followed by subsidence and submergence.

In the "Annals of Philosophy," for November 1823, p. 344, Professor Henslow endeavoured to shew, "that an increase of elevation, above the original surface of the ocean, has actually taken place," by water added to the earth at the time of the Deluge, by means of a comet: that in consequence of this elevation, beds of peat, containing vast numbers of trees, are now found in some situations, extending under the bed of the ocean.

In the same work, for April 1825, p. 255, Professor Sedgwick, without excluding the occasional operation of several of those agents which have been already referred to, has offered the following explanation of the phenomena of submarine forests.—"The mean elevation of the sea about every part of our coast, is unquestionably constant; but the actual level of high-water at any given place, is dependent on the velocity and direction of the tidal currents, the contour of the coast, and a number of circumstances which are entirely local. In proof of this assertion, it is only necessary to appeal to the fact, that in extensive bays and estuaries, the sides of which gradually diverge towards the open sea, the tides occasionally rise (through the operation of a common hydrostatical law) to an elevation which is many times greater than the rise of the same tides on more open parts of the coast. Any set of causes which greatly modify the form of a deeply indented coast, must, therefore, inevitably produce considerable local effects upon the level of high-water."

Such appear to be some of those views which different observers have entertained regarding the origin of submarine forests. They exhibit an unsettled state of opinion, which ought to excite to farther inquiry. Yet it need not be concealed that the phenomena themselves furnish satisfactory evidence by which several of the foregoing hypotheses may be successfully opposed.

If the mean level of the ocean be assumed as constant, and the submergence of the land be regarded as the consequence of a general subsidence, connected with earthquakes, we might expect to find the remains of forests occurring, indiscriminately, on all kinds of subsoil, or on such as trees and moss are associated with at present. But as far at least as my observations and reading extend, the submarine forests of this country occupy exclusively a subsoil of lacustrine silt, a deposit indicating satisfactorily the existence of a lake previous to the growth of the forest, and the formation of the peat. And if the waters of the ocean have risen in their level, in consequence of an addition to their mass, no matter from whence derived, and have overflowed tracts of land, clothed at the time with wood; the subsoil of these forests should certainly exhibit all the variety which would be displayed by any extensive wooded tract at present, if subjected to inundation or submergence.

The assumption of the permanence of the *mean level* of the sea, at any part of the coast, does not appear to be entirely free from objections. When we take into consideration the various currents which traverse the ocean, those rivers of the deep, as they may be denominated, such as the Gulf Stream, it does not seem unreasonable to suppose, that the mean level of the ocean, at those places against which the currents strike, may exhibit occasional irregularities. These may arise from changes either in the velocity or direction of the current, at the place, produced by alterations in the form of the headlands, or the distribution of the sand-banks, and altogether independent of the tidal wave. It appears to be owing to some such combination of causes, that the waters of the Red Sea maintain a constant elevation, of between four and five fathoms, above the neighbouring waters of the Mediterranean, at all times of the tide. In such circumstances, if the elevated waters of the Red Sea were either suddenly or slowly to assume the mean level of the Mediterranean, there would be left on its deserted shores, stratified or irregular deposits, containing the remains of marine animals, phenomena well calculated to puzzle the advocates for the universal permanency of the mean level of the ocean. On the other hand, were the waters of the Mediterranean to assume the mean level of the Red Sea, many tracts would be inundated permanently, and others during every flood, which at present are strangers to Neptune's influence. But leaving the question of the permanency of the mean level of the ocean, let us advert to the changes which may take place in the *mean level of flood tide*, as applicable to the case of submarine forests.

The mean level of the sea, at any place, may readily be determined by taking the excess of the mean of the two consecutive high water marks above the intermediate low water. If the ordinary neap tides of any place give a rise of ten feet of water, we may here assume an elevation or depression equal to five feet from the mean level. But if during spring tides, at the same place, the rise be sixteen feet, these grounds will be covered at the time of flood with three feet of water, which the neap tides did not reach, while a corresponding portion of the channel will be exposed at the time of ebb, which at the same period, in neaps, was submerged three feet. Should this condition of things be altered by any change in the form of the coast, or the extent and inclination of the inclined planes of the channel, the progressive motion of the tide wave may be altered in its velocity, direction, and elevation. If now, for example, instead of sixteen feet of tide, the waters rise to forty-two, as at King's Road, Bristol, the land on the neighbouring shore may be covered during flood with a column of water thirteen feet in height, which at a former period was beyond the influence of the tide.

If changes, such as have now been referred to, were to take place on a coast covered with a forest growing near the former level of high water, or with a bed of peat where a forest formerly grew, it is obvious that submergence of the spot would take place, and that a submarine forest would be formed.

If the opening of the connecting entrance of the Mediterranean with the Atlantic, at the Straits of Gibraltar, were by any means enlarged, high and low water marks, in the former, would be removed to a greater distance from each other, and places would be periodically covered by the one and uncovered by the other, which, at present, may be considered as protected, in consequence of the imperfect communication with the ocean diminishing the oscillations of the tidal wave. Similar occurrences must have frequently taken place in the creeks and estuaries of our own shores.

If the opening of the Red Sea into the Arabian Sea, by the Straits of Babelmandel, became more contracted, by the increase of coral reefs or sand-banks, the tidal waves of that gulf would experience a corresponding diminution, and instead of rising at high water, two or three feet above the mean level, they would become confined in their oscillations to a few inches, as in the Mediterranean at present. If the elevation of the mean level of the tide, at high water, became thus diminished, pools formerly filled with sea water and occupied by marine plants, might pass into fresh-water lakes, and a layer of peat might be formed of plants common to such a situation, covering or intermixed with the remains of the anterior marine vegetation. Such changes seem on the continent to have taken place at Linum, near Berlin, and in the vicinity of Drontheim. And there is some reason to suppose that similar changes had occurred at the Parret, in Somersetshire, where leaves of a zostera have been found, according to the observations of Mr. Horner.

But these changes which may take place in the level of high water, though they may afford an explanation of submarine forests situate *above* the mean level of the sea, furnish no evidence applicable to such as present themselves in an inferior position, or *below the mean level of the sea.* The submarine forest described by Dr. Correa de Serra, is stated as extending to the lowest ebbs in the year, or probably eight or nine feet below the mean level of the sea. The Somerset submarine forest is situate "considerably below the level of the sea, and now only to be seen at low water." Both the examples in Fife likewise extend below the mean level of the sea.

According to the views which I have adopted and illustrated in the paper already referred to, there occurs a considerable difficulty with respect to the barriers of those lakes which previously occupied the place of the forests and the silt of which at present forms their subsoil. Dr. Correa de Serra justly observes that "an exact resemblance exists between maritime Flanders and the opposite low coast of England, both in point of elevation above the sea and of internal structure and arrangement of their soils." To me it does not seem extravagant to connect the phenomena presented by the modern strata of both shores, and to lead the fancy back to that period when the space now occupied by the German Ocean was a fresh-water lake. To maintain such a state of things we have only to imagine the continuity of the chalk beds of Dover and Calais, and those of a similar æra of Sutherland and Jutland. The last is indeed no slight stretch of the imagination, and, in the absence of other proof, might deserve to be denominated extravagant. But in this neighbourhood there are other evidences indicating that fresh-water lakes existed where the sea now prevails, in the lacustrine silt over which it flows; and there are terraces and hills of fresh-water gravel which point out the former existence of sea-ward barriers of which not a trace remains. If such a lake ever existed, as the magnificent one alluded to, its drainage, and the consequent subsidence of its marshy margin, might serve to explain several interesting phenomena, as well as the character of those submarine forests which now present themselves below the mean level of the sea. But I fear that this notice has already extended too far to permit me to enlarge any farther on such topics.

February 15, 1830.

On the "Cystic Oxide" Calculus; and on the sensible, mechanical, and chemical Properties of the Urine in this Diathesis.—By ROBERT VENABLES, M.B., *St. Mary Hall, Oxford; Physician to the Chelmsford Provident Society,* &c. &c.

THE occurrence of calculi of this description is so extremely rare, that an opportunity of meeting with one may be considered as an era in practical medicine. Whether their occurrence be really so rare, or that many opportunities pass unheeded in consequence of the inattention so generally prevalent with respect to the more obscure forms of urinary disease, it is not my object on the present occasion to inquire. The cystic oxide was discovered by Dr. Wollaston for the first time in 1805; and in 1810, when he first published an account of its properties in the Philosophical Transactions, he had met with only two instances. Soon after Dr. Wollaston's description, Dr. Henry, of Manchester, discovered two specimens in his private collection*; but nothing seems to be known with respect to the histories of these cases.

Dr. Marcet met with three instances of this singular substance, and he has published a summary history of each, so far as he had an opportunity of becoming acquainted with it. For the details, imperfect as they are, I must refer to his work on "Calculous Disorders †." Mr. Brande met with two instances, the histories of which are also confined and unsatisfactory. The reader desirous of further information is referred to Mr. Brande's account ‡.

Dr. Prout, at the time of publishing the second edition of his very valuable work on the urinary organs, had met with only one instance. The detail appears to me the most valuable history extant, because it describes the general and very singular properties of the urine: objects of primary importance in calculous and all other urinary affections. Hence then it appears, that during a period of nearly twenty-five years—from 1805 to 1830—only ten instances of the cystic oxide have been noted in the whole of the medical experience of

* Henry, Med. Chirurg. Trans, vol. x. p. 140. † Pages 90—96.
‡ Royal Inst. Journal, vol. viii. p. 71.

Great Britain; and in only one of these have the properties of the urine been observed and described.

Although the instance, the particulars of which I am about to detail in this paper, will not perhaps be considered as adding much to the stock of information already extant upon this singular species of urinary concretion, still I am induced to bring it under the review of the scientific; not only because repeated observations upon so rare a form of urinary derangement must be valuable, but also having had the case under my immediate superintendence for a considerable time, frequent opportunities of attending to the properties of the urine occurred to me; and which, as tending to confirm in a great degree the very accurate description of Dr. Prout, will no doubt prove acceptable.

HISTORY OF THE CALCULUS.

Mechanical and sensible characters.—The calculus was about the size of a very large nut, and passed naturally with the urine through the urethra, by a female, a patient of Mr. Richard Cremer's, a respectable surgeon in this town, who gave it to me with a request that I would examine it, and ascertain its nature and chemical composition. It had a dull whitish appearance, and the outer surface was studded with a few sparkling shining crystals, which on examination proved to be the triple phosphate. The calculus itself in its external characters and appearance closely resembled the triple one. Its figure or shape approximated very much to that of the kidney. It weighed rather more than twelve grains. On being sawed through, it seemed of a waxy nature, though of much firmer consistence than wax, clogging the teeth of the saw, and giving occasion for frequent cleaning during the operation. Its texture was not laminated; its fracture was crystalline, and, as has been described, seemed to have a highly refractive density. The specific gravity was 1.714285.

Chemical characters.—Before the blow-pipe it gave out a peculiar fœtid odour, of a somewhat animal nature, but very distinct from that of the lithic acid, leaving a black spongy mass, which when further and more strongly urged, dissipated, leaving a minute portion of whitish ash, not alkaline. Heated with nitric acid upon a slip of laminated platina, it readily dissolved, and on being heated over a spirit-lamp till the acid was evaporated, a brownish, black, brittle residue remained, which burned, and gradually dissipated, on intensely heating the platina before the blow-pipe, leaving a whitish stain. It was insoluble in water, alcohol, the acetic, citric, and tartaric acids, and in the neutral carbonate of ammonia.

It was readily dissolved by the nitric, sulphuric, muriatic, and phosphoric acids, by the pure, and the carbonates of the fixed alkalies, and by lime and barytic water. The neutral carbonate of ammonia precipitated it from its acid solutions, and the citric, and acetic acids, precipitated it when held in solution by the alkalies and their carbonates.

Although these characters fully satisfied me that this substance could be nothing else than the cystic oxide, still, as I had never before seen a single specimen, I sent a portion of it to my friend Dr. Prout, who fully confirmed my views of its nature, and at the same time, strongly urged obtaining, if possible, the particulars of the case, and instituting an examination into the properties of the urine. The result I shall now proceed to detail.

HISTORY OF THE CASE.

The patient is a labourer's wife, residing at Mashberry, in this neighbourhood, aged forty-seven, stout, corpulent habit of body, sallow complexion, but in other respects healthy looking. Has had several children, living and healthy. Notwithstanding her general healthy appearance, she suffers very much from constant pains in the loins, mostly obtuse, resembling lumbago, but frequently assuming the acute character of active inflammation of the kidneys, requiring copious depletion and other powerful antiphlogistic measures for their relief. Mr. Cremer informed me that, at such times, the inflammatory affection of the kidneys assumed a very acute character.

She has frequently passed small calculi, similar to that given to me. The passing—if the calculi be of any size—is generally preceded by sharp and severe pains in the loins, extending in the direction of the bladder, and along the course of the ureter. These pains gradually become more and more severe, "till a feeling as if something dropping into the bottom part of the body*," when relief is obtained; and generally after

* I use the patient's own words in expressing her feelings.

an interval of a day or two a calculus is voided. Upon one occasion a great number of small ones, about the size of small peas, were passed, all of which were connected together like beads on a string. None of these, however, have been preserved, nor has their nature been positively ascertained.

OF THE URINE.

Sensible and mechanical properties.—On the 30th June, 1828, I for the first time obtained a specimen of the urine passed by this patient. It was passed in medium quantity. It was of a greenish-yellow colour, something like the rind of a melon when nearly ripe. The taste was slightly saline. The smell was very peculiar, and, I am satisfied, characteristic, as I had never before met with any thing similar. The only thing that I consider bears the slightest resemblance to it is the odour of the sweet-briar. If we imagine this odour adulterated with a fœtid urinous one, some idea, though I acknowledge an inadequate one, may be formed of the smell of this urine. Its consistence was oily, that is, the finest kind of oil, and its specific gravity 1.022. It was opalescent and turbid, apparently from the mechanical suspension of an opalescent impalpable powder. On being allowed to remain at rest, only a part of this amorphous pulverulent mass subsided to the bottom of the vessel. What separated seemed to subside enveloped in a kind of mucus mixed with coagulated fibrine; but a sufficient quantity to render the urine opalescent remained in permanent mechanical suspension, although left at rest for several days. The upper surface of the fluid, however, for the depth of about two lines, became rather clearer, so as to resemble a film of oil floating on a denser and more opake fluid. That the opalescence arose from the mechanical suspension of an amorphous powder, became evident by passing the urine through a filter, it passing through clear and transparent, of a deep sherry-wine colour, slightly tinged with green. What remained on the filter consisted principally of the cystic oxide*, intermixed with a

* Hence it may be inferred that the cystic oxide exists for the most part in a state of mechanical suspension, rather than of chemical solution, in the urine. This would still further appear from the fact that acetic acid and the other precipitating re-agents threw down very little—indeed scarcely any more—cystic oxide from the *filtered* urine, though this principle was abundantly separated by the

little animal matter—principally mucus and fibrine—and a small proportion of the triple phosphate.

Chemical characters.—This urine faintly reddened litmus paper; but this was clearly not the consequence of the re-agency of a free acid. No decided alkaline re-agency succeeded, even after an interval of several days, nor did the filmy deposit of the triple phosphate form on the surface *. Acetic acid and spirit of wine threw down a scanty precipitate of cystic oxide. Neutral carbonate of ammonia, however, threw down a copious precipitate, consisting principally of the triple phosphate intermixed with the cystic oxide in greater abundance; and a filmy layer of the triple crystals speedily formed on the surface of the urine. Ammonia produced nearly similar results, but the triple salt was more abundant, while there was no sensible portion of the cystic oxide mixed with the precipitate. When the urine was evaporated to one-half or one-third its original bulk, the acetic acid and alcohol threw down the oxide in quantities sufficient to recognize its properties and prove its identity.

This urine was also very deficient in *urea*; as not a particle separated on the addition of nitric acid, even when evaporated down to nearly the consistence of a thick syrup. Lithic acid was not separable even on the addition of the concentrated mineral acids to the urine evaporated to one-third its original quantity. This principle, however, was not wholly deficient, as was proved by evaporating to dryness, removing the phosphates, &c., and then treating the residue with nitric acid on a slip of platina, heating and subsequently evaporating to dryness. On exposing to the vapour of heated ammonia an ammoniacal purpurate was formed, easily recognized by its colour.

same re-agents before filtration. The mechanical suspension of the oxide may also serve to explain the readiness with which this substance separates and concretes into calculous masses in the kidneys, where the diathesis prevails; and it supports, if it do not absolutely confirm, the opinion advanced by Dr. Marcet, and confirmed by all the histories hitherto detailed, that calculi of this description are exclusively of renal origin.

* In the two last respects this specimen differed considerably from the one noticed by Dr. Prout. In the instance which occurred to him, "the urine soon was covered with a greasy-looking film and at the same time speedily became alkaline." But in this case the patient for a fortnight before had been taking alkaline remedies, which will easily explain the tendency to become alkaline, as observed by Dr. Prout. These characters, therefore, must be looked upon as *artificial*, rather than the *natural* and *genuine* result of the "Cystic Oxide Diathesis."

Serum in small proportion was an occasional, but not constant, ingredient.

The treatment consisted in the exhibition of the muriatic acid, and sulphate of morphia, with ipecacuanha, acetic extract of colchicum, and extract of hyoscyamus, in small doses, in the form of pills. The state of the bowels was also carefully watched, and any tendency to constipation was obviated by means of castor oil, or some other mild aperient. This plan was persevered in for a considerable time, Mr. Cremer having kindly undertaken to supply her with whatever medicines I might consider necessary for her relief. This plan was attended with considerable benefit, and I believe she has not experienced a severe attack of renal inflammation since that period.

On several subsequent occasions, and during the period of her taking the medicine, I have had opportunities of examining the urine. It would be useless to enumerate the individual results, as it would only lead to a repetition of what has been already stated. The only thing necessary to observe, is, that the specific gravity varied, being sometimes higher, sometimes lower, but never exceeding 1.025, nor falling below 1.020. There was less of the cystic oxide in mechanical suspension, and relatively more in solution, although the absolute quantity of this principle was diminished. The urine indicated more strongly an acidulous re-agency, and the quantity of lithic acid was sensibly, though not materially, increased; but what is of paramount importance, the patient's sufferings were very much alleviated.

OBSERVATIONS.

On reviewing the foregoing facts, several circumstances deserving of particular notice present themselves for consideration. We first observe a deficiency amounting almost to a total absence of two natural principles—urea and lithic acid—existing in comparatively great abundance in healthy urine. Hence we can readily admit the exclusive tendency of this diathesis, and easily explain the great purity of cystic calculi, as remarked by Wollaston *, Prout, Marcet, and others, who

* The first specimen discovered by Dr. Wollaston had a loose coating of the phosphate of lime. This might have been produced by an immoderate indulgence in alkaline remedies, or been furnished accidentally from the prostrate gland. That it was artificially, rather than naturally produced, may be inferred from the fact of the same patient—a boy about five years old—having died after the formation of another stone, which consisted almost wholly of lithic acid.

have enjoyed the most extensive opportunities of observation. The almost total absence of lithic acid prevents the possibility of any contamination with this principle or its compounds, while the tendency in the urine to alkalescence during the prevalence of the cystic oxide diathesis, would be unfavourable to the separation of the lithic acid, even if it existed in larger quantity. The absence of urea removes one source of the formation of alkali in the urine, and of course the precipitation of the phosphates. Urea, especially in mucous, and several other disordered states of the urine, speedily undergoes decomposition, evolving ammonia or its carbonate; hence the excess of phosphoric acid in the superphosphates of ammonia and of magnesia, &c., by which the latter is held in solution, being neutralized, the triple salt precipitates. But as the urea is so deficient in this diathesis, it is evident a very powerfully operative source of contamination with the phosphates is suppressed.

The faintly acidulous reaction noticed in this urine was owing unquestionably to the superphosphates of ammonia, magnesia, &c.; and it must be observed that the excess of acid in super-salts is in a very different state from that of a free acid; for although in *excess* as it is termed, it is still in combination, and so long as the combination exists, incapable of exerting the full chemical reagencies of a free or uncombined acid. It is upon these principles that we may explain the fact of the comparatively large proportion of the cystic oxide in mechanical suspension, and the small quantity of the same principle in actual solution. Probably the affinities of the oxide and excess of phosphoric acid for each other were inferior to those of this excess for the alkaline bases with which it was combined. Or, to speak more chemically, "the sum of the quiescent exceeded that of the divellent affinities," and consequently the intregrity of the super-salts was preserved.

Hence, too, we can explain the readiness with which the cystic oxide, when abundantly secreted, separates, as already observed, from the urine in the kidney, and concretes into calculous masses before reaching the bladder. When the specific gravity of the urine is high, and the quantity of cystic oxide secreted not superabundant—and this principle, even when concreted, not being of very high specific gravity,—in a state of minute or impalpable mechanical division its specific gravity will scarcely exceed that of the urine; it will remain in suspension and be evacuated with this fluid. But if the specific gravity of the urine be low, and the quantity of oxide secreted relatively abundant, of course there will be an immediate subsidence, and that in the kidney,—and a renal concretion will be the consequence.

All preceding observations infer severe disease of the kidneys in this diathesis. The present history fully confirms this inference. The frequent attacks of nephritis, the pains in the reno-lumbar regions, the coagulable fibrine, the albuminous and other morbid qualities of the urine, fully attest this conclusion, and indeed scarcely leave a doubt of its correctness.

Whether disease of the bladder be an essential consequence, the facts at present known are not sufficient to decide. In the present case, I think, there can be little doubt of its being affected in some degree; and as most of those cases, with the histories of which we are best acquainted, appear to have terminated rather suddenly—a very frequent occurrence in diseases of the bladder and kidneys, this may be considered as strengthening the presumption of the bladder being more or less diseased, or, at all events, liable to become so.

With respect to the medical treatment adapted to such cases we scarcely know any thing from experience. This arises from the limited field of inquiry necessarily presented, from the rarity of the affection, to those competent to the task. With but one or two exceptions, the opportunities of acquiring practical information occurred to those who probably had not devoted, and consequently had not qualified, themselves for inquiries of this description; and who possibly were unconscious at the moment of the nature of the disease entrusted to their care. "As to the remaining species of calculi," says Dr. Marcet, "and especially the cystic oxide, since these are soluble both in acids and alkalis, the use of the one or the other class of re-agents must be determined by collateral circumstances and by future trials *."

Dr. Prout observes, "With respect to the medical treatment to be adopted, this will depend on circumstances. In the first

* On Calculous Disorders, Ed. 2d., p. 181.

place, great attention should be paid to the digestive functions; and if the urine be acid, the alkalis may be taken with advantage; on the contrary, if alkaline, the muriatic acid *."

These precepts being founded on strictly chemical principles, and as being too exclusive, will not be found to answer in actual practice. In rare and obscure forms of disease, in which the sources of observation and experience are limited, and in which morbid anatomy and pathology have contributed but little to our instruction, we must found our principles of treatment upon reasoning and analogy. In all cases of serous urine; and in tendencies to an alkaline condition of this fluid, indicating positive or approaching disease of the bladder; I am disposed to regard mercury as highly prejudicial; and in the same light I view the alkalis, and those salts which tend to produce an alkaline condition of the urine. Of this description are the salts formed with an alkaline base, and a destructible or vegetable acid. I believe an alkaline condition of the urine, if kept up for any length of time, is of itself capable of inducing disease of the bladder †, even without any previous disposition; and this opinion is founded upon experience, not, however, sufficiently conclusive to be admitted as a general established principle. Upon these grounds I cannot but deprecate the use of the alkalis, and alkaline salts compounded with a destructible acid, in the cystic oxide diathesis in which there is a tendency to urinary alkalescence ‡, and to a deposition of the phosphates with at least a disposition to, if not positive disease, of the bladder. I advance it, however, only as a general principle, subject to occasional modification.

* On the Urinary Organs, Ed. 2d., p. 169.

† I believe it is an observed fact, that disease of the bladder is a much more frequent occurrence lately than in former periods. This perhaps some may be inclined to attribute to the increased attention of the present day, and superior methods of discrimination now prevalent. This will in part account for the observation, but not to the full extent. I think the increased prevalence may be in a great measure attributed to the empirical and inconsiderate indulgence in several fashionable medicines—as Seidlitz powders, &c.—and the popular practice of rendering hard malt liquor mild and brisk by the addition of carbonated alkalis.

‡ The urine, in most instances of the progress of the above case, was neutral, even during the exhibition of the muriatic acid; and when the quantity of lithic acid increased, it seemed to me combined with lime and soda, &c., forming lithates with these bases, but perfectly WHITE, and which of itself infers a tendency in the urine to become alkalescent. The very minute proportion in which these lithates existed, did not permit a satisfactory and unequivocal verification of their distinguishing characters.

The solution, and even permanent solution, of the cystic oxide, seems upon every principle of analogy and reasoning, to form an essential feature in the treatment. This, if my views be correct, cannot be safely nor advantageously attempted by means of the alkalis; therefore we must have recourse to those acids capable of exerting a solvent power upon this principle. Indeed they appear to me superior in every respect to the alkalis; and in this view, though possibly upon different grounds, I am supported by Dr. Prout, who says that the muriatic acid, "if the irritation present would permit it, might, perhaps in all cases, be employed advantageously, not only with the view of retaining the cystic oxide in solution, but of inducing the lithic acid diathesis*."

It is a problem of no easy solution, whether an acid introduced into the stomach be identically the same which, reaching the kidneys and mixing with the urine in a free state, gives to this fluid its acid reagency; or whether this effect be the result of more remote and less direct operations. Indeed the problem hardly admits of unequivocal demonstration. Analogy, however, would infer the affirmative proposition. We know that turpentine, nitre, and other similar salts, resist the decomposing powers of the stomach, making their way through the kidneys unaltered, and may be chemically detected in the urine. Hence, then, we may infer that the fixed or more undecompoundable acids will do the same. Upon these principles I prefer the phosphoric acid to all others, for it certainly seems to produce less irritation, and by holding the superabundant mucus in solution, thus favours its expulsion. However, I have not had much opportunity of trying it in the cystic oxide diathesis, although I have in the phosphatic and in catarrhus vesicæ.

Inflammatory action should be subdued and arrested by sufficient depletion and an active antiphlogistic regimen. Leeches, or occasional cupping the loins, sacrum, and the hypogastrium, will prove highly serviceable; and the insertion of permanent issues in the reno-lumbar regions will conduce much towards suspending the progress of organic disease in the kidneys.

* Op. Cit., p. 169.

The bowels should be kept moderately open, and the digestive functions be properly attended to and regulated. The due action of the skin should be promoted by an occasional resort to the warm bath, and the exhibition of mild diaphoretics.

I know of nothing superior to small doses of the sulphate of morphia, combined with acetic extract of colchicum, ipecacuanha, and hyoscyamus, hop, &c. These means I have found valuable in analogous conditions of the urine, and can recommend them from experience by no means confined. Such means steadily persevered in, I am inclined to hope, may prove highly beneficial; and in cases not marked by unusual severity, may possibly suspend the advance of the disease, or at all events, defer to a distant period the fatal termination.

P. S.—Although all the facts in the history of this disease, and all the information extant upon the subject, leaves not a doubt of the renal origin of this species of calculus, and fully prove the greater correctness of the name, "renal, or nephritic oxide," suggested by Dr. Marcet; yet, as neither he nor Dr. Prout, nor any of those who have preceded me upon this affection, have ventured to alter the name given to it by its discoverer, I have not deemed it prudent to attempt any innovation, and have, therefore, in this paper adopted the name originally bestowed upon it by the late Dr. Wollaston.

On the Coal-field of Sutherland. *By* J. Mac Culloch, M.D., F.R.S., &c. &c.

Although the "coal-field" of Sutherland was known to Mr. Williams, and has more recently been examined in a professional manner by an experienced surveyor, no account of it has yet been laid before the public. The singularity of its geographical position, nevertheless, and the peculiarity of its geological connexions, render it an object of great interest to a geologist; and they have induced me to draw up the following account. The circumstances of this Journal must be an excuse for not adding the map which belongs to it.

The total space which this deposit occupies is, as far as relates to its superficial area, very inconsiderable; but it extends for some miles along the shore, yet in an interrupted manner. In no place does it far exceed a mile in breadth; and, in some, it is not more than a few yards wide. It must therefore be considered that the real position of this "coal-field" is under the sea to the eastward; and that the part which alone is open to investigation is the edge or boundary, which reposes on the subjacent rocks that form the solid mass of the land on this side of Sutherland.

The interior land, which forms the boundary of this deposit, consists of a very irregular group of mountains, which are here divided nearly at right angles to the coast line by many valleys, giving passage to sundry streams from the higher country beyond it.

These mountains in one or two places protrude into the sea, and thus both terminate and intersect the "coal-field." In all parts they come down near to the shore, being thus separated from the sea only by the variable and often inconsiderable breadth of the secondary tract, which contains the coal. The elevation of this hilly ground is not very great, rarely reaching to one thousand feet; and the general outline is rounded and lumpish, unmarked by rocky protuberances or precipitous faces.

It was already remarked that a great tract of granite occurred on this coast, and I may now add that the hills thus described consist chiefly, through their whole extent, of this rock.

With respect to the aspect of the land which contains the secondary strata and the coal, it is far from being level in all parts. On the contrary, in many places it forms low undulating hills, and in others rises in an even manner from the flat shore, so as to conform in its inclination to the acclivities of the hills by which it is bounded. Where the rivers, which have been described as holding their courses through the intervals of the mountains, traverse those undulating or elevated parts of the secondary strata, they often form deep sections; by which the order of the stratification is exposed to view, and considerable facility afforded in determining the nature and succession of the whole series down to the subjacent rock. In other parts, where the shore is flat, the strata being elevated at different angles, that order can be traced, but to a much more limited extent and less satisfactorily, by following their elevated edges along the sea line, where the investing covering of earth has been removed. As the ebb is here considerable, the geologist will find his investigations much facilitated by choosing the time of low water for his operations.

The mass of granite described in the first part of this paper divides the sandstone of Caithness from this deposit; but while, on that side, the conglomerate strata occur in small quantity and are fine grained, often much resembling granite, and commonly composed of a very limited number of ingredients, no indication was observed of that well-known variety of conglomerate which is composed of many primary rocks, and in which the fragments are of various and generally of large dimensions.

But on that side of the granite where this coal-field lies, the first substance found reposing on it is a conglomerate of a coarse texture, formed of many different rocks; these being loosely agglutinated by gravel and sand of the same materials. The fragments are often so large as to reach to many hundred pounds in weight. The substances of which they consist are chiefly gneiss, granite, micaceous schist, argillaceous schist, and sandstone.

Where this conglomerate first appears, it forms an insulated high rock, now unconnected with the granite; but lower portions of it skirt the cliffs of that rock for a certain space along this part of the shore. Its extent, however, is not considerable; while, in many places, it forms a very thin bed. Tracing it along the granite, it at length disappears either partially or entirely, and in many parts of the coal-field it is altogether absent; so that the next strata in the order of succession come into contact with the fundamental granite of the mountains. I did not succeed in finding, in the northern part of this deposit, any of the ordinary red sandstone to which it must be supposed to belong, or any rocks resembling those which occur on the northern side of the granite; but to the southward there is found interposed between it and the granite, that series of red sandstone which extends into the southern parts of Sutherland and into Cromarty. It may be concluded that the conglomerate lying near the granite, on the northern part of the coal-field, is partial and evanescent, and that the secondary strata, which belong to the coal formation here, are in immediate contact with the granite throughout the greater part of their extent.

There is, indeed, no difficulty in ascertaining the truth of this statement by open examination; since I have, in one place, traced the coal itself within a few feet, or even inches of the granite; the interval being filled by a shale. In many other places, the sandstone, shale, or limestone, in the series may be traced, if not absolutely in contact with it, yet, to a distance so very short, as to render it probable that nothing is interposed, and to render it certain, at least, that not much of the conglomerate, if any, can be present.

Before describing the remaining substances that occur in this coal-field, it is necessary to mention all that could be discovered respecting the order of succession of the particular strata in the deposit. That, indeed, appears so irregular, that no accurate notion can be formed of it, nor any detailed and certain description given. As the strata are commonly thin, and the different substances are repeated many different times in alternation, it is not difficult to understand how this apparent irregularity arises. A certain order may really pervade the whole, although no two distant places may agree in exhibiting the same order of succession. From the tenuity of any one stratum, as it becomes diminished in its progress, it gradually vanishes; and thus, a new order appears to be the consequence; while the same taking place in other places, with regard to other substances on other strata, an appearance of complete irregularity is the result. And thus, in no two places does this series exhibit precisely the same number of beds, or the same proportions, or arrangement of the different substances of which it is composed. As similar appearances are, however, by no means uncommon, it is unnecessary to dwell on them; it is sufficient to have mentioned the fact, as an apology for not attempting to give with precision the order of succession among the remaining strata to be described. In describing them, I shall, however, notice these which, from occurring most generally, or always, in the lowest situations where they exist, may be considered as those which follow the fundamental conglomerate wherever they are found; or which, when that is not present, repose immediately on the granite.

The first of these to be noticed, is a conglomerate with a basis of limestone, containing generally fragments of argillaceous schist (shales) or of sandstone, or both, or fragments of other limestones. More rarely it contains fragments of gneiss and of granite, and occasionally also of quartz and of felspar. This conglomerate, from the places in which it occurs, seems immediately to follow the fundamental conglomerate already described; although, from the nature of its position in the sea, the contact cannot be traced. I could not discover it in those parts of the field most remote from the granite; although it may exist deep beneath the surface, where the strata are inaccessible. This position is indeed to be expected; as it approaches very nearly in character to the fundamental conglomerate; differing from it in little else than the smaller sizes of the fragments, and the calcareous nature of the basis.

The strata which seem to follow next in order to this, consist of a grey limestone, varying in its aspect in different parts. It is sometimes large, granular, or else of a fine grain, and very smooth, even fracture, and it is also occasionally schistose. In some places it alternates with thin beds of shale. In others it contains fragments of charcoal; or else the calcareous substance is intimately mixed with carbonaceous matter, or the coaly ingredient alternates in thin laminæ with the limestone. These parts of the calcareous strata, I must, however, remark, seem to be the upper part of this deposit.

The reasons why it is judged that these limestones follow immediately after the calcareous conglomerate are, that their position on the shore where they occur, seems to justify that conclusion, and because, in other cases, it is remarked, that where a bed of conglomerate, similar to the fundamental one of this place, is followed by a calcareous conglomerate, that is succeeded by simple calcareous strata. The causes of this order of succession must be very obvious.

Other limestones in this part of the series consist solely of a calcareous conglomerate; that is, the basis is a simple limestone, containing imbedded fragments of other limestones. But these also seem, like the carbonaceous beds, to belong to a higher part of this series.

The organic remains which occur in the calcareous strata, also appear to lie in the upper beds of this part of the deposit. They form a different set of calcareous strata; but the animal exuviæ are frequently intermixed in the same stratum with the carbonaceous matter which appears to have been derived from vegetables. The species of organic substances which I had an opportunity of examining, are far from numerous; and they are so very often obscure, from being mutilated and imbedded in an unusually compact limestone, that it was impossible to determine all their species, if indeed they are all to be found among those which naturalists in this department have ascertained.

In the genus ammonites, five or six well marked species, with a probability, from impressions and fragments, of there being even two or three more. Two gryphites. Two, or perhaps more, belemnites. Many apparent fragments of spiral univalves; and two, which prove to belong to turritella and nerita. Also a buccinum. Among bivalves, species in the genera pecten, modiola, plagiostoma, terebratula, mya, ostrea, trigonia, cardium, and apparently some others, which, from the imperfection and small number of my specimens, were unassignable. Besides these, I find abundance of the spines of echini, some flustræ, some joints of encrini, and other fragments which seemed to surpass all powers of analysis. I need not be more minute, as the purposes of geology, such as I view that science, are accomplished, as far as my purposes are concerned, by this enumeration. The question of extinct zoology is of a far different nature: but I see no necessity for confounding them; and this does not form the present pursuit. As to the vegetable fragments, it is abundantly easy to describe forms, and stripes, and much more: but I have not discovered to what this tends, where the remains are so very obscure as they are here, or as I at least find them.

After leaving these calcareous strata, the strata which succeed consist of various shells and sandstones, with occasional small laminæ and larger beds of coal, and some thin and partial beds of sand, apparently resulting from the decomposition of some of the most friable sandstones. It would be quite fruitless to attempt to describe the order of succession in these repeated alternations; since they present every where that irregularity of recurrence and of dimensions which I noticed at the beginning of this part of the subject.

The sandstone is sometimes of a pure white, but rarely very solid; much oftener it is extremely friable. Occasionally it contains fragments of the same kind of sandstone, or presents, what is not very common, a simple white sandstone conglomerate. In one or two places, I remarked that it was intersected in every direction by laminar veins of great tenuity, reticulating in an intricate manner, and of a whiter colour than the body of the rock. They are also harder; since, on the exposed surfaces, they protrude in the manner so often seen in granite, where similar veins exist. I should also here observe, that among these sandstone beds, there occur conglomerates with a sandstone base, containing fragments of shale or of limestone, or of both. The variety of appearance which they hence present is considerable, but requires no further description. The shales are of various colours and of different degrees of solidity or tenacity. Bluish-black, black, and paler grey, are among the most common; but red, yellow, and purple are not unfrequent. That which is requisite to be said respecting the coal, will better find its place in the historical account of the workings at Broras.

Such is the variety of substances, and the general order of succession in this coal-field, as far as it can be ascertained, and as far indeed as it seems either necessary or useful that it should be known.

For those who are desirous of seeing more particularly the minute arrangements occurring in the upper part of the series, I shall content myself with referring to the working section of the Brora coal. I shall, in terminating this part of the subject, content myself with summing up, in the most general manner, the order of succession in the inferior parts, as far as that could be deduced with any probability. And, to render it more useful, I shall exhibit it as it occurs in different places.

Granite.
Coarse conglomerate of various rocks.
Calcareous conglomerate of various rocks in a calcareous base.
Grey compact, and granular limestone.

Grey limestone, with charcoal in fragments, or with carbonaceous matter in laminæ, or diffused through the rock.
Limestone, with various organic remains; also bituminous.
White sandstone conglomerate.
White sandstone, shales, coal, and limestone in numerous and very irregular alternations.

This enumeration comprises as complete a general series from the granite upwards, as this coal-field appears to afford; but it does not pretend to give the number of the strata.

The next series shews a simpler order of things, the lower conglomerate being absent: and of the following, I may as well remark here, that they are detailed to shew what takes place in those cases where the granite touches the coal-field in such a manner as to exclude the lower strata; or else, where, in the progress of deposition of the successive strata, the lower have gradually disappeared—

Granite.
Calcareous conglomerate.
Limestone.
Limestone, with carbonaceous matter and shells.
Sandstone, shale, limestone, and coal, in different alternations.

Granite.
Limestone, with or without shells and carbonaceous matter.
Sandstone, coal, and shale, in alternations.

Granite. Shale. Coal. Sandstone. Shale. Sandstone, &c.

Granite. Sandstone. Shale. Sandstone. Limestone. Sandstone.

Considering the phenomena which often take place at the junction of granite with the primary rocks, it will naturally be asked whether these strata are any where fractured or fissured, or whether they any where contain granite veins. Some fractures have been found in the coal strata at Brora, but none of the latter appearances were observed during my researches; and from the want of such appearances in the red sandstone, formerly described as similarly situated with respect to this granite, it is probable that there is no interference of the granite with this set of secondary strata. It must, on the contrary, be considered, that they have been deposited on the previously formed basis of the granite; whether or not they may have, since that period, undergone any change of position. I am thus led to consider the present state of their inclination, which is the last subject connected with the history of these strata yet remaining to be noticed.

This inclination is exceedingly irregular and inconsistent. At the coal works at Brora, it is about 12°, or varies from 10° to 15°; dipping to the south-east. Here I must remark, they lie at some distance from the foot of the hills. Near the northern parts of the field, the inclination varies from 20° even to 60° and 70°; and in some places, they are even vertical. In one or two situations, I observed them reversed in contact, although no intervening veins were present. It seems also to be a sort of general rule, that as they approach the hills, the angle of inclination increases; although exceptions to this law occur. If the dip is not invariably to the south-eastward, that must still be considered the predominant one; a circumstance which might be expected from the position of this field with respect to the hills. The exceptions, however, which occur, are neither unfrequent nor trifling; as the dips are often found to be north or south, and at every possible angle, even in a very narrow space.

Before proceeding further in the history of these strata I must interpose a few remarks, as a continuation of the same fact which forms a remarkable circumstance in the first portion of this paper; the granite is succeeded by what will presently be seen to be a very remote portion of the secondary strata and without the intervention of any primary strata. By the manner in which these strata succeed to the granite, it is plain that the order is not entire throughout; but that different members of the series, where it is complete, come into contact with the fundamental rock. As it has also been already inferred that this granite is not posterior to the strata, but that they have been deposited on that rock, it follows from the circumstances stated in the preceding paragraph, that the inferior strata have, in certain parts, ceased in succession to be deposited, thus admitting the upper members to come into contact with the fundamental rock. This occurrence is, however, so common in other cases where secondary strata occupy the geological situations commonly called basins, that it needs excite no surprise. The present fact is, however, as yet a solitary occurrence; it is the only instance known, in which strata so high in the usual order of succession, are found in contact with that rock which is the lowest of all, whether secondary or primary.

The last general consideration in this case relates to the disturbance of the coal strata in this field, which I have shown to be considerable. It is undoubtedly a common occurrence everywhere; yet, as a great variety of substances, reaching to a great depth, are in all such cases found beneath those strata, the nature or the places of the cause which have led to them are entirely out of the reach of conjecture. Here, the lowest rock, the granite, is immediately in contact with the secondary strata; and if these disturbances have been produced by partial elevations, or subsidencies in the foundation, it is in that rock that they must be sought. In other cases, among the primary strata, where it seems probable that the disturbances of these have been produced by changes in the condition of the subjacent granite, that probability rests chiefly on the intrusion of this substance in the forms of veins, on its peculiar and irregular obliquity to the strata at the places of mutual contact, and on circumstances which have so often been discussed that it is unnecessary to repeat them. But here no veins have been discovered; and it seems, on the contrary, probable, that the secondary strata in question have been deposited originally on the solid granite, whatever posterior changes they may have undergone. The granite, therefore, not having been protruded in a fluid state beneath them, whatever changes of figure it may have undergone capable of producing the disturbance of the strata, must have been effected by causes acting at a greater depth within the earth, and sufficiently powerful to alter the form of the solid rock above, together with the angles or inclinations of the strata that lie upon it. I need only add, that these phenomena, on this side of the granite mass, are ample evidences of what, in the former part of this paper, where the sandstone alone is concerned, might have appeared conjectural.

I shall conclude these general remarks on this "coal-field," with the following observation. These secondary strata contain fragments of other secondary strata of similar character, imbedded in the base. These also are not of the nature of local conglomerates, or they are not such fragmented rocks as are found on the surfaces merely, or at the junctions of discordant strata. On the contrary, being of the character of general conglomerates, they prove that these secondary strata have been formed from the ruins, at least in a certain degree, of previous secondary strata of a similar nature. To what important conclusions this fact may lead respecting the nature of former states of the globe, it is unnecessary to suggest; since it is evident that, like some other analogous facts, they prove a series of revolutions far more complicated than geologists in general have as yet thought fit to admit.

I shall now subjoin an account of the working of the coal at Brora; being indebted to the Marchioness of Stafford for the facts on which it is founded.

The first coal-pit was sunk and wrought at this place by Jane, Countess of Sutherland, in 1598; since which time the workings have been occasionally carried on, but, till lately, with no great energy. The first working was made near the sea-shore; and it is probable, from the thinness of the stratum, and the pyritical nature of the coal, that it was the uppermost bed of this part of the field, and that which at different times brought discredit on the nature of the whole produce. That bed crops out to the southward of the present high road, and near the old salt pans; and it appears also to be found at Strathsteven, westward of this place.

It is unnecessary to trace the intermediate history of this work throughout to the present time; but, forty years ago, a company from Portsoy undertook to work it, and found a second stratum three feet thick, of a better quality, which was wrought by a pit forty feet deep. The outburst of that coal is now to be seen on the banks of the river, near the present pit.

This last attempt was commenced in 1813, in which year the sinking of the engine and draw-pits, now in use, was completed. The stratum which is wrought, is the third in the order of succession downwards, and lies, at the pit, about two hundred and forty feet beneath the surface. The dip is to the south-east, and the angle, in the miners' phrase, one in four. The thickness of the stratum varies from three to four feet. It appears that the workings have extended about seven hundred yards forward, on the rise of the stratum; and that about ten acres have been excavated. It is found, moreover, that a cubic yard of the bed produces about a ton of large coal; and the operations are in such activity, as to furnish thirty tons in the day.

The quality of this stratum, which is, as already remarked, superior to that of the two which lie above it, is intermediate between that of Newcastle and Staffordshire. It has been exported to the neighbouring coasts of Inverness and Cromarty; but a large quantity is consumed on the spot, in the salt pans and potteries which have been established on this estate.

The engine-pit has been sunk forty-five feet lower than the present coal; and in the course of this proceeding, there have been discovered two thin seams of coal, one of them nine inches, the other sixteen inches thick. In the same pit has been found a bed of fine brick clay—a stratum which does not appear in the other parts of the series.

Four *faults* have been found in the present workings, consisting of a simple subsidence of the roof, as it occurred in the course of the excavation, but not amounting to more than three or four feet. To the eastward of the pit, it appears particularly subject to these faults or slips, and is gradually becoming more irregular: but to the west, where the level has been driven for seven hundred yards, it is more regular, though not quite free from troubles of various kinds.

The following is the miners' section of the Brora pit, as it stood in August 1820. I am sorry that I cannot interpret the word bass, as the substance was not shown to me when there; but it is not of great importance.

Journal of Sinking a new Coal Pit at Brora, finished in August 1820.

			Feet.	Inches.
1819.				
April	15	Black soil . . .	1	,,
	17	Light rich sand . . .	3	,,
	20	Gravel	4	,,
May	26	Common dark blue . .	4	,,
	27	Live sand . . .	,,	2
	31	Blue	8	,,
	,,	Common coal . . .	,,	$\frac{1}{8}$
August	13	Common dark blue . .	99	,,
1820.				
January	15	Black bass . . .	6	,,
	,,	Hard blue rock . . .	9	,,
	,,	Mixture of limestone and white spar	3	,,

1820.			Feet.	Inches.
January	15	Bass and shells	39	,,
August	10	Black bass	6	,,
	,,	Common blue	9	,,
	,,	Shelly grey rock	5	,,
	,,	Black bass	7	,,
	,,	Hard blue rock	7	,,
	,,	Common blue	3	,,
	,,	Very hard rock	1	,,
	,,	Common blue	4	,,
	,,	Roof of the coal, blue, intermixed with shells and petrifactions	3	,,
	,,	Coal	3	,,
	,,	Parrot	3	,,

It remains to inquire into the proper geological relations of this deposit, to which I have hitherto applied the popular term coal-field. I consider it to be that lignite formation which belongs to the oolite limestone*, and this is proved by the character of the strata, as also by the general nature of the organic remains contained in them: this latter species of proof, however, being one on which I lay somewhat less stress than conchologists, particularly in the comparison of strata distant in geographical place, and for reasons assigned in a paper on Organic Remains in the Edinburgh Encyclopædia.

The lias, and the oolite, therefore include its limestones, and among these presumed unvarying rocks we must seek the different beds here found, as well as we can. The minuter parts of this task I gladly leave to those who, as they can feel no satisfaction till they have reduced the whole world to the model of England, can, I hope, always find, if it is even in their imaginations, the means of satisfying themselves.

I apply the term lignite to this formation, without hesitation; and the reasons are given in a paper intended for your journal hereafter†. And as I shall then have occasion to enter on the whole subject of these higher coal deposits, I will not touch further on it now. I need only say, that the presence of complete coal does not remove this or any other such deposit from that division, as the chief coal deposits of the continent belong to it, and as the essential character is found in the

* This was pointed out already in this journal, in a paper on Lignites, where I also traced rapidly the general extent of the same series in Scotland, as it had been formerly described in detail, in my account of the Western Islands.

† This paper is the one above alluded to.

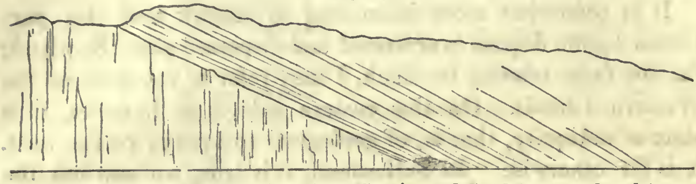
geological position. English geologists will then perceive that the coal, or lignite, of Sutherland, has its well-known analogues in the oolite coals of England, and I need not therefore waste space in extending this comparison.

It is somewhat more interesting to remark how this particular lignite deposit is scattered and dispersed about Scotland; for the facts relating to which, I may refer to my work on the Western Islands. On this eastern side, it is, however, in a state of antiquity, that is, of geological antiquity; on the west, it is far otherwise. In Sutherland, it is true, we saw but the very edge of some basin, of the extent of which we cannot conjecture; and, therefore, the quantity is very small, since the sea has either destroyed or contains the rest. Yet its traces, at least, extend further, even into the Murray Frith, where, however, they are but traces, appearing at two or three points to the south of Cromarty harbour, for the last time.

In the Western Islands, a very different interference has reduced it to the dispersed fragments which it now presents; and even these are, as we may say, reduced still lower in space, by occurring commonly on the very edges of the sea. The great interference consists in the mountains, or masses of trap-rocks, by which it is everywhere overwhelmed; and while these have in some measure protected it from the sea, they have but had the power, generally, of protecting its thin edges, though a few exceptions occur, as in Skey, and elsewhere. As might be expected, every species of disturbance, and obscurity, and destruction, is also added to the mere fact of overwhelming, rendering the whole a task to investigate, which I observe has become abundantly easy, since it has been done; since the entire analysis has been given, and every, the most disjointed atom, traced out, and referred to the general deposit. How often it had all been passed by before, as inexplicable, appears to have been forgotten by those who now find the whole so plain and easy. If I can yet point it out where I have reserved it for others to discover, I should have been little satisfied of the originality of any discoveries, had they discovered these places also.

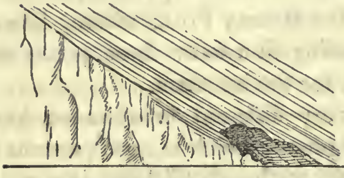
But I may terminate this paper, referring to that work for details and drawings which will enable any one now to produce a map of the lignite and oolite formation of Scotland, with the exceptions at which I have hinted. Of these, the Sutherland coal-field is at least the most continuous and conspicuous portion.

Fig. 1.



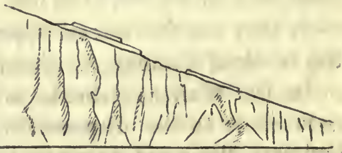
General view of the Junction of the Granite and Sandstone; reduced to a plain surface.

Fig. 2.



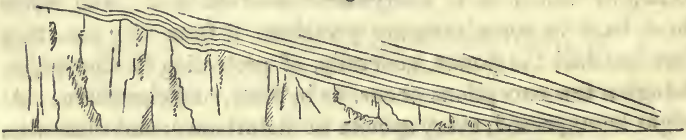
Enlarged view of the Junction.

Fig. 3.



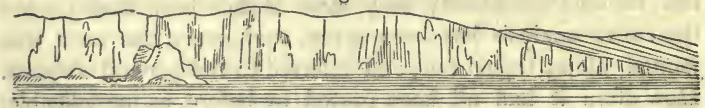
Portions of Granite assuming the appearance of stratification.

Fig. 4.



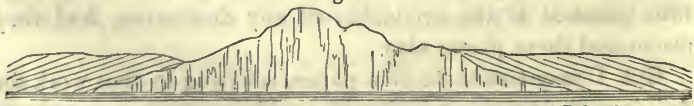
View of the undulations in the Sandstone where near the Granite.

Fig. 5.



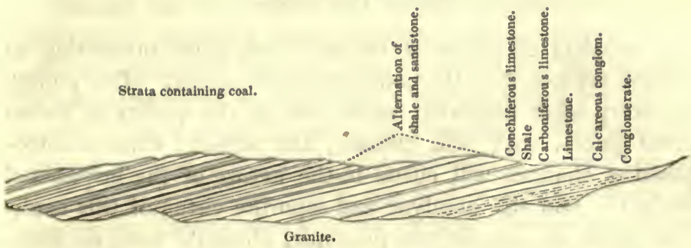
View of the commencement of the Conglomerate of the Coal-field.

Fig. 6.



Relative position of the Coal-field and the Red Sandstone to the Granite.

Fig. 7.



General notion of the nature of the Sutherland Coal-field.

EXPLANATION OF THE CUTS.

Fig. 1. This represents the general view of the whole line of the junction, reduced to an imaginary section by the omission of all the picturesque irregularities. The extent is also condensed. The apparent stratification of the upper surface of the granite is represented near the cave, where it is accessible to the hand.

Fig. 2. Is an enlarged drawing of the interesting part near the cave, where the apparent stratification of the granite is more distinctly represented.

Fig. 3. Represents other portions of the surface of the granite. The sandstone strata have disappeared here for a certain space, so as to leave the granite forming a step or shoulder; and on it are seen portions of the same apparently stratified granite as that which occurs near the cave.

Fig. 4. Represents a point where the depressions or irregularities in the surface of the granite are attended by corresponding inequalities in the incumbent sandstone.

Fig. 5. Sketch of a view on the coast, pointing out the commencement of the conglomerate.

Fig. 6. General notion of the mode in which the granite separates the sandstone of Caithness from the coal-field of Sutherland.

Fig. 7. A general imaginary section of the coal-field, founded on the succession of strata observed in different parts of it. The true position of the granite to it could not be represented, even in many sections, from the peculiar mode in which the strata are related to that rock; but the present mode of delineating it serves to shew that it is in contact with all the members.

Observations on Opium and its Tests. By Andrew Ure, M.D., F.R.S., &c.

Few subjects of chemical research are more interesting to medical science than the constitution of opium. The poppy, like every other vegetable, must vary in the quality of its secreted juices, with soil, climate, and season; whence corresponding changes will ensue in the nature of the inspissated product. Did the anodyne and soporific virtue of this medicine reside in one definite principle, chemical analysis might furnish a certain criterion of its powers. It has been pretty generally supposed that this desideratum is supplied by Sertürner's discovery of morphia. Of this narcotic alkali not more than seven parts can be extracted by the most rigid analysis from one hundred of the best Turkey opium; a quantity, indeed, somewhat above the average result of many skilful chemists. Were morphia the real medicinal essence of the poppy, it should display, when administered in its active saline state of acetate, an operation on the living system commensurate in energy with the fourteen-fold concentration which the opium has undergone. But so far as may be judged from the most authentic recent trials, morphia in the acetate seems to be little, if any, stronger as a narcotic than the heterogeneous drug from which it has been eliminated. Mr. John Murray's experiments* would, in fact, prove it to be greatly weaker; for he gave two drams of superacetate of morphia to a cat, without causing any poisonous disorder. This is perhaps an extreme case, and may seem to indicate either some defect in the preparation, or an uncommon tenacity of life in the animal. To the same effect Lassaigne found that a dog lived twelve hours after thirty-six grains of acetate of morphia in watery solution had been injected into its jugular vein. The morphia meanwhile was entirely decomposed by the vital forces, for none of it could be detected in the blood drawn from the animal at the end of that period†. Now, from the effects produced by five grains of watery extract of opium, injected by Orfila into the veins of a dog, we may conclude that a quantity

* Edin. Phil. Journ. vii. 388.

† Annales de Chim. et Phys. xxv. 102.

of it, equivalent to the above dose of the acetate of morphia, would have proved speedily fatal.

Neither can we ascribe the energy of opium to the white crystalline substance called *narcotine*, extracted from it by the solvent agency of sulphuric ether; for Orfila assures us that these crystals may be swallowed in various forms by man, even to the amount of two drams in the course of twelve hours, with impunity; and that a dram of it dissolved in muriatic or nitric acid may be administered in the food of a dog without producing any inconvenience to the animal. It appears, however, on the same authority, that thirty grains of it dissolved in acetic or sulphuric acid caused dogs that had swallowed the dose to die under convulsions in the space of twenty-four hours, while the head was thrown backwards on the spine. Oil seems to be the most potent menstruum of narcotine; for three grains dissolved in oil readily kill a dog, whether the dose be introduced into the stomach or into the jugular vein.

Since a bland oil thus seems to develop the peculiar force of narcotine, and since opium affords to ether, and also to ammonia, an unctuous or fatty matter, and a resin (the caoutchouc of Bucholz) to absolute alcohol, we are entitled to infer that the activity of opium is due to its state of composition, to the union of an oleate or margarate of narcotine with morphia. The meconic acid associated with this salifiable base has no narcotic power by itself, but may probably promote the activity of the morphia.

Hence, though the weight of morphia obtainable from a given variety of opium may by no means represent the total essence of the drug, yet its quantity is most probably proportional to the powers of the opium. But morphia exists in the state of a meconate, and its quantity must be in equivalent ratio to that of the meconic acid. On this principle, a ready mode seems to offer of trying the comparative narcotic powers of different opiums. Let a grain or two of each be dissolved in a little dilute alcohol, and then diffused through such a body of water as will make the liquid nearly colourless. Pour each liquid into a graduated glass cylinder, and add to it a few drops of red muriate (or tincture of muriate) of iron. The characteristic brown-red tint will immediately appear, of a depth proportional to the meconic acid, and equivalent to the morphia present; for the previous dilution with water has been so great as to remove the inequalities of colour in the original spirituous solutions. Let the darker shades be now lightened with water till the tints of the whole be uniform; and the relative volumes of the liquids will afford an approximate measure of the qualities of the several opiums. It is obvious that a double quantity of any given opium will take a double volume of water to bring its meconate of iron to the standard shade. By this means different tinctures of opium may be very expeditiously compared in narcotic power.

I have tried in this ready way Turkey, English, and East Indian opium, and have found the results to harmonize sufficiently with their known powers determined by other methods. An improved East Indian opium, of which Dr. Chambers gave me a specimen, approaches by this test very closely to the quality of fine Turkey opium.

The employment of red muriate of iron as a re-agent for detecting the meconic acid of opium has been frequently resorted to, under different modifications, since Vogel first pointed out the singular sensibility of that acid to the peroxide ferreous salts. I have found solution of acetate of lead, faintly acidulated with vinegar, the preferable re-agent for separating meconic acid, in the form of a meconate of lead, from solution of opium. The slight excess of acetic acid prevents any of the morphia from falling down with the oxide of lead. Twenty-seven grains of washed, but still impure meconate of lead, may be obtained from one hundred grains of good opium—a result which I obtained both from the Turkey and the above East Indian. By treating this insoluble salt, diffused in water, with the equivalent quantity of sulphuric acid, or by a stream of sulphuretted hydrogen gas, the meconic acid is set free, and may be procured in small crystalline grains by slow evaporation of the filtered liquid. These grains, once concreted, are very difficult of solution in water, and may therefore be washed with this fluid. Of the washed grey-white grains, a solution perfectly colourless strikes a deep brown-red with a drop of permuriate of iron.

Another process for procuring meconic acid has been prescribed. The magma obtained by boiling magnesia in a watery infusion of opium, is to be washed first with proof spirit, to extract the narcotine and resin, and then with strong alcohol to dissolve out the morphia. The residuary meconate of magnesia is to be digested in dilute sulphuric acid, and the meconic acid is to be thrown down from that solution by acetate of lead. The meconate of lead is to be washed, then diffused in water, and decomposed by a stream of sulphuretted hydrogen gas. The meconic acid is set free and dissolved, and may be procured, it is said, in impure, scaly crystals, by evaporation.

On this process it may be remarked, that the sulphuric acid of the sulphate of magnesia is unnecessarily dragged along, to the injury of the meconic acid; for sulphate of lead is formed simultaneously with meconate, on adding the acetate of that metal to the mixture of the magnesian sulphate and meconic acid; and these two insoluble salts, the sulphate and meconate of lead, afterwards evolve their acids simultaneously to the sulphuretted hydrogen gas.

Whereas, by throwing down the meconic acid by the just quantity of acidulous acetate of lead, washing the precipitate, and decomposing it, either by the equivalent dose of sulphuric acid or by sulphuretted hydrogen, we at once obtain a relatively pure meconic acid.

From the circumstance of magnesia precipitating both the meconic acid and morphia from an opium solution, it may be inferred, that meconic acid will form an insoluble compound with magnesia. But this is by no means the case, for if we heat a solution of meconic acid with magnesia in excess, no meconic acid is withdrawn from the liquid, for it strikes as deep a red, with permuriate of iron, as before the magnesia was presented to it; but acetate of lead separates the whole of the meconic acid from solution or tincture of opium; so that the supernatant liquid occasions merely a faint, greenish-brown colour, with red nitrate of iron.

Among the criminal abuses of the diffusion of knowledge which characterize the present times, the administration of opium, or its tincture, concealed in various vehicles, by the lower orders, with the most felonious purposes, holds a conspicuous place. An atrocious crime of this nature was brought specially under my notice, about a year ago, in examining, by desire of the magistrates of Glasgow, the contents of the stomach of a man who had fallen a victim to these murderous devices. Here the laudanum had been largely mixed with strong beer, and was sensible to the smell, in the liquor extracted by the stomach-pump. One portion of that liquor, treated with acetate of lead, afforded an insoluble precipitate, from which an acid, strongly-reddening permuriate of iron, was separated by the agency of the sulphuric. Another portion afforded directly, with a few drops of the permuriate of iron, an evident reddish-brown tinge, very different from the drab or fawn-coloured precipitate occasioned in strong beer of the same quality by the same salt of iron. Other experiments were made, which it is unnecessary to detail at present. The chemical facts, joined to a body of circumstantial evidence, led to a conviction of the guilty pair, a man and wife, who were accordingly executed.

It was suggested, by the ingenious counsel for the culprits, that muriate of iron, as a test for opium, was fallacious, since it would give the same redness with sulpho-cyanic acid, a substance present in human saliva, as it does with the meconic acid of opium. I was not then aware that this curious acid, of modern discovery, did exist in the saliva, and thought it merely a *ruse de plaideur.* But, even if ambiguity had been occasioned by this test, the characteristic smell of opium could not be set aside.

Since that period, the elaborate work of Tiedemann and Gmelin, *Sur la Digestion,* has come in my way, which contains proofs, apparently sufficient, of the existence of sulphocyanate of potash in the saliva of man. Treviranus, indeed, in his "Biologia," published in 1814, had remarked that the human saliva gave a sensible redness to the permuriate of iron.

I have recently repeated and varied Gmelin's researches, and have found them entitled to confidence. My own saliva, and that of many other persons, in its natural flow, as well as provoked by smoking tobacco, acquires a blood-red hue, with a few drops of permuriate of iron, such as would give to water merely a faint, straw-yellow tinge. Saliva, simply distilled in a glass retort, at a gentle heat, which did not brown a particle of the mucus, afforded a colourless water, that reddened litmus paper, and grew red with a few drops of the ferreous salt.

The distilled liquid was also heated, with a few grains of chlorate of potash, and muriatic acid, in order that the resulting oxide of chlorine might acidify the sulphur of the sulpho-cyanic acid. This actually happened; for the liquid now precipitated a sulphate of barytes from the muriate.

The preceding mode of simple distillation which I adopted, obviates those sources of fallacy in Gmelin's experiments, on which Berzelius comments in his *Scientific Annual**. He observes, that in his own researches on the saliva, made some time back, he had tried to produce, with the peroxide salts of iron, the reaction noticed by Treviranus, *without success;* but that he had not treated the dried extract of the saliva with alcohol, as Tiedemann and Gmelin did. "What share," asks he, "may the boiling with alcohol have on these phenomena? That sulpho-cyanogen can be formed from sulphuret of carbon, and ammonia, with alcohol, we know from Zeise's investigations. May it not be inferred, that an analogous product at least, if not the same, may result from the re-action of alcohol on the dried constituents of saliva?"

When so skilful a chemist as Berzelius doubts of the real presence of sulpho-cyanic acid in the simple saliva, after he had seen Tiedemann and Gmelin's evidence of the fact, my doubts, in entire ignorance of their work, will not appear unnatural. That a member of that poisonous family of acids, at whose head stands the formidable Prussic acid, should be swallowed by man, not merely with impunity but with advantage, every day of his life, is very marvellous. But that it is so, my experiments prove beyond suspicion, since no such re-action as Berzelius alludes to can have place in simple distillation.

If into a little saliva, contained in a wine-glass, a drop or two of the red muriate of iron be poured, a few rusty-brown spots may be all that appear; but on adding a few drops more of the muriate of iron, and stirring the mixture, a florid blood-red colour will result in the whole liquid. Gmelin has shewn that the sulpho-cyanic acid is associated with potash in the saliva of man; and with soda, in that of sheep.

From the similarity of colour between saliva treated with permuriate of iron, and blood diluted with water, it occurred

* Jahre's Berichte, vol. vii. p. 301.

to me that the iron known to exist in the blood is probably in the state of a sulpho-cyanate. A series of experiments was in consequence instituted to determine the truth of this conjecture; but the results have not hitherto enabled me to determine whether sulpho-cyanic acid be one of the constituents either of human blood, or that of the sheep, however liberally it is supplied to the stomachs of both by the saliva.

Blood freed in a great measure from its fibrin and albumen, was rendered slightly alkaline by carbonate of potash, and then passed through a filter, with the view of separating the oxide of iron from the sulpho-cyanate of potash, possibly formed. The filtered liquor was next slightly supersaturated with phosphoric acid, and the mixture was distilled in glass at a gentle heat. A colourless liquid came over, which did not change the colour of litmus paper, but afforded, with a drop of permuriate of iron, a tint faintly inclined to red, when compared with an equal volume of water, to which a drop or two of the same muriate had been added.

It deserves to be noted, that the red colour produced by the action of permuriate of iron on meconic acid, or a weak solution of opium, has a brownish tint, distinguishable from the deep orange-red of sulpho-cyanate of iron, diluted to the same degree with water; and by further dilution, the meconate of iron merely pales its shade, but the sulpho-cyanate changes it, somewhat abruptly, to a golden yellow.

When opium is dissolved in porter (good London), the detection of the drug becomes much more difficult than when it is dissolved in strong beer; for permuriate of iron produces with porter (lightened with an equal volume of water) nearly the same brownish colour, whether it be used as delivered by the brewer, or mixed with laudanum to the extent of thirty drops in two-ounce measures. A very copious grey-coloured precipitate is thrown down from London brown stout by solution of acetate of lead—nearly as copious, in fact, as from porter drugged, as above, with tincture of opium. And when these two precipitates, washed on filters, are decomposed by a little dilute sulphuric acid, they afford two liquids, which strike nearly the same red-brown tints with permuriate of iron. It is difficult to resist the evidence thus disclosed of the presence of opium in *genuine* London porter. Tincture of hop, diffused through water, becomes, with a few drops of permuriate of iron, a greenish liquid, quite different from the diluted porter treated in the same way.

Porter becomes turbid when supersaturated with water of ammonia, and lets fall a brown sediment, which, collected and washed on a filter, bears some resemblance to impure morphia, but possesses a very remarkable peculiarity: it neither reddens with nitric acid, nor does it suffer morphia mixed with it to be thereby reddened, or at least the redness is merely momentary, and passes on the slightest heat into a light yellow shade. This precipitate I shall make the subject of future researches. Tincture of hops, which becomes slightly turbid on mixing with water, is rendered limpid by supersaturation with ammonia.

It might be imagined that bone-black (animal charcoal) would decolour porter, so that the agency of permuriate of iron on its supposed meconic acid might be made more manifest; but this process is at best fallacious; since bone-black, boiled with a portion of dilute solution of opium, deprives it almost entirely of the power of affecting permuriate of iron; while the corresponding portion receives from that salt a deep red-brown colour.

Whenever morphia can be obtained apart, its identity may be determined by decisive characters; the bright red colour imparted by it and its acetate to nitric acid, and the greenish-blue tint, to red muriate of iron.

I have not found the tincture of galls the delicate re-agent for morphia, even to $\frac{1}{15000}$ part, which Dublane, the suggester of this test, announced. It affords, with a solution of acetate of morphia, a grey precipitate, which reddens with a drop of nitric acid; but tincture of galls cannot be used where gelatine, and other animal matters, attractive of tannin, are present. Even aided by alcohol, prescribed by Dublane for dissolving out the tannate of morphia from the tannates of gelatine and albumen, it will not answer; for Vauquelin tried, in this way, two portions of urine, one which contained morphia, and the other not; and he had the same result from both—because alcohol dissolves a great deal of the animal matter precipitated by the tincture of galls, and thus complicates the experiment.

Glasgow, Dec. 19, 1829.

Memoir on the Geology of the Shore of the Severn, in the Parish of Awre, Gloucestershire.

(Communicated by the Rev. Charles Pleydell Neale Wilton, M.A., of St. John's College, Fellow of the Cambridge Philosophical Society, one of His Majesty's Assistant Chaplains in the Colony of New South Wales, and Editor of the Australian Quarterly Journal.)

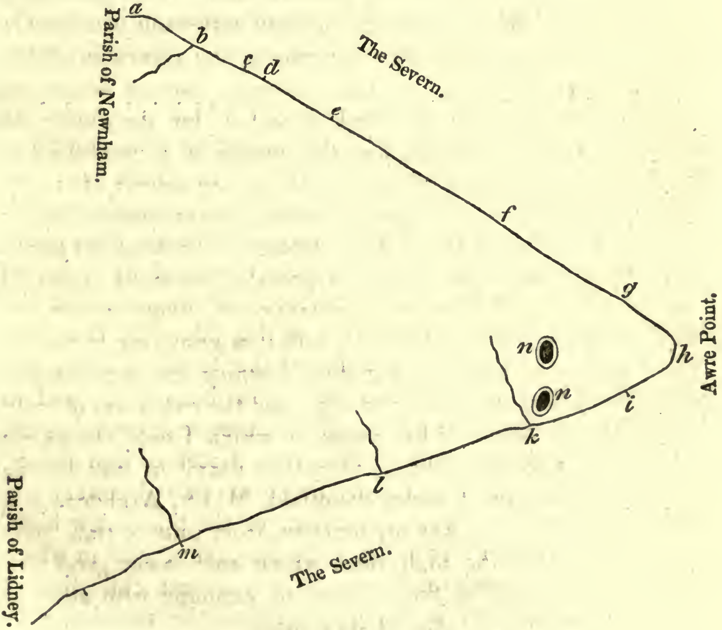
The interesting researches of individuals, of whatever description they may be, if not made generally known, will lose all their value, and must of necessity die with them. As it is by the concentration of the investigations of many, that we can hope to arrive at any degree of perfection in scientific subjects, and as that which may remain unnoticed by one man may be developed by another, so is it the duty of every one to contribute his own share to the general fund. The present is not the age in which to sit down quietly within our own homes, framing theories, and then wondering at the creatures of our imagination; but it is, on the contrary, that of active and spirited research—an age in which fact is, for the most part, substituted for hypothesis, and the results of a careful investigation for the visions of fancy. In no one subject of science does the public mind appear to take greater interest at the present day, than in the study of geology; and however parties may disagree as to the period or periods, the mode or modes, in and by which the present appearance of things on the surface of the earth has prevailed, still it is gratifying to observe them going on amicably together, keeping the same end in view, the enlargement of knowledge and the extension of truth.

During the period of five years, in which I held the curacy of Awre in Gloucestershire, when that excellent and learned man, the late Ven. Charles Sandifold, M.A.*, Archdeacon of Wells, was Vicar, it was my practice, from time to time, more particularly after the high tides which wash away great portions of the bank of the Severn, to examine with attention the geological phenomena of its shore.

In my researches, several new and interesting particulars, which form the subject of the present paper, were presented to my notice. In many instances, however, from not possessing

* Formerly Tutor of Trinity Hall. See a memoir of him in the Gentleman's Magazine for June, 1826.

the advantage of examining arranged collections, or of consulting the larger and more expensive works upon the subject, I have been under the necessity of giving only the generic names of some of the fossil bodies, and of leaving the determination of their species until a more favourable opportunity. In the annexed plate will be seen the whole extent of the shore of the Severn, in the parish of Awre, from its most extreme point north, adjoining the parish of Newnham to its extreme point west, bordering on the parish of Lidney, and in which the particular spots mentioned in this memoir are pointed out. The whole extent of the shore rather exceeds six and a half miles, and the scale in the plate is that of two inches to a mile.



a, Commencement of the Parish of Awre.
b, Box-Pile.
c to *d*, Bed of Clay.
d, Hampstells.
e, *f*, *g*, Muddy Shore.
h, The Dumble.
i, The Woodend.
k, Bream's Pile.
l, Gatcomb.
m, The Stream at Purton Passage, which divides the Parishes of Awre and Lidney.
n, *n*, Barrows.

The strata in which the organic remains, &c., are respectively found, are, in the order of their position,

1st, Alluvium.

2d, Diluvial Gravel.

3d, Blue Lias.

Upon reference to the different histories of the County of Gloucester, I find no further account of any fossil organic remains being *then* known to have been found in the parish of Awre, than that of the remains of the *pentacrinite,* in the notice of the point *k,* in the accompanying drawing, where Sir R. Atkyns informs us "pentagonal stones are found," (Atkyns's History of Glou., p. 123, fol.) Bigland, speaking of the same point on the shore, observes (p. 102), "Pentagonal stones, which, when immersed in vinegar, seem to have motion, are found on this strand." Rudder, also, in his history, (p. 248,) notices the existence of the *pentacrinite* in the same locality.

The parish of Awre, in that part of it which is bounded by the Severn, commences about 150 yards above the point *b* at *a.* From *a* to *b* the shore is muddy and the bank low, exposing about half a foot of clay beneath the vegetable mould. From *b* to *c* is a cliff of red marl, varying in height from about forty to seventy feet, in some parts traversed by veins of a greyish blue, the shore being covered with fragments of the same, washed down by the action of the tide. In the clay, which prevails from *c* to *d,* I found the teeth of deer, one about three and the other two feet below the surface; and in the interval between the places where these teeth were dug up, I discovered, at the depth of nine feet, a sort of iron shovel, much corroded, accompanied by fragments of red pottery and carbonized wood*. From the action of the tide below high-water mark, several feet beneath the stratum of clay in which the above-mentioned teeth were found, a vast collection of wood and hazel-nuts is brought to light—the remains, probably, of trees which once grew near the spot where they now lie. The incursion of the tide which first deposited them, and probably at no very remote period, is still in course of

* See Quarterly Journal of Science, &c., No. XL. p. 413.

action*; and sudden inundations, of but recent date, have been known to take place. In the old Register Book† of the parish the inundation of the Severn, in the great storm of November, 1703, is recorded by the vicar. It has been observed, that "those who perished in the waters on that occasion, in the floods of the Severn and the Thames, on the coast of Holland, and in ships blown away and never heard of afterwards, are computed to have amounted to 8000;" and "in one level in Gloucestershire, on the banks of the Severn, 15,000 sheep were drowned." Of the 9th of January, 1737, the same Register contains the following remark: "This night, about nine, a violent storm of wind arose, and, it being high water, the sea-wall was broken, and the whole level was five feet under water‡."

But to continue the description of the shore. From *d* to *e* the bank is high, consisting of clay, incumbent immediately about *d* upon the remains of branches and roots of trees, &c., and from thence to *e* upon blue lias, and the shore is generally covered with mud, although, at certain periods, when the wind blows strong upon it, the blue lias appears, presenting to the geologist specimens of several species of petrifactions hereafter to be enumerated. From the point *e* to *h* the shore is marshy and flat, containing no organic remains of any kind. At the

* About thirty-three years ago, a house of no inconsiderable antiquity was standing upon a stratum of gravel at the Woodend, the point *i* in the drawing. The bank of the Severn has since that period been gradually washed away, and to such a degree, that the spot where this house formerly stood, is now nearly the edge of low-water mark, at a little above which, remains of the well, attached to the house, may now be seen on the surface of the blue lias.

† The Register of the parish of Awre commences, 'Anno Regis Octavi XXX°. Anno Dom. 1538,' that is, one year after the dissolution of monasteries. It is altogether a curious relic. From the year 1687 to 1725, the entries of marriages, baptisms, and burials, have each annexed to them the sign of the star then predominant; and the entry of a marriage in 1736 has before it the figure of a comet —doubtless Cupid was, in this instance, more than ordinarily ardent. The ancestors of the most ancient family in this parish, and who were formerly seised of considerable lands within it, whose first representative in this country probably came over with William the Norman, have their name written in the reign of King John, as *De Awre*—thus, *William de Awre;* and in several parts of this Register, as *of Awre*—thus, *Charles of Awre*. This has been of late years, as it is at present, abbreviated to *Awre*—thus, *William Awre*, who is now residing with his family in the parish.

‡ Pepys, in his Memoirs (vol. i. p. 133), in speaking of the forest of Deane, which bounds the parish of Awre on its north-eastern side, observes, 'Feb. 17, 1661, 2.—Great talke of this late great wind. We have letters from the forest of Deane, that above 1000 oakes, and as many beeches, are blown down in one walke there.'

point *h*, which is called the *Dumble*, is a bank composed of a *subfluvial* forest, being in the thickest part from sixteen to twenty feet in depth below the wall erected to resist the incursions of the tide. At the top of this bank of roots, branches, &c., to the depth of a foot below its surface, I have found hazel-nuts in great abundance; and at the bottom of it, reposing on the blue lias, lie the trunks of large trees, retaining their shape and bark, but easily to be broken asunder, being thoroughly saturated with water.

The only remains found in the *alluvium*, near the point *k*, or *Bream's Pile*, are those of the teeth of horse, deer, ox, and dog, in a mixture of mould and clay, besides the jaw-bones and teeth, with other bones, of deer, stag, ox, and hog, discovered amidst ashes *, pottery, &c., and one tooth of deer amidst iron-slag; these teeth exactly corresponding with others found in the *subjacent diluvial gravel.*

From the point *k* to *m* are cliffs of red marl, varying from about twenty to eighty feet in height, against the base of which the tide beats at high water, covering the shore with detached fragments of the cliff. These are again oftentimes buried

* In one of my walks along the shore of the Severn, near the point *k*, I was struck with the appearance of layers of ashes on the side of the bank, which had been exposed to view by a late fall of the earth, occasioned by the action of a high tide. On digging down into the bank, from the surface, I came at once upon a sort of burying-place, in which, mixed with ashes, the bones above mentioned, and carbonized wood, were several large iron nails, much corroded, fragments of black and red pottery, and the greater part of an ancient quern, or hand-mill, in gritstone. At about the distance of two miles from this spot, between the points *i* and *k*, the bank of the river, for a considerable length, is one continuous line of ashes, cinders, and iron-slag, mixed with fragments of similar pottery. In the adjoining forest of Deane, are many undoubted remains of iron-mines, which are commonly reported to have been worked by the Romans. The antiquity of these is mentioned by Pepys, in his Memoirs, vol. i. p. 157. He is recording a discourse which he held with Commissioner Pett, "most of which," he observes, "was concerning the forest of Deane, and the timber there, and iron-works, with their great antiquity, and the vast heaps of cinders which they find, and are now of great value, being necessary for the working of iron at this day, and without which they cannot work." The spot on the river, where this bed of ashes, &c., is found, is distant about five miles from the parish of Lidney, where are to be seen, in the park of the Right Hon. C. B. Bathurst, the remains of a Roman station, where have been dug up coins of the several Roman emperors, from Augustus to Honorius. In this gentleman's extensive collection of antiquities, discovered there, are fragments of pottery and querns, exactly similar to those found at Awre. Not far from the river, in the same parish of Awre, it may be remarked, are two large barrows, marked *nn* in the drawing. Under all the circumstances, I am induced to conclude, that Awre was a Roman-British village or settlement, and that the iron-ore, which was dug from the mines, in the neighbouring forest of Deane, was conveyed to the banks of the Severn, in the parish of Awre, where it was smelted and shipped from thence to Glovernia, the ancient city of Gloucester, and to other parts of the country.

either by sand or mud, the change in deposition arising from the variation of the wind, whether it blows on or off the shore. During the very hot weather in the month of July, 1825, the salt of the waters of the Severn was precipitated to the depth of one-tenth of an inch over this part of the shore at low water, and the ledges and hollows of these cliffs were covered with a similar substance—a phenomenon which was noticed by me in the twentieth volume of Mr. Brande's *Journal of Science*, &c.

The organic remains of the *alluvium* on the shore of the Severn in the parish of Awre are but few, being, as it was before stated, with the exception of those discovered amidst the pottery, &c. at the point *k*, teeth of horse, deer, ox, and dog.

We now come to the fossils at the *gravel*, which lies immediately beneath the clay or mould. From *a* to *h* there is no stratum of gravel—from *a* to *e* the clay is found reposing on lias, burned trees, &c.; and through *e*, *f*, *g*, the shore is covered with mud and rushes. From the exponement of the subfluvial wood at *h* to the south, the gravel commences incumbent on the sand, which reposes on the blue lias, extending from *h* to *k*.

The organic remains, &c., of the gravel are as follows:—

Ammonites Birchii.
A species of Astrea.
" Caryophyllia.
" Fungia.
Belemnite.
Madrepora Porpites.
Mya.
Small rounded fragments of Oolite.
Pentacrinite—very rare, only two small specimens of the vertebræ of this animal having hitherto been found in the gravel.
Petrified wood—siliceous.
Stones, pierced by Teredo Navalis.
Serpula.
Terebratula lampas.
" dentata.
Gryphæa *arcuata (incurva* of *Sowerby)*, in great abundance.

[A respectable farmer of the village of Awre informed me that he makes use of this fossil as a medicine for oxen or cows. When from heat they are troubled with what he termed *red* or *bloody water*, he beats the gryphæa to a fine powder, with which, mixed with whey, he drenches the animal, and the complaint is immediately cured.]

Large blocks, composed of fragments of shells, mixed up together by a calcareous cement, containing specimens of *Pecten, spines of Echinus*, *Ostrea Edulis, Serpula Buccinum, Mya*, and *Pentacrinite.* These blocks are found at the bottom of the gravel, immediately reposing on the lias, and some of them have been known to weigh from 80 lbs. to 1 cwt.

Rib of Horse.
Teeth of do.

Fragments of the horn of a stag, *Elephas Cervus* of *Cuvier*, ten feet below the surface of the bank of the river, at the bottom of the gravel, imperfectly mineralized.

Jaw of Deer.
Teeth of do.
Fragments of Os Frontis of Ox.
Radius of do.
Horn of - - - - do.
Metacarpus of do.
Astragalus of do.
Teeth of do.
Teeth of Hog.

The organic remains of the *blue lias* are,

Ammonites gigantea.

I have found a specimen of this species measuring two feet eight inches in diameter*.

* In the New Monthly Magazine for July, 1827, p. 316, we read the following notice:—"As some labourers were lately taking down the vicarage-house at Awre, in this county (Gloucester), they discovered several extraneous fossils imbedded in claystone. It is very probable they came from Church Rock, in the river Severn, which is not far distant. The house, it appears, has been standing four or five centuries, and the stones exactly correspond with those now seen in the remains of the abovementioned rock, which is impregnated with numerous fossil shells of various species. A large Cornu Ammonis, more than three feet in diameter, and a beautiful specimen of the Plagiostoma Gigantea, are removed from the old vicarage walls into the collection of Mr. R. Ryder." This gentleman (Mr. Ryder) has resided in the parish of Awre for upwards of twenty-six years, at but a short distance from the Severn. During that period he has constantly visited the shore, and has made a most valuable collection of its fossils. Specimens (I believe) of all the organic remains enumerated in this memoir are in his possession; and I know that he feels peculiar pleasure in shewing his collection to any one who, like himself, takes a lively interest in geological inquiry.

Innumerable casts of the same in lias.

Ammonites Parkinsoni.
,, Bucklandi.
,, Also in Pyrites.

The fragment of an ammonite in limestone I found, having in its solid interior, when broken, other shells—*Serpula*, *Ostrea-Pectunculus*, *Pentacrinite*, and spines of Echines—a circumstance which is recorded in the eighteenth volume of Mr. Brande's Journal.

Astarte.
Avicula inequivalvis.
Belemnite.
Entrochius.
Gryphæa Aricuta, very abundant, in entire beds.
Modiola.
Nautilus Obesus.
Ostrea.
Patella.
Pentacrinite—a few capitals of this species have been found, and one of them very beautiful, and attached to the stem.
,, Another species.
Pecten.
Plagiostoma Gigantea.
covered with Ostrea.
,, transversa.
,, costata.
Spirifer.
Terebratula dentata.
Trochus Similis.
Casts of the same.
Casts of Solen.
Fragment of the spine of a fish resembling *Balistes*.
,, chelate claw of a crab.
Spines of Echinus.
Vertebræ of Ichthiosaurus.
,, Plesiosaurus.
Bovey Coal, in small pieces.

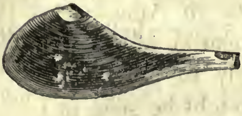
Besides the abovementioned catalogue of fossils, five of a new description have been discovered in the blue lias :—

1. A species of Alcyonium.
2. Cast of an unknown tooth.

3. A shell (see the annexed figure) which was discovered by Mr. R. Ryder, of Awre, (see note, p. 70,) and which I have, therefore, named

b

a



1 1-5th inch, the largest-sized Ryderia hitherto found.

Shewing the hinges of (*a*).

c

1-half inch, the smallest specimen hitherto found.

Ryderia. Respecting this shell, the late Mr. Parkinson, author of the "Organic Remains of a Former World, &c.," writes thus to Mr. Ryder. "It is curious, that amongst those fossils with which you favoured me, there is not a single separated valve, by which, of course, I am prevented determining the genus; but which, I believe, is new. They are very interesting indeed, from their long, and even fistular terminations." Only one single valve of this shell has hitherto been found. This was only a very minute one, and broke to pieces immediately, on being touched. The only locality of this fossil is at the point *i*, or the *Woodend.*

4. Specimens of a sort of carbonized wood, much resembling Bovey Coal; in which occurred, traversing its fractures in small lamellar pieces, a white substance, not hitherto met with in that matrix, and which, on examination by Mr. Brande, proved to be sulphate of barytes. This was found near the point *i.*

5. The petrified trunk of a tree, fourteen feet long, not far distant from the locality of No. 4, traversed by veins of sulphate of barytes, presenting a form exactly similar to that of the coal of No. 4. Upon this mineral, Mr. Brande, in writing to me, observes: "The veins in the petrified wood, of which I duly received the specimens from Mr. Powell*, are, as you very reasonably anticipate, sulphate of barytes. It is an interesting fact, that the same substance should be found traversing both the wood and coal."

From the preceding memoir, it is worthy of observation, that we find *three* periods distinctly marked out, in which the same species of animals have existed alive on the earth, and probably not far from the spot where their remains are found, *viz.*, prior to the Deluge; at the time of the formation of the alluvial deposition; and during some portion of the period

* My very good and particular friend, the Savilian Professor of Geometry at Oxford.

when the Romans held possession of Great Britain. The extermination of these from the neighbourhood of the locality of their remains is not to be wondered at, when we consider, among other instances, the total extirpation, by the chase, of the wolf, and the bear, the boar and the beaver, from England—the gradual destruction of the wild turkey in America, as the population advances, and of the kangaroo of New South Wales—an animal which is now rarely to be met with in situations where, twenty years ago, it might be seen in herds. The remains also of the Elephas Cervus—a species resembling the red deer, or stag, which have been found abundantly in peat-bogs and sand-pits in England, France, Germany, and Italy*, were, it appears, discovered on the shore of this parish, in their well-known locality, the diluvial gravel—one of the matrices, containing the fragments of the other species, as before mentioned, collected. It might be a question, perhaps, not altogether irrelevant to the subject of the present memoir, to put to the reader, whether it be more reasonable to suppose, that the animals, whose remains have there been found, and who have existed alive subsequently to the Deluge, were descended from the corresponding antediluvian animals duly preserved in the Ark, from the surrounding ruin, or that they were newly created, together with those of extinct or unknown genera and species, after the Noachic catastrophe †.

On the Decay of Timber, especially of Oak; with an Account of some rudimentary Experiments projected as a Test whereby to compute its probable Duration.

[*To the* Editor *of the* Quarterly Journal of Science.]

My dear Sir,

Many experiments have been made by architects, engineers, and others, to ascertain the comparative strength of timber used in buildings, machinery, and such like important works of art; but I know not that any have been devised, by

* See Professor Jameson's Mineralogical Notes, annexed to Cuvier's "Theory of the Earth."

† See my "Remarks on certain parts of Mr. Granville Penn's Comparative Estimate of the Mineral and Mosaical Geologies," on this subject—of a New Creation, p. 38. Rivington, 1826.

which, previous to using them, their probable durability may be determined,—a subject of at least equal, if not greater, importance; and to some rudimentary trials on this point I beg shortly to call your attention.

The experiments by which the hardness, gravity, stiffness, elasticity, toughness, tenacity, &c., of different kinds of wood, and of differently treated specimens of the same kind, may be computed, are so simple and so easily performed, that almost every work on carpentry teems therewith; hence it would be foolish here to repeat in detail such as but confirm previous observations, and increase the number, rather than add to the extent, of facts already known and published. Suffice it, then, to generalize these points in the words of Tredgold, who observes, that "the oak is universally allowed to be the best of woods;" not that it is either the hardest, the heaviest, or the toughest—in single qualities it yields to many; it is in their conjunction that its superiority consists: thus many kinds of wood are heavier, as guiacum and teak; many harder, as ebony and box; many tougher, as yew and ash; many easier to work, as fir and poplar; but none other as yet is known in which the several most important properties are combined in so great a degree, or so apportioned to be useful, as in the oak, and no oak is equal to British oak. "No nation," (said Bacon long ago, in his advice to Villiers,) "no nation doth equal England for oaken timber, wherewith to build ships."

The experience, however, of the last fifty years would lead almost to the denial of this boasted, this so long cherished superiority: when we find on record, that some of our ships have rotted on the stocks, needing repairs even before they have been launched; that others, more fortunate, have, nevertheless, in three, five, seven, or ten years, proved not seaworthy; and that, of the best modern-built ships, the average duration seldom exceeds twelve or fifteen years. Much discredit has consequently attached to oaken timber, and especially, though most injudiciously, to British oak; it having once been given in evidence by some ship-builders (but on very insufficient grounds) that it is less durable than that of foreign growth. It has also been disparagingly compared to locust and other woods, which have been found, in some cases, to outlast oak as posts and spurs, not considering the other qualities in which oak is so decidedly superior; and that the brittleness of locust (which is yet a very valuable wood) renders it useless for many of those more important purposes to which oak is peculiarly adapted and applied. Nichols, who was formerly surveyor of the royal woods, speaks decidedly on this point: he says, "oak is the only timber of any consequence made use of for building ships for the navy, or, I believe, ever will with good effect."

It is to the durability of oak, however, that I propose chiefly to restrict my present observations; for when I find, as I do, from specimens in my own possession, that oaken timber built into Prince John's palace at Eltham; into Windsor Castle, of the time of Edward III.; from the Spanish Armada, wrecked in 1588; from Greensted church, built A.D. 1010, are all good, strong, firm, and sound, after being in their variety of situations several centuries, I cannot but contend that oak, in conjunction with its other qualities, is, when properly selected and applied, a most enduring wood. Eltham Palace and Windsor Castle give evidence of its durability in ordinarily dry situations for upwards of five hundred years at least; the wreck of the Armada, '*croped*' up a few years since in Tobermoray Bay, had been submerged between two and three hundred years; and the rough oaken walls of Greensted church have been exposed to heat and cold, wet and dry, for about eight centuries and a quarter, and are still so strong and sound as to defy all calculation as to the ages they yet may probably endure.

I have said that the sensible qualities of oak have been long made the subject of inquiry; and what, to ship-builders, millwrights, &c., is of equal, if not greater import, its power of endurance, especially its endurance when subjected to the influence of weather, to the frequent changes from wet to dry, and from heat to cold, to which ships and mill-work, and such like most important structures, are constantly and inevitably exposed, has occasionally, though with much less success, forced itself upon their attention. The extreme variability in the duration of wood thus subjected to atmospheric changes is so notorious, that certain kinds have long been almost exclusively appropriated to certain purposes, and the oak has been preeminently famed for endurance between wind and water, where most other woods, however tough, or stiff, or heavy, they may be, rapidly decay. But this general principle has been too *universally*, far too *empirically* pursued, and oaken timber has been contracted for merely *as oak*, and used merely because it is *called oak*, in docks, wharfs, ships, campshoding, &c. &c., without due reference being had to the *kind of oak*, its *maturity*, seasoning, &c.; and hence the astonishing facts that some ships have lasted upwards of a century, while others have been worthless even before they have been fully built; that dock and flood-gates, posts, &c. &c., are often found to be rotten and useless in ten or fifteen years, while others last sound and strong for ages. These matters have not unfrequently excited the attention of the public, and have occasionally been discussed in the highest assemblies of the state.

Many causes have been assigned for this premature decay (for premature it must surely be esteemed); as, when we know that vessels, built in the sixteenth, seventeenth, and early part of the eighteenth century, lasted from fifty to a hundred years, no absolute necessity can exist for those of the nineteenth being often utterly worthless in a fifth—aye, in less than a twentieth part of the forementioned time.

From the facts elicited by the investigations which have hence been instituted, the most probable causes of decay, in our modern oaken structures, may fairly be attributed to the use of *immature, ill-chosen*, and *worse-seasoned* wood. The inordinate supply which, during the last war, was demanded by our dockyards, led to the introduction not only of inferior foreign timber, but also to the premature and almost indiscriminate application of our own; whence resulted that lamentable decay, [destruction?] to cure and to account for which so much has been written, said, and done. Essays have been published upon the felling, the seasoning, and the choice of wood; treatises have appeared on rot, *i. e.*, upon ordinary decay; and volumes have abounded on that appalling scourge, misnamed the dry-rot: the which, if they have not greatly explicated the problem, have at least gone far to shew that much, very much, may be written on a subject, about which little, very little, is truly known.

Perhaps the authority of Linnæus, who arranged all the British oaks as but varieties of one and the same species, which he called *Quercus Robur*, not a little favoured the indiscriminate application of their timber for naval purposes, as we find that, in the charter of the Shipwrights' Company, *red* wood was proscribed as inadmissible to the dockyards, and, along with sappy timber, was ordered to be removed; and both "strictly prohibited and restrained from being used in or upon any ship or other vessel." Martyn, however, as Ray and Bauhin had done long before him, emphatically insisted upon the distinction being made between our sessile-fruited and stalk-fruited oaks, the latter of which gives a lighter and more lasting wood, when exposed to atmospheric influences, than the former; and the Durmast or Downy oak would seem to yield a darker and less enduring timber than either of the others.—(Vide Flora Rustica, Botanical Diversions, &c.)

These important distinctions, now generally established, were formerly either little known or less attended to; for Nichols, in his book, observes, "The plantations made in the New Forest, about the year 1700, were of the Durmast oak, the timber of which is not so durable as the true English kind. And we are told in a late Number of the *Quarterly Review*, which, nevertheless, confounds the Q. sessiliflora with the Q. pubescens, that the wrong oak still "abounds, and is propagated vigorously in the New Forest, and other parts of Hampshire, in Norfolk, and the northern counties, and about London." This picture seems to me far too highly coloured: certainly in the neighbourhood of London, and within twenty or five-and-twenty miles thereof, it is comparatively rare to meet with either the sessile-fruited or the downy oak. Since the botanical characters have been popularly familiar, the preference has most properly been given to the cultivation of the stalk-fruited oak (Q. pedunculata;) but as I have elsewhere observed (vide "Amœnitates Querneæ,") the timber of the sessiliflora is far from worthless, it being both tougher and stronger than the pedunculata, though less enduring and elastic; and the pubescens affords, for many purposes, a valuable wood.

I cannot forbear correcting a botanical error which seems to have lately become prevalent, for it has been repeated in several influential publications of the day, namely, that the *Q. sessiliflora* is not a native species. It is probably true, that the *downy* oak is not indigenous to Britain; but even this is doubtful. Miller tells us, that it was brought to this country by the Duke of Richmond; and hence the mistake has probably arisen, as many botanists believe Q. pubescens only a variety of Q. sessiliflora; which, however, in its ordinary type, would appear to have been always the prevailing oak of our northern counties, as the pedunculata is of the south and eastern shires. Miller, at one time, considered it to be the "common oak of England," but this could only have arisen from some very partial survey, though less so than that which induced M. Fougeroux to describe the pubescens as our common native species. The sessiliflora being, as I am informed, the common oak of our northern provinces, as the pedunculata is of the southern, may not improbably justify, or at least explain, the prejudice which has so long existed against north-country-built ships, as less durable than those constructed in our southern ports.

Much confusion hath existed, and doth still exist, as to the names of our native oaks. All three were considered varieties of his Q. Robur by Linnæus. When subsequently distinguished, the term *Robur* has been considered synonymous with *pedunculata* by some botanists, and, by others, with both *sessiliflora* and *pubescens*. Wildenow, Aiton, and most continental authors, call the sessile-fruited oak *Q. Robur;* Smith, Salisbury, and most English writers, give the title *Robur* to the stalk-fruited kind. I am persuaded, that it belongs more properly to the Downy oak, *Q. pubescens,* than to either of the others; that is, the qualities of the pubescent wood agree more closely with those ascribed by ancient authors to the timber of their *Robur,* than does that of either of the former species. It will be remembered, that the terms *Q. Robur*, *Q. Suber*, *Q. Ægilops*, &c. &c., were unknown to the ancients; that *Quercus* indicated one tree, *Robur* another, *Suber* another, and so on. The assembling the whole under the common generic term *Quercus,* with the adjunction of specific names, has been a modern invention; and although Vitruvius says the *Robur* is less liable to warp than the *Quercus,* Pliny declares, "as for the *oke Robur,* it will corrupt and rot in the sea."—"*Robur* marinâ aqua corrumpitur." He also observes, "*Robur* exalburnatum;" and Martyn states, that the Durmast forms much less alburnum, in comparison to its heart-wood, than the other oaks. Again, Festus Pompeius writes, "Materiam quæ plurimas venas *rufi* colori habet *Robur* dictam;" and thus the colour of the timber of the Durmast oak, called by the French, from its darkness, *chêne noir,* will rather accord with the name *Robur, à Robus*, the obsolete form of rubeus, red, than does the lighter, hard timber of *pedunculata*, which, although more durable, is not, as shewn by Tredgold, so strong as (*i.e.* it breaks with a less weight than) the *sessiliflora*. Moreover, Pliny decides the question, by recording, of the ancient *Robur*, "Fluviisque demersum nulla marcoris tabe conficitur, sed in *mare putret*," which can never apply to our British naval oak.

"It is readily confessed, that pedunculata is a very unsatisfactory name, very indeterminate, as many other oaks have peduncled acorns, and very trifling for so important a tree." Hence is it, that as one oak has several names, and one name signifies several oaks, I have proposed, in my "Amœnitates Querneæ," to avoid the confusion in which this nomenclature seems involved (and which is even increased by the assertion lately made, that the Quercus pedunculata of the Continent is different from our native pedunculated oak, although both have similar names), by calling the British naval or pedunculated oak, Q. navalis, or ship-oak—the free-growing and more stately oak, with sessile acorns and smooth leaves, Q. Regalis, or royal oak; and the slower-growing oak, with downy leaves, which holds its foliage much later in the season, and yields the reddest and darkest wood, Q. Robur. The other oaks, sometimes named as species and sometimes as varieties, I am inclined to believe the latter, perhaps mule plants, formed by the mutual impregnation of the three before-named species.

The evidence already within our reach, although not wholly unexceptionable, would seem to prove, that much of the variability in the duration of oaken timber, formerly attributed to the nature of the soil on which it grew, and to certain diseases affecting trees, is in a great measure referrible to the *kind* of oak, and the purpose to which the wood of the several species may be applied. The timber of the Durmast oak, the darkest in hue and most curiously veined, seems well fitted for furniture and ornamental purposes; the sessiliflora, being both tougher and stronger than the pedunculata, and, as well as the pubescens, very durable, if not exposed to weather, is best adapted for domestic architecture; its more rapid growth and very elegant form will also fit it for shrubberies and home plantations; while the endurance of the pedunculata between wind and water, where the others are most perishable, will ever establish its pre-eminence as the naval oak; for when thus exposed to atmospheric influence, as Nichols writes in the work already quoted, "the true English wood, for firmness, strength, and durability, is preferable to any other for ship-building, and is well known all over the world."

As the botanical diagnosis, however, serves only when we can procure either fruit or leaf, and as soil, disease, or too rapid growth, may probably, I think frequently, deteriorate the timber even of the true naval oak; as cupidity may fell trees that are immature, and mistake or fraud purvey, as true naval oak, timber of the other species or of foreign growth, a test by which the prospective durability of wood designed for important uses, as for ship-building, &c., may be computed by experiments on samples, cannot be esteemed a useless application of science to the common purposes of life; and I doubt not your willingness to devote a few pages of the *Journal of Science* to the present observations, which shall conclude with a notice of some rudimentary trials which promise fairly to achieve this end, and which, although it would be wrong to generalize on so comparatively few experiments, I am induced to publish in the present form, that my views may be corrected or verified sooner, and on a larger scale, than they could be by individual means; and that persons, having opportunities to procure specimens of timber from different countries, from different soils in this island, and of different kinds, ages, &c. &c. &c., even if they do not wish to be at the trouble of examination themselves, may forward them for me to the Royal Institution, that I may test their relative value.

It may not be improper to state, for the information of those unversed in physiological enquiries, that wood, even of the most solid, firm, and hardest kinds, is entirely composed, as to its intimate texture, of tubes and cells, differing in size, form, and situation, in different tribes of plants, and more or less replete with matter of very different sorts, upon which its chief strength and substance, and all its most important characters as timber principally depend. In the light and porous woods of many quickly-growing trees, as the poplar, the ligneous matter deposited one season, is, in great part, reabsorbed to support the rapid increase of the next; but in the heavy, firm, and solid wood of more slowly-growing trees, for example, take the oak, so far from living on the principal, as does the spendthrift poplar, although some change takes place in the heart-wood for many years, the cells are not left void, but more and more substance is deposited in these ligneous receptacles, by which the alburnum becomes converted into duramen, and the duramen becomes gradually matured; for, although vegetable anatomists in general distinguish only two kinds of wood, the sap and the heart, or spine, in large trees there are many gradations of each; and although the vital functions are more energetic in the one, the other is not devoid of activity, and ought not to be considered dead. When life is totally extinct in the centre of a tree, chemical changes, more or less rapidly, ensue; the duramen is unable to resist external influences, and it absorbs extraneous and often hurtful substances, which the living parts eschew. You may remember shewing me a specimen of a beech-tree, some of the heart-wood of which was dead, and which had become stained by water draining through forge ashes, that had been cast in a heap near its trunk, but the alburnum and the outer live rings of the duramen had resisted that external impression, to which the whole were equally exposed, but which the lifeless portion had no power to avoid.

Much too little stress has hitherto been laid upon the premature cutting of timber; we hear, indeed, the practice reprobated, of falling fourscore young oaks, when their aggregate contents were considerably less than what is often found in a single tree; but there, the objection seems rather to have been made to the quantity than the quality of the wood. As great an error occasionally, though not so frequently, occurs, of allowing trees, designed for timber, to stand too long. This has also been, and very properly, objected to; but here the objection has been made to the chances of injury that the tree may get, the diminution of timber by the loss of limbs, or the hollowing of the trunk: but, besides these, another most important objection would seem to me to consist in allowing the heart-wood to become too far indurated, by which that balance of qualities, which constitute the great superiority of good English oak, is considerably interfered with, and if any decay occur, the durability must also be correspondingly impaired.

Ordinary decay in timber, and, indeed, in most vegetable substances, consists in a change occurring in the contents of their intimate cellular structures, by which the matter therein contained is either dissolved and carried away, leaving the cells more or less empty; or decomposition, and the formation of new chemical combinations, is favoured, by which the quality of the wood, or whatever it may be, becomes essentially altered. By maceration in different menstrua, the matter deposited may be dissolved, and the cellular structures thus exhibited afford a number of very beautiful anatomical preparations: and thus it happens in natural as well as artificial processes, that vegetable substances kept perfectly dry or immersed in menstrua, in which their peculiar matters are insoluble, or which, instead of favouring, check their proximate principles from undergoing decomposition, and producing new compounds, will last unhurt, or but little changed, for ages; but if the matter be soluble in the menstruum applied, or the reverse of the former circumstances occur, then solution and decomposition, that is, decay will, more or less, rapidly ensue.

Timber exposed to atmospheric changes is subject, more or less, to all these influences, and those woods, the ligneous matter of which is the most soluble in water, will, *cæteris paribus*, the most speedily decay; but it often happens, that the decomposition (as in fossil timber) produces a matter less corruptible than the original, at least on the outer surface, and thus defends the internal parts—sometimes the whole becomes thus changed. More frequently, however, the decompositions that take place, generate various gases, *e. g.*, carbonic acid carburetted hydrogen, &c., &c., in abundance, the elasticity of which cannot fail to rupture the delicate tissues of which the cells are formed; and these fissures, minute and almost inappreciable as they may be thought, in fact are potential capillary tubes: moisture is again applied, is again absorbed, and by these means pervades the intimate structures, even more readily and more extensively than before. By sea-water, salts are also carried in, which often crystallize; or during cold weather the water freezes, and either of these processes will sufficiently account for many of those cracks and fissures, which do not occur from violent exsiccation. The solution and deposit is sometimes so complete and general, as to transform a block of wood into stone, as may be seen in almost every museum; and the fissures just noted, which are always occurring in their slighter forms, in very severe winters such as that just passed, produce much more notable effects. In this country, as well as in the south of France, many large and hollow trees, especially cork-trees, have been split, and their trunks rent in pieces, by the congelation of water contained within them, and this has taken place with so sudden and so great a force, that the noise produced has been said to have been heard at a considerable distance, resembling the discharge of a musket.

A just estimate of these various circumstances will tend to explain the successive stages of decay, and to elucidate some phenomena apparently paradoxical; *e. g.*, that a light soft wood, as cedar, should be more lasting than harder, heavier, and more solid timber, than elm, many kinds of oak and beech; that fir, when exposed to weather, should so rapidly decay, although the resin with which it abounds is insoluble in water; that some woods are almost exempt from the attacks of worms, while others are so peculiarly prone thereto; and that immature and ill-seasoned timber becomes speedily infested by dry-rot; which, in fact, is the decomposition of the ligneous material, favoured by *heat*, *moisture*, and *confined air:* in which circumstances, wood, the matter of which is but half elaborated, assimilated, or matured, either from too early, or wrong seasoned felling, almost inevitably is found to perish.

Such being the philosophy of decay, the practical application of these principles is all that now remains, and it is so obvious that it need not detain us long. To test the probable durability of any untried timber, or any doubtful specimens of a known kind, which was the object mooted at the commencement, shall form the conclusion of the present essay; and the means by which wood may be subjected in rapid succession, and during a short period, to similar or equivalent influences to those which occasion its decomposition when exposed to the atmosphere for ages, are those experiments the results of which have suggested the foregoing observations.

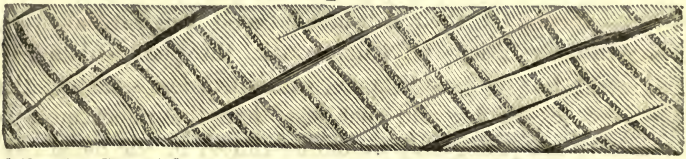
The strength, elasticity, stiffness, tenacity, toughness, &c., being ascertained in the usual way by taking rods of the wood to be examined, of 2 inches square, and 3 feet long, with 24 or 30 inches between the fulcral points, and suspending weights to the centre of each rod, noting in what time, in what degree, and by what weight each bends and breaks: the probable durability should be tested by taking equal portions of each rod, and steeping them for equal periods, both in hot and in cold water, then drying them and steeping them again for eight or ten times, once a day, and the quantity of extract contained in the respective waters will be a tolerable criterion of the relative solubility of ligneous material possessed by each.

Dutch wainscot, American clap-board, French and Norway oak tinge the water deeply, and all part with a considerable proportion of their substance; good English oak yields comparatively little. Of English oak, the pedunculata parts with much less of its substance, and much less speedily, than does the sessiliflora, and that less than the pubescens; yet specimens of young and immature timber of the naval oak I have found to be little better than the inferior foreign plank.

I purposely avoid indulging, at the present stage of the inquiry, in any prolix details of the various experiments, to a statement of the general results of which I have endeavoured to confine myself: however, to take a few examples, I find, from notes made by Mr. Waterworth, who assisted in the manipulations, the average solubility of the ligneous material, in six specimens of oak submitted to experiment, to have been as follows:—1·; 2·; 2·5; 3·; 4·; 5·; to 5·25; *i. e.* when equal weights, say 450 parts, of mature, well-seasoned British naval oak; of the same, ill-seasoned; of the same, immature; of pubescent oak, of choice Norway oak, and of right Dutch wainscot, were steeped in equal quantities of water, say 1800 parts, at the same temperatures, and for the same periods, the relative losses, as ascertained by the increased densities of the several infusions, and the weight of the extracts obtained by evaporating equal quantities of each, averaged, of the Norway, double that of the good British naval oak; of the Dutch, four times as much; and of the pubescent, five to six times as great: the loss of the immature and ill-seasoned British naval oaks, as compared with mature and well-seasoned specimens, averaged two and a half and three times as much: therefore, if the loss of the good naval oak be taken as 1·, that of the Norway will be 2·; of the ill-seasoned, 2·5; the immature, 3·; the Dutch 4·; and the immature pubescent, 5· to 5·25.

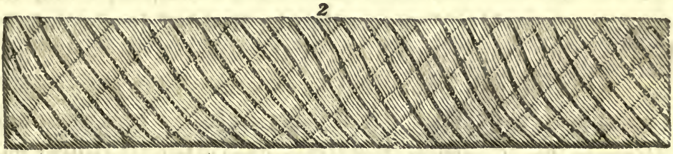
Subsequently to repeated infusions in water of different temperatures, the specimens still wet should be put out of doors during a sharp frost, and alternately frozen and thawed ten or twelve times, by which their relative powers of resisting variations of temperature will be tested; as by the former experiments it is proposed to ascertain their capacities for enduring the change from humidity to drought. In the trials which I had made this last winter, the specimens of Norway and French oak, though both good of their kind, after being frozen eight times exhibited the appearance shewn in the accompanying sketch:

1



while the English, submitted to similar ordeals for the same periods, and at the same times, withstood the effects both of drought and moisture, heat and cold, with but little change, I might almost say totally unhurt: of this the second figure may serve as an example.

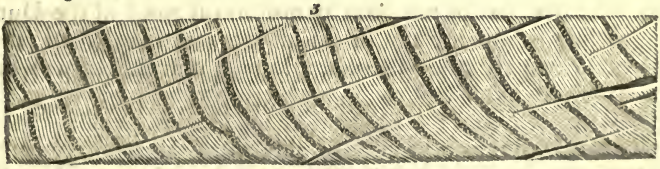
2



(The medullary rays, which pass downward from right to left, are, however, in the wood-cut, certainly too strongly marked.)

Other similar specimens were also steeped in a saturated solution of Glauber's salt, and then the salt allowed to crystallize, which process, alternated with the dissolving of the crystallized salt, was repeated several times with an effect similar to, though less powerful than, the frost; fig. 3. has been drawn from a specimen thus treated.

3



It would be wrong, as I have before observed, to generalize too boldly from the few trials already made, which, however, have been sufficiently numerous and satisfactory in their results to encourage the belief that the experiments detailed promise fairly to become useful as a test of the probable durability of timber when exposed to atmospheric influences; and hence I forward them to the Journal of Science in their present rudimentary state, both in order that they may be submitted to more speedy and extensive proofs than individual opportunities can offer, as well as with the hope that persons who may meet with samples of oak, and yet not wish to be at the trouble of the experiments, will send them to me for examination.

Believe me to remain,

Yours, ever truly,

22, *Great Marylebone-street.* GILBERT T. BURNETT.

Microscopic Illustrations of a few new popular and diverting Living Objects, with their Natural History, &c., conjoined with accurate Descriptions of the latest Improvements in the new Microscopes, the best Methods of constructing their Mountings, Apparatus, &c., and complete Instructions for using them, illustrated by highly finished coloured Engravings from magnified Drawings of the actual living Subjects. By C. R. GORING, M.D., *and* ANDREW PRITCHARD. *Legent hæc nostra nepotes.* London, Whittaker and Co. Royal 8vo. boards.

MICROSCOPES having of late undergone a complete revolution in their construction, it is, therefore, natural to expect new works upon them: the present is one of such a class, of which it forms a first part;—it may be it is written somewhat in advance of the spirit of the present age; rather for posterity than for the present generation: for the new microscopes are, as yet, only in the possession of scientific persons, and have not found their way to the public at large; nevertheless, we conceive that the present publication cannot fail, in its own time, to recommend itself generally, for it is associated with elaborate descriptions and beautiful coloured engravings of a variety of diverting and popular objects, many of which are the same, if we mistake not, which have already conferred much high gratification on the public, in the exhibition of the achromatic solar microscope.

The exordium or preface of this work is strongly tinged with that admiration of the Supreme Intelligence which the contemplation of the microscopic world so naturally inspires; and contains some arguments which appear to us to be original. Chapters II. III. and VII. are devoted to the description of the larva and pupa of a beautiful species of gnat, the larva and chrysalis of the ephemera marginata, and the larva of a British hydrophilus, or water-devil.

The rest of the work is occupied with a dissertation on the best possible ways of constructing the mechanical parts and apparatus of microscopes,—a description of the operative, aplanatic engiscope, (achromatic, compound microscope,) and the most improved methods of observing with it, &c. Dr. Goring wishes to introduce a more accurate definition of the names of microscopes, (p. 48) by terming all those instruments which operate by means of a magnified *image*, *engiscopes*, and the rest simple, or compound microscopes, according to their nature. We wish him much success in this hopeful scheme, but think he should have learnt to spell this said word engiscope, right; we beg to acquaint him, that the Greek *upsilon* is always rendered in English by a *y*, and that the correct orthography of the word is *engyscope*.

With respect to the operatic, aplanatic engiscope, we shall observe, that it is, of course, one of the *new* microscopes; yet, we must confess, that, with a few immaterial exceptions, we see very little new about it; at least in its mechanical structure and apparatus, we are confident that there is nothing in it which has not already been executed, in detail at least; it would appear that he has merely selected, from a variety of constructions, both English and foreign, those parts which he conceives to be most valuable and effective and combined them together; and, perhaps, in this case, he has acted more wisely than if he had, by striving after originality, produced a construction much more different than this from those of our best makers.

The instructions for managing it (and it includes the simple microscopes, and Amician reflector) are, however, certainly both novel and elaborate, and written with great perspicuity. No one could have delivered them, who had not been long and intimately acquainted with the best methods of observing with microscopes; and whatever may be the description of the instrument which a microscopist may use, he cannot, we think, fail to derive both pleasure and advantage from reading them. We shall conclude by selecting two passages, which we think are a fair example of the quaint but forcible style in which the book is written.

"Now, courteous disciple, I shall endeavour to instruct thee how to manage thy tackle, and will, moreover, have the extreme complaisance to suppose thee (in all microscopic matters, at least) one of the *awkward squad*, as stupid as an owl, and as ignorant as a cart-horse. I will tell thee as well as I can, all that thou art *to do*, and all that thou art *not to do*. I will try to make thee know the right end of thy instrument from the wrong one; how to put a fly's eye before the object-glass, and a fool's eye before the eye-piece; with many other things equally curious, important, and interesting; and if perchance, I shall succeed in teaching thee how to deal with the instrument under consideration, together with the Amician reflector, the management of *all others* thou canst meet with will be as easy to thee, as the guidance of a cock-boat to the seaman who can work a line-of-battle ship."—p. 61.

"Courteous reader, I have endeavoured to supply the place of a *viva voce* lecture on the instrument, and to infuse into thee such knowledge as I possess, touching the management of the aplanatic engiscope, &c. I hope it will be found sufficient for ordinary objects; but under the head of *Test Objects*, I shall enter into the subject still more minutely, and give still more precise and specific instructions, as the case may seem to require. I cannot help thinking, however, that the directions already given will be found much more explicit, clear, and intelligible, as well as more full, particular, and diffuse, than any others hitherto given in print. *Valeant quantum valere possunt.* By their assistance, thou shalt be enabled to enter into a course of researches, *very nearly* as profitable to thyself and fellow-creatures, as if thou wert engaged in the sublime and important occupation of determining whether the small star of ε Bootis is of a greenish-blue, or bluish-green, or whether some nebula is very gradually, or very suddenly, much brighter in the middle."—p. 90.

On Thorina. By Professor Berzelius.

[Continued from p. 302 of last Volume..]

2. EXAMINATION OF THORINA AND ITS METALLIC RADICAL.

Thorina is reduced neither by carbon nor by potassium; but thorium can be *isolated* either when the double fluoride of horium and potassium, or when anhydrous chloride of thorium s mixed with potassium and heated. The latter mode succeeds best, and gives the purest thorium. Chloride of thorium is prepared from thorina, by mixing with carbon, and heating to redness in a stream of chlorine gas. The decomposition of the chloride of thorium takes place with a very slight detonation, accompanied, if the chloride of thorium be altogether free from water, only by the development of heat without light, and it can be safely enough performed in glass. Even the fluoride produces with potassium a very slight detonation.

To satisfy myself that thorina is not reduced by potassium, I mixed anhydrous sulphate of thorina with potassium in slight excess, and heated the mixture in a covered porcelain crucible. The decomposition took place, with an exceedingly violent detonation, which heated the crucible from within to whiteness; and the excess of potassium was driven between the crucible and the lid, and there burned with a very bright flame. After cooling, water separated sulphuret of potassium, and left a snow-white earth.

When chloride of thorium is detonated with potassium, there is obtained a dark-gray mass, which at first, as is usual in such reductions, gives out hydrogen gas. This, however, soon ceases, and a gray, heavy metallic powder remains. This powder is dark-lead gray; after drying, can be pressed together into a compact mass, and, when pressed with a polished agate, becomes of an iron-gray colour, takes the metallic lustre, and seems to have the same degree of *metallicity* as aluminum. It is not oxidized by water, either warm or cold; but, when gently heated in the air, it kindles and burns with extraordinary lustre. It changes at once into a body of fire, which can be likened to nothing more than to the phenomenon which takes place when a bubble of oxygen gas is introduced over mercury to melted phosphorus. It is accompanied with a strong evolution of light; so that the burning mass appears like a single bright flame. Small particles of thorium, in the flame of a spirit-lamp, burn with a white light; and in the moment of illumination seem to swell out to many times their former volume. The thorina, which remains after burning, is snow-white, and has not the slightest appearance of fusion or of cohesion among its parts.

If thorium be treated with dilute sulphuric acid, there arises a slight action with evolution of hydrogen gas, which speedily ceases, and the mixture can afterwards be warmed without the thorium being sensibly dissolved. By digestion, in this way, with dilute sulphuric acid, a mixture of thorium and thorina may be separated, and the thorium obtained pure. By long-continued action, however, the thorium is diminished, and at length entirely dissolved. The action of nitric acid upon thorium is still less than that of sulphuric acid. Thorium may be boiled in it, without the solution proceeding much more rapidly. On the contrary, thorium dissolves with great ease in muriatic acid, especially when aided by heat; and the solution is accompanied by the evolution of hydrogen gas. Hydrofluoric acid acts upon it as slightly as sulphuric acid: caustic alkalies, in the moist way, do not act upon thorium.

2. *Thorina*, which is formed by the union of the metal with oxygen, and seems to be its only state of oxidation, has the following properties:—It is colourless; heavy; soluble only in concentrated sulphuric acid, aided by a high temperature.

Preparation of thorina from thorite.—The mineral is dissolved in sulphuric acid, in the way stated above in the analysis. The solution is treated with sulphuretted hydrogen, and the earth precipitated by ammonia. The precipitate, collected on the filter and well washed, is dissolved in dilute sulphuric acid, and the solution evaporated by heat. By this means, there is formed a bulky sulphate, which is separated before the liquid is completely evaporated, washed with boiling water, pressed between folds of paper, dried, and heated to redness: what remains is pure thorina.

The mother liquor and the washing still contain thorina; the excess of acid is saturated, as nearly as possible, with caustic ammonia: oxalic acid is then added, so long as any precipitate falls; and the precipitate, washed with water, slightly acidulated with oxalic acid. By this process, the manganese, iron and uranium remain in solution, and the oxalate of thorina is obtained on the filter. This gives, after burning, an earth tinged with yellow, arising from a small taint of peroxide of manganese, which attaches itself more strongly than other substances to this earth.

Thorina may also be precipitated from this solution by adding dry crystallized sulphate of potash, as long as any is dissolved. The thorina falls in the form of a double salt; and by this process it is more completely thrown down than by oxalic acid.

To obtain the hydrate of thorina, the sulphate of thorina, after washing with boiling, is dissolved in cold water. This salt dissolves very slowly, but at length completely: the solution is precipitated by caustic potash, and washed upon the filter; the precipitate is gelatinous, like the hydrate of alumina, but falls easily; during washing and drying it absorbs carbonic acid from the air; dried in the open air, it forms itself into glassy lumps; in a vacuum over sulphuric acid it is obtained in the form of a white powder; it loses its water by a low red-heat. The moist hydrate dissolves easily in acids; when dry, it dissolves with great difficulty and after long digestion; but, after heating to redness, it becomes altogether insoluble in nitric and muriatic acids.

The hydrate of thorina is insoluble in caustic alkalies. On the other hand, both the hydrate, the carbonate, and the *subsalts* dissolve with ease in the carbonated alkalies, and in carbonate of ammonia. Very little is dissolved by the alkali, if the solution is much diluted; but they dissolve abundantly and with ease if the solution be concentrated. If a solution of thorina, in carbonate of ammonia, be put into a flask and heated to + 50 (Celsius = 122 Fahrenheit) or thereabout, the liquid becomes turbid, and much thorina is thrown down, which is again completely taken up when the solution is allowed to cool. The addition of caustic ammonia does not precipitate the thorina; and if, previous to the addition of ammonia, the liquor were muddy, from the commencement of precipitation, the ammonia restores its transparency.

If thorina be heated to redness with a caustic or carbonated alkali, it does not fuse with it, but becomes insoluble in muriatic and nitric acids, which take up only the foreign matters with which it may be contaminated, and which, previous to this heating with alkali, could not be removed by acids. It separates in the form of a white milk-like mass, which, in washing, passes through the filter, like titanic acid; an effect, however, which may be prevented by mixing muriatic acid or sal ammoniac with the washing water.

Thorina, by heating to redness, becomes hard, and is afterwards difficult to reduce to fine powder. Its specific gravity is greater than that of any other earth, and approaches that of the oxide of lead: I found it 9.402. The specific gravity of the mineral thorite is therefore considerably less than might be expected from that of the pure earth.

Before the blowpipe, it remains unchanged; with borax it melts with very great difficulty, and the clear glass does not lose its transparency by *flaming**; but it may be saturated so much as to become milky on cooling. In phosphoric salt, it dissolves with great difficulty, and with carbonate of soda it does not fuse.

The saturating power of thorina I have endeavoured to determine by the analysis of its combination with sulphuric acid. The sulphate, precipitated by boiling, was afterwards dissolved in cold water, and the solution precipitated by pure caustic potash in slight excess. The earth, fully washed and

* An intermitting application of the flame.

heated to redness, weighed 0.6754 grammes. The alkaline liquid which passed through, being saturated with muriatic acid, and precipitated by chloride of barium, gave 1.159 grammes sulphate of barytes. In another experiment I obtained the proportions—

1.0515 thorina,
1.832 sulphate of barytes.

To determine the number of atoms of oxygen it contains, I analyzed the double sulphate of thorina and potash: 0.801 grammes of the salt in crystals were dried on the sandbath; they became opaque milk-white, and lost 0.0365 grammes of water. This loss was not increased by a heat which melts tin. The remaining 0.7645 grammes were dissolved in warm water, precipitated by caustic ammonia, and heated to redness. The earth obtained, weighed 0.265 grammes: the solution which was left gave, by the usual treatment, 0.3435 grammes sulphate of potash; so that the sulphuric acid, united to the earth, was 0.156, or the same as is contained in the sulphate of potash. This analysis for the determination of the atomic weight affords two grounds of calculation—namely, from the sulphuric acid and from the sulphate of potash. Calculated from the former, the atom becomes 851.3, from the latter 841.73: the formerly mentioned analysis of the sulphate gives—the first, 849.664, and the second, 836.86; the mean of all four$=$844.9, is probably nearest the truth.

As alumina and the oxide of iron give, with sulphuric acid, salts, in which the oxygen of the acid is only twice as great as that in the base, and as these salts unite with sulphate of potash in such a proportion that the quantity of sulphuric acid in each of the component salts is equal, *the same most likely holds, also, in the case of thorina**—a circumstance here so much the more probable, as the sulphate of thorina, thrown down by boiling, seems necessarily to be a basic (sub) salt. In this case, the earth contains three atoms of oxygen, and one half more per cent. than is denoted by the preceding analysis. I therefore analyzed the salt, which crystallizes by a spontaneous evaporation from an acid solution of sulphate of thorina, but I found the base and the acid to be here in the same ratio, the water of crystallization only being very different. I digested

* I put these in italics, because they are not in the original, though they seem necessary to complete the sense.

then, with sulphuric acid, a determined weight of the sulpho-salt, thrown down by boiling, and afterwards evaporated over a lamp till fumes ceased to be given off. In the greater number of experiments, fumes ceased to be given off at a point which denoted an increase of one half in the quantity of acid contained by the salt; but this quantity was by no means precise: sometimes a little more was obtained, sometimes less; but, in the last case, a portion of the salt did not immediately dissolve in water. In every case, I found an anhydrous salt formed by the presence of more sulphuric acid.

To get out of this labyrinth, I prepared and analyzed a portion of anhydrous chloride of thorium. This analysis gave for the atomic weight of thorina, .838. I regard this number, however, as less worthy of credit than the formerly obtained mean, since in this case the earth was coloured by some foreign substance, probably iron.

If we take the mean of the results obtained from the sulpho-salts, as nearest the true atomic weight, then thorina in 100 parts will consist of

$$\left.\begin{array}{ll}\text{Thorium} & 88.16 \\ \text{Oxygen} & 11.84\end{array}\right\} 100$$

And the hydrate of thorina will consist of

$$\left.\begin{array}{ll}\text{Thorina} & 88.25 \\ \text{Water} & 11.75\end{array}\right\} 100$$

The symbol for an atom of thorium $= 744.9$ may be Th, for thorina $\dot{\text{T}}\text{h}$, and for its hydrate $\dot{\text{T}}\text{h}\,\dot{\text{H}}$.

Thorina is distinguished generally from the other earths by its forming with sulphuric acid a compound which, by boiling, lets fall a white salt, dissolving again, though slowly, on becoming cold. In applying this test, however, it must be remarked that this precipitation is prevented by the presence of those bases with which thorina forms double salts, from which by boiling no appreciable quantity falls.

From *alumina* and *glucina* it is distinguished by its being insoluble in caustic potash, by which these earths are taken up.

From *yttria*, by its forming with sulphate of potash a double salt insoluble in a saturated solution of sulphate of potash, by which means it may sometimes be separated *quantitively* from yttria.

From *zirconia*, by these two circumstances, that zirconia precipitated hot by sulphate of potash becomes afterwards, in a great degree, insoluble both in water and acids; and that thorina is precipitated by the cyanide of iron and potassium, by which the salts of zirconia are not troubled.

From *protoxide of cerium*, by these, that, in drying and heating to redness it does not assume the colour of the peroxide of cerium; and that before the blowpipe with borax and phosphor salt it does not give a coloured glass, either when hot or cold, provided the earth has been previously perfectly free from oxide of iron.

From *titanic acid*, as well by its precipitating with sulphate of potash as by the characteristic properties of titanic acid before the blowpipe.

From the *common metallic oxides*, among which, from its high specific gravity, it might be ranked, by its not being precipitated by sulphuretted hydrogen.

The properties which I have formerly stated it to possess in common with the sub-phosphate of yttria are the following:— i. Its salts have a pure astringent taste. ii. The crystallized sulphate treated with warm water becomes opaque, and leaves a white skeleton of the crystalline form. iii. Most of its salts are precipitated by boiling, and attach themselves strongly to the sides of the glass like a white enamel. iv. Its hydrate strongly attracts carbonic acid from the air while drying. v. And dissolves in carbonated, but not in caustic alkalies. vi. And the solutions of both are precipitated by prussiate of potash, &c. But it is easily distinguished from yttria, both by the abovementioned test and by this—that the chloride of thorium is not thrown down by boiling, like a solution of subphosphate of yttria in muriatic acid.

3d. *Thorium and sulphur*. When a mixture of thorium and sulphur is heated, first the sulphur begins to distil over, and afterwards the metal takes fire in the gaseous sulphur, and burns with nearly the same lustre as in the open air. The product is a yellow powder, which by pressure becomes shining, but does not exhibit the metallic streak. Heated in an open glass-tube, it gives thorina and sublimed sulphur, (even after the sulphuret of thorium has been heated to redness in a stream of hydrogen gas,) but it does not burn with any degree of lustre. Digested with dilute acids, it gives off at first a small quantity of sulphuretted hydrogen, but is not sensibly dissolved even when heated. Nitric acid also acts upon it very slightly. In cold nitro-muriatic acid also it suffers no change; but when heated it dissolves without residue, giving off nitrous acid gas. The solution contains sulphate of thorina.

4th. *Thorium and phosphorus.* When thorium is heated in gaseous phosphorus, they unite with the development of heat and light. The phosphoret of thorium is dark-gray, has the metallic lustre, and resembles graphite. It is insoluble in water. When heated, it takes fire, and is converted into the phosphate of thorina.

5th. *Salts of thorium.* The salts which thorium gives, as well with *salt-formers** as in the state of oxide with the *oxy-acids*, are distinguished by a strong and pure astringent taste, which is not accompanied by anything of sour, sweet, or bitter, and which most resembles that of pure tannin. In taste they also resemble nearest the salts of zirconium. Their solutions are precipitated by oxalic acid, and by the cyanide of iron and potassium, of a white colour, and are rendered muddy by sulphate of potash, which is dissolved by them.

These three re-agents distinguish them from all other unmixed salts, except those of the protoxide of cerium, from which salts they are distinguished by this—that the colourless precipitate by caustic alkali does not become yellow in the open air, as is the case with the cerium salts. The salts of thorina are decomposed by a red heat, and leave the earth in an isolated state, and they lose their acids more easily than zirconia.

(A.) Haloidsalts.

Chloride of thorium is formed by mixing thorina with pure sugar, charring the mixture thoroughly in a covered platinum crucible, afterwards, when heated to redness in a porcelain tube, passing over it a stream of dry chlorine gas. The decomposition takes place very slowly, and the chloride of thorium is not particularly volatile. The most of it is deposited where the tube ceases to be red; the mass to be decomposed, therefore, should not be allowed to reach so far, if we wish to make a distinct separation. The chloride of thorium deposits itself in the form of a white crystalline ring, and white fumes,

* The following are what Berzelius calls *saltbilder*, *salt-builders*—chlorine, iodine, bromine, cyanogen, fluorine, and sulpho-cyanogen—the base of the hydro-sulpho-cyanic acid.—*Translator.*

passing over, condense on the sides of the glass receiver, into which the porcelain tube opens. It forms there an uncrystalline mass, dissolving only partially in water, and leaving on the glass a transparent thorina, which cannot be washed off, and which, after the glass becomes dry, adheres so strongly, that it might be mistaken for a consequence of the action of acid upon the glass. It is dissolved off by sulphuric, but neither by muriatic nor nitric acids. The cause of this phenomenon may be that the pulverulent chloride of thorium given off is changed at the moment of contact with the moist air into a subsalt, (in what way I do not understand;) so that the earth, separated by the action of water, will be in the same state of insolubility as that which is obtained by burning.

With water the neutral chloride of thorium developes much heat, and is entirely dissolved when the compact, half melted portion is selected.

The hydrate of thorina dissolves with ease in muriatic acid. Evaporated to a certain degree of concentration, particularly if it contain an excess of acid, which renders the salt less soluble, it congeals on cooling, into a straw-like crystalline mass. If the evaporation be continued to dryness by a gentle heat, a deliquescent saline mass is obtained, which, even in dry air, neither crystallizes nor dries. Heated more strongly, it is decomposed, the muriatic acid is driven off, and thorina remains behind. The *hydrous chloride of thorium dissolves* in strong muriatic acid, almost as easily as in water; the *chloride of zirconium*, on the contrary, is almost *insoluble* in muriatic acid. It dissolves also, with ease, in alcohol.

The chloride of thorium combines with the chloride of potassium, forming a double salt, so soluble in water, as to be almost deliquescent. It may be dried and heated to redness in a stream of muriatic acid gas, during which process some chloride of thorium is sublimed, a little is decomposed by the still adhering water, but the greater part remains unchanged. I made use of this method, among others, for the reduction of thorium with potassium*. The double salt may be obtained

* The attempt to obtain by the same process, an anhydrous chloride of kalium, and aluminum for reduction, always failed, only a very small part of the chloride of aluminum remaining undecomposed.

crystallized, although, from its being so easily soluble, with great difficulty.

Bromide of thorium is prepared by dissolving hydrate of thorina in hydro-bromic acid. The solution, which contained an excess of acid, was left to spontaneous evaporation; a tenacious, gummy-like mass, was formed, which, on driving off the excess of acid, became of a deep orange-colour—a hue which was not changed by several days' direct exposure to the solar rays, at a temperature of +30° (Celsius=86° Fahrenheit*.)

When a little bromide of potassium is added, a double salt is formed, and the bromine speedily evaporates.

Fluoride of thorium is insoluble in water, and in hydrofluoric acid. It is obtained by dissolving the hydrate in fluoric acid. When the supernatant excess of acid is evaporated, there remains almost no residue. The fluoride of thorium is a heavy, enamel-white powder, which is not decomposed by a red heat, and very imperfectly by potassium.

The *fluoride of thorium and potassium* is a salt, insoluble in water, which falls when a salt of thorina is mixed with fluoride of potassium. It is not decomposed by heat; but potassium reduces the thorium, though always silently.

Cyanide of iron and thorium is obtained when a salt of thorina, containing no excess of acid, is mixed with a solution of cyanide of iron and potassium. The slightest trace of thorina is discovered by this means. The precipitate is heavy and enamel-white. Acids dissolve it, and alkalies decompose it, separating hydrate of thorina. By the *red* cyanide of iron and potassium the salts of thorina are not troubled.

* I have endeavoured to find out the cause of this colouring, and found it to be derived from a property which iodine has in a high degree, bromine in a less, and which chlorine altogether wants, namely, that they give higher degrees of combination than correspond with those of oxidation (än som svara emot oxiderna). Iodine has this property, even with the strongest bases, potassium and sodium, and gives very soluble crystalline high degrees of iodic combination, with calcium, magnesium, &c., which, with the earthy hydrates, form insoluble subsalts. Digestion, with much water, decomposes these, and separates the earth. Bromine exhibits these high degrees of combination, which are decomposed by water, with the weaker metallic bases only, of which we have an example in the bromide of calcium. Hydrate of lime, treated with bromine in excess, and afterwards evaporated in the vacuum of an air-pump, over dry caustic potash, gives a solid, cinnabarred mass, which is decomposed by water in such a way, that a yellow powder is separated, and a bleaching liquor formed; but all colour is speedily lost, and with it the bleaching property. The liquid then contains bromide of calcium and bromate of lime. In an analogous way, the orange bromide of thorium contains a chemical combination of bromine, with neutral bromide of thorium.

B. Oxysalts.

Sulphate of Thorina.—This salt is obtained when thorina, previously heated to redness, is rubbed to fine powder, digested with a mixture of equal parts of water and sulphuric acid till all the water is evaporated, and the excess of acid driven off by gentler heat. The salt which remains has an earthy appearance. In a large quantity of cold water it dissolves immediately; but, if the quantity of water be so small as to cause the development of heat when poured upon the salt, it takes a much longer time to dissolve. The solution, left to spontaneous evaporation at a low temperature, deposits transparent crystals, and leaves at last a very sour mother liquor, which contains almost nothing but sulphuric acid, and gives a very slight precipitate when saturated with ammonia.

The crystallized salt is neutral sulphate of thorina in rhomboedral crystals. These crystals undergo no change at the common temperature and humidity of the atmosphere, but in very warm and dry air they become milk-white without falling asunder. They contain 29.4 per cent. of water, the oxygen in which is five times that in the earth. When they fall to powder, or are gently heated, they lose three-fifths of this water. The salt, like the sulphate of yttria, dissolves very slowly in water, so that the crystals may be very long in that liquid without losing the sharp edges. In powder it dissolves more easily, and water afterwards takes up a large quantity of it. Thrown into hot water, the crystals lose their transparency, and become milk-white; and if the water be heated to boiling, they become covered with a white deposit, which dissolves as the water cools. If a much-diluted solution of the salt be heated to boiling, the water becomes opalescent; but if it be in a flat vessel, and be blown upon, it becomes clear. These phenomena are derived from the property which this salt possesses of losing, at a temperature varying with the degree of concentration of the solution, a portion of its combined water of crystallization—becoming, from a combination of five, one with only two atoms, and which new combination is exceedingly difficult of solution, and continues so till it again takes up the three other atoms. For this reason it may be washed out, without great loss, after the manner already mentioned, with water having a temperature above that at which it is changed from $\dot{\mathrm{Th}}\dddot{\mathrm{S}} + 5\dot{\bar{\mathrm{H}}}$ to $\dot{\mathrm{Th}}\dddot{\mathrm{S}} + 2\dot{\bar{\mathrm{H}}}$.

If a solution of sulphate of thorina be evaporated by a heat of about 25° (Celsius = 77° Fahr.), it begins, at a certain point of concentration, to deposit a snow-white, woolly, very bulky mass, which is a congeries of exceedingly fine, flexible, microscopic crystals of the salt just mentioned, precipitated by the boiling, and the formation of which is not prevented by excess of acid. It dissolves very slowly in cold water, especially when the quantity is small; and commonly leaves a transparent crystalline wool, looking like the result of a decomposition, but which also is at last entirely dissolved. The sulphate of thorina is insoluble in alcohol, and is precipitated by it from its solution in water. If the precipitate be thrown down in the cold, it contains five atoms water; if it be boiled in a mixture of water and spirit, it contains only two atoms.

The distinction between these two salts is analogous to that already shown by Mitscherlich in regard to several crystallized salts, which at different temperatures assume different quantities of water of crystallization.

The salt is composed per cent. of

Sulphuric acid	26.260 = 1	or 31.90 = 1
Thorina . .	44.273 = 1	or 53.78 = 1
Water . . .	29.467 = 5	or 14.32 = 2
	100	100

I have already shown that thorina seems to form an acid anhydrous salt, which made me uncertain how far the atomic weight deduced from the analysis of the sulphate could be depended upon.

One gramme of the sulphate of thorina, thrown down by boiling, dried in the air at 24° (Celsius = 76° Fahr.), mixed in a platina crucible with distilled sulphuric acid, which was afterwards evaporated over a spirit-lamp till all fumes ceased to be given off. The weight then was 1.055 grammes. It had therefore taken up 19.77 gr. sulphuric acid more than formerly, which was something more than half as much as it contained before. In another experiment, 1.192 sulphate of thorina, prepared in the same way, gave 0.6345 gr. thorina, which approaches very near $\dot{\mathrm{Th}}^3\dddot{\mathrm{S}}^2$; but here there had evidently been formed a neutral salt, which dissolved with great difficulty and slowness.

In several other experiments I obtained always varying results, since the proof of the evaporation of the excess of acid is always uncertain. At all events these experiments seem to prove the existence of an anhydrous acid salt, which probably contains twice as much acid as the neutral salt, and the character of which is, that it fully dissolves in cold water in a few moments, giving a solution, the evaporation of which, in whatever way, affords the neutral salt, leaving the excess of acid in the mother liquor.

To determine if thorina forms a *sub-sulphate*, and how it is composed, I mixed a solution of sulphate of thorina with less caustic ammonia than was necessary to precipitate all the earth. The precipitate was at first re-dissolved—it was very gelatinous, and semi-transparent. During the washing I observed that, when the washing-water ceased to leave a trace on evaporation, it still gave a precipitate with chloride of barium. I took, therefore, a portion of the precipitate, analysed it, and obtained 100 parts thorina and 68 parts sulphate of barytes. The washing was then continued a couple of hours, with boiling water, containing no trace of sulphuric acid—after which the remainder, analysed, gave me 100 thorina and 50 sulphate of barytes, from which it appears that water in washing decomposes this subsalt, extracting the acid and leaving the earthy hydrate.

Sulphate of Thorina and Potash.—When in a solution of thorina, sulphate of potash is placed in the solid form, there is at first no precipitation, but after some time the water begins to be opaque; and, as the salt dissolves, there is deposited on the inside of the glass, and precipitated through the solution, a snow-white crystalline powder, which is this double salt. If the solution of the thorina salt be neutral and much concentrated, the whole thorina it contains is not precipitated in this way, since the salt soon becomes covered over with a thin layer of the double salt, which may, indeed, be separated by shaking, but which is never found when the salt is nearly all precipitated. This was the case in the formerly-detailed analysis. If, instead, a boiling hot solution of sulphate of potash be taken, and added so long as any precipitate is formed, we have, on cooling, a solution entirely free from thorina, even when it contains acid in excess. This salt is entirely insoluble in cold saturated solution of sulphate of potash. It dissolves with difficulty in cold water, but very easily and readily in warm. Left to spontaneous evaporation, the solution deposits clear colourless crystals, which I once obtained in the form of rectangular four-sided prisms united lengthwise into a cross, formed of the plane terminations of the prisms. But these crystals seemed to be hemitropic, and had re-entering angles on the projecting (ut ăt vände) sides of the prism. These crystals I have generally obtained too small to determine their form more nearly.

The aqueous solution of this salt, boiled in a platina vessel, speedily covers the metal with a layer of thorina, and deposits a subsalt insoluble in water; but this decomposition goes on only to a certain degree, and that which is deposited speedily loses a portion of its acid. The salt is insoluble in alcohol. It contains water of crystallization, which is dissipated by a gentle heat, and leaves the crystals opaque and milk-white. It is not changed by exposure to the air. It consists of

$$\left.\begin{array}{ll}\text{Sulphuric acid} & =39.312 \\ \text{Potash} & =23.138 \\ \text{Thorina} & =33.139 \\ \text{Water} & =\ \ 4.412\end{array}\right\} = \dot{K}\,\dddot{S} + \dot{Th}\,\dddot{S} + \dot{\bar{H}}$$

I have been unable to form any double salt of these constituents in any other ratio. Even the acid sulphate of potash, melted with thorina, produces this salt; but it is not dissolved by melting in an excess of this acid salt, as is the case with zirconia, tantalic acid, titanic acid, &c.

Nitrate of thorina dissolves easily in water and alcohol. In the open air the solution becomes syrupy and semi-fluid. Over sulphuric acid, in a close vessel, it concretes into a crystalline mass.

Nitrate of thorina and potash is very easily dissolved in water. After spontaneous evaporation to the state of syrup, it shoots out all at once into a mass of strawlike crystals. It is soluble in alcohol.

Phosphate of thorina is insoluble even in excess of phosphoric acid. It falls in the form of a white flocky precipitate, melting with difficulty before the blow-pipe.

Borate of thorina is a white flocky precipitate, insoluble in excess of boracic acid.

Carbonate of thorina is precipitated by carbonated alkalies, with the development of free carbonic acid, and is a subsalt, whose composition I have not more nearly investigated. It is insoluble in water impregnated with carbonic acid. The hydrate of thorina attracts carbonic acid from the atmosphere, and, after long drying in the air, dissolves in acids with effervescence. This does not happen when it is dried in a vacuum over sulphuric acid.

Arseniate of thorina is insoluble in water and arsenic acid. It is thrown down in the form of a white flocky precipitate, both by neutral and by acid arseniates.

Chromate of thorina is a beautiful bright-yellow flocky precipitate, which in excess of chromic acid dissolves, and forms an acid salt.

Molybdate and tungstate of thorina are thrown down both by neutral and by super-salts of these acids. They are in the form of white flocky precipitates.

Oxalate of thorina is a white, heavy precipitate, insoluble in excess of acid. In other free and diluted acids, it is very sparingly soluble. If it is collected on the filter, and washed with water, it speedily begins to pass milky through the paper, which is prevented by the addition of a little oxalic acid to the water.

Oxalate of thorina and potash is also a white powder, insoluble in excess of acid. It is distinguished from the former by becoming black on burning; and, after the carbon is burnt off, it falls, when put in water, to a milk-white mass, and the solution contains carbonate of potash.

Tartrate of Thorina.—Hydrate of thorina dissolves by digestion in tartaric acid. If so much be added that a portion remains undissolved, a neutral salt is obtained, white, flocky, and sparingly soluble in ammonia, which only takes up a portion of it. The acid solution has more of a sour than astringent taste, and gives, after evaporation, an acid crystalline salt. It is soluble in alcohol, leaving a neutral salt; but the solution in alcohol still contains thorina, which seems to show the existence of a still more acid salt. Neither the acid tartrate, nor any of the other salts of thorina to which tartaric acid is added, are precipitated by caustic ammonia in excess; and there is no sure method for separating thorina from such a solution but by evaporating to dryness, and destroying the tartaric acid by a red heat.

Tartrate of thorina and potash is formed when bitartrate of potash is digested with hydrate of thorina and water. It is a difficultly soluble crystalline salt, which is not precipitated by alkalies, and is only rendered opalescent by prussiate of potash.

Citrate of thorina.—When citric acid is digested upon hydrate of thorina, a white flocky insoluble neutral salt is obtained, while super-salt remains in solution, which may be evaporated to a transparent syrupy mass, but does not crystallize. Its taste has more sourness than astringency. Both the neutral and the super-salt dissolve with ease in caustic ammonia, without any sign of precipitation; and if the solution be concentrated, there is obtained from both a transparent gummy-like mass, which is soluble in water. To obtain the thorina, therefore, the citric acid, like the tartaric, must be destroyed.

Acetate of thorina.—If hydrate of thorina, still wet, be digested with dilute acetic acid, a thick pasty opaque mass is formed; and if carbonate of thorina be digested with concentrated acetic acid, it is decomposed with effervescence, a white powder remaining at the bottom, and a small quantity being taken into solution. Either of these being evaporated nearly to dryness by a gentle heat, the acetate of thorina becomes insoluble in water; and it may in this way be freed from other earths, which dissolve in the form of acetates, while only a slight trace of thorina is taken up. The acetate is heavy, enamel-white, and goes like milk through the filter, unless the washing-water contain a little muriate of ammonia. From the neutral nitrate of thorina, acetate of potash causes no precipitate, not even by boiling, which seems to indicate the formation of a soluble double salt.

Succinate of thorina.—Succinate of ammonia throws down from the neutral salts of thorina a white flocky precipitate. The hydrate of thorina, digested with a solution of succinic acid, changes into a thick enamel-white neutral salt, like the acetate. Excess of succinic acid dissolves only a trace of the neutral salt.

Formate of thorina.—Formic acid dissolves the hydrate of thorina, and the salt crystallizes by spontaneous evaporation of the water and excess of acid. The crystallized salt is slightly soluble in alcohol. By boiling water it is dissolved, without being disturbed by boiling; but cold water decomposes it in such a way, that the solution becomes acid, and a certain portion of a white sub-salt remains undissolved. The formic acid, employed in this experiment, was prepared after the method of Dobereiner.

C. Sulphur-Salts.

Thorium seems, like aluminum, to form no sulphur-salts by the moist way. When I precipitated sulphate of thorina with pure sulpharseniate of sodium, a smell of sulphuretted hydrogen was given off; and when the yellow precipitate was washed, and afterwards treated with muriatic acid, thorina was extracted without the development of sulphuretted hydrogen; from which it seems to follow, that the sulphur-salts only throw down a mixture of the hydrate of thorina, with the electro-negative metallic sulphuret.

On the Astacillæ of Cordiner, a Genus of Crustaceous Animals. By the Rev. John Fleming, D.D., F.R.S.E.

[Communicated by the Author.]

There are few employments, in which a naturalist can engage, of a more disagreeable character than the collation of synonimes. In some cases, he is compelled to witness, with regret, the successful labours of original observers overlooked by those who, unconscious of their existence, would willingly, under other circumstances, have done ample justice to their merits. In other cases, his attention is directed to individuals in whom selfishness appears to predominate, who exhibit a cold indifference towards the labours of their predecessors, and who seem more disposed to secure the temporary applause given to discoverers, than the honest fame of faithful recorders. There is so great a degree of drudgery connected with the investigation of synonimes, and apparently so little discredit attached to inaccuracy regarding them, that the task is usually avoided as too irksome for execution. Should it be found impracticable to avoid noticing the labours of those who have investigated the subject, many individuals but too easily succeed in saving themselves the trouble of scrutinizing the evidence, by deriving their information at second hand. The reader, in ordinary cases, will not likely detect the indolence of the compiler, who, in such circumstances, but too frequently claims and gains credit for great industry of research, when all his labours consisted in transcribing. It is true, that an occasional typographical error, in the reference to a page, when it can be traced through the synonimes of subsequent authors, may reveal the secret. But few travel in a course where such occurrences are to be met with; and those who may happen to detect them are generally in a state of mind to afford a delinquent the greatest sympathy.

In the present state of society, it is perhaps impossible to avoid the multiplication of synonimes. The number of observers, in almost every branch of science, has increased to such a degree as to render it unattainable for any individual to become acquainted with all the results which they have gained; and it is equally impracticable to be aware of all the truths which these observers may record in the numerous publications now open for the reception of scientific truths. It would require, for each department of science, the lifetime of an active individual, placed in favourable circumstances, in order to collect the facts which have been ascertained, and apportion to each observer the share of discovery which may be due. Few, however competent for the task, can be expected to exhibit such devoted patriotism. But there are many individuals, at present engaged in scientific inquiry, who are studying Nature or tracing the labours of her votaries, and who occasionally have it in their power to point out the progress of discovery, and thus contribute to the formation of correct annals of science. Under the conviction that every result of this kind is of value, the following notices have been recorded, which may probably have an interest with those who are engaged in the study of the crustaceous animals, or who take any pleasure in watching the progress of the British Fauna.

In the year 1784, the Rev. Charles Cordiner, of Banff, began to publish in London, in numbers, a very interesting work, entitled, "Remarkable Ruins and Romantic Prospects of North Britain." Though the professed object of the work was the elucidation of the remains of antiquity, the author was induced to publish twenty-four plates, containing the figures of marine animals, chiefly obtained from the Moray Frith. A few of these objects had been previously described in the writings of Mr. Pennant, and other British naturalists, but the greater part constituted additions to our Fauna. Though the figures in general display the taste of the painter rather than the discernment of the naturalist, and the descriptions offend by their inflated style and want of precision, I have been able to identify several species, of which Cordiner's figures are the first indications. To these I have referred at the proper places among the *mollusca* and *radiata*, in the first volume of "British Animals."

Attached to the twenty-first number of the work there is a plate marked "Astacilla, Purple Doris," &c., 1793. This contains four figures of a crustaceous animal which was dredged up a few leagues from the coast of Banff. The figures express some of the attitudes and habits of the species, and sufficiently indicate its want of accordance with any of the species or genera at that period recognised by naturalists. The description annexed merely states, that "the diminutive Astacilla, of the generic name of lobsters, is applied at present as a common characteristic, until one more particularly appropriate may be fixed on to distinguish it by; for these are a species that do not appear to have been yet recognised among the varieties of British insects." It is also added, "the eye appeared as a regular arrangement of bright specks, in circular rows." In reference to their dwelling he observes, "The Astacillæ, having their residence among the tender corals, which can only spread their beautiful forms in the calm and tranquillity of those regions of the deep where winter-storms never have power to agitate the bottom, they only can be brought ashore with those peculiar clusters of corallines among which they dwell; and the many chances against falling in with any one rare and particular species, in the wide extent of these their dwellings, so remote from human eye, is an obvious cause why many of these have been so long, and may yet remain, undelineated and undescribed." In the plate to which we have referred, these astacillæ are represented with their projecting antennæ and four anterior pairs of natatory legs free; while the three hind pair of prehensile ones are grasping the stems of the *tubularia indivisa*, growing with other corallines on a dead shell of the *cyprina islandica*. The attitudes of the animal, exhibited in the figures, are remarkably expressive, and appear to accord with its structure, especially the arrangement of its legs.

In the year 1805, the late Mr. James Sowerby, without apparently being aware of the previous labours of Mr. Cordiner, again introduced the astacilla to the notice of the public, as an addition to our Fauna, in his valuable "British Miscellany," No. III., plate xv. He there designated it under the title *Oniscus longicornis*, with the following specific character:—Segments of the body, 10; the fourth is the length of six others. Antennæ consisting of five joints. The eight fore-legs hairy in the inside, the others smooth; eyes black." This species was transmitted by the late Mr. T. W. Simmons, as having been "entangled in the nets off Dysart, near Inch Keith," in the Frith of Forth. The figures bear a sufficiently close resemblance to those of Cordiner, though on a more reduced scale; and the only addition, of consequence, to the history of the animal, which was at this time communicated, was included in the following sentence: "The eggs are red, and adhere to the under side of the largest segment of the body."

This singular species of crustaceous animal, thus prominently brought under the notice of British naturalists, appears to have been altogether overlooked by that truly zealous and successful systematical zoologist, Dr. Leach, in his different treatises on crustaceology, which successively appeared in the Edinburgh Encyclopædia, the Linnæan Transactions, and the Supplement to the Encyclopædia Britannica; and it was equally excluded from the collection of these articles published by Mr. Samonelle, in his "Entomologist's Useful Compendium." Nor did it find a place in the "Considérations Générales sur la Classe des Crustaces," of M. Desmarest, Paris, 1825. This omission is the more remarkable, as Dr. Leach, under the article *Hippolyte Sowerbæi*, refers to the "British Miscellany," and Desmarest enrols the same work in his "Bibliographie Carcinologique." The latter author, however, never probably examined the work; as Dr. Leach having, by mistake, in quoting the "Miscellany," under the species just referred to, named the twenty-first plate instead of the twenty-third, and this typographical error has been transferred to the pages of Desmarest.

In the thirteenth volume of the Edinburgh Philosophical Journal, conducted by Professor Jameson (for October 1825), the Astacilla of Cordiner was produced a second time, by Dr. Johnston, of Berwick, not only as a new species, but as constituting a new genus, under the title *Leacia lacertosa.* This zealous observer perceived, as Cordiner had previously done, that, in its characters, this crustaceous animal differed from all the genera which had been previously instituted; though he considered it as associating best with the Asellides of Lamarck, a group which includes the genera *Asellus, Idotea, Sphæroma, Cymothoa,* and *Bopyrus.* The name of the genus was intended as a compliment to my valued friend, Dr. Leach, and the following character was assigned to it. "Antennæ, four; the superior very short; the inferior nearly as long as the body. Body linear, of nine segments; the four first, and the four last, short, transverse, and brading appendages; the mid one naked, half the length of the body. Legs of two kinds; those attached to the anterior segments, formed for swimming, and those to the posterior, for creeping. Caudal segment mucronate, with two lamellæ beneath, inclosing divided styles."

Had this generic character been accurate in all the particulars of which it consists, a very useful addition would thereby have been made to the systematical arrangement of crustaceous animals. But, unfortunately, the mistakes which have been committed in its construction are of such a kind as to destroy its value, and to excite a regret that, amidst several successful efforts at minute description, there should occur any room for censure.

The *body* of the animal, exclusive of the head and the tail, consists not of nine, but of seven segments, the ordinary number of its congeners. The middle segment of the body, or the fourth, instead of being naked, or, as it is stated in the description, "resembling in form the middle shield of the lobster tribe, but peculiar in having affixed to it neither feet nor other appendage," in reality possesses a pair of natatory feet. These, however, having their origin very near to the anterior margin of the segment, may, on a cursory view, be regarded as in connexion with the antecedent segment—a deceptive appearance, which, in the present instance, has misled. In consequence of this mistake, the first pair of feet are assigned to the *head*, though they belong to the first segment of the body. In the specimen now before me, the inferior antennæ, instead of having only seven joints, as represented by Dr. Johnston in his description, possess eight, the three last appearing to constitute a single segment.

Among the marine productions collected during Captain Parry's first voyage to the Arctic Seas, and noticed in the Appendix which was published in 1824, a crustaceous animal is described and figured by Captain Sabine, under the name of Idotea Baffini, as brought up by the trawl in considerable numbers from twenty fathoms depth, coarse sandy bottom, on the west coast of Baffin's Bay, in latitude 71°. The propriety of inserting this species along with the Idoteæ could only have been justified, in consequence of the resolution of the author to follow the system of Lamarck, in which the genus *Idotea* is characterised in such general terms, as to admit animals differing widely in structure from the typical species, as in the present instance. The same animal is retained in the genus Idotea by Lieutenant J. C. Ross, in the appendix to Captain Parry's Third Voyage, p. 117.

The general connexion between the Astacilla and Idotea Baffini occurred to me at once, on comparing the very expressive delineations which Mr. Cordiner has given of the one, and Mr. Curtis of the other; and their generical connexion was ascertained without difficulty, by comparing together specimens of each. The example of the Arctic animal in my possession was much infested with the *campanularia volubilis.* It differs chiefly from Cordiner's animal in the greater number of segments in the last joint of the under antennæ, and the greater brevity of the fourth segment of the body.

Viewing all these circumstances in connexion, I was disposed to consider the *Astacilla* (assuming the name employed by Cordiner, from its priority) as a well-marked genus in the family Idoteadæ, and sufficiently distinguished from *Idotea* and *Stenosoma* by the greater length of the fourth segment of the body, and the natatory character of the four anterior pair of legs.

The recent publication of the third edition of the invaluable "Règne Animal" of the modern Aristotle, has unfortunately added another synonime to the group, the history of which we have been attempting to trace. The deservedly celebrated entomologist, M. Latreille, in the fourth volume of the work referred to, has instituted the genus *Arcturus*, which is identical with *Astacilla*, and adds, "Je n'ai vu qu'une seule espèce (*Arcturus tuberculatus*) et qui a été rapporteé des mers du Nord, dans l'une des dernières expéditions anglaises au pole arctique," p. 139.

It would appear, from this notice, that M. Latreille was unacquainted with the zoological value of our Arctic voyages, and suffered himself to describe as a new species, in the year 1829, an animal, the description and figure of which had been published in this country five years before; and in a work, too, which might have been supposed calculated to attract the notice of the continental observer.

The genus Astacilla consists at present of two species:

1. *A. Longicornis*. The fourth segment occupying one third of the length of the body; terminal joint of the antennæ composed of three rings.

   Astacilla, Cordiner's Ruins, No. xxi. Tab. iv.

   Oniscus Longicornis, Sowerby's British Miscellany, I. p. 31., T. 15. Leacia Lacertosa. Johnston, Edinburgh Philosophical Journal, vol. xiii. p. 220.—*East Coast of Scotland*.

2. *A. Baffini*. The fourth segment of the body double the length of the third; terminal joint of the antennæ composed of about twelve rings.

   Idotea Baffini, Sabine, Appendix to Parry's First Voyage, p. 228., T. 1., f. 4. 6.

   Id. Ross, Parry's Third Voyage, Appendix, p. 117.
   Arcturus Tuberculatus Latreille, Règne Animal, iv. p. 139.—*Arctic Seas*.

Manse of Flisk, 12th January, 1830.

Fragments on Egyptian Literature.

i. *Egyptian word for God.* Among the hieroglyphical characters whose meaning was first discovered by the late lamented Dr. Young, is the *hatchet* or *battle-axe*, which he interpreted "God," and read "Nout," a Coptic word of similar signification; he, of course, considered the character as ideographic or symbolic. In this he has been closely followed by M. Champollion, who says of the *hatchet*—" Noute *dieu*, caractère symbolique." I agree with these gentlemen as to the signification of the character, which, when it stands alone, beyond all controversy signifies God. I have observed, however, that, when combined with other characters, it is often decidedly phonetic, representing the vowel O, or the diphthong OU; and I conclude that it was by this simple monosyllable, and not by the Nout or Noute of the Coptic manuscripts, that the ancient Egyptians expressed the idea of "God." In fact, the Coptic word would appear to be a *derivative* from the Egyptian one. It seems to have *properly*, or, at least, *originally* denoted the adjective "divine," not the substantive "God." In this sense it occurs on the Rosetta Stone, in the last line of the hieroglyphic text. Where it is said that the decrees shall be engraved in *sacred* characters, I read the adjective employed phonetically—NOUT, that is, "divine."

This old Egyptian word O, or OU, forms a part of several proper names, which are preserved in Greek characters; it is represented in them by ο, ω, or ου, with the article π or φ sometimes prefixed. Thus, in Αρωηρις, Αρποχρατης, and Σεμφουκρατης, the syllables ω, πο, and φου signify "God," or "the God." I will not enter at present upon an explanation of the remaining parts of these words; suffice it to say, that the meaning assigned to these syllables is not *a mere conjecture*. We have a compound similar to these in the proper name, Sempo-eris, deciphered by Dr. Young from the Enchorial MSS. of Mr. Grey (*Quart. Journ., N. S.*, I. 402). Another compound word, in which this element is found, is the well-known title of the ancient Egyptian kings, of which so many interpretations have been given. I have no hesitation in resolving it into the elements Pha-Ra-O, that is, "The Sun God." It is the enunciation of that symbol, *the solar disc*, with which the mystic titles, or *prænomens*, of all the kings, who were thus entitled, commenced.

I am not sure whether the *crosier* is generally admitted to be a character for God; I am satisfied, however, that it is so. It frequently occurs in the titles of kings, accompanied by some symbol, simple or compound, which stands for an epithet, or a combination of epithets, such as good, just, warlike, &c. For example, the sovereign, the cover of whose sarcophagus is at Cambridge, is called "Rameses, the Warlike God."

Either of these characters (the *hatchet* and the *crosier*) followed by a *semicircle* and an *oblique oval*, (a group, which Dr. Young pointed out as characteristic of the feminine gender,) or by a *semicircle* only, forms the phonetic word, OUTHiS or OUTH, which signifies "a Goddess." In this instance, and probably in the greater number of native words, the feminine characteristics were sounded, a short vowel being supplied between the two letters; but in other cases, especially, as we may suppose in foreign proper names, they are pleonastic.

The sitting figure, representing a God or a Goddess, which is generally subjoined to the phonetic name of a divinity, is in most cases pleonastic, so far, I mean, as respects the pronunciation. Sometimes, however, it is required in order to complete the name, and must then be read OU, OUTH, or OUTHiS. An instance of this will be given in the next fragment; another is the following. The name of the deified mother of the defunct, who occupied Sir T. Henniker's sarcophagus, is represented hieroglyphically, if Mr. Salt's plate be correct, by the three letters PH, I and L, followed by the figure of the Goddess Athor. It appears, from the Greek inscription, that her name was in the genitive case φιλουτος, which would come, according to the Alexandrian dialect, from a nominative φιλουτ, as well as from φιλους. I am inclined to think that the former was her true name, and that the figure of the Goddess is in this instance to be sounded OUT.

I will only remark farther, that, although the *hatchet* is to all intents and purposes a *letter*, and as such has the same power as the *lituus* or *quail*, it appears to be only used in words connected with religion. It is used occasionally as the first letter of the word OUeB, "a priest," and as the middle letter of the word MOUTH, a mother, when applied to a Goddess.

In the ordinary use of this word, to denote an earthly parent, a *lituus* is substituted for the *hatchet;* or, more commonly, the word is abbreviated. It is scarcely necessary to say that I consider the *vulture* as a phonetic character, representing the letter M. It is stated by Horappollo, that this bird *alone* expressed the idea of "mother;" and I have sometimes been inclined to think that the Egyptian word for that idea was simply Ma, and that MOUTH was a contracted compound, signifying "mother-goddess." If this be the case, M. Champollion has only *partially* explained the Greek word θερμουθις, which before his time was so completely misunderstood. Its correct interpretation will be ZHeR-M'-OUTHiS, that is, "the great mother-goddess."

ii. *Names of Osiris and Isis.* The hieroglyphical names of the deities Osiris and Isis have been universally supposed to be symbolic. The impossibility of reading them according to the corresponding Greek names, by assigning any phonetic value to the *throne*, the character with which they both commence, is very obvious. I am, nevertheless, decidedly of opinion, that the names are phonetic; and I consider the *throne* to represent the letter S. The characters which compose the name of the male deity are a *throne*, an *eye*, a *solar disc,* and a *hatchet*, or *figure of a God*. Of these, however, either the second or third is generally omitted; the former as a vowel, the latter for abbreviation's sake; just as the final N is omitted in writing the word SoTeN, *king*, and other letters in other instances, where the characters which are written are sufficient to determine the meaning: but all the four elements above enumerated occur, amongst other places, on a *cippus* in the Borgian Museum. (*Précis, pl.* xii.) The three first of these characters may be read SIRE; the fourth, we have seen, in the last fragment, is O or OU; and it was, by different persons and in different places, pronounced with or without the article, and placed before or after the distinguishing appellation of the deity. Hence were formed the several words, Sireou, Sirepou, Ousire and Pousire, all signifying "the God Sire;" which were pronounced by the Greeks, some slight changes having been made in the vowels, and the terminations having been accommodated to the genius of their language, Σειριος, Σεραπις or Σαραπις, Οσιρις and Βουσιρις. The signification of these four words was vulgarly supposed to be very different; and it is no wonder that they should appear to differ widely in their origin, when the meaning of the elements, *ο* or *ου* and *π* or *β*, was unknown or unattended to. Plutarch, however, in his Treatise on Isis and Osiris, expressly identifies Sirius with Osiris; and it is abundantly manifest that Osiris and Serapis, each of them the consort of Isis, and the God of the Infernal regions, were one and the same deity. It has been said, indeed, by some moderns, that Serapis was a Grecian deity, introduced into Egypt by Ptolomy Soter. This, however, is a mistake. Ptolomy procured from the town of Sinope, and transported into Egypt, a *statue*, which was worshipped as that of "the Infernal Jove." When it was brought to Alexandria, it was called "Serapis," which was the name of the Egyptian deity, who had similar attributes; but we are no where told that it was worshipped under the name Serapis at Sinope, nor that Serapis was previously unknown to the Egyptians. Authors have *inferred* the latter from his being omitted by Herodotus in his enumeration of the Egyptian deities; but this omission may be explained in an equally satisfactory manner, by supposing him to have been mentioned by the Father of History under *another name*, namely, that of *Osiris*. The identification of Busiris with this many-named deity is less easy, as the latter name has been seldom mentioned; I consider it, however, to be a highly probable conjecture. I do not speak of Busiris, the name of a place, for which a different derivation must be sought.

We have thus *three*, and possibly *four*, different ways of reading this phonetic name; a name which is compounded of two parts, one signifying "God," the other, "Sire," of uncertain signification. I have formed an opinion as to the meaning of this last word; but as I see a very different derivation of it, in support of which plausible arguments may be adduced, I shall for the present suspend my judgment. I will only add, that, when the name of this deity is prefixed to that of a deceased person in funeral records, the character for "God" is sometimes omitted. It should then, in my opinion, be read simply—Sire.

I now pass to the hieroglyphic names of the wife and sister of Osire. One of these consists of *four feathers*, the *second* of which is *reversed;* and is read without difficulty ESI. It is a considerable time since I have read this name in the inscription copied by Mr. Salt from the little temple which he excavated at Philœ, and published by him in his Essay. (*Pl.* v. *fig.* 3: an obvious inaccuracy occurs in the figure.) I am not aware that this name has been read by any one else; nor have I observed it elsewhere.

The name of the Goddess is most commonly expressed by a *throne*, with the *semicircle* and *oblique oval.* Sometimes we have a *star*, a *reversed feather*, or a *bent line* (such as occurs in the name of Ptolæmous on the Rosetta stone) in place of the *throne.* *All* of these (as Mr. Salt has long since remarked respecting the three last-mentioned) represent phonetically the name SoTHiS, by which this deity was commonly known*. Properly, indeed, this appears to have been the name of the dog-star; but we know from Diodorus and Horapollo, that the Egyptians supposed this star to be the dwelling of—or rather a personification of—the goddess Isis.

It would seem that the astronomers have confounded the names of the husband and wife in speaking of this star; it is now called Sirius, in place of Sothis. *Anciently*, Sirius was a name for *the Sun*, and it is so used by Hesiod, and others of the Greek poets; but Virgil applies it to the dog-star, and in later times the error, for such it doubtless is, has been almost universal.

iii. *Pedigree of Rameses the Great.* In the present state of things, that person must be considered as very rash who should give an opinion, founded on merely probable grounds, respecting any point in Egyptian history or chronology. It is hard to say how soon some monument may be discovered in that country which may decide the question one way or other, or which may, at least, furnish additional grounds on which an opinion may be founded. In the present instance I do not mean to commit the fault that I have pointed out. Suspending my own opinion as to the parentage of the Egyptian hero, I would merely point out the unwarrantable inferences that

* In one of the figures (37) of M. Champollion's letter to M. Dacier, there is a still different variation of this name. The first S seems to be the *weight* or *roller*—the character which is used in the name of Osorchon, the son of Shishonk; and the second is the *broken line* or *two sceptres* M. Champollion, most unwarrantably, asserts that the signs of this name are *incomplete.*

have been made by M. Champollion and others on the subject; I would refer to the authorities on which they propose to rely, and shew that these very authorities would rather authorize an opposite conclusion.

M. Champollion always speaks of Rameses the Great as the grandson of Rameses Meiamoun, and as the son of Amenophis, who succeeded that prince; and who, it has been thought, was the Pharaoh who was drowned in the Red Sea. This he does on the authority of Manetho, as quoted by Josephus, which, he says, is confirmed by the genealogical table in the palace at Abydos. "Manetho," says M. Champollion (*Précis*, p. 275), "giving an account of the second invasion of the Shepherds into Egypt, in the reign of Amenophis III., father of Sethos, says, in effect, that 'the king, troubled at the news of the arrival of these strangers, set out in order to fight with them, after having intrusted his son Sethos, who was then five years old, and who was also called Rameses, from Rampses his father, to a sure friend.' Further on, Manetho relates that 'Amenophis the third, not having been able to resist the Shepherds, retired with his son into Ethiopia, where he remained many years; but at length, having gathered together an Ethiopian army, he re-entered Egypt, along with his son Rampses, who himself commanded at that time a body of troops.'"

M. Champollion has here given, accurately enough, the words of Josephus (*Cont. Apion.*, i. 26, 27); but he has completely *misapplied* them. Had he attended to the context, he would have seen that the Amenophis and Rampses, who are spoken of in this passage, are not, as he imagines, the last king of the xviiith dynasty and the first of the xixth, but the third and fourth kings of the xixth. Josephus expressly says that this Amenophis reigned 518 years after the first expulsion of the Shepherds, that is, after the commencement of the xviiith dynasty; and he proves this from the words of Manetho. The princes of the xviiith dynasty reigned in all 393 years. Then came the two brothers Sethos and Hermeus, who commanded the xixth dynasty, and whom he identifies with the Egyptus and Danaus of the Greeks. Sethos, says Manetho, cast the other out of Egypt, and reigned fifty-nine years, as did his son Rhampses after him sixty-six years. Amenophis is mentioned by Manetho as the next king; and the commencement of his reign is subsequent to the expulsion of the Shepherds by Tethmosis 393+59+66=518 years. (See Josephus, *Cont. Apion.*, i. 26.)

Now, whatever opinions may be entertained respecting the correctness of Manetho's statements, in the whole or in part, it is plain, from what has been said, that he does *not* state that Rameses, or Sethos, the founder of the xixth dynasty, was the son of Amenophis; and, in the absence of such a statement, his mention of the dynasty being changed, would seem to imply, that he was not so related to his predecessor.

I turn now to the table of Abydos, which has been supposed to confirm this pedigree of Rameses, but which appears to me to furnish a strong presumptive argument against it.

It is well known that, in the lowest horizontal line of this table, the prænomen and name of Rameses the Great are arranged in alternate ovals; each occurring, when the table was complete, ten times. Over these are two other lines, which seem to have originally contained thirty-nine different prænomens, and a single name, following the last prænomen, which is precisely the same as that in the lowest line. The sovereign, who bore this last name and prænomen, is admitted on all hands not to have been the *father* of Rameses the Great, but his ancestor in a remote degree. Why then, it may be asked, are the intermediate names omitted? It is difficult to give a satisfactory answer to this question, if we suppose, that the kings who reigned during this interval were interposed in genealogical succession between the two Rameses mentioned in the table. But the difficulty would be removed, by supposing that Rameses the Great was of a different family from his immediate predecessors, but equally descended from the former Rameses (Rameses II. of Champollion-Figeac). I would not venture to advance this supposition, as *proved* by this genealogical table. I merely say, that the table should not be appealed to as *confirming* a *different* theory, when it is *at least equally* reconcileable with that which I have mentioned.

I cannot, however, help remarking, that the *anxiety* of the great Rameses to *prove* his descent from the royal stock, is, in my mind, a strong presumptive argument against his being the son of his predecessor, and of course universally admitted to belong to it. Not only did he cause this genealogical table to be constructed in his palace, but among the titles in his first oval he selected "of the race of the Sun*," in addition to the matter-of-course "Son of the Sun," *between* his ovals. It is remarkable, that in this particular he was afterwards imitated by Shishonk, who, it can scarcely be questioned, became the founder of a dynasty by usurpation or conquest†.

iv. *Hieroglyphical Tablet commemorative of Julius Cæsar.*—In M. Champollion's letter to M. Dacier, and Mr. Salt's Essay, there is a tolerably complete series of the names of the Roman emperors, from Augustus to Commodus. Some inaccuracies occur, indeed, in both of these publications, especially in Mr. Salt's ‡; but, in general, the readings published are correct. M. Champollion, in his Egyptian tour, has continued the series, having read the names of Severus and Geta; and it is probable, that some of the names "Antoninus," which occur on the temples, may be intended for the emperor whom we call Caracalla.

* So the concluding qualification is *now* interpreted by M. Champollion, as appears by his letters from Egypt. In his *Précis* he explained it to be "*approved by* the Sun." I presume he has good grounds for his present opinion, but am ignorant what they are; those assigned for his former one were so weak, that I never assented to it. I was disposed to interpret the unknown combination of characters "belonging to," or "a votary of,"—equivalent to the PT at the commencement of some proper names; but, considering the advantages M. Champollion enjoys in Egypt, I readily yield to his *corrected* opinion.

† The fact of Shishonk being the founder of a *new* dynasty, for which we have the express testimony of Manetho, explains an *apparent* inconsistency in the sacred writings, as has been already remarked in the Christian Examiner for January, 1829. Solomon married the daughter of Pharaoh; yet Solomon's enemy, Jeroboam, fled for refuge from him to the court of Shishak, king of Egypt, by whom he was favourably received. This Shishak, it is observed, was of a *new* family, unconnected with Solomon, and who had probably dethroned his brother-in-law. There may be some weight also in the remark which is there made, that the reason why the Jewish historians do not call this prince "Pharaoh," as they do the other kings of Egypt, was, that they considered him to be an *usurper*, and consequently to have *no right* to that title, which, without regarding its interpretation in the Egyptian language, was understood by foreigners to denote "the *legitimate* sovereign of Egypt."

‡ The principal errors in Mr. Salt's explanations are the following:—the two first shields in his fig. 19, are not "Marcus Verus," but "Lucius Aurelius," (LUKI AURLI;) fig. 21 is not "Adrian," but "Œlius," (ALI,) with a qualification that I may hereafter explain; fig. 23 is certainly not "Verus," it is badly executed or copied, but seems to have been intended for "Œlius," (ALIOS.) Mr. Salt was reluctant to read characters as L, which he considered to represent R; and he confounded the characters for A and U: the two latter are always distinguished in foreign proper names, while the two former are represented by the same characters indiscriminately. It is to these two mistakes that the false interpretations of the ingenious and lamented gentleman are principally to be attributed.

The series of hieroglyphical legends of Roman Emperors is thus extended from Augustus, or the Christian era, to the beginning of the *third* century. I am not aware, however, that a Roman name, prior to that of Augustus, has been, as yet, read in hieroglyphical characters by any one but myself; and yet it is a fact, that there is a tablet in the British Museum, bearing the name of the first and greatest of the Cæsars. The gentlemen who have published the valuable paper on Egyptian Monuments in the Transactions of the Royal Society of Literature, have, indeed, explained this name, which occurs in their thirty-fourth figure, as Verus, or Severus; they think, but are not positive, that the letters compose the word BEERES. The first, however, a *feather*, is A, E, or I; it has neither the shape nor the reversed position of the feather that represents S; the third is an L or R; the fourth an I; and the fifth and last an S. The last two characters, at the end of foreign proper names, universally represent IUS or ÆUS, never a simple US. The characters, therefore, whatever may be the value of the second of them, cannot possibly represent either of the names suggested in the paper; nor is there any which was borne by a Cæsar, (and the name which accompanies this is, unquestionably, "Cæsar,") which can correspond with the characters already specified, except AELIUS and IULIUS. The *second* character is a *leg*. In the name "Berenice," it represents a B, as well as in some other decisive instances; it is, therefore, unlikely, that it should come to represent an E, or rather to be superfluous, as would be the case if the *former* word were intended; but B, V, and U are similar letters, and easily interchanged; and, in point of fact, are *actually interchanged* in phonetic hieroglyphics. There can, therefore, be no objection to the name being read IULIUS; nor is there any other name to which it can correspond.

I have dwelt, perhaps, too much on the component parts of this name, but I was induced to do so, partly by the interesting nature of the discovery, and partly by the strangeness, as it must appear, of a name, which has been so long before the public, and which is written in an alphabet, which has been for so many years, to a great extent, known, not having been correctly read until now. Of the contents of the tablet I can say nothing, as I am not aware that a copy has been taken, and I have had no opportunity of examining the original. From the sketch given in the Transactions of the Royal Society of Literature, it would appear to be *votive;* and I have little doubt that it contains, at least, an *allusion* to the Egyptian victories of the Roman general, who is represented as kneeling before the sun and the god of war. It is to be hoped, that a copy of this interesting inscription will be given to the public in the next Number of the Collection of "Hieroglyphics," if not at an earlier period.

E—— H——.

K——h, February 22, 1830.

Effects of Animal Charcoal on Solutions. By Thomas Graham, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow.

The property of withdrawing matters from a state of solution, possessed by the charcoal of bone-black, has been investigated in the case of soluble colouring matters of a vegetable and animal origin. It is known, that the discolouring faculty resides entirely in the charcoal, for the earthy matters and portions of azote combined with it, possess by themselves no such power, and the charcoal discolours without them. This property is also greatly exalted by the state of extreme division and porosity of animal charcoal, arising from the interposition of foreign particles of earthy and saline matter between the particles of carbonaceous matter in bone, which effectually prevents the aggregation of the carbon during calcination. The bright, hard charcoal from the calcination of dried blood has no discolouring power; but the charcoal from the calcination of dried blood, mixed with carbonate of potash, as in the manufacture of prussiate of potash, proves the most efficient discolouring form of charcoal we possess, after the alkaline carbonate is washed out. A very intense heat, however, destroys entirely the discolouring power of bone-black.

The colouring matters are not destroyed or decomposed by the charcoal, but merely withdrawn from a state of solution, in combination with the surface of the charcoal, and may be again dissolved out and made to appear by the action of a more powerful solvent.

M. Lowitz first discovered this property of charcoal in 1791. He used only charcoal of wood. M. Guilbert observed, that the discolouring power of wood charcoal was improved by exposing it for a considerable time, in a wet state, to the rays of the sun. In 1810, M. Figuier, professor of chemistry at Montpellier, discovered that animal charcoal discoloured with much greater power. It has subsequently been used very extensively by the sugar-refiners of France in clarifying their syrups. Of bone or ivory-black, one-sixth of the weight of the raw sugar is boiled with it for ten minutes. The charcoal and impurities are separated by filtering, and the syrup is filtered a second time to separate a little charcoal which comes through the first filter, (Payen) In the *Journal de Pharmacie,* tom. iv., pp. 301—7, there is a distinct account of the mode of preparing bone-black, by M. Cadet de Gassicourt; and in the same work, tom. viii., pp. 257—277, an excellent memoir on charcoal, considered as a discolouring substance, by A. Bussy, which was crowned by the Society of Pharmacy of Paris, and contains everything known on the subject. It is followed by another memoir on the same subject by M. Payen, to which a second prize was adjudged. The substance of the preceding memoir is given in this Journal, vol. xiii., pp. 406—16.

But the action of animal charcoal on solutions has been considered hitherto only in reference to the removal of colouring matters. More determinate results, however, might be expected in solutions of saline and other chemical bodies, of which the composition is known. The investigation is also interesting, from the light which it may throw upon the state of combination in which bodies exist in cases of ordinary solution, as salt in water, to which the doctrine of definite proportions seems wholly inapplicable. If a solid body, such as carbon, destroy such a combination, and take down the saline matter attached to its surface, we may conclude that there is an analogy between the combination of the salt with the water, and the combination of the salt with the charcoal, and that the former as well as the latter processes have something of a mechanical character.

The same property is possessed by other solid bodies, in a state of minute division, as when newly precipitated, although not in so great a degree. And, in analytic researches, its interference must be guarded against, as it may contribute, in some cases, to increase the weight of precipitates.

The animal charcoal, employed in the following experiments, was prepared from common bone, or ivory black, by boiling dilute muriatic acid upon it, and afterwards washing it with hot water till the water came off tasteless. No more than ten or twelve per cent. of charcoal remained after dissolving out the earthy salts. On burning this charcoal, it left a grey ash, amounting to about one-twelfth of the original weight, insoluble in water and acids, and almost entirely silica. Charcoal, prepared in this way, M. Bussy found to go no farther in discolouring than one and a half times its weight of the original ivory black.

In my first experiments, it was found that the prepared charcoal, in great excess, had no sensible effect in impoverishing a saturated solution of common salt at natural temperatures. The proportion of salt remaining in solution was always as great as water was found capable of retaining, at the same time, at the lowest temperature which had occurred during the experiment.

A solution of nitrate of lead, with the charcoal repeatedly agitated, and occasionally tested with carbonate of soda, gave a distinct precipitate the first day, a much less distinct the second, and the merest trace the third day. But, on heating the water, the charcoal part of the nitrate was re-dissolved, and afforded a copious precipitate, with carbonate of soda and with sulphuretted hydrogen.

The dinitrate of lead, which is soluble, was taken down completely by the charcoal, so that no trace of it was perceived by means of sulphuretted hydrogen. But on heating the water over it to 200°, part was re-dissolved, as in the previous case, but again taken down completely by the charcoal on cooling. The action of the charcoal on the cold solution of the dinitrate was immediate, and much more energetic than in the case of the nitrate. The former salt, however, is much less soluble in water than the latter. Other soluble subsalts were tried.

2. Three grains diacetate of lead in one ounce water, with twenty grains common ivory black; taken down completely, and not re-dissolved in any degree on boiling.

Four grains trisacetate of lead; same results.

Four grains tartar emetic in one ounce water, with twenty grains of the prepared charcoal, in the cold; agitated occasionally for several days; still a copious precipitate, with hydrosulphuret of ammonia. After a second addition of twenty grains of the charcoal, only a trace of antimony, with sulphuretted hydrogen.

Lime-water was deprived entirely of the lime which it contains, in the cold, as Dr. Paris previously observed, so that the liquid remaining did not act on reddened litmus.

Arsenious acid was not taken down entirely in six weeks by great excess of the charcoal, no heat being applied.

No quantity of the charcoal could take down bisulphate of copper.

Ammonia was added in excess to bisulphate of copper, so as to form the deep-blue solution of ammonio-sulphate: the latter was readily taken down by the charcoal, and the liquid became perfectly colourless. Strong ammonia was digested in the cold upon the charcoal containing the salt of copper, and also boiled upon it, without dissolving a trace of it, as the ammonia did not become blue even when poured off and exposed to the air. In a certain experiment, the deep-blue colour of five grains bisulphate of copper in half an ounce of caustic ammonia, diluted with one and half ounces water, was much impaired by twenty grains of the charcoal. Increasing the charcoal every second day, by five grains at a time, with thirty-five grains, the colour had become very slight, and was entirely destroyed by forty grains; nor did the supernatant ammonia contain any protoxide of copper.

Five grains of nitrate of silver, in the same quantity of ammonia and water, with twenty grains of the charcoal. Next day no trace of silver in solution could be detected: two and a half grains nitrate of silver added; agitated occasionally with the charcoal, but after several days there was still silver in solution. On examining the phial containing the above materials some time afterwards, shining metallic spangles were perceived among the charcoal.

The solution of chloride of silver in ammonia was also taken down completely by the charcoal.

A solution was made of ten grains hydrated protoxide of lead in caustic potash, which was diluted with water till it amounted to three ounces. Twenty grains of the charcoal, added to the above solution, in a phial, which was then corked up, took down so much of the oxide of lead that the white colour of the latter substance was quite discernible among the charcoal. Here we have the colour of the charcoal disguised in the compound. Making successive additions of charcoal, the oxide of lead in solution was reduced to a trace by ninety grains; the last additions of charcoal floated over the heavy portion containing the oxide of lead; the supernatant solution, which had a greenish tinge, was poured off, and the charcoal washed, thrown on a filter, and dried at a heat which did not exceed 212°. When dry, innumerable metallic particles were visible in it; so that the oxide of lead is easily reducible by the charcoal attached to it.

The oxide of zinc was withdrawn entirely by the charcoal from solution in caustic ammonia.

A deep-red solution was made of five grains iodine in fifteen grains pure hydriodate of potash, dissolved in two ounces water. Forty grains of the charcoal were added before the colour of the iodine was wholly removed from the solution; the liquid acquired a faint acid reaction: the carbon was washed, and dried in a filter on the sandbath without exhaling any iodine vapours; but on heating it strongly in a flask by a lamp, iodine rose in vapour, and condensed on the sides of the flask with some moisture. The iodine was afterwards re-absorbed by the dry charcoal when cold.

Labarraque's disinfecting fluid (chloride of soda with bicarbonate of soda) may be boiled without being materially injured; but I was surprised to find that ebullition for a few seconds of a large quantity of that fluid, in contact with a few grains of the charcoal, completely destroyed its bleaching power.

The same effect took place in the cold, on agitating the fluid and the charcoal together for a few minutes. No gas was emitted in either case. On evaporating the saline solution to dryness, it was found to contain no notable quantity of chlorate of soda. Twenty grains of carbon were adequate to destroy the bleaching power of a pint of the disinfecting fluid recently prepared.

A solution of common bleaching powder, chloride of lime, was destroyed by charcoal with nearly equal facility, particularly when hot.

A pound of water, recently impregnated with an equal bulk of chlorine gas, was heated rapidly to the boiling point, in contact with twenty grains of the charcoal, in a glass flask provided with a perforated cork and bent glass tube, for the purpose of collecting any gas which might be given off. Gas was collected, but it was entirely carbonic acid, and most of the charcoal disappeared: muriatic acid was found in the liquid. On collecting the unconsumed charcoal in this and other cases, and washing it several times after being dried on a sandbath, it gave out a few drops of strong muriatic acid, when heated in a glass tube by means of a lamp.

Observations on the Mullets of the Coast of Guiana; and the Grey Mullet of the British Coast: with incidental Remarks on the Air-bladder and Stomach in Fishes.—By Dr. J. Hancock, Corr. Memb. of the Zool. Soc., &c. &c.

There are two species of Mullet in Guiana,—one called Queriman, and the other Trench Mullet. In the Queriman, the gill membrane is six-rayed; the first dorsal fin has four spinose rays at the middle of the back; posterior dorsal, nine soft rays; pectoral, sixteen; ventral, six,—the first ray sharp-pointed; anal, ten,—the three anterior spinose; caudal, twenty; upper lip protractile; head flat and blunt; shoulder broad; pectoral fins approaching the shoulder, connected below by a membrane; tail forked; scales large and rhombic, with lanciform scales also at the base of the back fin; no tongue nor teeth; the hyoide, or pharyngial bones, are large and rough, and nearly close the passage to the stomach, answering, as it were, the purpose of a strainer; body, above, dusky-greenish,—below, silvery; eye large, black, and prominent; grows to the extent of 28 or 30 inches in length.

In respect to internal structure, its *stomach* is very thick, muscular, and fitted for triturating its food—as one would say, from its gizzard-like structure; in shape, similar to that of the British mullet, but rather conical, rugous, or with many folds of its inner membrane; it has also a reservoir, or cœcum-like process, at its posterior part, containing chyle or chyme: it is flattened at the anterior part, where the gut and several small pyloric cœcæ are inserted into it. Intestines, in a number of convolutions joined together by a plexus of vessels; nothing in the stomach and intestines but mud and chyle; heart small and angular, close to the gills; liver, gall bladder, and pancreas large; the gall having a peculiar bitterness, which is warm and stimulant, and not unpleasant on the tongue. In a queriman of 26 inches the intestine was $8\frac{1}{2}$ feet long—nearly four times the length of its body.

It lives entirely by suction, frequents soft, muddy bottoms near the shore on the coast of Guiana, and escapes by leaping over the nets of the fishermen, as mentioned of the European mullets, to which they have a strong resemblance in their external form, as well as habit. At times they are seen in great numbers on the Pomeroon coast*, leaping out of the water to the height of two or three feet, whether in sportive exercise, or to escape some voracious fish, as the shark and byara, is not well known: the latter supposition is most probable, judging from their behaviour to the fishermen. The young of this species are at times found in the trenches, along with the Trench mullet.

The gall of this fish (as well as that of the shark and gilbagre) is said to be a useful ophthalmic remedy in amaurosis, &c., as mentioned of the uranoscopus, by some ancient writers; and to remove specks and nebulæ from the cornea, by letting fall a single drop into the eye once a day.

I have been told by natives of Barbados, that the *queriman* is very frequently caught around the coast of that island; but the species of this genus resemble each other so nearly, that we cannot depend on reports of this nature. I had never an opportunity to ascertain the fact; but, from what we are told of the remarkable transparency of the sea-water, and the sandy bottom around the island, it will probably be found to be a distinct species.—On the Guiana coast this fish is only found in the most turbid waters, or where there is abundance of *drift mud*, so called,—consisting chiefly of an argillaceous earth which is probably brought down by the rivers, floated

* I have known them to leap into the boat, and with such violence, during this turmoil, as almost to knock the rowers overboard, such is the extraordinary elastic power in the tail. In like manner, the first flying-fish I had an opportunity to examine, was one which flew on board the ship, about the latitude of Barbados.

about, and successively deposited in banks,—forming indeed the alluvial soil of the European settlements, and one of the most inexhaustible and fertile soils in the world.

In the Trench mullet, Mugil incilis, as we may designate this species, (being chiefly found in the trenches or ditches dug for draining the flat lands of the coast of Guiana), the scales are small,—the anal fin has twelve rays; grows to eight or ten inches in length; is of a lighter colour than the queriman, but otherwise differs very little from a young queriman of the same size; the structure of the stomach is also the same, being a sort of gizzard.

Like the latter fish, it lives entirely by suction. It delights in water that is slightly brackish; and although it is often found on the coast, yet a sudden immersion in sea-water soon kills it. I once observed, at Cape Batave, (the property of Mr. Gilgeous) on the west coast of Essequibo, great numbers of mullets swimming with their heads, or snouts, out of the water. On inquiry, I found that the front dam had given way in the night, from a high spring tide, and nearly filled the trenches with salt water.

It appears extraordinary that this fish, although it constantly inhabits the fresh-water trenches, is never found (not to my knowledge at least) in the *natural* pools or rivulets of fresh water; and I am not certain whether it is ever found in the proper salt water of the ocean,—for the water of the coast is seldom very salt, owing to the abundance of water brought down by the great rivers from the interior; it appears, indeed, like a sort of voluntary domestication, not like those shut up in ponds; this is not common in respect to fishes, or other animals, although there are many plants which make their appearance in certain places, only after the soil has been put under cultivation.

It is rather singular, too, that this small species should obtain the name of mullet amongst the English colonists, in preference to the queriman, which resembles much more correctly the British mullet.

Both species are very excellent viands, and constitute articles of essential importance as food in Guiana, both fresh and smoked.

The only obvious distinctions between the queriman and trench mullet, appear to be in the anal fin, and the scales on the back of the head; the anal fin in the queriman having only eleven, while the trench mullet has constantly twelve-rays. The scales on the back of the head of the former are marked with *concentric circles*, but the trench mullet shews no trace of this character. Its scales are smaller and quite smooth; the head is not so angular, is less flattened, of a lighter colour, and more delicate in appearance, *i. e.*, taking a full-grown trench mullet and a queriman of the same size for comparison: the scales in the latter are stouter, and much more developed. But in these respects, you require to compare them together, to observe the difference, and that with somewhat careful attention; being so near alike, that many think them the same species,—that the mullet is the young of the queriman, in the same manner as the white-bait (Clupea latulus, Cuv.) is sometimes mistaken for the young of the shad. The lips are protractile in both. I observed very fine setæ in the lips in both species, but less crowded in the mullet than in the queriman. The body of the mullet is more soft and flexible than that of the queriman, and its taste is also different, having a peculiar delicate flavour, different from that of other fishes. It has a gall-bladder very small, and oval; the queriman has a large, oblong, pointed gall-bladder; in both, the liver is situated close to the anterior part of the stomach.

The guiana mullets have twenty-four dorsal vertebræ, that is, if we include the fan-shaped bone of the tail. The grey mullet has the same; and Hill says the mugil cephalus has twenty-four vertebræ. It is probable, that all the true mullets have about the same number; a great similarity usually prevails in this respect in closely allied species; as, for instance, the three species of gadus, cod, whiting, and haddock, have each respectively, fifty-three, fifty-four, and fifty-five.

Specimens of the queriman and trench mullet are deposited at the Museum of the Zoological Society, in Bruton-street, from which drawings may be made. Both of these are fish of the finest flavour. The queriman is remarkable for having a very large roe, or ovaria, the finest and most delicious, perhaps, of any fish known; it is composed of two oblong, cylindrical bodies, slightly connected at the anterior extremities.

In July 1828, I, for the first time, met with a specimen of the common grey mullet of the London market, and was surprised to find it a non-descript species, or at least, not to be the Mugil cephalus, as marked in Turton's translation of the Syst. Nat. and as universally believed. Its gill-membrane, or gill-flap, as more properly called by Dr. Fleming,* is six-rayed, not seven, as stated in the Systema Naturæ; and Cuvier's *Elem. Hist. Nat.*; dorsal fins, 4, 8; pectoral, 18; ventral, $\frac{1}{5}$; anal, $\frac{3}{9}$; caudal, 20; 1st dorsal at the middle of

* I have here to acknowledge an error I had fallen into, in respect to this part; having taken the ossicles, or arched rays, for the gill-membrane, a mistake, owing to the bad definition of authors, some others have committed. I had inadvertently done this, by following one who has lately published a descriptive enumeration, and superbly figured many of the Demerara fishes.

" In every species that has yet come beneath the author's observation, the branchiostegous membrane has *uniformly* consisted of *four* bony rays. This construction appears to be universal in the increased temperature of the tropical waters, as the sea-fish have it equally with those of the rivers." See Hilhouse on the Indians of Demerara—and " Ichthyology of the fresh-waters of the interior," —p. 107.

It is a misfortune to be regretted, that we are almost as liable to embrace the errors of anterior writers, as their best truths. I might have adduced from the same author, on the Demerara fishes, most interesting observations respecting the cobitis anableps, the viviparous siluri, their singular habits, &c.; and of which, in an unpublished paper on the latter genus, I have not failed to take advantage.

It is not Mr. Hilhouse and myself alone who have fallen into the errors just alluded to, by a misapplication of the terms, or nomenclature of ichthyologists. I have found skilful anatomists here, in the same error. Had we merely been informed that the *gill-membrane* forms the lower part of the *operculum*, or gill-cover, no one could have mistaken it.

By Willoughby and all the great ichthyologists prior to Linné, as Aldrovand, Gesner, Aristotle, it was *this* part, the ossicles or gills themselves, and not the flap or cover which was so constantly alluded to as an essential part of the description—and these indeed furnish a much greater variety of characteristic distinctions than are obtained from a simple enumeration of rays in the gill-flap: for instance of the tunny, p. 177, he says, " *Branchiæ.* Radiosæ *branchiarum* carunculæ pectiniformes exiguæ sunt et rotundæ," &c. Of the shad, p. 227, " Branchiæ utrinque quatuor radiis pectinatis longis ex una tantum parte donatæ; ex altera tum globulis tum aristis carent." Of the river-trout, p. 199—" trutta fluviatilis—branchiæ 4, prima longisismis aristis ex parte exteriore pectinata, ex interiore nullas habet." And of the smelt, p. 202, " *Branchiæ* quaternæ simplici radiorum serie pectinatæ."

It may be remarked, that the Baron Cuvier has, in some measure, revived the ancient characters, and thereby rendered his method more full and satisfactory.

The great work of Cuvier, now in progress of publication, furnishes a more complete elucidation of this and other anatomical structures in fishes, than are to be found elsewhere.

the back,—all four of its rays are strong, sharp-pointed spines; 2nd dorsal, unarmed, consisting of soft divaricate twin rays, that is to say, composed of two pieces each, as in those of the caudal fin; tail, forked; the pectoral fins being set high up towards the back, pointing upwards and backwards; head rather conic, or tapering, not quadrate, as said of the M. cephalus; the callus at the corner of the mouth less obvious than in the queriman—lips thin and protractile, the upper one for near an inch; no teeth, unless the setæ in the lips may be considered as such; dark-greenish, grey back; silvery belly; about twenty inches in length (said to be a full-grown fish). The roe, or ovaria, are small, in proportion to the body; not half the size of the M. cephalus or that of the queriman.

This fish, the common grey mullet, is not described in the Systema Naturæ. It agrees in no respect with the M. cephalus, which is marked for it in the English translation; nor with any of the species there noticed, except in the position of the anterior dorsal fin, which is near the middle of the back, *i. e.*, equidistant between the snout and the insertion of the tail; a circumstance common, perhaps, to most of the species of this genus. Is this fish, then, (the common grey mullet) still a nondescript? or where is it described?

In its general contour, size, and appearance, it closely resembles our queriman, of the Guiana coast, the labranch of the French and Spanish colonists. On more minute inspection, however, we find it to be a very distinct species. Unfortunately, the entrails had been taken out, and I had no opportunity to observe the internal structure, which I was desirous of comparing with those of the Guiana mullets, in both of which, the stomach is a strong, muscular viscus, like the gizzard of gallinaceous birds; of the figure of a short cone. The gullet is inserted into it on the under and posterior part; the forepart is flat and circular, the centre perforated with the gut. Yet those fish live entirely by suction: no ingesta is ever found within them, except soft mud and mucus.

It is commonly supposed that the gizzard, or muscular stomach, of certain birds, has been given them as a means of grinding down the hard seeds on which they feed*; but it is difficult to conceive how any triturating power should be required for the substances which form the nutriment of these fishes. I think it is asserted by Spallanzani that he could never detect anything indicative of a triturating power in the stomach of fishes. It is doubtless a striking anomaly; yet it seems that a species of trout, the gallaro, is also provided with it.

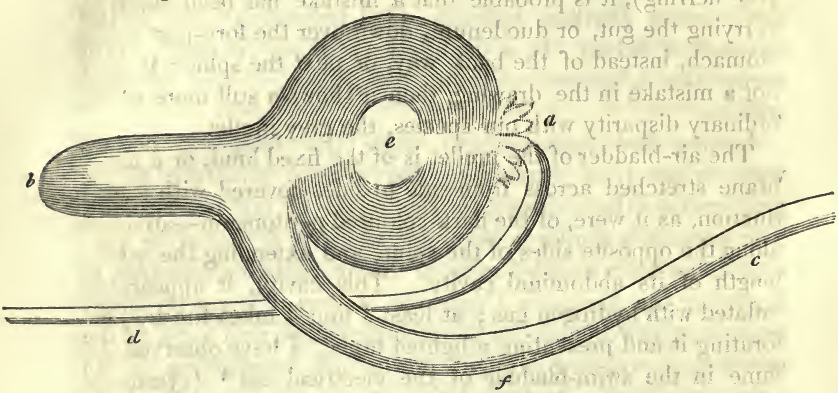
Having had opportunities, since the foregoing notices were written †, of examining the interior structure of the grey mullet, the following is the result of my observations. The stomach is similar in *shape* and *structure* to that of the queriman, but more rounded; that of the M. cephalus, however, as figured in the Anatomie Comparée, is very different in shape, being long and slender—a mere continuation, as it were, of the œsophagus—running straight down, and terminating in an acute *cul de sac;* its form being that of the stomach of the herring, to which M. Cuvier compares it (and is confounded, as he observes, with the œsophagus); having a round globular body attached to its middle, or at the pylorus, on which stand several slender radiated appendages, or cæca.

The stomach of the mugil albula is of similar shape with the cephalus, but rounded at the lower end, or *cul de sac,* and has no radii attached to the muscular ball. It is only necessary to compare the different forms of the stomach of the grey mullet and that of the M. cephalus, as given in the Anat. Comp., tom. v., p. 353, pl. 43, in order to show a specific difference which is more remarkable than in any of the external characters; distinguishing it most clearly from the M. cephalus.

* I observed the gizzard of a fowl filled with gravel, but not a grain in the whole course of the canal, either above or below. What becomes of the gravel swallowed by gallinaceous birds?—We never observe it in the fæces: some think it is digested, or worn down to a powder; but whoever examines the gravel will see this is not the case, as there are no signs of detritus, the pebbles, or gravel-stones, retaining their corners and sharp edges.

† The substance of this paper was wrote in July, 1828, and left at the Museum, with Mr. Vigors, the learned secretary of the Zoological Society. Since this period, I have seen many other specimens of the same fish; and I have met with no other species in London. Possibly, the M. cephalus (that from which the botargo is prepared in the Levant) may inhabit some parts of the British coast, but it is not probable, for, in that case, the rapid system of boating, and inland carriage, would bring some of them to the London markets.

The following is a sketch of the stomach of the grey mullet of the natural size, the fish being a young one, fifteen inches in length, as it appears externally and internally, supposing it to be transparent;—



a The anterior part; pyloric cæca, six or eight in number; being reservoirs of chyle, or where chylification appears to be completed.

b The posterior part, cul de sac, or first receptacle of the food.

c The gullet, or esophagus.

d The intestine.

e The central cavity of the stomach, or gizzard, surrounded by its thick muscular wall, which has almost the consistence of cartilage, and has the peculiar colour, density, and firmness, of the gizzard, or ventriculous bulbosus in birds; its lining membrane is rugous, and peels off, as in those of fowls.

f The part next the spine, the figure being reversed, as the fish laid upon its back when opened.

The intestine, sixty-eight inches in length; much sand in the stomach—*i. e.*, in the first and second cavities only!—not a particle of sand could be found beyond this, or in the whole course of the intestinal canal, which below was filled with a thick ingesta that seemed as it were only inspissated chyle, like thick cream; for it loses its colour entirely from the entrance of the duodenum, or from the pyloric apparatus, reservoirs, or cæca. These reservoirs are, in a mass, united at the base, digitated or divided into several lobes, which are unequal, and surround the insertion of the duodenum; the gut, running up between the spine and the stomach, is inserted into the anterior part, or that next the mouth. In the M. cephalus, it consists, according to the figure, of four slender rays, which are distant and entirely unconnected (see fig. 23, pl. 43, tom. v. of the Anat. Comp.). In this, as well as fig. 24, and perhaps 19 (the herring), it is probable that a mistake has been made by carrying the gut, or duodenum, down over the fore-part of the stomach, instead of the back way, or next the spine: if it be not a mistake in the drawing, then it shows a still more extraordinary disparity with our species, the grey mullet.

The air-bladder of the mullet is of the fixed kind, or a membrane stretched across from side to side, covered with a production, as it were, of the dark-coloured peritoneum—attached along the opposite sides of the spine, and extending the whole length of its abdominal cavity. This cavity, it appears, is inflated with hydrogen gas; at least I find it takes fire by perforating it and presenting a lighted taper. I have observed the same in the swim-bladder of the electrical eel * (gymnotus

* The air-bladder in this fish is large and perspicuous, although said by Bloch to be destitute of this viscus. I may also notice here that many errors of this kind are fallen into by Linnæus, and former writers, even by the most distinguished naturalists of the present age. MM. Blumenbach and Cuvier observe, that the common mackerel (Scomber scombrus) is destitute of the swim-bladder: and they assert the same of all the pleuronectes, and several others. In these, however, I find the swim or air-bladder; that of the mackerel is precisely of the same kind, although smaller, as in the mullets—*i. e.*, a fixed bladder, as it were a duplicature of the peritoneum, and similar to what we observe in the gadus, or cod kind.

But what is rather singular, this fish (the mackerel), in which the greatest naturalists deny the existence of such an organ, contains, in fact, not one merely, but two air-bladders, running one above the other immediately under the spine; they are nearly cylindric when blown up, for they are commonly found empty; having the usual attachments—*i. e.*, on each side the spine—to the ovaria, or, in males, to the milt, to the gills, and anterior dorsal vertebræ; they seem to be distinct, or without communication, as appears by inflating each cavity alternately with a quill or small tube. I had previously observed the same structure in a sparus, sent me by Mr. Hempson, No. 304, Oxford Street, in May, 1829, who called it a silver starling. It appears to be a variety of S. pagrus, or a near species; not yellow, as in the gilt-head, but the whole body silvery. The same, also, was observed in a species of clupea which was sent to me for a shad.

The remora, or sucking-fish (echeneis), the mullus, the cottus, and calyonymus genera, &c., are said to want this organ. Several of these, it is true, I have either neglected, or had no opportunity to examine; having, however, found the air-bladder in most of those fishes in which authors deny its existence—in all those, at least, which I have examined: I am inclined even to doubt whether any true fishes exist which are not provided with some hydro-pneumatic apparatus of this kind; in its form and structure exceedingly diversified in different fishes. I even find it in the lamprey, a fish presenting one of the most simple species of internal organization. It consists, in this fish, of a slender tube, nearly the diameter of a goose-quill, running along under the spine, and having the same attachments as in the mullet, the cod kind, &c.: in all, it occupies the part next the spine. Here Creative wisdom appears in a clear light: the buoyant power thus preserving a electricus), as also in the cod. It burns with a brilliant flame, giving out the smell of gunpowder in its combustion. The varieties exhibited in the form and structure of this organ, in different fishes, are very curious. In many, it is an extremely thin, light, and pellucid membrane, or *bladder*, properly so termed; in others, it is thick and spongy, or of a cellular structure. In the gilbagre, it seems as it were a solid mass of glue, weighing at least a pound in a full-grown fish*. At the same time, it may be much inflated by blowing with a quill, inserted into the anterior part, where it appears to be connected with the gullet, and the gill apparatus.

In some fishes the bladder is not obvious, until inflated, and this I have recently found to be the case with respect to the cottus scorpius:—On opening this fish, I observed, as I thought, an ovarium, which was attached by a membrane along the spine, and by some cords to the anterior dorsal vertebræ. Having observed this to be the usual attachments of the air-bladder in other fishes, although there was no appearance of a bladder in the present instance, I was induced to insert a tube on the side next the spine, by which means the part I had taken for an ovarium, or a membrane in contact with it, was

due position in the water; as, in loading a vessel, no one would place the lighter articles at the lower part of the hold.

In the pleuronectes, as in all other fishes, the bladder is placed next the spine, or vertebral column; but the spine in these fishes approximates nearer the anterior part of the abdomen than to the back, which brings the bladder likewise somewhat forward of their centre of gravity; and this may be the reason why these fishes swim on *one side* and affect the bottom—at the same time their extended fins enable them to preserve their horizontal position and equipoise in the water. I may further observe, that what is here stated is more to fix the attention of naturalists to these apparently neglected points, than to enunciate any opinions of my own.

* It might afford a profitable article of trade taken to Europe, one sound containing as much fine ichthyocolla as is sold in London for ten or twelve shillings; and the fish may be caught in vast abundance on the coast of Guiana.

The gilbagre is a viviparous silurus; its colour is yellow, which can be easily wiped off, as the colouring substance consists merely of a coat of slime or mucus, which invests the body of the fish. A most extraordinary kind of articulation exists in the junction of the strong spine of the anterior dorsal fin with the scapular bony helmet; forming as perfect a hinge as could be made by art—and in respect to composition, the two pieces are quite entire and inseparable, like two links of a chain. This catenated joint (unknown to anatomical science) may perhaps be found in some others of the armed siluri, but I have not observed it.

Two bones of great hardness and density lie in contact with the brain—doubtless of the kind which are considered as the ossicula auditus, and contributing to the sense of hearing; although they are more than thrice the size of those in the cod, which, M. Cuvier says, are larger than in any other fishes; each bone weighing in a full grown fish about seventy or eighty grains—not twenty, as I had stated by mistake in a former paper.

instantly inflated into the most perfect bladder—bent behind and extended forwards in two lobes, having an oblong horseshoe form.

Being one of those fishes which are said to be destitute of the swimming bladder, I forwarded it, with the viscus inflated, for the inspection of Mr. Bennett, of the Zoological Society; he stated his belief, that it was the *urinary* bladder, which is represented by M. Cuvier, in his present work, as very large in this fish. This opinion may be correct,—it certainly comes from the best authorities, but the viscus appeared to me to be destitute of all the analogies of such a viscus,—and possesses all those of a *swimming* bladder; it is extremely thin and transparent, and attached by cords, as before observed, to the *cervical vertebræ,* or at the gills,—a situation where I have constantly observed such attachment of the air-bladder; but in no instance have seen a urinary bladder so connected. It is true, having no idea of this sort in mind, it did not occur to me to search for urethra or ureters; but, had such existed, they would probably have been detected in blowing up this receptacle.

An elderly person at Billingsgate, a few days since, observing me busied in examining the viscera of certain fishes, wished to know what the object was; I told him it was to learn the internal structure,—of the intestine stomach, &c.; and the air-bladder—this he said, although every one had it, yet I should not find it in all; for, in some fishes, it is so *thin* and *tender*, as to break when the fish is caught.

This was a person having no affectation of *science*, but of long practical observation, which is infinitely better, and who has been employed either in fishing, or at Billingsgate market, for upwards of twenty years past. He further added (and I thought it a shrewd remark, whether it be exactly true or not), that in such fishes, as soon as taken out of the water, the *air rushes* in, and causes the bladder to burst. The poor knowledge possessed by physiologists on this point will, I presume, hardly justify a contradiction of this simple statement of the fisherman.

It appears to me, that, in the cetaceæ or aquatic mammalia, this organ is supplied by the peculiar structure and magnitude of the lung, which has capacious air-cells, lined with a strong capsular membrane, are of firm texture, unlike those of quadrupeds, and invested by a strong tunic which enables this viscus to support the requisite hydrostatic pressure.—In the manati of the interior of Guiana, the lung is one-third the length of the whole body—in its texture similar to the swim-bladder in the gilbagre. M. Humboldt, reflecting on the size of the lung in the manati, thought it strange the animal should be compelled to rise so often to blow or spout water; vide Pers. Nar., v. iv., p. 448. But it seems to me a fact sufficiently evident, that the lungs in those animals answer the double or rather triple purpose of respiration and oxygenising the blood, and as that peculiar kind of gasometer or buoyant bladder found in the true fishes. This will, I presume, be admitted by competent judges as the most rational view of the subject; and in which there is nothing strange, unless it be, that so many sagacious physiologists should have passed the affair quite unheeded.

This fish (the grey mullet) appears to be much more nearly allied to the mugil albula, as given by Linnæus, than to the cephalus; and it may be observed, that the different species of this genus resemble one another so closely, that they are with difficulty distinguished by the external characters, unless we except the *number of rays in the fins.** Inattention to such circumstances has led naturalists into frequent errors.

Another character which has been recommended as affording the most certain distinction, I find to fail here, that is, the number of bones in the vertebral column; in several cases, distinct species are found to have the same number. However valuable this may be to the anatomist, as a specific character, it is not applicable to general use, as, for the progress of natural history, more ready and obvious external characters are requisite.

Pennant's figure (Br. Zool., vol. iii., p. 436) appears to be

* With respect to the numerous *siluri* of the tropics also, it is quite impossible to distinguish them by other means. I have found the number of rays in the fins to be remarkably constant, both in these two and in other genera, although some assert the contrary.—The method of those, therefore, who neglect such specific characters must be very defective, especially for genera consisting of numerous approximating species.

that of the grey mullet, and a good delineation. At the same time, we may observe, it would answer equally well for the queriman, (and doubtless for other species,) their resemblance being so exact: his description is partly applicable to this and partly to M. cephalus; the two species being evidently confounded.—This, the common grey mullet, or M. britannicus* and the queriman, are remarkably near species in shape, size, and colour. The number of rays of the fins afford the only well-marked difference, and in this respect they approach very near: their disparity, however, with M. cephalus is very evident on comparison with its description or assigned characters.

I have since consulted the "Règne Animal" of Cuvier, and, from what I observe there stated, it appears probable there are other species besides these two confounded under the same name. He says there are three species met with in the Mediterranean, which resemble each other very much, one of which is the M. Cephalus of Linnæus; he also alludes to the existence of a thick carneous stomach, p. 292, tom. ii. (1st. ed.) It is probable that this carneous or muscular structure of the stomach is, in some measure, common to all the true species of this genus.

In Fleming's recent and valuable work, British Zoology, the M. cephalus is likewise indicated as identical with the common mullet, with some alteration of the characters; but if Linnæus's description be anything near correct, the mugil cephalus and the common British mullet are two very distinct species. In the Systema Naturæ, the rays of the fins stand thus:—

M. Cephalus	Dors. $5-\frac{1}{9}$,	Pect. 16,	Vent. $\frac{1}{6}$,	Anal, $\frac{3}{12}$,	Caudal 12.
In the Br. mul. they are thus	4—8,	18,	$\frac{1}{5}$,	$\frac{3}{9}$,	20.

Being favoured by the kindness of Mr. Wood, of the Strand, with the loan of Willoughby's "Historia Piscium," I observed that justly esteemed author has referred to certain *interior waters of France,* as situations where the mullets are caught, and describes the form of the trap, made of reeds,

* It may be so called, I presume, since it is the only one known or usually caught on these shores.

which is used for this purpose:—" Locus ubi capiuntur. In lacum quendam propries *Martegues* in *Gallia* magna vis mugilum quotannis stato tempore, nimirum exeunte vere ingreditur," &c., p. 275. The species here alluded to, however, must be regarded as very uncertain; we can hardly suppose it identical with our common grey mullet, which, I believe, is chiefly caught only at sea, and that far from land.

In respect to the number of the rays of the fins, in his statement, they are as follow:—

Dorsal, $\frac{5}{11}$; Pectoral, 18; Ventral, $\frac{1}{5}$; Anal, $\frac{3}{10}$;

which compare with those at page 136. The *small muscular* stomach and *appendices* are also mentioned. This description appears chiefly to be taken from Rondelet, and not from his own observation. He adds, " Alga tantum ac herbis mugilem vesci aiunt, vel limo. Piscibus certe omnino abstinet. Amnes subit testibus Aristotele, Strabone, Galeno, Plinio. Idem nos experientia docet; nam in *Garumna, Rhodano, Sequana, Ligeri,* capiuntur. Rondeletius."

I imagine that, from some cause or other, the mullet was seldom brought to this market in Willoughby's time, otherwise that industrious author would have made us better acquainted with it. Others, mentioned by Rondelet, are not sufficiently defined to enable us to form any tolerable opinion as to the species.

The grey mullets are said to be taken abundantly at Torbay, and off the Isle of Wight, frequently along with the mackerel, with which it appears to associate, or to have similar haunts and habits; that they enter the mouths of rivers, especially in a dry time when the water is brackish, in the Thames up to Woolwich, and the Medway to Rochester bridge, &c.; but I do not know if the grey mullet ascends by the small streams, into ponds and stagnant pools, to breed or deposit its ova in the manner of that described by Willoughby?

Respecting the *M. cephalus* we find many allusions amongst the Roman writers, which may be ranked with those cited by M. Cuvier, and in Dr. Brewster's *Journal of Science* for January, 1830, vide pp. 63 and 64, respecting " the mullets of

Europe," not the genus under consideration, but the *Mullus*, or surmullet kind, the two genera having no affinities but in name. Such, however, I conceive, are scarcely worthy the notice of the naturalist, or man of science, as, for instance, the puerile caprices, or downright idiotcy, displayed in their disgusting expressions of pleasure, or affected delight, at the sight of the dying mullets, with all the epicurean affectation of a debauched and effeminate people.

The name of Cephale, or cephalon, was probably given it by the Greeks in reference to the size of the head, as it were, great head, (macrocephalus,) or more analogous to the attributive nouns in the Spanish, as from cabeça, the head, they say cabeçón, one with a great head. It has been suggested, however, by some of the older writers, as deriving its name from the alleged property of causing headache when eaten at supper.

The head of the M. cephalus is represented square, with a blunt snout, and several peculiarities in which it emulates the Guiana queriman.

Since the foregoing observations were written, I have been rather surprised to find, that a very considerable number of the British fishes are either mistaken or ill described;—even the common shad of the river Thames, if not a nondescript species, is at least *mistaken* for the Clupea alosa of Linnæus. Indeed it appears there are two or three species on this coast confounded under the name of Shad. Cuvier says the shad (Clupea alosa) is without teeth, and has a single black spot behind the gills.

A Description of Commander Marshall's *new mode of Mounting and Working Ships' Guns, &c.*—Murray, Albemarle-street, London.

Considering that manual power is nearly a constant quantity, and that dexterity in the use of instruments is acquired by practising with them, it will appear that the efficiency of naval force depends chiefly upon two concurring circumstances, *viz.*, the excellence of the ships of which it is composed, and the powers of its artillery equipment.

The military qualities of ships of war will therefore be in proportion to the knowledge and exertions, generally, of two distinct classes of persons,—those who construct them, and those who have the control, contrivance, and management of their warlike apparatus.

The business of the naval constructor is to furnish a ship of a given force with the best sea-going qualities, amongst which, velocity and celerity of evolution should be of pre-eminent consideration. He is, in fact, the contriver of a vast locomotive machine, by which a warlike apparatus is conveyed to a certain sphere of action; but the efficacy of this apparatus depends, not only upon its intrinsic powers, but also upon those who handle it and manœuvre the ship. Now, admitting the skill, manual strength, and dexterity of these agents, it ultimately appears that *the effect of this apparatus must depend on its own capabilities.*

The capabilities of naval artillery include its quality or calibre; its range; the celerity with which it may be served; the power of easily directing its fire on remote points or passing objects; and the safety with which it may be attended.

No one, we presume, will pretend to doubt that the larger the calibre, and the longer the range, the more destructive will be the blow, and the wider will be the extent under command; nor will any one scruple to admit, that the crew which handles its guns with the most celerity (supposing that accuracy of fire is not sacrificed) will stand the best chance of coming off victorious: we shall proceed, therefore, to examine how far this object, and the no less important one, the power of directing the fire of a ship on remote points and passing objects, have been, until now, effected; for it is by an entire departure from old principles that the author of the work before us has effected decidedly the most important improvement that has yet been made in the means of rendering the heavy ordnance used on board ship easily manageable, and equally efficient, in every position.

We are informed by Froissart, that guns were first used at sea by the Spaniards in the year 1372*, that is, twenty-six years after Edward the Third had profited by them at the battle of Cressy, which was the first occasion that these terrible engines of destruction had come into anything like general notice in Europe. The more general introduction of cannon into naval combats took place in the wars between the Venetians and Genoese. The latter part of the fourteenth century was distinguished by the most rancorous contests between these rival maritime states; and no doubt a novel instrument of warfare, as the cannon must then have been, was taken with avidity to assist in settling their dispute for naval supremacy.

As the first guns were but very small, little difficulty and labour were experienced either in transporting or managing them in action; but, as their use and power were increased, it became necessary to make them much heavier; and hence additional contrivances became requisite to make them transportable and manageable. The commonest experience taught the first contrivers of these weighty engines of destruction, that there was no other means of transporting them readily from place to place, but upon rollers, trucks, or wheels, the adoption of either of which methods depended upon the degree of locomotion that was required; the two first being sufficient when the distance to be moved through was small, and the last being evidently the best calculated for assisting in military operations in the field, and passing over great distances. In all these methods, however, of rendering pieces of ordnance moveable by means of sledges with rollers, trucks, or wheels, we find that, as it was necessary to give them a different direction in a vertical plane, a contrivance by which this might be readily effected, was common to all. This consisted in making them move round a horizontal axis at right angles to the axis of the piece, and placed in such a position as to have the weight towards the muzzle of the piece nearly equal to that towards the breech. Such necessity gave birth to the addition of the trunnions, which are to be found in almost all the ancient pieces that have been preserved.

We perceive, therefore, that the mode of mounting guns naturally suggested by these wants, is obviously the same in principle as that which is used to this day, and we venture to say, that the present standing garrison-carriage has varied

* In a battle off Rochelle with the English and their allies, the people of Poitou.

but little, even in detail, from that in use upwards of three and a half centuries ago;—that is, the gun is placed between, and allowed to rest by, its trunnions, on the edges of two upright thick slabs of timber, extending from a little beyond the trunnions to the hind extremity of the gun, and connected with and supported by two massive axletrees, on which iron trucks or small wheels are placed *. The part of the gun in the rear of the trunnions, which slightly preponderates, is supported by a smaller slab, placed on the axle under the breech, and the elevation or depression is regulated by a wedge inserted between this and the breech.

It is most probable that the guns used on board ship were at first entirely of the lighter natures; for, until the invention of port-holes, their use was confined to firing over the gunwales of the gallies in intervals left between the oars. Such were merely lashed on a skid, or block of wood, hollowed out to receive them—a method of mounting which probably continued down to the middle of the sixteenth century, the lighter natures of the ordnance of that famous English ship, the Harry Grace de Dieu, built in 1515, being mounted in that rude manner.

When the invention of port-holes had rendered it possible to carry heavy guns on one or more decks between the gunwale and the water, it is clear that, as these were capable of being transported through the greatest distances by the ship itself, the requisites necessary to the mounting of guns for shipboard would appear very naturally to be those belonging to garrison-guns, with some *obvious* additions, for the purposes of restricting the recoil, and for working and securing the guns in an agitated sea.

Such were, doubtlessly, the reasons which introduced the land gun-carriage, used in permanent defences, on board ship, with the addition of two or three ring and eye bolts in its sides for the breeching-ropes and tackles used in the service of the gun, and with its iron trucks exchanged for wooden ones; and, strange to say, that not an improvement of importance has been made since their first introduction, more than three centuries ago. They remain in principle just as they were: a few necessary alterations in the detail may have been made, but that is all. Upon this singular fact, Captain Marshall, in his introductory remarks, very naturally observes,

"That either the established carriages answer, in the most effective manner, all the purposes required of them, or that, from the principle upon which guns are mounted, they are incapable of improvement."

* See diagram in page 146.

Which of these two suppositions is correct, our author, we think, incontrovertibly settles in the following passages, by which it will appear that, however sufficient the common standing garrison-carriage may be in a battery on shore, whose point of defence or attack is determined by the direction of the embrasures, the same, when removed on board ship, is, and must be, most lamentably deficient.

"In the first place, a ship's gun requires to be moved quickly in different directions, and is subject to be checked very suddenly in its motions. It is, nevertheless, placed upon a carriage whose axletrees are immoveably bolted parallel to each other, and upon which its weight is so disposed, that nearly the whole of it rests upon one extremity of the carriage. When a gun is so improperly placed, upon a carriage so imperfectly constructed, the inconveniencies in working it, as regards the violence with which it tilts up, and the difficulty which exists in laterally moving the fore part of the carriage, are consequences which naturally follow."

The author then observes, that as an action becomes prolonged, and the guns get heated, these difficulties become increased; whilst, of course, a less number of men, and those comparatively exhausted, are left to contend with them, and the difficulty with which the fore part of the carriage is laterally moved, necessarily renders the oblique pointing of a ship's guns very slow. Upon this most important particular Capt. Marshall observes,

"It is evident, that the further guns can be pointed towards the bow or quarter, the more powerfully will they defend the ship and assail the enemy. It will consequently be supposed that the size of a port will favour the limit to the angle at which a gun can be trained or pointed across it; but this is not the case: for the form of the old carriage generally prevents the guns traversing through an arc so great as might otherwise be obtained, by from 18° to 24°; and thus, as it were, the application of the instrument is limited by the clumsiness of its handle."

And with regard to the *accuracy* of fire, our author remarks, that

"In order to discharge a correct shot from a ship in motion, the elevation of the gun, and the line of its direction, must both be accurately adjusted at the instant the gun is fired."

This again serves to expose the deficiency of the old carriage; for, whilst the elevation (as Captain Marshall observes) is undergoing constant change by the rolling of the ship, the line of direction is also altered by the ship's locomotion.

The comparatively little destruction made in naval combats from such immense quantities of *heavy* ordnance, has evidently proceeded from difficulties naturally attendant on their service, from the extreme defectiveness of the common system of mounting, which absolutely renders firing guns on board ship, in an agitated sea, at an opponent, almost a waste of powder and shot. Captain M., in developing these causes of inaccuracy and non-effect, very justly says, that

"it is absurd, as a principle, to calculate that the rolling of a vessel shall produce a proper elevation, at the same instant that the motion of its head is not interfering with the true line of the gun's direction;" and hence, "it becomes necessary that, whilst taking aim, the marksman should have the power of changing the elevation, or the line, of his gun, at the same instant that the motion of the ship may be producing the other required movement; and also, that he should be able to fire at the moment these joint operations have produced their desired effect."

Those who are ignorant of the difficulties *peculiar* to the service of *naval* ordnance, will, no doubt, think that the foregoing observations are nothing more than elaborate in exposing minute defects; but in reality the causes here brought before us by Captain Marshall, trivial as they may appear, are those which chiefly conduce to the general inaccuracy and consequent non-effect of a ship's fire.

Captain M., in winding up this able exposition of the properties required in naval ordnance, comes to the obvious conclusion, that,

"contrary to all other artillery exercises, it hence appears that the practice of naval gunnery requires an art similar to that of firing at moving objects; . . . . and, consequently, before any general system of correct firing can be hoped for in cases of this nature, seamen must be provided with a gun-carriage better calculated than the old one to meet the peculiar circumstances in which they are so frequently placed."

A short but very able discussion then follows, in which it clearly appears, from mechanical principles, that it is impracticable to obtain, with the present adaptation of the standing garrison-carriage, the qualities required for the proper service of a ship's gun.

It appears, therefore, that to produce the desired facilities, either a carriage upon a perfectly novel principle must be adopted, still mounting by the trunnions, or else a modification of the present carriage must be effected by bidding adieu to the ancient method of mounting.

Adhering to the old practice, many ingenious men have endeavoured to form a gun-carriage adapted to the wants of the naval service, but they have generally failed in some important particular. Gover, and the celebrated Swedish naval architect, Chapman, who both adopted inclined slides*, certainly effected in a measure the objects proposed, but at the expense of simplicity and lightness †. Sir W. Congreve also turned his talents to the same important subject, and certainly produced a carriage wanting in nothing but stability ‡. He placed trucks on the trunnions themselves: these trucks moving in and out on a traversing platform attached to the ship's side, gave, in conjunction with the traversing motion, every facility to the simple service of the gun. Had he been acquainted with the disagreeable accidents liable from want of base, no doubt he would have even remedied this sole defect; but even then there would have been this very insuperable objection to the general and immediate adoption of his system, viz., that the enormous quantity of carriages always kept in store for our navy must all have been thrown aside. Captain Marshall, by adopting *two* points (or rather *axes*) of support for the gun, of which one is shifting, instead of the single one, the axis of the trunnions, has been enabled by a very simple modification of the *old* carriage to increase all its *good* qualities, and to deprive it of all the serious defects that rendered it so inapplicable to sea service. But here our author shall again speak for himself, in his usually clear and demonstrative style.

"The new gun-carriage consists of two distinct parts, whose movements are independent of each other; and which, though jointly supporting the gun, have separate duties to perform: one is termed the breech-carriage, the other the breast-carriage.

"The breast-carriage A, in figure 1, consists of a block of elm, in which two plates of nearly similar form are let in flush upon the top and bottom surfaces, and secured in their places by the clenched bolts, 1, 2, 3, 4, (fig. 1 and 2). The upper plate is made much thicker than the lower one, and the bolt 1 stouter than the others. By means of these plates, termed eye-plates, the breast-carriage is attached to the centre of the port, by the breast bolt G (figs. 1 and 3), passing through the holes E, E, and through the gudgeons C, D, which are fixed to the ship's side. In the eye-plates, the holes X, X (figs. 1 and 2), form a socket in which the spindle of the crutch H (figs. 1 and 4) works; a hole having been made through the breast-block sufficiently large to prevent the spindle of the crutch from ever becoming tight in it. Bolted to the under part of the breast-block, is an iron axle, *d*, upon which a bushed wooden truck works, and the end of the breast-

* Chapman's slide traversed on a pivot connected with the deck.

† Gover's 24-pounder carriage was more than 5 cwt. heavier than the common 24-pounder carriage.

‡ Congreve's carriage would be about 2 cwt. lighter than the common carriage for a 24-pounder. Vide Congreve's "Elementary Treatise on the Mounting of Naval Ordnance."

carriage traverses. The upper gudgeon C, is fixed to the ship's side, by bolts passing through the timbers, (figs. 1, 2, and 3,) and the top eye-plate rests upon it. The lower gudgeon, or a socket placed upon the waterway, as most convenient, is only required to steady the heel of the breast-bolt, and does not support any of the weight of the gun."

Fig. 1.

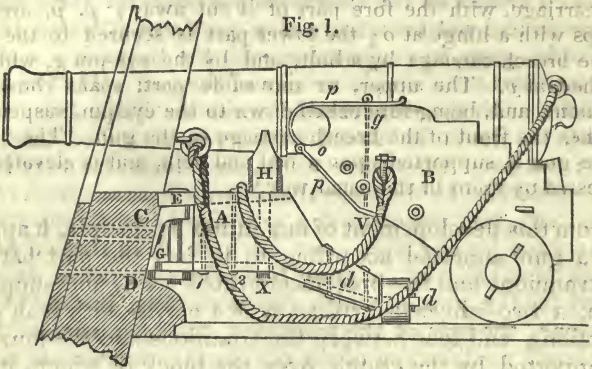


Fig. 2.

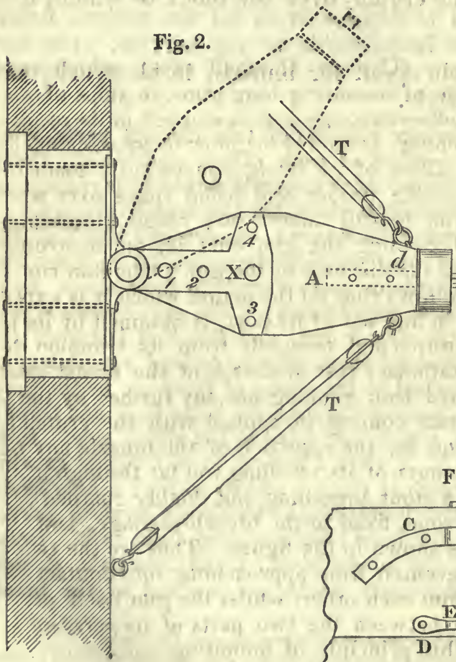


Fig. 4

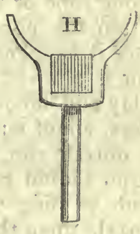
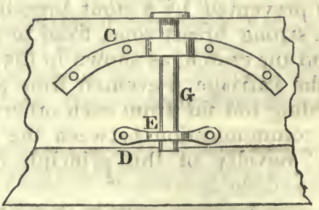


Fig. 3.



" The crutch H (fig. 4) is formed of wrought iron, and receives within the lower part of it a small block of wood, upon which the gun rests and works. The block is made a little concave, to receive the gun; and, in the other direction of its upper surface, is so cut away, or made convex, as not to interfere with the gun's lodging on nearly the centre of the block, when it is elevated or depressed.

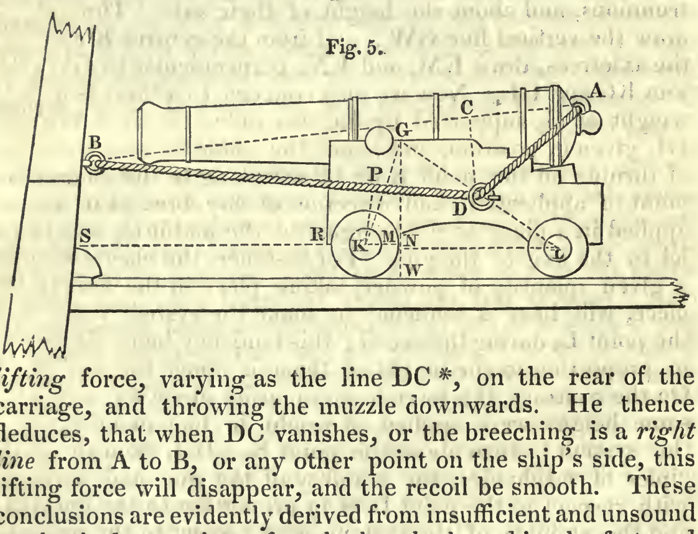
"The breech-carriage B (fig. 1) is formed similarly to the old gun carriage, with the fore part of it cut away; *p*, *p*, are iron clamps with a hinge at *o*; the lower part is secured to the front of the breech-carriage by a bolt, and by the eye-pin *g*, which is clinched at *v*. The upper, or moveable part, spans round the trunnion; and, being forelocked down to the eye pin, suspends or attaches the front of the breech-carriage to the gun. The breech of the gun is supported upon a bed and coin, and is elevated and depressed by them in the usual way."

From this developement of our author's principle, it appears that a gun mounted according to it, has the part between the trunnions and the breech connected with, and supported upon, a two-wheeled carriage, which moves with it in every direction; and that between the trunnions and the muzzle it is supported by the crutch, over the block of which, it runs in and out.

"The circumstance (Captain Marshall says) which renders practicable this mode of mounting long guns, in ships of war, is, that contrary to land service, the gun is stopped in its recoil by a strong rope or breeching, as soon as the muzzle arrives sufficiently within the port to allow of its being conveniently loaded; for since it happens that the proper and usual space over which a gun is allowed to run in (all unnecessary recoil increasing the delay and labour of running the gun out) is, on an average of cases, about equal to the distance of the gun's trunnion rim from its muzzle rim, it follows that all the action which it is expedient a gun should have, in and out of its port, is obtained by its being made to slide backwards and forwards from its trunnion to its muzzle upon the stationary rest or crutch of the breast-carriage. The gun is prevented from running out any further by the trunnions or trunnion rim coming in contact with the crutch; and when the gun is run in, the approach of the muzzle any nearer the crutch, or the danger of its recoiling too far through it, is not only prevented by a stout breeching, but doubly guarded against by a strong breast rope, fixed to the breech-carriage, and passed round the crutch, as shown in the figure. Thus are the two parts of the carriage prevented from approaching into contact, or of receding too far from each other, whilst the gun itself preserves the communication between the two parts of its carriage. The chief novelty of this principle of mounting consists in having removed the bearing of the gun upon its carriage, from the trunnions (which have now nothing to do with supporting the gun), to a fixed point at the breech, and a moveable point somewhere between the muzzle and the trunnions."

From this description of the principle, it plainly appears that the crutch not only becomes the axis of revolution in elevating or depressing the gun when run out, but also supports the muzzle and prevents it from dropping at the instant the recoil is stopped by the breeching, which mischievous tendency is very strongly developed in the old construction of carriage, and more especially so from the bad manner in which the breeching is applied. Much erroneous discussion has been put forward on this disturbance in the recoil of a ship's gun. Sir W. Congreve, in his "Elementary Treatise on the Mounting of Naval Ordnance," attempts to account for it by the breeching being led so as to make two sides AD, DB, of a triangle, of which the third side, AB, is the right line drawn from the loop at the breech of the gun to the ring-bolt B, in the side of the ship;—thus, he says, exerting a

Fig. 5.



lifting force, varying as the line DC *, on the rear of the carriage, and throwing the muzzle downwards. He thence deduces, that when DC vanishes, or the breeching is a *right line* from A to B, or any other point on the ship's side, this lifting force will disappear, and the recoil be smooth. These conclusions are evidently derived from insufficient and unsound mechanical reasoning; for whether the breeching be fastened to the ring D in the side of the carriage, or passes through it to the loop A, at the moment the recoil is stopped the effect will be the same: that is, the action on the carriage is not dependent on that portion of the breeching between the ring D and the loop A; but depends entirely on the direction and position of that part between the ring D, and the ring-bolt B, in the side of the ship †.

* We are not told how the line DC is drawn, but imagine that it is intended to be perpendicular to AB.

† This will immediately be admitted, if we conceive the gun and carriage to *fall* from the ship's side and to be sustained in its fall by the breeching, in which case B would be the point of suspension, and D the point where the rope ADB is applied to the mass, whether it terminates at D, or passes through to A.

Dupin also, in his "*Force Navale de la Grande Bretagne,*" discusses this question erroneously. He arrives at the conclusion that the muzzle is thrown *upwards* when the recoil is checked, unless the plane of the breeching, when tight, passes through the common centre of gravity, G, of the gun and carriage; most likely not knowing that he was denying a notorious fact from an imperfect, and, no doubt, hasty consideration of the subject.

As this is a very interesting question in the service of naval artillery, we shall here endeavour to clear it of the obscurity with which it has generally been treated.

Taking the preceding diagram, representing a sea-service gun, on a carriage of the usual construction, when the ship is upright, let G be the common centre of gravity of the gun and carriage; this point being a little in the rear of the trunnions, and about the height of their axis. Through G, draw the vertical line GW; and from the centres K and L of the axletrees, draw KM, and KN, perpendicular to GW, and join KG and GL. Now we may conceive that there is a given weight at G, supported by the two inflexible lines GK and GL given in position, and hence the whole system is capable of turning on the point K or L, according to the magnitude, point of application, and direction of any force that may be applied in a plane at right angles to the platform, and parallel to the axis of the gun. For instance, the elastic force of a given quantity of powder, taking place in the axis of the piece, will have a tendency to make the system turn about the point L, during the recoil; this tendency being developed in proportion to the height of the axis above the said point. On the contrary, if a force in an opposite direction, and at the same height, were applied, it would, in like manner, cause the system to turn about the point K. But, from the principles of mechanics, the stability of the gun and carriage, with respect to the point L, is in proportion to the line LN; and the stability of the system, with respect to the point K, is proportional to the line KM. Hence, as the line WG is generally very much nearer to K, than to L, making KM much less than LN, it follows, that it requires a proportionably larger force to overcome the stability, with regard to the point L, than the stability with regard to the point K*.

* It is evident, from this step of the investigation, that if the carriage be much shortened in the rear of the line GW, the line LN may be so much diminished, that the gun and carriage may be thrown backwards—a circumstance by no means unfrequent, when it has been foolishly attempted to mount lighter and shorter pieces of ordnance on short carriages of the same height. For, with the same charges of powder, the initial velocities suffer but a small decrease, and consequently, the momentum of recoil is nearly the same as in the larger pieces;

Now, as soon as the momentum of the gun and carriage, in the action of recoil, is overcome by the tension of the breeching, applied at a point D, above the centre of the fore axletree, its reacting force, in the direction DB, pulls the mass not only towards the ship's side, but also gives it a rotatory motion round the point K, or the fore-axle of the carriage; and if $W =$ the weight of the gun and carriage, $v =$ velocity of recoil, and $y =$ the line KP, drawn from the point K, perpendicularly to the direction DB of the breeching when tight, we have the effort to produce rotation expressed by yWv. But the stability of the system, with regard to the same point K (putting $KM = a$), is expressed by aW: hence, it depends on the relations which the two quantities yWv, and aW, bear to one another, as to the effect the breeching will produce when the recoil is stopped. The tendency in the case before us, to turn about the fore-axle, can only vanish when y or $KP = o$, or when the direction of the breeching passes through the centre of the fore-axle. If, however, the velocity of recoil be known, the breeching may be so adjusted as to make the effort to turn about the fore-axle just equivalent to the stability of the gun and carriage. For example, if a 32-pound shot be fired with an initial velocity of 1600 feet per second, from a long gun, weighing 55 cwt., mounted on a common carriage, weighing 8cwt. more, it will appear, by making $yWv = aW$, and supposing that the ship is upright, that the quantity y, or the line KP, should be between $\frac{1}{7}$ and $\frac{1}{8}$ of a, or the line KM, when the effort to produce rotation about the fore-axle is just equivalent to the stability of the gun and carriage. But although the breeching then ceases to *turn* the system about the fore-axle, it may, by its direction, exert a *lifting* force on the fore-part of the carriage, if it be elevated above a parallel to the plane of recoil; and thus exert an effort to produce rotatory motion about the *rear* axle: it may, therefore, be generally adopted as a maxim, that to divest the breeching of these mischievous effects in the recoil, it should never lead through a point in the carriage more than $\frac{1}{8}$ of KM above the centre of the fore-axletree, and be carried to the ship's side, so that its direction may be parallel to the deck. The best way, perhaps, of applying the breeching, according to this

hence, these short guns require, with regard to the hind truck L, nearly, if not quite, as long a carriage as the longest guns of the same calibre.

It is also equally evident, that when the deck becomes inclined, and the gun is on the lee-side, that the vertical line GW will still more approach the point K; and, consequently, the stability of the carriage, with respect to this point, be proportionably diminished. On the contrary, in a weather gun, it will in like manner be increased.

principle, would be to pass a ring-bolt through the middle of the fore-axletree of the carriage, making it sufficiently long to pass through the rear axletree, and forelocking on it, as well as on the fore axletree. The breeching may then be led from this, to a single ring-bolt in the ship's side, placed just above the thick waterway in the middle line of the port. In this way, a gun would require about half the length of breeching that it does at present; but, being a single rope, should be stouter than when the breeching is led to two ring-bolts. If, however, two ring-bolts should be preferred, both in the carriage and in the side of the ship, and there are some cogent arguments for having them, those in the carriage should be placed in the middle line of the port, so that one should be just as much above the height before indicated, as the other is below it; the planes of the rings being so disposed, as to revolve about a horizontal axis; then, if the breeching be led through both these rings, and fixed to two others in the ship's side, similarly disposed, in the middle line of the port, the object in view will be effected, without neutralizing our primary intentions, and giving, as in the first method, a breeching, which, although consisting of two parts, from the circumstance of being in *one* plane, perpendicular to the platform, and bisecting both port and carriage, will never require adjustment when the carriage is trained.

The breast-carriage in Captain Marshall's plan will be seen, by the most superficial investigation, to afford the readiest means of traversing or training the gun from one side of its port to the other, and also, of ensuring the nicest adjustment of aim at a moving object, by means of the tackle TT, fig. 2 of our explanatory diagrams. Indeed, with so much facility is this operation performed, that a long 12-pounder, worked only by *three* men, was traversed from an angle of 54° before the beam of the port, to 54° abaft; or through an arc of 108° in the short space of 25 seconds.

Captain Marshall states, that the aggregate of the weights of his two carriages for a 24-pounder gun, is only 1cwt. more than the weight of the old carriage; so that we have not here to complain of the immense additional weight accompanying almost all other attempts to improve the mounting of naval ordnance; nor have we to make the serious objection, common to all systems that we have seen, except Congreve's, that the deck is encumbered with a large fabric, and the free circulation of air prevented thereby. We think that the increase of weight might be nearly, if not quite, got rid of; for, no doubt, there is, at present, an excess of strength in the cheeks, or sides of the breech-carriage, since so much violent and percussive action is avoided, and their thickness might safely be reduced. No objection can arise to this, from the strains to which the carriage is liable when secured at sea, for all those are diminished, in consequence of the gun, in this circumstance, being considerably nearer the deck than on the old system of mounting.

Every person, in the least acquainted with artillery exercises, is aware of the Herculean task of mounting a heavy gun without the apparatus called a gin;—an apparatus which it is impossible to use on board a ship, and the want of which can only be supplied by strong and complicated tackling attached to the beams over head, and by the assistance of many men. This arises from the circumstance of having the whole weight of the gun to be lifted, whilst, by the contrivance of our author, which throws the weight on either the breech or breast-carriage, as occasion may require, this service may be executed with the greatest ease, by a few hands, in an almost incredibly short time. On board the Isis, of 50 guns, the operations of dismounting and mounting were performed with a 24-pounder gun, in the space of 20″, with *seven* men.

There is one property of Captain Marshall's carriage, which, if made available, will contribute in no small degree to the comfort and health of those who man our frigates. We allude to the property of securing fore and aft. The main-deck guns, from always being run out, cause the deck, in rough weather, to assume the character of a reservoir of water: for the half-ports can never be rendered sufficiently tight to prevent it from coming in. The circumstance, too, of the guns being frequently under the waves, and consequently, dragged through them, occasions no small impediment to the ship's velocity. Now, these inconveniences are unavoidable with the common carriage, which, if secured inside, fore and aft, instead of projecting the muzzles outside the ports, cannot be ready for action without serious delay. But with the new carriage this objection does not exist, and, moreover, it possesses the additional good property of *lowering* the gun.

As the very extreme ports, at either bow or stern, are usually armed with guns, from the nearest broadside port, when occasion requires, it would appear, at first sight, that Captain M.'s carriage was not capable of such an operation, since the breech-carriage only presents two wheels to effect it. However, our author has here again derived a singular advantage from what certainly appears to be an unpropitious circumstance. He applies an axle with trucks to the breast-carriage, lashes the crutch to the muzzle-ring, or astragal of the gun, and thus constructs a four-wheeled carriage, of which the gun itself becomes the perch; and as the breast-carriage and its trucks turn about the crutch-bolt, the whole system is capable of taking any direction, with much greater facility than the old carriage with its four trucks can ever possess.

We have thus very briefly analysed the two first sections of Captain Marshall's book, in which the detail of his construction of gun-carriages necessarily includes the developement of its properties; but, in the following section, the author more particularly enters into the *training*, *elevating*, and the action of *recoiling*, on all which subjects he displays both true reasoning and professional knowledge of the wants of naval artillery—wants that land artillerists cannot be supposed to have any practical acquaintance with; and no doubt it is owing to the circumstance of our naval ordnance being organized, as it were, in a department especially devoted to the pursuits and inquiries of land artillery, that so little has been done in accommodating heavy cannon to the service of our marine. This is perfectly natural in result:—how can it be supposed that men, whose ideas from education and long habits are indissolubly connected with a solid and immoveable platform, and a defined routine of action, can appreciate and know how to contend with a service where everything is changing its position, and fresh exigencies continually arise? Not only the battery itself, but its very platform, and the object at which its fire is directed, have each their distinct motions of translation and rotation, and require from their combinations a rapid and varying adjustment. What would be said if the numerous improvements which are daily taking place in the equipment of our land service artillery were to be laid before a committee of naval officers to decide upon as to efficacy? would it not be regarded by the most common capacity as utterly inconsistent with reason? The question need not be answered here; but do we not behold as gross an anomaly in making a committee of land artillery officers arbiters in a case of which they can have as little practical knowledge as a committee of naval officers would have of the efficiency of a new travelling-carriage and its limber for a siege or field gun? The fact is, that all improvements attempted to be made in ordnance or its equipment are referred, no matter whether affecting our naval or land service, to men who are by profession only capable of pronouncing on matters connected with the latter, and this has been the main cause of placing the equipment of sea-service ordnance so far behind that of the land service. No one afflicted with ill health would call in a doctor of music to improve it; he would naturally fly to the doctor of medicine; in like manner, if we wish to improve our naval artillery, we must apply to naval artillerists, and not to *land* ones. If the Admiralty wish naval artillery to improve in an equal ratio with that of the land service, they must have a committee of the comparatively few amongst naval officers who have studied a subject, hitherto, it must be confessed, too little regarded, and, what is worse, generally returning nothing to its cultivator but disappointment and chagrin, from coming in contact with those naturally incapable of appreciating his motives and ideas.

We think that Commander Marshall has settled the long and vehement dispute regarding square and rounded sterns very satisfactorily in a military point of view. As his gun-carriage enables the broadside guns to be pointed 45° or 50° abaft the beam, and also the stern guns 45° or 50° from a fore-and-aft-line, it immediately appears that a square-sterned ship can produce a parallelism of fire between the broadside and stern guns at the greatest traverse, and that whether the stern be square or rounded, there is no point of impunity. It therefore now becomes a question purely of naval construction, and naval architects must decide which stern contributes most to the sea-going qualities of ships.

Persons who are acquainted with the method of applying the *force* of artillery on land, are aware that this force is always brought to bear on some one point. The *focal* fire of a battery of only a few pieces of field artillery are terribly destructive, and in siege batteries for breaching the walls of a fortress the same principle is adopted, with similar efficiency; but, on board ship, with the present system of mounting naval ordnance, in which the training is limited to an arc of 70°, it is scarcely possible to effect anything like *focal* firing, excepting at such distances as to render it a matter of no importance. We cannot forbear citing here the very just remark of our author on this important particular, in which his gun-carriage has so much the advantage over the common one.

'Let, for instance, five well-directed broadsides, double shotted, be all concentrated into any part of the enemy, which may be effected in five minutes by the new system, and there can be little doubt but that much more decisive effects would be thus produced on the fate of an action than the customary results of a first onset.

"These principles of combined and controlled effort might also be applied with advantage, when closely engaged abreast, by the bow-division (of guns) firing on the quarter, and the stern-division on the bow of the enemy: by this means her decks would be crossed by the shot diagonally, and their effect be increased in the proportion of an hypothenuse to a perpendicular, or of radius to the cosine of the angle of inclination."

The great increase given to the arc of traverse by Commander Marshall's carriage also produces another important advantage, *viz.*, that of enabling the ship provided with them to direct its guns to bear on objects at greater angular distances, and thus affording an immense advantage over an enemy's ship furnished with the common carriage. This subject, however, our author treats of in an earlier part of his work.

At the conclusion of the fourth section, under the head "Working," it is justly observed, from the previous investigations and demonstrations,

"That a gun may be worked with more expedition upon the new carriage than upon the old one; and, as the more laborious parts of the exercise are rendered easy and simple, it may also be inferred, that a brisk fire may longer be kept up, and the physical strength of the men be longer preserved from exhaustion."

The experiments which have been made on board the Prince Regent, of 120 guns; Isis, of 50 guns; and Galatea, of 42 guns, amply confirm these deductions. On board the first-mentioned ship *three* men only worked a long 12-pounder more quickly than *six* men by the old carriage. A number of experiments at sea, on board the Isis, have established the fact, that *seven* men with the new carriage are able to fire about a third greater number of shot from a 24-pounder in a given time than can be accomplished by *ten* men with the usual carriage. It was also equally verified on board the Galatea, that *six* men can handle an 18-pounder with greater facility than ten can with the ordinary means.

It appears that, with Commander Marshall's carriages, a line-of-battle ship is more effectually manned with one-sixth less men than the usual complement at present allowed in the English service. Here the invention assumes other interesting features. The facility of increasing the number of ships at sea with a given number of seamen is an advantage of vital importance, and with a given number of ships it is evident that a considerable economy results in being able to dispense with a portion of the present crews *. But there is another question of no less importance, hinging on the introduc-

* Our navy in commission at present employs 21,000 sailors, and it could be at least equally well manned with 18,500 with the new carriages.

tion of this invention into our naval service, and that is, the improvement of our ships in sailing qualities. The naval constructor, who, in preparing the design for a seventy-four gun ship, has only to provide provision and store-room for 500 men, may indeed be said to work with a great advantage over him who has to provide for a crew of 600 men. But even a great amelioration of qualities would be immediately derivable in the ships we have already by us, though considerably less than that which would result from a construction *de novo,* made, under such favourable circumstances, by those who know how to profit by them.

The fifth section of his work our author has chiefly devoted to a number of official reports of experiments made on board his Majesty's ships the Regent, of 120 guns; Isis, of 50 guns; and Galatea, of 42 guns; all of which are confirmatory, in the highest degree, of the truth of the inventor's proposition, which appears, from the hint in the Introduction of the book, to have made its way against the prejudices always attendant on the introduction of novel plans, literally by the force of conviction, and by that alone. This has, indeed, operated so powerfully as to induce the Admiralty to order one broadside of the upper deck of the Donegal, of 74 guns, to be entirely armed with guns mounted on the proposed system. At the conclusion of this part of the book the following parallel is drawn between the properties of the two modes of mounting;—properties which are incontrovertibly confirmed by *experiment.*

Properties of the New Carriage.	*Properties of the Old Carriage.*
"It allows the gun to be pointed at the greatest angle across its port, which the width of the port or the length of the gun will admit, that is, through an arc of from 90° to 100°, upright sides, with broad waterways, &c., present no obstacles to the training of the guns.	"Prevents its gun training through a greater arc than about 68° under general circumstances; in upright sides, with broad waterways, a smaller angle of training can only be obtained.
"It enables a ship powerfully to defend her quarters by the fire of her stern and broadside guns, and thus to command every point of attack from the stern to the bow.	"Does not enable a ship to defend her quarters by the fire of her stern or broadside, an arc of about 20° on the quarter being left unprotected.
"A great portion of the broadside guns may be brought to bear on what is termed the point of impunity; and a vessel in chase may so place herself on the quarter of her enemy within point blank range, as to get all her broadside guns to bear, without those of the enemy, on the old carriages, being able to return the fire, the ships continuing on parallel lines of sailing.	
"A gun trained to its greatest angle may be fired repeatedly in that direction with the same expedition as in any other.	"The continued fire of a gun is very much retarded by the difficulties which attend its being trained when engaged

	much before or abaft the beam of its port; these difficulties the irregular direction of the gun's recoil frequently occasions.
"A greater number of accurate shot in a given time can be fired.	
"3 men are sufficient to work a long 12-pr. "4 Ditto ditto a Congreve 18-pr. "5 Ditto ditto a long 18-pr. "7 Ditto ditto a long 24-pr. Exclusive of powder-boy. Hence the reduced crews of ships of war on the peace establishment are more than sufficient to work their guns.	"6 men are required to work a long 12 pr. "7 Ditto ditto a Congreve 18 pr. "9 Ditto ditto a long 18-pr. "10 Ditto ditto a long 24-pr. Exclusive of powder-boy. Hence, the full complement of men are required to work a ship's guns.
"The services of about fifty men from each deck of long guns being dispensed with, much less confusion and carnage at quarters will prevail; by requiring fewer men, economy may be promoted, more ships may be equipped, or the present crews made more efficient.	"In the confined space between the guns so many men are stationed to work them, that the decks are inconveniently crowded, and the enemy's shot amongst them very destructive.
"When disabled in action, it may have its parts quickly renewed.	"If disabled, cannot be replaced in action without such difficulty, as to prevent its being attempted.
"The breeching of the gun can never get foul of the carriage, and little or no caution is necessary to keep the side tackle falls clear of the recoil.	"Serious inconveniences may arise when the ropes attached to the carriage get under the fore tracks, and to keep them clear requires much attention.
"A gun, engaged at a great angle of elevation or depression, does not require its quoin to be moved in order to load it.	"When working a gun much elevated or depressed upon its carriage, the muzzle often requires to be levelled each time it is loaded.
"Guns, in the regular sized ports, may be elevated or depressed through an angle of at least 28°.	"Guns in their ports can only be elevated and depressed through an arc of about 18°.
"A ship heeling over 10° or 12°, may lay point blank her weather guns.	"A ship laying over 6° or 7° cannot point her weather guns horizontally.
"Weather and lee guns may be worked more easily, a part of their weight only requiring to be moved up and down the inclined plane of the deck.	"When the decks are inclined, the whole weight of the gun is moved up and down them.
"However heated or heavily shotted the gun may be, it never jumps up at the breech.	"The gun, after a few rounds, kicks or tilts up, by which means the bed or quoin is moved, and the decks violently shaken.
"When the guns are all laid at their proper level on going into action, they will continue in this position.	"However carefully the elevation of the guns may have been inspected and adjusted, two or three rounds will move the quoin.
"By a new mode of altering the line of the gun, by moving the breast block instead of the breech, the gun is steadily drawn in a line with its object, and may be fired at the same instant, the men whilst in the act of moving the gun being clear of the recoil.	"The rough motion of a handspike applied to the breech occasions delay in accurate pointing*, and the men being required to stand clear before the gun can be fired, an interval of time elapses, in which the motion of the ship may alter the direction of the gun.
"Guns when secured are much nearer the deck and lower in the ship.	

* The old French carriage for ships, having only the two fore tracks, had much advantage in this respect over those with four tracks.

"Very little practice is required to work the gun with the greatest celerity in all cases; a uniform proficiency in naval gunnery may, therefore, throughout his Majesty's service, be more easily attained."

"To work a gun smartly in different directions requires very great practice and skill, and to keep up a brisk fire for any length of time, very exhausting efforts."

The sixth and last section, of the work under our notice, refers to the application of the proposed new system of mounting naval ordnance to steam-vessels, gun-boats and merchant ships, and particularly to those of the latter description belonging to the East India Company. We cannot afford the space to enter into these particulars; but there is an interesting subject brought forward in the same section with regard to *restoring*, the author says, the Congreve gun to the service of his Majesty's ships, from which they have "lately been discontinued" on account of "the unsteady and unsafe action of these guns upon their carriages."

We cannot help thinking that Commander Marshall must here labour under some misinformation, and that it is hardly possible for those who have the management of our naval ordnance to have contemplated such a line of conduct, much less to have put it in execution, and thereby throw away the services of nearly one thousand pieces of ordnance eminently calculated by their powers* and properties for sea service. These guns armed the upper deck of the Queen Charlotte at Algiers, and were supposed to have fired at least one hundred rounds a piece. In this severe trial we are told that not the least failure or disappointment occurred whatever. Their excellence was so completely confirmed by experiment, as to induce the Admiralty to adopt them very largely in the armament of our three decked ships of the line, and in the 46-gun frigates. As we cannot imagine that the decease of their talented inventor could have invested these guns with any new properties or have neutralized their powers, we naturally infer that their now discovered bad properties depend not on the gun, but on some defect in the mode of mounting. As they did not show any disposition to unsafe action at Algiers in 1816, why should they do so now in 1830? If, from having been mounted on improper carriages, they are found to develope such bad symptoms, an alteration should be made to correct them. Any gun mounted upon too short a carriage will evince a disposition to upset, and it is a well-known fact that carronades, when mounted on short and comparatively high carriages with four trucks, will do so. Here then the remedy

* From official experiments made in 1813, on Sutton Heath, it appeared that the *point blank* range of Congreve's 24-pounder of 41 cwt. and 7½ feet long was 505 yards, and that of the long 24-pounder of 50 cwt. and 9½ feet long was 368 yards.—*Vide* the "Concise Account" of the Congreve 24-pounder gun.

is obvious, and it should be applied. An infinite deal of pains has been taken, and with much success, to render the carronade manageable, and the same ought to be taken with the Congreve 24-pounder and other guns of this construction, if they really have become so unsteady and unsafe. But we are informed by Sir W. Congreve himself in his "Concise Account" of his 24-pounder gun, that the "more conical form of my construction" was, subsequently to its first introduction, adopted by those who contested the palm of excellence with him; and indeed any one who looks at the newly-constructed 32-pounders, of nearly the same length and weight as that proposed by Congreve, must acknowledge, that, with the exception of the ugly, pernicious, and useless mass of metal about the muzzle, they possess in a great degree the *conical* form of Congreve, and therefore its properties, bad or good. Hence a proportionate care in mounting them also will be required to deprive them of unsafe action on their carriages, which no doubt will be given. If, however, what our author says with respect to the rejection of the Congreve construction of ordnance be correct, and that, from a forgetfulness of its valuable properties, a defect arising from equipment may have condemned it, the new carriage returns it to its proud position of excellence, and with an augmented developement of good properties. This has been amply proved with the 18-pounders of this construction with which the East India Company's ship, the Earl Balcarras, has been armed, mounted on the proposed carriage. Commander Marshall says, "as they may be worked by nearly the same number of men as carronades of similar calibre, line of battle ships carrying even fewer men (than we have before supposed) may now change their main deck guns for Congreve's 32-pounders."

Our author then proposes, with much propriety, to increase the force of our 28-gun frigates by giving them 18-pounder guns of Congreve's construction, as they may be worked on his carriages "without any augmentation of their crews or inconvenience to their narrow decks," being capable of stowing fore and aft.

We must refer our readers to the book and its numerously well executed and illustrative plates for a great quantity of detail and interesting discussion. We think that the proposition has completely put into our hands the power of raising the force of our ships of war, either by advancing the calibres to what Chapman (the Swedish naval constructor) has proposed, or by introducing, at least, the calibre of 38 throughout our navy, which, in a former Number of the

Quarterly Journal of Science, we have shown from the analysis of *facts* to be as perfectly practicable as it is desirable; and it is to be hoped that other maritime powers will not be the *first* to derive the advantages of an invention, whose discussion has not only thrown a new light on the badly understood subject of naval gunnery, but is in itself so well calculated to produce a great simplicity and facility in some of its operations—operations which have hitherto been to a degree complicated and laborious; and, what is still worse, eminently deficient in effecting their purposes.

We cannot take leave of Commander Marshall's work without expressing our conviction that all professional men of science will set that value on its contents which true reasoning will always command; and we are fully persuaded, that, with a "fair field and no favour," his system of mounting naval ordnance will be found to excel all other systems as much in practice as it does in principle.

On Indigo. By Andrew Ure, M.D., F.R.S., &c.

Among the vast variety of vegetable products, there is probably none so interesting to science, by the curious complexity of its nature, and the protean shapes it may be made to assume, as indigo; and, certainly, there are few more important to British commerce and enterprise, since it constitutes the most valuable article of export and remittance from Hindostan. At the four quarterly sales appointed by the East India Company, no less than twenty thousand chests of this dyeing drug are, on an average, brought annually into the market. A very considerable quantity of indigo is also imported into Europe from America and Egypt. It is not long since the Caraca and Guatimala indigo held a much higher character, and commanded a much better price than that of India; but the improvements due to the intelligence of our planters in the East have, within these few years, enabled them to prepare an article very superior to the finest American. The sequel of this paper will present satisfactory proofs of this assertion.

Indigo is procured from many different species of plants, belonging to Tournefort's natural family of leguminous, included for the most part in the genus called indigofera by Linnæus. According to Heyne, the *indigofera pseudo-tinctoria* cultivated in the East Indies, produces the best indigo; but others extol the *indigofera anil*, the *ind. argentea*, the *ind. disperma*, which yields the Guatimala kind, and some the *Mexicana.* About sixty species of the indigofera are at present known, but those above named are in peculiar esteem. My object in stating these differences here is chiefly to shew that a drug obtained from such a variety of vegetable species must necessarily vary in composition. The matter which affords the indigo is confined entirely to the pellicle of the leaves, and exists in largest quantity at the commencement of maturation, while the plant is in flower; at a somewhat later period the indigo product is more beautiful but less abundant; afterwards, much less of it is obtained, and of a worse quality. The plant is remarkable for giving a blue tinge to the urine and milk of cows that feed upon its leaves; a circumstance which accords with the known permanence of the dye. The statement of Mr. Weston, in this Journal (No. XXVII. p. 296), agrees with these observations on the ripening of the blue principle. He shews that the developement of this matter in the *indigoferas* goes on in the leaves, even after they are separated from the plant and dried. When packed up for a few weeks, more or less according to their preceding state of ripeness, the leaves assume a light lead colour, which gradually deepens into a blackish hue. The planter studies to seize the period at which the *maximum* portion of colouring matter is formed, that he may then transfer the leaves to the steeping vat.

Three different processes are employed for extracting the indigo, each of which must modify more or less the nature of the product. In the first and second, the dried leaves are operated on; in the third, the recent plant. For the perfect success of the two former processes, the plant should be very speedily deprived of its water of vegetation; hence the *indigofera* is reaped only in fair weather. An hour and a half before sunset, the plants are cut down, carried off the field in bundles and immediately spread on a dry floor. Next morning, at six o'clock, the reaping is resumed for an hour and a half before the sun acts too powerfully on vegetation, and the plants are treated in the same way. Both cuttings become sufficiently dry by three o'clock P.M., to permit the leaves to be separated from the stem by threshing. The leaves are now thoroughly dried by exposure to the sun, then coarsely bruised, or rather ground to powder in a mill, and packed up for the use of the manufacturer of indigo.

From these powdered leaves, the dye stuff is extracted either by simply digesting them in water, heated to 150° or 180° F., in as small a proportion as may be practicable, and subsequently beating the infusion with paddles till the blue indigo granulates, as Roxburgh recommended; or by mashing the ground leaves with twice their bulk of water, at the atmospheric temperature, drawing off the liquor into a vat, where it speedily undergoes fermentation, and is beat as above with paddles or oars, till the blue indigo forms. Some persons prescribe the addition of lime-water at this stage of the process; others reject its use.

In operating on the recent plant, it is laid in bundles in the steeping trough (*trempoir*), which contains sufficient water to stand about two inches above plants slightly pressed down by crossing bars of wood. A brisk fermentation soon begins, with copious extrication of air-bubbles. This process is suffered to proceed till the liquor has become green, and casts up a pellicle of a copper-red hue. A sour smell is now perceived, and the blue colouring particles seem ready to separate. This happens commonly at the end of from ten to twenty hours, according to the temperature of the weather. The liquor is then run off into the beating vat, and lime-water is added, or not, according to the fancy of the operator. In all cases of fermentation, whether the dried leaves or the recent plant be employed, it is proper to watch the progress of that change with solicitude; because, when too violent, it not only decomposes entirely some of the indigo blue, but introduces much foreign vegetable matter into the precipitate; when too feeble, it is said to leave some indigo unextracted.

From the differences which exist in the nature and culture of the *indigofera*, and of its treatment by the manufacturer, the product, *indigo*, as found in commerce, differs remarkably in quality and chemical composition. In this respect, it forms a complete contrast to the simple crystalline product sugar, Besides impurities accidentally present, from a bad season, want of skill or care, the purest commercial indigo consists of no less than five constituents—1. *Indigo-blue*, a very singular vegetable compound of carbon, hydrogen, and oxygen, with fully 10 *per cent.* of azote; 2. *Indigo-gluten*, a yellow, or brownish-yellow varnish, which differs from wheat-gluten by its solubility in water. It has the taste of osmazome, or of beef-soup, melts when heated, burns with flame, and affords an empyreumatic oil along with ammonia by distillation.—3. *Indigo-brown*. This constituent is more abundant than the preceding. It is extracted by a concentrated water of potash, made to act on powdered indigo, previously digested in dilute sulphuric acid. Chevreuil's indigo-green seems to have consisted of this substance, mixed with some alkaline matter, and indigo-blue.—4. *Indigo-red*. This is readily dissolved by boiling alcohol out of indigo previously subjected to the action of an acid or alkaline menstruum. The alcohol acquires a beautiful red tinge, and leaves by its evaporation the red principle in the form of a blackish-brown varnish.—5. *Phosphate of lime*. I have found the bone phosphate in notable quantity in some fine indigo, constituting another feature of resemblance between this vegetable and animal products. Hence, also, the charcoal of indigo is most difficult of incineration, and requires, for perfect combustion in some cases, the deflagratory powers of nitric acid.

Pure *indigo-blue* is most easily obtained from the blue vat of the indigo-dyer; the yellow liquid of which being acidulated faintly with muriatic acid, and exposed, with occasional agitation, in a shallow basin, soon deposits the blue precipitate, mixed, however, with a considerable quantity (more or less according to the quality of indigo used) of indigo-red. This must be removed from the dried blue powder, by the solvent action of boiling alcohol, applied in successive quantities.

In my paper on the "Ultimate Analysis of Vegetable and Animal Substances," which the Royal Society did me the honour to read at their Meeting in June, 1822, and to publish in the volume of their Transactions for that year, I gave an analysis of *indigo-blue*, to which I appended the following remarks:—" I had intended to pursue at considerable detail my researches on this azotized product of vegetation, but the subject having been lately taken up by my pupil and friend, Mr. Walter Crum, I was induced to leave it in his hands." I then thought it likely that some slight modification might require to be made in the weights of the constituents given by me, for "I did not (then) resume the subject of indigo, after I had become most familiar with the manipulations." I have found since that my mode of analysis was not in fault, but the revived *indigo-blue*, which I employed, had not been entirely purged of the red principle, by sufficient ebullitions with alcohol; for it adheres very tenaciously. Hence that resinous matter introduced a little oxygen and hydrogen, more than absolute indigo-blue contains. But the error will appear inconsiderable, if we compare the result with the analysis previously published by Dr. Thomson. The following is a view of the ultimate constituents of *indigo-blue*, as given by different chemists:—

	Thomson.	Ure.	Crum.	Royer and Dumas.
Carbon .	40.384	71.37	73.22	71.71
Oxygen .	46.154	14.25	12.60	12.18
Azote .	13.462	10.00	11.26	13.45
Hydrogen .	0.000	4.38	2.92	2.66
	100.000	100.00	100.00	100.00

That pure indigo contains hydrogen, I have recently placed beyond a doubt, by heating a mixture of it and calomel in a green glass tube, the open end of which terminated in an inverted tube, filled with nitrate of silver. Copious fumes of muriatic acid were evolved, and chloride of silver was precipitated in its characteristic curd.

The liquor of the dyer's vat (for calico-printing) contains indigo deoxidized by protoxide of iron, and dissolved in limewater. This solution, in its average state of richness, has a specific gravity not appreciably higher than that of distilled water, and affords out of 1000 parts, by weight, not more than 3 parts of indigo-blue, and nearly the same quantity of carbonate of lime, equivalent to about a grain and a half of quicklime in 1000 of the liquid; which is the proportion in common lime-water.

If that yellow liquor be introduced into a glass globe, with a graduated stem, previously filled with hydrogen, by plunging the vessel into the vat, we may transfer a portion of deoxidized indigo conveniently to the mercurial pneumatic trough, and measure the quantity of oxygen which a given bulk of it absorbs in becoming blue. This quantity will be proportional to the strength and purity of the vat-liquor. I have lately instituted a series of experiments, the results of which will, I hope, prove interesting in reference to the problem for determining the quality or purity, and strength, of different commercial indigoes; but they are not yet mature enough to meet the public eye. The rigid mode of examining this drug is to eliminate the *indigo-blue* from the other substances, by the readiest artifices of analysis, and to weigh it apart. It may be objected to the analysis of indigo, that it is too complex and operose a process to be practicable with the despatch and to the extent which the public quarterly sales of indigo require. But I conceive this to be a mistake. When only one object is pursued, various arrangements may be contrived for readily attaining it. Under this conviction, I ventured to state, ten years ago, in the introduction to the first edition of my Dictionary of Chemistry, that "the result of numerous researches made with that view, has shewn me the possibility of rendering analysis in general a much easier, quicker, and more certain operation, than it seems hitherto to have been in ordinary hands." My experience since has fully justified that statement.

Accordingly, about three years ago, I suggested to the Honourable Court of Directors of the East India Company, the propriety of establishing an Assay Office for Indigo in Calcutta, to guide them in their purchases of that article, and to enlighten the manufacturers in Bengal about the value of their various products and processes; and I again submitted to their consideration, last autumn, a memorial to the same effect, in which I detailed the advantages likely to accrue from such establishment to indigo-planters, dealers or brokers, and consumers. But the Court did not think it expedient at present to make any alteration in their indigo department. How much an office of this kind is wanted in London, in connexion with the quarterly sales of indigo, the following series of analyses, lately executed by myself, will sufficiently demonstrate. The quantity of indigo required for the assay need never exceed ten grains, provided a very delicate assay-balance be employed; and by a suitable system of arrangements, the average quality of 500 chests may be accurately determined in the course of a day, by a diligent chemist, with four or six ordinary assistants to follow his directions.

I.—*East India Indigoes;* prices as at the last October Sales:—

No.	Price. *s.*	*d.*	Real Indigo in 100 parts.	
1.	3	9	42	Broken, middling violet, and coppery violet spotted.
2.	3	6	56.5	Ditto, a little being coppery violet and copper.
3.	3	3	46.0	Ditto, middling red violet, dull violet and lean.
4.	4	3	54.5	Large broken and square, even, middling red violet.
5.	4	2	75.0	Much broken and very small, very crumbly and limy, soft, good violet.
6.	4	9	60.0	Square and large broken, ½ middling violet, and ½ good coppery violet.
7.	5	3	70.0	Large broken, very good; paste, a little limy, good violet.
8.	6	6	60.0	Square and large broken, soft, fine paste, fine violet.
9.	6	0	66⅔	Square, ditto, good red violet.
10.	7	0	75	Square, ditto, fine purple and blue.
11.	2	3	37.5	Middling ordinary Madras.
12.	3	6	60.0	Good Madras.
13.	4	3	58.0	Very fine ditto.
14.	2	0	—	Low, pale, Oude.
15.	2	4	27¾	Middling ordinary Oude.
16.	3	3	54	Good Oude.
17.	1	9	29	Lundy, very low quality.

II.—*American Indigoes.*—Wholesale prices at present.

		s.	*d.*				*s.*	*d.*	
Caraca Flor.	.	6	0	54½	*Guatimala.*—No.	5.	5	0	50
Guatimala.—No.	1.	5	0	33½	,,	6.	5	3	35
,,	2.	3	2	19	,,	7.	4	8	46
,,	3.	4	6	32½	,,	8.	4	8	33½
,,	4.	5	4	50	,,	9.	5	4	50

March, 1830.

On the Velocity of Sound and Variation of Temperature and Pressure in the Atmosphere. By John Herapath.

I.—Velocity of Sound.

Having communicated the discovery of some theorems, relative to the velocity of sound and the decrease of temperature and barometric pressure in ascending in the atmosphere, to several scientific friends, I have been prevailed on to give them to the public before the work of which they are intended to form a part.

It is pretty well known in the scientific world, that in pursuing Newton's hints of the cause of gravitation, I have been led to a theory of the nature and constitution of airs very different from that generally embraced. This theory, after unfolding to me the laws of an immense variety of phenomena, I was anxious to apply to solving the celebrated problems of sound and atmospheric temperature and pressure. No difficulty whatever occurred in developing the general laws; but this was not enough. If the theory to which I had arrived was right, I felt assured there must be some method of getting at the exact quantities of the phenomena, without drawing on experiments for more than indispensable elements. For instance, in estimating the velocity of sound, I conceived no just theory ought to require more from experiment than the elastic force and specific gravity of the air. The same elements only, I apprehended, ought to be sufficient for determining the exact rate of diminution in temperature and pressure at any elevation. For a long time my efforts were unsuccessful. At last, however, a very simple idea, which I am surprised should have so long eluded my attempts to reduce the equations of comparison I had previously used to equations of equality, enabled me to solve the hitherto refractory problem of sound, and with it several of much more importance and utility.

What probably will appear not the least remarkable is, that this problem, which has obstinately resisted the abilities of Newton, Euler, Lagrange, Laplace, and other eminent mathematicians for 150 years, and the highest powers of analysis, should at last yield to a process scarcely requiring simple equations of algebra; and at the same time open solutions to other phenomena, with which I think I may venture to say no analyst ever expected it had the remotest connection. But the theorems will speak best for themselves.

Let, as usual, S, E, D, g, denote the velocity of sound, elasticity of the air, its density, and the velocity acquired by a falling body at the end of the first second. Then, by the theory alluded to,

$$S = \sqrt{\frac{E\, g\, \sqrt{2}}{D}} \qquad (1)$$

And if S be the velocity of sound at any elevation x, and P, p the barometric pressures at the lower and higher stations,

$$\frac{p}{P} = \left(1 - \frac{g\, x}{3\sqrt{2}\, S^2}\right)^6, \qquad (2)$$

and

$$3\sqrt{2}\,(S^2 - s^2) = g\, x. \qquad (3)$$

For, comparing these formulæ with observations, we have

$$D = \frac{E\; 488}{r\, h\, (F + .448)},$$

in which h is the altitude of the barometer at the temperature of water freezing; $r = 10463$ by Biot the ratio of the specific gravity of mercury to that of dry air, at 32° Fahr., and barometric pressure $h = 76$ metres, the metre being 3.28085 English feet, and F the Fahr. temperature. Therefore since $g = 32\frac{1}{6}$ feet,

$$S = 1089.41 \sqrt{\frac{F + 448}{480}} \text{ Eng. feet.} \qquad (4)$$

Now, from a mean of Captain Parry's experiments in the north, at − 17°.72 Fahr., it appears the velocity was 1035.2 feet per second, or, allowing for the difference in the value of g in that high latitude, probably about 1034 feet reduced to our latitude; our theory gives 1031.5. The French Academicians, in 1738, at about 42°.8 Fahr., found it 1103.5 feet; our theory makes it 1101.6. Dr. Gregory, by the mean of his observations, determined the velocity to be 1107 feet at 48°.62 temp.; by our theory it should be 1108.1. In 1821, Arago and his colleagues made the mean at 60°.62 to be 1118.43; our theory gives 1121.5. And from an article on sound not yet published, with which the author has kindly favoured me, it appears Moll's late experiments, reduced to dry air at 32°, give 1089.4 feet, the same as our theory. In the same article, some of Goldingham's experiments at Madras, when reduced to 32°, it seems make 1089.9; the mean of his other experiments differing as much as 10 feet.

Collecting these observations together, we have—

OBSERVERS.	Temp. Fahr.	Observed in Eng. Feet.	Computed in Eng. Feet.	Diff. Feet.
Parry .	−17°.72	1034	1031.5	−2.5
Academicians	+ 42.8	1103.5	1101.6	−1.9
Gregory .	48.62	1107	1108.1	+1.1
Arago .	60.62	1118.4	1121.5	+3.1
Moll .	32.	1089.4	1089.4	+0.0
Goldingham	32.	1089.9	1089.4	−0.5
		Mean Diff.	. .	+0.12

I have no more time to devote to this part of the communication, than just to notice the extreme minuteness of the mean difference of our theory, only $1\frac{1}{2}$ inch, compared with that of the old, amounting to 53 metres, or 174 feet. Laplace has, indeed, contrived to reduce this difference to 13 or 14 feet, by an ingenious, but very questionable, hypothetical assumption. The theory from which I have deduced the preceding formula requires no such assistance; nor I believe any thing beyond the simple definition of an air.

II.—Diminution of Temperature.

Let us now turn our attention to the other formulæ. Supposing f the Fahr. temp. at the higher station, F being that at the lower, and substituting for S and s their values in (2) and (3), we get

$$x = \left\{1 - \left(\frac{p}{\mathrm{P}}\right)^{\frac{1}{6}}\right\} \cdot \frac{r\,h\,(f + 448)}{80} = 326.1264785\left\{1 - \left(\frac{p}{\mathrm{P}}\right)^{\frac{1}{6}}\right\} \cdot$$

$$(f + 448)\,;^{*} \text{ and } \mathrm{F} - f = \frac{80x}{r\,h} = \frac{x}{326\frac{1}{8}} \text{ nearly.} \qquad (5)$$

From the latter of these theorems it appears that the Fahr. temp. decreases uniformly at the rate of 1° for every $326\frac{1}{8}$ feet.

* These computations, and those that follow, are extracted from a letter in which the metre was reckoned to be 3.281 Eng. feet.

The difference may, therefore, be easily computed: "Take a $\frac{1}{100}$th of the altitude in yards; subtract a $\frac{1}{10}$th of this from itself; and then add $\frac{2}{10}$ths of the part so subtracted." Thus, if the altitude was 7600 yards—

$$\begin{array}{r} 76 \\ -\ 7.6 \\ +\ 1.52 \\ \hline 69^\circ.92 \end{array}$$

And in centigrade degrees—"To a $\frac{1}{100}$th of the alt. in fathoms add twice a $\frac{1}{100}$th of itself, and then a $\frac{1}{10}$th of this corrrection." For example, in the preceding instance:—

$$\begin{array}{r} 38 \\ .76 \\ .076 \\ \hline 38^\circ.836 \end{array}$$

Applying this rule to the cases extracted by Mr. Ivory from Ramond's collection of observations, we shall find it agree with the observations much better than the observations, probably by different individuals, at the same place, agree with one another, as the following table shews:—

PLACES.	Heights, yards.	Differences of Temp. Cent. Observed.	Computed.	Differences.	Mean Diffs.
Lussac's Ascent	7630	40°.3	39°.0	..	—1.3
Chimborazo	6427	26.9	32.8	..	+5.9
Montblanc, Geneva	4782	31.2	24.4	—6.8	—5.8
	4782	29.2	..	—4.8	
Pic de Teneriffe	4077	16.5	20.8	..	+4.3
Montblanc, Chamouny	4070	25.9	20.8	—5.1	—5.5
" "	..	26.6	..	—5.8	
Etna	3540	18.7	18.1	..	—0.6
Montperdu, Tarbes	3408	18.7	17.4	..	—1.3
Col du Géant, Geneva	3346	20.4	17.1	..	—3.3
Maladette	3174	17.4	16.2	..	—1.2
Pic du Midi, Tarbes	2858	15.9	14.6	—1.3	+1.6
" "	..	11.0	..	+3.6	
" "	..	14.4	..	+0.2	
" "	..	13.1	..	+1.5	
" "	..	10.7	..	+3.9	
" "	..	15.1	..	—0.5	
" "	..	10.5	..	+4.1	
Col du Géant, Chamouny	2606	17.1	13.3	..	—3.8

PLACES.	Heights, yards.	Differences of Temp. Cent. Observed.	Computed.	Differences.	Mean Diffs.
Montperdu, Bareges .	2354	18°.1	12°.0	..	−6.1
Pic d'Eyré, Tarbes .	2347	10.3	11.9	..	+1.6
Pic de Montaigu .	2244	11.4	11.4	..	0.0
Pic du Midi, Bareges	1808	10.3	9.2	−1.1	−3.3
" .	..	13.9	..	−4.7	
" .	..	13.1	..	−3.9	
" .	..	12.5	..	−3.3	
" .	..	13.4	..	−4.2	
" .	..	10.8	..	−1.6	
" .	..	13.1	..	−3.9	
" .	..	13.2	..	−4.0	
Puy de Dome, Clermont	1163	6.9	5.9	−1.0	−2.1
" . .	..	7.0	..	−1.1	
" .	..	6.9	..	−1.0	
" . .	..	9.6	..	−3.7	
" .	..	9.4	..	−3.5	
Bédat du Bagneres, Tarbes	611	2.9	3.1	..	+0.2
Pont du Berges, Clermont	537	3.2	2.7	..	−0.5
La Barrague, ditto .	415	1.8	2.1	..	+0.3

How the altitudes here given were found I have not read, but probably from barometric observations.

The mean error or difference from observations is 1°.1. This is a degree of coincidence which, in observations subject to so many causes of error as these are, could scarcely be expected. Were we to allow only 1° for the superior radiation and influence of bodies at the surface of the earth, even this trifling difference would disappear; or were the observations at the same places to be repeated at night instead of in the day, the time at which it is very likely they were made, it is highly probable the difference would be reversed, and be positive instead of negative. For my part, I have no doubt that the apparently more rapid diminution of temperature near the surface than higher up, is owing to the observations having been made in the day time; and that the contrary would happen were they made in the night, especially if the weather was calm and clear. Dr. Wells's experiments, which in the night manifested an increase of temperature of sometimes 12 or 16 degrees at the elevation of only a few feet, are a strong confirmation of this.

III.—Barometric Depression.

Unluckily I have not a single case by me of an elevation determined trigonometrically and barometrically, so that I am incapable of comparing the other formula with direct experiment. However, as Laplace's empirical formula is said to agree exceedingly with observations, its comparison with ours will afford a tolerably good indirect test.

For the ease of calculation, we may suppose the temperatures of the upper and lower barometer to be the same, and at 32° Fahr., or 0 Cent. With these conditions, Laplace's formula (Playfair's Outlines, vol. i. p. 240) is in Eng. fathoms

$$x = 10050 \left(1 - \frac{2\,C}{1000}\right). \log \frac{P}{p};$$

C being the negative Cent. temp. of the higher station. And our theorem in fathoms and logs. is

$$x = \left\{1 - \left(\frac{p}{P}\right)^{\frac{1}{6}}\right\} rh, \text{ or } \log x = \log\left\{1 - \left(\frac{p}{P}\right)^{\frac{1}{6}}\right\} + 4.4164760.$$

Now Laplace's formula affording us no assistance in determining the value of C, we have no resource but to compute it from that theorem which we have shewn to agree so well with observation. Assuming, therefore, $\frac{p}{P} = \frac{28}{30} = \frac{14}{15}$, our formula gives $x = 298.29$ fathoms, from which C = 3°, and consequently by Laplace $x = 299.3$, or 1 fathom above ours. Putting $\frac{p}{P} = \frac{25}{30} = \frac{5}{6}$, ours gives 780.88 and C = 8°, and hence, Laplace's 782.47, or 1.59 above ours. Again, when $\frac{p}{P} = \frac{20}{30} = \frac{2}{3}$, we have from our theorem 1704.8 fathoms, and C = 17°.42, and from Laplace's 1708.1, or only 3.3 more, in an altitude of nearly two miles. In Gay Lussac's great ascent, the temp. sunk from 30°.8 Cent. to − 9°.5; the barometer from 1000 to 432; the density of the air from 1 to $\frac{1}{2}$; and we are informed the height ascended, doubtless determined from these data by Laplace's formula, was 7630 yards, or 3815 fathoms. From the barometric condition, our formula gives 7600 yards, or 3800 fathoms, that is, 15 fathoms less in a height of $4\frac{1}{3}$ miles. The depression of temp., as we have seen in the table, differs likewise only about $1\frac{1}{4}$°.

It should here be observed, that if Laplace's formula coincided perfectly with observations at moderate heights, yet it could not be true at greater elevations; and the greater the height the more it must diverge from nature: for by that formula, the atmosphere must be infinite in extent, a palpable absurdity, which Laplace himself acknowledges. However, the differences which we have shewn to exist between the two formulæ, are much within the limits of error to which probably the best observations could pretend in such heights.

There is a source of error in barometric admeasurements, which it will be difficult for any theory to estimate or avoid; namely, the unequal distribution of vapour in the atmosphere. This will in general tend to depress the lower barometer too much, and consequently to give the altitudes too little. It is probable this may never occasion an error of serious moment, but it will undoubtedly always have some influence.

If we suppose D, d to denote the densities of the air corresponding to P, p, the combination of our two theorems gives

$$\frac{d}{\mathrm{D}} = \left(\frac{p}{\mathrm{P}}\right)^{\frac{5}{6}}; \qquad (6)$$

or in Lussac's $\frac{d}{\mathrm{D}} = (.432)^{\frac{5}{6}} = \frac{1}{2.0126}$, differing from his $\frac{1}{2}$ by only a $\frac{1}{310}$th part.

From these instances some idea may be formed of the perfect fidelity with which the theorems I have given represent phenomena. Probably it will not be hazarding too much to affirm, that the success of them is greater than could have been anticipated; and that there is scarcely a parallel instance in science in which investigations, begun and conducted so absolutely independent of experimental aid, have been so thoroughly confirmed by phenomena. Their mathematical analysis will appear in the work already alluded to.

Several important consequences flow from these theorems, besides those we have mentioned; some of which we shall here notice.

1st. The velocities of sound and the transmission of heat by the air are the same.

2d. The total altitude of the air is equal to

$$\frac{r\,h\,(\mathrm{F} + 448)}{80};$$

or, at a medium, better than 30 miles; and at other times varies directly as the Fahr. temp. + 448.

3d. Since r and h are estimated at a common temperature, when air is constant, the other must be constant too in the same air; and therefore the quantity of air has nothing to do with its total altitude. This would be the same whether there was a half, a third, or a 100 times the quantity.

4th. Other things being alike, the altitude of an atmosphere is reciprocally proportional to the attraction of the body it surrounds at its surface, and the specific gravity of the air under a given pressure and temperature conjointly. If, therefore, our globe was surrounded with hydrogen, its altitude would be about $14\frac{2}{3}$ times higher than our atmosphere is.

Hence a means of determining the altitude of the atmospheres of any of the celestial bodies; and reciprocally, having the altitudes and the nature of the airs, their attractive forces. And hence, too, a proof of the small attractive forces of comets, which have been found by other methods, with a means of computing them, at least approximatively.

5th. By (1) reduced to (4), it appears that the velocity of sound at the surface is independent of the pressure of the atmosphere; and by (2), that the pressure in the higher regions is dependent on this very velocity, and varies with it, being greater or less as this is greater or less. This apparent paradox is easily explained: for at the surface the pressure results from the total quantity of air, but at a given altitude from the total quantity minus that below, which depends on the temperature at the surface, and thence on the velocity of sound.

6th. Our barometric formula (2) requires no aid from the temperatures of the external air. It includes all that is needful within itself, and merely requires that the barometers be of one or reduced to one temperature. Even this it might do without. But as I have elsewhere remarked, Laplace's formula in this respect is singularly helpless; it not merely affords no means of finding the difference of temperatures, but cannot do without it.

7th. By the help of the formulæ here given, the time sound takes to travel over any given space, oblique as well as horizontal, a problem, I believe, that has never been attempted, may easily be determined. For instance, if a be the altitude of the generation of sound, b that of the auditor, and q the distance between the two, the time in seconds is

$$\frac{6\sqrt{2}\,.\,q}{g\,(b-a)}\left\{\sqrt{S^2-\frac{g\,a}{3\sqrt{2}}}-\sqrt{S^2-\frac{g\,b}{3\sqrt{2}}}\right\}; \quad (7)$$

and the time of travelling vertically from the top to bottom of the atmosphere, or the contrary, $\frac{6\sqrt{2}}{g}$ S, in which S is the horizontal velocity at the surface. If, therefore, S = 1089.4, as we have computed it at 32° Fahr., this time is 4′ 47″. 4.

I might here observe, by way of conclusion, that should any one feel disposed to compute a table from our barometric theorem for the more easy measuring of heights, it would be advisable to do it for 52° Fahr. The altitude being taken for this temperature, and multiplied by twice the number of degrees which the temperature of the lower barometer may be above or beneath 52°, $\frac{1}{1000}$th of the product will be the only correction required, and is to be added or subtracted to the preceding altitude, just as the lower barometer's temperature happens to exceed or fall short of 52° Fahr.

For the Cent. thermometer, the table had better be computed for 0 temp. or the freezing point.

Proceedings of the Royal Institution.

January 22nd.

Mr. Faraday on the Chevalier Aldini's proposed method of preserving men exposed to flame.—We gave an account in our last volume* of the plan which Aldini has proposed, and put into practice, for the purpose of obtaining the above object. Since then the Chevalier has arrived in town with his fire-proof clothings, the power of which it was the object of Mr. Faraday to illustrate to the

* Quarterly Journal, N. S. vol. vi. p. 398.

assembled members. The philosophy of flame, its extinction, and the effect of non-conductors, were first distinctly laid down; and then the powers of the apparatus already described, practically shewn. All the experiments that could be repeated in a lecture-room were made; and, in place of those with double hedges of flame, the fireman, clothed and guarded, was exposed to a large and powerful flame from the mouth of a condensed gas vessel. The specimens of asbestos cloth and clothing, laid by M. Aldini upon the lecture-table, were upon so large a scale as to surpass, probably, all that had ever been seen before them.

In the Library, Captain Grover explained a small pocket azimuth and altitude instrument of Captain Kater's invention; and, as Captain Kater has not as yet given a detailed description of this exceedingly useful instrument, the following brief account may not be unacceptable.

The great advantages it possesses, are—extreme portability; the ease and accuracy with which it can be used; and its cheapness.

A circle, three inches in diameter, is fixed to a hollow cone, which moves upon a solid axis, and the whole is supported by a tripod stand, into which this axis screws. At the back of the circle is fixed a spirit level. A telescope, magnifying about eight times, to which are fixed two opposite verniers, moves upon the circle; there is, also, a tangent-screw for slow motion. A ball and socket is screwed into the back of the instrument, which serves as a counterpoise when vertical angles are taken.

In the focus of the telescope are placed one vertical and three horizontal spiders' threads, which are illuminated by a very ingeniously contrived reflector, forming a portion of a hollow cone, silvered inside, which fits upon the object end of the telescope.

The motion in azimuth is given by two projecting pieces attached to a tube, which fits rather tightly on the conical axis; these pieces serve also, by being brought in a line with one of the radii of the tripod-stand, when the telescope is directed to a star, to turn the instrument 180° in azimuth, so as to bring the star into the field of view when the face of the circle is changed from left to right.

To use this instrument, it must be carefully levelled, and the telescope being directed to a star, or other object, so that it appears upon the horizontal wire, and upon or very near the vertical wire, the verniers read off the apparent elevation to minutes. The circle is then turned half-round in azimuth, and the angle being read off by the two verniers, we have the altitude deduced from the mean of four readings.

To take horizontal or oblique angles, the vertical column is unscrewed from the stand, and the ball and socket joint screwed in its place.

With this instrument the time may be deduced from the sun's altitude, taken under favourable circumstances, to within three-tenths of a second of the truth; and the latitude deduced from a *single* observation of the pole-star will seldom differ more than twenty seconds from the truth, or, by a mean of seven or eight observations, it may be determined to five seconds.

The three horizontal threads with which the telescope is furnished make it a very efficient equal altitude instrument.

The great advantages of this instrument over a sextant, to persons travelling on land, are the facility and expedition with which it can be used, requiring none of those troublesome adjustments which in all *reflecting* instruments are necessary; being a perfect circle, with two verniers, any errors of eccentricity are corrected; it can be used when the sun or star is in the *zenith*, which gives it an immense advantage in tropical latitudes over the sextant, and it renders unnecessary that troublesome auxiliary, an artificial horizon. If the sun's altitude be taken, and a different limb be brought to the horizontal wire, when observing with the instrument turned half round, the mean will give the apparent altitude of the sun's centre, consequently there will be no allowance necessary for semi-diameter.

All the parts essential to accuracy are finished in the best manner; and those parts, where a high finish would only add to the expense, are left in a rough state, and painted. The whole is packed in a mahogany box, seven inches long, by four wide, and three deep, which also contains a zenith eye-piece. Mr. Robinson, of Devonshire-street, Portland-place, is the maker, and he charges seven guineas.

This instrument, when intended for more accurate surveys, has a horizontal as well as a vertical circle. The horizontal circle is furnished with three equidistant verniers, and a lower telescope, which, when directed to a fixed object, indicates any accidental derangement which might take place in the position of the instrument. The price of the instrument thus constructed is ten guineas.

Numerous presents were upon the tables, and amongst the rest, some very fine specimens of crystallized glass, from Isaac Cookson, jun., Esq., of Newcastle.

January 29th.

This evening Mr. Fordham gave an account of his proposed method of transferring the power of water-mills, stationary steam-engines, or other cheap first movers, to locomotive engines and carriages, intended to travel on common turnpike roads. His proposal is to compress air by the power of these motors, and then employ its elastic power in propelling the carriages. The following is a brief prospectus of his plan.

The air will be condensed by the power of steam-engines, water-mills, or any other cheap prime mover. The air, when condensed, will be contained in strong but light iron vessels, called recipients; a certain number of these recipients, fixed in a frame, and opening into one common main pipe or tube, will be called a reservoir. Each reservoir will contain a quantity of condensed air sufficient to propel a carriage of a certain weight, one stage of eight or ten miles. The carriage in its appearance, or external form, will resemble a steam-boat in miniature. The wheels will support, and also give motion to the vehicle: the reservoir will be suspended beneath the axle; and the bottom of the frame should not be more than nine inches from the ground. The machinery will consist of two or more cylinders, with pistons, connecting rods, and the apparatus for communicating motion, which is commonly used in high pressure steam-engines. The valves must be made to close at any part of the stroke, for it is necessary to let the air expand in the cylinders, and it will be advantageous also to let the air pass from one cylinder to the other; working in each or all expansively; and permitting it to escape from the last into the external atmosphere. With these conditions in view, a carriage for conveying the mail may be made of the following weight:

	Cwts.	Qrs.	Lbs.
Reservoir, containing 68 cubic feet	13	1	0
Machinery	2	3	0
Carriage	13	0	0
Condensed air	2	0	0
Engineer and guard . . .	3	0	0
4 Passengers, and bags . . .	8	0	0
	42	0	0

The velocity or rate of travelling of such a carriage as this, may be fourteen miles per hour; the expenditure of air on ten miles will not amount to 2000 cubic feet; the reservoir will contain upwards of 3000 cubic feet.

A carriage intended to convey passengers, and not the mail, may be made on the same plan, but the proportions of the several parts will be different.

	Cwts.	Qrs.	Lbs.
Reservoir, containing 140 cubic feet	26	2	0
Machinery	4	0	0
Carriage	16	0	0
Condensed air	4	0	0
Engineer and guard .	3	0	0
20 Passengers, with their luggage .	30	0	0
	84	0	0

The rate of travelling should be at the least ten miles per hour; the expenditure of air will be 4000 cubic feet, the reservoir will contain 6000 cubic feet. It should be observed, that in bad weather the reservoir may be charged with more air than the quantity above mentioned. The expense of compressing the air will vary with the cost of the power employed in condensing it, and the quality of the machinery; but, in general terms, it may be stated that the power of steam produced by the combustion of one bushel of coals will condense 2000 cubic feet of air, under a pressure of 36 atmospheres, or of 36 times 15 lbs. per square inch.

To conclude; on roads of great traffic, the capital invested at present in horses and carriages will be sufficient to erect stationary engines and condensing machinery, and also to construct the locomotive carriages; and, in some cases, the capital required by the proposed plans will be less than that which is now employed.

Some clever little instruments intended to give facilities in nautical surveying were laid upon the library-table by Captain Grover; and amongst them, one intended to lay a boat between two given objects, in which, as in the camera lucida, one half of the eye was occupied in looking forward, whilst, by means of a reflector, the other half was looking directly backward.

February 5th.

Mr. Burnett, on the oak, and especially the naval oak, of Great Britain.—This subject being far too extensive to be fully entertained at a single meeting, he selected some few practical points, which most needed the exhibition of specimens and experiments for their illustration, and contented himself with referring for other information, which might as well be acquired in the study as in the theatre, to his work, the "Amœnitates Querneæ," then lying on the table. These points were the following:—

1. The comparative durability of oak, British and foreign; and of the several native species.
2. Experiments, by which the value of timber for endurance between wind and water, hitherto chiefly judged of empirically, or only discovered by premature decay, may be ascertained previously to its employment in naval architecture, and other important works.
3. The botanical character of the several British species, and their varieties.
4. A notice of the many other trees included by the ancients under the common term oak; and of the use of acorns as food.
5. Recollections of some of the most remarkable oaks, for size, age, &c. &c.

The two first heads were illustrated by numerous specimens, both of native and foreign oaks, exemplifying the very different qualities of their timber, as to strength, stiffness, elasticity, &c.; and some of the examples of long enduring wood may be esteemed antiquarian curiosities: for there were upon the table specimens from the roof of Westminster Hall (thought by many to be chestnut, but shewn by analysis to be oak;) from Windsor Castle, of the time of Edward III.; from Prince John's palace, at Eltham, which was an ancient house, when repaired by the Bishop of Durham, A.D. 1310; from the piles of the old London bridge; from a gun-carriage belonging to one of the vessels of the "Invincible Armada," wrecked A.D. 1588; from Greensted Church, built A.D. 1010—all quite sound; besides reference being made to the tomb of William de Valance, and the shrine of Edward the Confessor, both in Westminster Abbey, which trace upwards of 550 years. The oaken coronation chairs, one of which was made for Mary II. 340 years ago; and the other, the date of which is lost in antiquity, has been in its present situation more than 530 years; Arthur's round table, in the County Hall at Winchester; the ancient vessel lately found in the former channel of the Rother; the canoes discovered in the fens of Lincolnshire; the stakes at Coway, which it is said the ancient Britons drove into the bed of the Thames, to impede the progress of Julius Cæsar, may be likewise named as remarkable oaken relics; but the wooden figures, found by Belzoni in the tombs at Thebes, are probably, as Tredgold observes, the oldest now known that bear the traces of human labour.

For an account of the *rationale* of those experiments, by which the probable durability of timber was designed to be prospectively computed, see a letter on the subject, page 73, of our present number.

After demonstrating from specimens, aided by enlarged diagrams, the botanical character of the several British oaks, and their most notable varieties, Mr. Burnett took occasion to notice, that the ancient signification of the name was much more comprehensive than that which we assign to it at present; formerly the word *oak* meant not only the modern genus *quercus*, but also was applied to many large and sacred trees. Thus, Hesychius writes, Δρῦς παν ξύλον η δένρεον.

Hence, when estimating the value of acorns, as a former luxury, by their present taste, regard must be had not only to the greater size and sweetness of those produced in more southern climes; but also to the variety of other fruits included under the term 'GLANS,' by us translated '*acorn*,' *e. g.* glans Sardinia, the chestnut; glans Phœnicia, the date; glans fagi, beech mast; Jovis glans, or juglans, the walnut, &c. &c.; and in the same manner from Δρῦς and πεπτω, or πιπτω, Δρυπετής, signified all ripe or fallen fruits, Δρυπεπής, an olive, &c.: and hence, Plutarch's Βάλανοφαγοι, or acorn eaters, had no despicable bill of fare.

But the ancient synonymes Φαγὸς or Φηγὸς, and Esculus, for certain species of oak, that is to say, the tree of eating, clearly indicate the absolute use of the true acorn as a staple article of food; and parallel with these runs the text of Isaiah, " to be eaten as the tiel tree and the oak." Mr. Burnett confessed, that he could not speak so favourably of British acorns, as of the timber of the British oak; other persons, however, were of a different opinion. Evelyn, when writing on the subject, with the energy of an epicure, emphatically exclaims, " the young acorns found in stockdove's craws, as well as the incomparable salads taken out of the maws of partridges at a certain season of the year (which gives them a preparation beyond the art of cookery,) are a delicious fare;" upon this Mr. Burnett observed, " There certainly is no accounting for taste."

The bitterness of acorns may, however, be in a great measure removed by maceration in hot water; or subdued by allowing them to germinate, and then, suddenly checking their growth, as is done in malting barley, by which means a considerable saccharine formation is ensured: and, as a proof that acorns make far from despicable bread, a loaf and three dozen small biscuits were placed upon the table as samples thereof; and, as evidence of the practicability of their use in time of scarcity, were eagerly sought after, and speedily consumed by the persons present, many of whom declared (and their opinion has been re-echoed in several of the public prints) that they form not merely an esculent, but a very palatable bread.

Mr. Burnett's observation, that few persons, save those to whom habit has rendered it familiar, form any thing like just estimates of the true size of trees, is certainly correct; and even figures convey to many but an imperfect conception of length and breadth, height and girth; more familiar types should be joined thereto in popular descriptions. When told of an oak, seven or ten feet diameter, it scarcely arrests our attention; but when we reflect that the smaller of these has a width of trunk as great as the carriage-way of Fetter-lane, near Temple Bar, or of Bedford-street, in the Strand, we become convinced of the surprising magnitude of such a living mass of timber. Many such illustrations were given, to enumerate all of which would swell this report beyond its due proportions; let one or two suffice. The long oaken table in Dudley Castle, a single plank cut out of the trunk of an oak growing in the neighbourhood, measured considerably longer than the bridge that crosses the lake in the Regent's Park. The famous roof of Westminster Hall, the span of which is among the greatest ever built without pillars, is little more than one-third the width of the Worksop spread oak; its branches would reach over a Westminster Hall, placed on either side of its trunk, and have near thirty-two feet to spare; the rafters of Westminster Hall roof, though without pillars, have massive walls on each side to support them, but in the tree boughs of sixteen feet more extent are sustained at one end only. The Duke's walking-stick, in Welbeck Park, was higher than the roof of Westminster Abbey. The arch in the venerable Greendale oak, through which there is a road, and through which a carriage can be, and often has been driven, is higher than the entrance to Westminster Abbey (the Poets' Postern). The ground plot of the Cowthorpe oak, now standing, is greater than that of the Eddystone Lighthouse. Upon Arthur's round table might be raised a church of equal capacity with the parish church of St. Lawrence in the Isle of Wight; and if the basement of the Cowthorpe oak were substituted for the table, there would be plenty of room, not only to build the parochial church, but also to allow for a *small* cemetery beside. Indeed, with reference to the last named oak, and also some of those which the ancient Germans used as castles and forts, and in one of which a hermit had his cell and chapel, Mr. B. observed, that St. Bartholomew's, in the hamlet of Kingsland, between London and Hackney, which, beside the ordinary furniture of a place of religious worship, viz. desks for the minister and clerk, altar, staircase, stove, &c., has pews and seats for 120 persons, (upwards of 100 have been in it at the same time.) This chapel is nearly nine feet less in width, and only seventeen inches more in length than the ground plot of the Cowthorpe oak,—in fact, the tree occupies upwards of thirty square feet more than does the chapel.

In the library, amongst many other interesting objects, were some beautiful architectural models in plaster of Paris, by Mr. Day, of Rathbone Place.

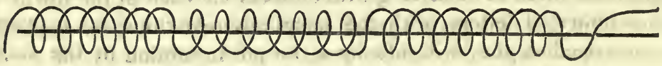
February 12th.

The subject this evening was in the hands of Mr. Ritchie. It was Electro-dynamics, and especially Ampère's proposed application of it to the purposes of a telegraph.

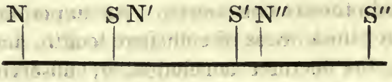
After a short view of the progress of discovery connected with Electro-magnetism, Mr. Ritchie examined at some length the nature and general properties of magnets. A temporary magnet, he remarked, might be made by a powerful magnet, in the same manner as a metallic rod might have its electricity decomposed by the influence of another electrified body. Let P be a metallic body electrified positively, and N P′ an insulated metallic rod, brought near P, but not within the striking distance. Then the electricity of P will attract the negative portion of the natural electricity of N P′ to N, and repel the positive to P′. Let us now substitute a bar magnet for the electrified body, and a piece of soft iron for the rod N P′, then the magnetic fluid (which is in all probability nothing more than common electricity) belonging to the soft iron, would be decomposed by that in the magnet. The pole P would either bring the opposite atoms towards itself, and repel the same kind to the other extremity, or arrange them in opposite directions on each side of the middle—and hence the soft iron would become a real magnet. When the electrified body was removed, the insulated rod exhibited no signs of electricity; and when the magnet was removed, the soft iron returned to its unmagnetised state. The lecturer here remarked a striking analogy between induced electricity and induced magnetism.

Mr. Ritchie then proceeded to remove false notions which are sometimes entertained with regard to the *poles* of a magnet, as if the whole magnetic influence were concentrated in those points. He shewed that the poles of a magnet were nothing more than those points at which the attraction of one half of the magnet, diminished by the repulsion of the other half, was a *maximum.* He candidly confessed, that there were some facts connected with magnetism, which could not well be accounted for on any theory. If a piece of very hard tempered steel be formed into a magnet, and then broken in the middle, *two* distinct and perfect magnets will result; and if each of these be again broken in the middle, four distinct magnets will result, and so on, according to the number of fractures—a fact, which has yet received no satisfactory explanation.

The lecturer then proceeded to state, that the natural electricity of steel might be decomposed by a current of common electricity passing at right angles to the needle to be magnetised, and that powerful magnets might be formed by making the electric current circulate about a spiral of copper wire, having the needle in the axis. By changing the direction of the threads of the spiral, as in the annexed figure, and



placing a long steel wire in the axis, he shewed that any number of magnets might be formed on the same pieces of steel wire. The annexed figure exhibits the magnets thus connected.



For this curious fact we are indebted to M. Arago. He then shewed, by experiment, that magnets might be formed by voltaic electricity even more effectually than by electric discharges. When the discharge from an electric jar is made to circulate round the needle placed on the axis, the north pole is always developed at a certain ending, depending on the direction of the current, and the form of the spiral. When the voltaic influence is made to act in the same manner by means of a single combination of copper and zinc, the north pole is developed as if a current of positive electricity passed along the spiral from the *copper* plate to the *zinc*, or, in other words, that, as far as the metallic conductor was concerned, the *copper* plate acted as the positive side of an electric jar, and the *zinc* as the negative side. Mr. Ritchie thought that considerable ambiguity still prevails with regard to the poles of the compound battery, since the extreme plates of zinc and copper are superfluous, and derange the regular order of the three elements when the circle is completed. Thus, when the circle is completed in the common trough, the arrangement of the elements is, zinc, copper, and fluid— zinc, copper, and fluid. . . . . zinc, copper; zinc, copper: whereas by simply striking out the extreme plates, the regular order of the elements is restored. The lecturer then dwelt particularly on the fact, observed by Oersted, that the magnetic needle invariably places itself at right angles to the conducting wire, provided the needle be rendered astatic. He then shewed, by the ingenious contrivance of M. Ampère, the direction which the needle invariably assumes, when acted on by a voltaic current. Mr. Ritchie concluded, by exhibiting the Electro-magnetic Telegraph, proposed by Ampère, by means of which, rapid communication might be carried on between distant towns in every state of the weather.

Let a number of thick copper rods or strong wires be laid below the public road between distant towns, and let them be connected with the wires of delicate galvanometers in each of the towns. If a number of letters, with the usual abbreviations, be applied to those needles, they will, of course, be put in motion by the passage of powerful voltaic currents. By observing the needles which are successively put in motion in one of the towns by a current sent along the proper wire from the other, it is obvious that a newspaper, printing in London, may be printed at the same time in Edinburgh and other remote towns. As experiments have not yet been made with thick wires of sufficient length, and batteries of intense powers, the lecturer concluded, by observing, that, in the present state of the inquiry, we cannot pronounce, with absolute certainty, with regard to the success of this ingenious project.

Arthur Kett Barclay, Esq., M.R.I., who has lately returned from the northern parts of the continent, placed a magnificent specimen of native platina, from the Ural mountains, upon the library-table. It was in one piece, and weighed upwards of three-quarters of a pound. It had evidently been rubbed or beaten over all parts of the surface.

February 19th.

Mr. Ainger, in a notice on the 'Economy of the Steam Engine,' alluded to the misapprehensions which had at various times existed, as to the saving of fuel which would result from substituting ether or alcohol for water, as the vaporizable material; and he endeavoured to show, that a very simple calculation applied to the known facts, in regard to those substances and their vapours, would have prevented those misapprehensions, and would, indeed, have furnished the same results as have been obtained from experiment. The reasons usually assigned for proposing to use these liquids instead of water, have been the lower temperature at which they assume the state of vapour of a given elastic force (alcohol, for instance, boiling at about 170°, and ether at about 100°); and, also, the smaller latent heats of their vapours, as compared with steam. The boiling point of a liquid, and the latent heat of its vapour, form, however, only a small part of the consideration required for calculating its economy. The cost of a certain quantity of force derived from a given bulk of liquid, depends on the boiling temperature, the specific gravity, and the specific heat of the liquid, and, on the latent heat, the actual weight, and the specific gravity of the vapour. These being known, the relative costs of a certain quantity of power derived from two or more liquids may easily be deduced, as in the following comparison between water, alcohol, and ether.

It may be assumed, that these substances are all supplied to the engineer at the same temperature, say 50°. To raise them to their boiling points, they will require the following additions:

	Boiling Point.
Water . .	212 − 50 = 162
Alcohol .	170 − 50 = 120
Ether . .	100 − 50 = 50

Multiply these numbers by the specific gravities of the liquids, respectively.

	Specific Gravity.	
162 ×	1000 =	162,000
120 ×	800 =	96,000
50 ×	740 =	37,000

These results would require to be multiplied by the specific heats of the three liquids; but, as the specific heats are not very perfectly ascertained, and, as far as they are known, do not appear to differ very considerably; and, further, as the cost of heating the liquid forms a small part of the whole expense, the specific heats may be safely neglected, leaving the numbers, 162, 96, and 37, to represent the expense of elevating to the boiling temperature equal volumes of water, alcohol, and ether.

The cost of vaporizing them will be given by multiplying the actual weights (represented by their specific gravities) of the three liquids by their latent heats, which are about 1000, 450, and 300.

	Weight.		Latent Heat.		
Water .	1000	×	1000	=	1000
Alcohol .	800	×	450	=	360
Ether .	740	×	300	=	222

Add these numbers to those representing the cost of heating up to the boiling points, respectively:

$$162 + 1000 = 1162 \text{ Water}$$
$$96 + 360 = 456 \text{ Alcohol}$$
$$37 + 222 = 259 \text{ Ether}$$

then the last results will express the whole cost of vaporizing equal bulks of the liquids in question; the advantage, so far, appearing greatly in favour of the ether and alcohol, as compared with water. But it is now necessary to introduce another element into the calculation, namely, the specific gravity of the vapour, or the volumes of vapour produced from equal volumes of liquid. These are nearly as the following numbers:

Water . . .	1700
Alcohol . .	610
Ether . . .	300

That is to say, one cubic inch of water becomes about 1700 inches of steam, at atmospheric pressure; and single cubical inches of alcohol and ether become 610 and 300 inches, at the same pressure. The quantity of power is obviously as the bulk of the vapour, and the cost is of consequence inversely as that bulk. If, therefore, the cost of vaporizing be divided by the bulks of vapour respectively, the quotients will represent the relative expense of equal units of power derived from the three liquids.

$$1162 \div 1700 = .6714 \text{ Water.}$$
$$456 \div 610 = .7475 \text{ Alcohol.}$$
$$259 \div 300 = .8633 \text{ Ether.}$$

From which it appears, that, independently of the original cost of the liquid, supposing, indeed, that alcohol and ether were supplied spontaneously, as accessibly, and at the same temperature as water, even then water would be the most economical source of power.

From this it appears, that the temperature at which a liquid vaporizes, and the quantity of latent heat absorbed in the process, form no criterion of its eligibility for the production of mechanical force; and that, therefore, there is no reason at present to expect that power can be obtained from liquid carbonic acid gas, or any other of the gases liquefied by Mr. Faraday, more cheaply than from water, merely because of the low temperatures at which they become highly elastic. Analogy, it is evident, would lead to a conclusion exactly the reverse, and would induce an expectation that the vapour of mercury, or even of metals vaporizing at a much higher temperature, would furnish the most economical motive power.

Mr. Ainger then described a mode of increasing almost indefinitely the power, or, in other words, of decreasing almost indefinitely the expense of the steam-engine, which has not hitherto been suggested, and which appears to require for its realization, only the discovery of a succession of liquids, whose boiling points should differ about 100° of Fahrenheit; whose nature should not alter by repeated distillation; and which should exert no injurious action on the substances composing the machinery of the steam-engine. The difficulty of finding such a series of liquids is probably insuperable; if it were not so, there can be little doubt that the cost of steam power would be susceptible of an immense reduction. If, for instance, a succession of liquids could be obtained, whose boiling points were 612°, 512°, 412°, 312°, and 212°, and if the furnace were applied to the first, and its vapour were employed to work a condensing engine, it is clear that the vapour which was condensed at 612°, could be made to evaporate the second liquid, by condensing the first on the surface of the vessel containing the second, the vapour of which would, in its turn, work a steam-engine. The condensation of the second vapour at 512° might, in like manner, evaporate the third liquid which boils at 412°, and so on, till the water which boils at 212° was evaporated, and which might be condensed by injection in the usual way.

It may perhaps be thought that a cooling surface at 512° will not sufficiently reduce the tension of a vapour at 612°, to leave any effectual difference between the pressures on the two sides of the piston; but it must be recollected, that a depression of 100° reduces the elastic force of a vapour produced at 612°, as much as of one produced at 212°. The elastic force of common steam at

112° is equal only to $2\frac{1}{2}$ inches of mercury; the elastic force, therefore, of the vapour produced at 612° would, when cooled to 512°, be also equal only to $2\frac{1}{2}$ inches of mercury. There is, it must be confessed, a difficulty in condensing by mere contact with a metallic surface, as compared with condensation by an injection: but this difficulty would, in the proposed case, be much less than in the various schemes which have been projected to use alcohol, ether, and liquid carbonic acid, because in the former it is proposed to cool a less easily vaporized substance by one more easily vaporized; whereas, in the latter cases, water, which has been the intended cooling material, is less easily vaporized than the substances it is required to cool; a circumstance obviously unfavourable to the production of the effect. But for this difficulty, it is probable that the heat employed to vaporize water might, by the condensation of the steam, be transferred to alcohol, and from this again to the ether; but the question then arises, how is the heat to be abstracted from the ether; we have no other means than the contact of a vessel containing cold water, a means which is found insufficient for cooling common steam, and which would, therefore, be doubly inefficient in cooling the vapour of ether. These considerations will suggest other difficulties in the construction of engines to use alcohol and ether, beyond the absolute defect of economy, which has been before explained.

February 26th.

This evening Mr. Watson developed his plan for preventing ships from foundering at sea. This plan, as is well known, consists in introducing air-tight copper tubes into various parts of the ship, so as to be out of the way, and yet, by their buoyancy when immersed in water, prevent the ship from sinking when full of the fluid. Mr. Watson illustrated his plan by experiments on the buoyancy of different kinds of wood, and also by models of an 80-gun ship, furnished with air-tubes. The plan is so fully before the public in various forms, that we do not think it necessary to enter into further details here.

In the library Captain Grover exhibited a Wollaston's double microscope, made after the model left by Dr. Wollaston to Captain Kater, and the first that had been constructed. Its performance was admirable.

Captain Blake, who purposes shortly setting out on a journey over land to India, also placed his portable barometer and other instruments upon the table.

A scull, with other things, was sent by Octavius Morgan, Esq., M.R.I., to be laid upon the table. It was accompanied by a note, of which the following is part:—" I have also sent with them a curious, and, as it seems to me, a non-descript scull, which I found in an old closet at our house in Monmouthshire: it is apparently a diluvian fossil, but has no orbits, and instead of a suture has a strange crest. Unless it be of the Saurian tribe, I am at a loss to give it a name, and should feel obliged if some of the scientific gentlemen who attend our Friday Evenings could give me information respecting it."

March 5th.

The subject this evening was the transmission of musical sounds through solid conductors, and their subsequent reciprocation. It was delivered by Mr. Faraday for Mr. Wheatstone, and was a continuation of the phonic demonstrations, which have been proceeding for the last two seasons. We expect from Mr. Wheatstone a correct account of the new matter introduced this evening, and also of the subject generally, which shall appear in the next Number of our Journal.

MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *Transparent Watch.*—A watch has been presented to the Academy of Sciences of Paris constructed of very peculiar materials, the parts being principally formed of rock crystal. It was made by M. Rebellier, and is small in size. The internal works are all visible; the two teethed wheels, which carry the hands, are rock crystal; the other wheels are of metal, to prevent accidents from the breaking of the spring. All the screws are fixed in crystals, and all the axes turn in rubies. The escapement is of sapphire, the balance-wheel of rock crystal, and its spring of gold. The regularity of this watch, as a time-keeper, is attributed by the maker to the feeble expansion of the rock crystal in the balance-wheel, &c. The execution of the whole shews to what a state of perfection the art of cutting precious stones has been carried in modern times.—*Revue Ency.*, xliv. 796.

2. *On the Elastic Force of Vapour at high Temperatures.*—A committee, appointed by the Academy of Sciences, has been engaged in carrying on experiments to determine the elastic force of vapour at high pressures: the labours have principally devolved upon MM. Dulong and Arago. The results have been obtained experimentally up to 25 atmospheres, and extended to 50 by calculations. That no error dependent upon the use of valves should interfere, it was resolved to estimate the force exerted by the columns of mercury sustained. A glass tube was therefore prepared by MM. Thibaudeau and Bontemps, consisting of 13 pieces, 2 metres (78.74 inches) each in length, 5 millimetres (0.2 of inch) in diameter, and the same in thickness. Each piece was sustained by counterpoises, so that the lower should not be crushed by the upper, and the whole was erected in a square tower, which is the only remains of the ancient church of St. Genevieve.

Fearing that if the steam from a boiler were made to act directly upon such a column of mercury as this tube would sustain, it might, from intermission of its force, occasionally produce such sudden agitation in the metal as to endanger the safety of the whole, it was resolved to form a kind of manometer, in which the compression of a given volume of air should be ascertained, first, by the column of mercury, and afterwards used as a measurer of the elasticity of vapour at various temperatures. In this way the estimations would be as accurate as if made directly by the column of mercury. The preparation of this instrument gave an opportunity of examining the law of Mariotti, namely, that all gases are compressed in volume in proportion to the energy of the compressing force. Boyle and Muschenbroek thought they saw errors in this law, even when the force was not above 4 atmospheres. Robison and Sulzer carried the force to 8 atmospheres, and agreed in giving the same departure from the law, namely, that when compressed eight times, instead of exerting a force eight times that of the common air, it was only six times greater. Oersted, on the contrary, found the law true to 8 atmospheres, and even up to 60 atmospheres; but his mode of experimenting is not satisfactory to the French commissioners, though the results were correct.

In the preparation of the manometer the experiments were carried to 27 atmospheres, and the law found to be *correct.* It was intended to ascertain if it held good with other gases than air, but the authorities forbade the use of the old church tower for this purpose.

There appears to have been much fear about steam at the pressure of 24 or 25 atmospheres; and, lest the boiler should explode, and blow up the old vaults, and even destroy neighbouring buildings, it was determined to have it in the court-yard of the observatory, and make the experiments there. Ultimately, therefore, the manometer was transferred, though with great difficulty, and finally placed in proper communication with the boiler.

Some important precautions were now taken to ascertain the temperature accurately. The first was to take account of the cooling effect of the air on that part of the thermometer exterior to the boiler; this was done by retaining it constantly at the same temperature. The next was to prevent alteration in the capacity of the bulb, by allowing the vapour to press upon it. This was effected by putting the thermometers into gun-barrels, made thin, closed at one extremity, and filled with mercury; these, when fitted to the boiler, were made to descend, one to the bottom of the boiler nearly, to give the temperature of the water; the other to within a few inches of the water, to give the temperature of the vapour.

The temperature and pressure were then experimentally ascertained up to 24 atmospheres; after which formula was sought for, by which they could be extended to higher pressures, and the following one adopted:

$$e = (1 + 0.7153\, t)^5$$

e being the elasticity; t the excess of temperature above 100° C, taking for unity 100° of the centigrade thermometer. This formula nearly represents the results given by experiment up to 24 atmospheres; the greatest error has been at 8 atmospheres, and was then 0.9 of a degree. It was more accurate for the higher pressures, being calculated from them, and the commissioners have no doubt that at 50 atmospheres the error is not more than 0.1 of a degree.

Elasticity of the Vapour taking the Pressure of the Atmosphere as Unity.	Temperature. Centigrade.	Fahrenheit.
1	100.	212.
1½	112.2	233.96
2	121.4	250.52
2½	128.8	263.84
3	135.1	275.18
3½	140.6	285.08
4	145.4	293.72
4½	149.6	301.28
5	153.8	308.84
5½	156.8	314.24
6	160.2	320.36
6½	163.48	326.26
7	166.5	331.70
7½	169.37	336.86
8	172.2	341.96
9	177.1	350.78
10	181.6	358.88
11	186.3	367.34
12	190.0	374.00
13	193.7	380.66
14	197.19	386.94
15	200.48	392.86
16	203.60	398.48
17	206.57	403.82
18	209.4	408.92
19	212.2	413.96
20	214.7	418.46
21	217.2	422.96
22	219.6	427.28
23	221.9	431.42
24	224.2	435.56
25	226.3	439.34
30	236.2	457.16
35	244.85	472.73
40	252.55	486.59
45	259.52	491.14
50	265.89	510.60

The members of the committee remark, that they could find only one English table of the force of high pressure vapour; it had been given to M. Clement by Mr. Perkins, but it was found sadly erroneous: for instance, at the temperature of 215° C., or 419° F., the force in it is given as 35 atmospheres, whereas it is really only 20, or little more than one half. In Germany a table has been constructed by M. Arzberger, of Vienna, which rises to 20 atmospheres, and is much nearer the truth than Mr. Perkins.

It is about 3 atmospheres wrong at the highest pressure.—*Bib. Univ.* xlii. p. 338.—*Bull. Univ.* A. xii. 407.

3. *On the Motion of Currents in Liquids.*—M. Dutrochet has made some singular remarks on the influencing causes of the motion of currents in liquids, and has found light to affect them considerably. He finds that difference of temperature is the efficient cause, and that 1-800th of a degree of difference is sufficient, when aided by light. In the absence of light such motion will cease. When the windows of the experimental room are closed so much as only to admit light enough to distinguish, whether the circulating motion continues or not, it soon ceases; when re-opened, the motion recommences. When completely suspended by the absence of light, if the table be struck with slight blows, the vibration communicated immediately causes the motion to recommence; when the liquid was again at rest, the sound of a violoncello, or a bell, was occasioned, and the vibrations communicated to the liquid, again caused it to acquire circulating motion. Hence it would appear that the vibration of the particles of a liquid favour the circulating motion which a slight difference of temperature is competent to produce; that this previous vibration is a necessary circumstance, and that light consequently only acts by producing it amongst the particles of the fluid. From whence M. Dutrochet concludes, that, in the phenomena of circulating motion in liquids, two causes intervene—the *efficient* one, difference of temperature, the *favouring* one, light, or any other circumstance which can communicate feeble vibrations to the particles of fluid.—*Revue Ency.*, xlv. 234.

4. *On the Expansive Force of Freezing Water.*—Experiments on the force exerted by water when it becomes solid, have been made in the Arsenal at Warsaw, during the winters of 1828 and 1829. Howitzer shells were used for the purpose; they were of cast iron, 6 inches 8 lines in diameter, the aperture 1 inch 2 lines in diameter, and the thickness of metal 1 inch and 2 lines. One, 46.29 cubical inches in capacity, was filled with water at 41° F., left open and exposed to the air at 21° F.: the frozen water formed a projecting cylinder of ice of the diameter of the aperture, which in two hours was 2 inches and 2 lines in length; this was the maximum effect, and shewed that the water, by freezing, had undergone an expansion of 2.31 cubic inches, or $\frac{1}{20}$ of the whole volume. Another shell, after being filled with water, had the aperture closed by a wooden plug; it was then exposed to the same cold as before, the plug was driven out, and the ice occupied its place. Another shell, after being filled with water, was closed by an iron screw, perforated by a hole 3 lines in diameter, the temperature of the air was the same; after seven hours the shell was burst into two unequal parts, the larger being thrown a foot, and the smaller 10 feet from the place; the ice had been formed to the thickness

of 6 lines only, the rest of the water remained fluid. Another shell was filled with water at 46° F., the aperture closed by a metallic screw, having a passage 6 lines in diameter; it was exposed to the air at 28° F., the shell burst into two pieces, one of which was thrown 4 feet off, the thickness of the crust of ice was 13 lines, and the water within remained liquid. Another shell was filled with water at the same temperature, closed by a solid screw, and exposed to the air at 28° F. as before; the shell was burst into two parts, the smaller being thrown about a foot from the place, and the thickness of the coating of ice within was 5 lines.—*Bull. Univ.* F. xiii. 314.

5. *New Hygroscope.*—This instrument is the invention of M. Benout, and although he calls it a hygroscope, it still has the power to a certain degree of *measuring* moisture present in air. Its indications appear to be exceedingly delicate; its action depends upon the expansion and contraction of paper, when exposed to moisture and dryness; that used is what is known by the term *papier végétal,* which is exceedingly hygrometric, very thin and very homogeneous. The principal piece in the instrument consists of an exceedingly thin metallic plate about 10 inches long, and $\frac{8}{100}$ of an inch in width; it is formed into a helix, and the external part is covered with a plate of the paper above mentioned, of the same size, united to it by means of a cement unacted upon by moisture. When this spiral is in moist air, its external surface being hygrometric expands, whilst the inner does not, and a motion of torsion takes place, which may be rendered evident by a needle attached to the lower coil, and traversing over a graduated circle. The contrary motion occurs as the instrument becomes dry. The length of the spiral is conveniently made such as to allow a motion of the needle, in one revolution from 60° to 100° on Saussure's scale.

This instrument is liable to certain objections, amongst which are the following: i. As the paper becomes moist, it expands, overcoming the elasticity of the metal, but ultimately it becomes so soft, that though it is becoming damper, the force of the metal overcomes the paper, and the needle returns towards dryness; this effect only happens near to extreme moisture, at 95° of Saussure's scale. ii. The elasticity of the metallic plate varying with the temperature, causes that changes in heat are sufficient, by influencing the quality, to produce motions in the needle. iii. The two parts of the spiral, the metal and the paper, being of unequal expansion by heat, the instrument acts like Breguet's thermometer. The effect of heat is the same with increase of moisture, as to the motion of the needle; the effect of cold is the same as increased dryness.

The sensibility of the instrument is very great; an experiment like one of Saussure's was repeated with it; the instrument was put under the receiver of a good air-pump, and when stationary, rapid exhaustion was effected; in 6 or 7 seconds the needle had risen from 45.5 to 59; it then became stationary for a moment, and afterwards proceeded to indicate dryness with such rapidity, that in 12 seconds the needle had traversed the circle. The exhaustion being discontinued, and the air allowed to enter, it returned to the point from whence it parted in 8 seconds, giving a motion over 105° in less than half a minute.—*Recueil Industriel,* xii. 45.

6. *Adhesion of Metals.*—M. Prechtt states that the force with which two plates of the same metal adhere, is the same as that with which one of the plates will adhere to a plate of another metal, having a less adhesion, however, for metal of its own nature; thus if two plates of copper will adhere with a force of 21 grains, then one of these copper plates will adhere to a similar plate of bismuth, zinc, tin, or lead, with the same force, although two plates of any of these metals will adhere with a less force.—*Bull. Univ.* A. xii. 440.

7. *Effect of Solar light upon Magnets.*—This subject has been investigated experimentally by M. Zantedeschi, with all the care he could give to it, for the purpose of ascertaining, not the effect of the decomposed and separated beam of light, or any particular part of that beam, but the effect of the sun's light generally. He has been, in part, anticipated by others; but his results, if exact, still add something to what was before known. The scientific world is, however, now well aware, that numerous influential causes, besides those which are purposely brought into force, are frequently active, and that an unreserved assent can hardly, as yet, be given to any of the statements which have been made relative to the action of light, in producing and affecting magnetism.

M. Zantedeschi calls the phenomena electro-magnetic: if this term is used in contradistinction to ordinary magnetism, we see no reason for its use here; for no reference is afterwards made to any thing like electrical phenomena, or electrical conditions. Barlocci has said that a magnet, able to carry 1lb. 6oz., after being exposed for three hours to bright sun-light, raised 2oz. more, and, in twenty-four hours, had its power *doubled.* A similar magnet in a dark place remained unaltered; after two days, the power did not increase. This and similar effects were repeated and confirmed by M. Zantedeschi, who obtained the same results with natural magnets. A dry, cloudy day produced no effect of the kind.

It appeared that magnets which were invested with a film of oxide were thus strengthened by sun-light, but that those which had been freed from oxide, on the contrary, became feebler, unless the polish of the metal had been carried to such a degree, as to enable it perfectly to reflect the sun's rays, and then no alteration took place.

A great difference appeared when the sun's light was thrown upon the north, or upon the south pole; in the first case, power was gained, and in the second lost, but the gain was, on an average, greater than the loss. The heat communicated in these experiments tended to diminish rather than increase power, and cooling was found to be favourable. The loss of power in a south pole became less when it was removed out of the sun's light; the gain of power in the north pole was increased by the same circumstance.

In the midst of these details, however, M. Zantedeschi states, that he met with anomalies, and says, that magnets are a sort of proteuses, which change under the eyes of the most careful observer.

A very singular fact, however, verified by numerous repetitions, was that on days when the sun was hid by thin clouds, the south pole, subjected to its concentrated rays, increased in power; whilst the north pole, by similar treatment, lost force. This extraordinary effect is hypothetically accounted for, by supposing that light has a *negative polarity,* which is inversely as the layers of clouds floating in the atmosphere.

Experiments were then instituted, like those of Mr. Christie, in which it was found, that the direct light of the sun diminished the arcs of oscillation of a vibratory magnetic needle. These effects were obtained, and Mr. Christie's remarks confirmed. M. Zantedeschi endeavoured to ascertain whether here also, the exposition of the north or south pole to the light made any difference. A series of experiments, repeated above thirty times, shewed that when the north pole was exposed, a greater number of oscillations and a smaller amplitude were obtained, than when the south pole was exposed. Still, it was remarked that, on those days, when a thin veil of clouds covered the sun, the results were inverted.—*Bib. Univ.* xlii. 193.

8. *Non-interference of different Electric Currents.*—Professor Marianini has been led to examine what might take place when electric currents are so directed as to cross each other, with a view of ascertaining whether there might be any interference; but he finds none, and in this respect, draws a strong analogy between electricity and light; the rays of the latter, as is well known, crossing each other in *almost* all directions, as if quite indifferent.

At first, by attaching two zinc and two copper plates to the sides of a cube of wood, and connecting them into cross pairs by wires, and plunging them into a vessel of acid, it was found that whether one or both pairs were connected, not the slightest difference resulted in the strength of the current in the wires when examined by a magnetic needle: here, therefore, was no interference.

Whether the currents were equal, or unequal; of one, or of many hundred pairs of plates; put in action together, or successively; when three currents were used in place of two; when they were made to intersect each other at acute angles, or to pass parallel to each other; even when they were made to pass through the same metallic communication—still no interference or alteration was observed.

M. Marianini considers these effects as favourable to the Franklinian theory of electricity. In a note, he also quotes other effects, which he thinks far more easily explained by the theory of one electric fluid. In an electro-motive pair, if the negative plate be more deeply immersed in the fluid, the effect is greater than if the positive plate be immersed. If a plate of tin, or some other metal, 18 or 20 square centimetres in surface, be formed, at one side, with a narrow projecting band, and then placed so that the plate is in one vessel of water (*a*), and the band in another (*b*)—if then, a plate of zinc be placed in the vessel (*b*), and a plate of copper in (*a*), neither of them touching the metal—if then a galvanometer be connected with the zinc and copper plates, a feeble deviation will be obtained; but if the zinc and copper plates be reversed, a much stronger effect will be procured.

"It is in vain that I endeavour to explain this fact by the theory of two electric fluids, since, if on the one hand, when the plate of zinc is in (*b*), the passage is rendered difficult for the vitreous fluid, and facilitated for the resinous fluid; on the other, when the copper plate is there, the passage is rendered difficult for the resinous fluid, and facilitated for the vitreous. There is no reason, therefore, for a difference of results. But in the theory of one fluid, it is easy to conceive, that, in the first case, the electric fluid which is diffused, as if by radiation, through the liquid, would find the passage more difficult than in the second case, and consequently the electro-magnetic effect, which is known to depend principally upon the rapidity of the electric current, should be less in the first case than in the second."—*Ann. de Chimie*, xlii. 131.

9. *Heated Air used in Iron Furnaces.*—Heated air for blast furnaces has been used for some time at the Clyde iron works, and with great success. Experiments have proved that iron is smelted by heated air with three-fourths of the quantity of coal required when cold air, *i. e.*, air not artificially heated, is employed for that purpose, while the produce of the furnace in iron is at the same time greatly increased. All the furnaces at Clyde iron works are now blown with it. At these works, the air, before it is thrown into the blast-furnace, is heated to 220° Fahrenheit, in cast-iron vessels, placed on furnaces similar to those of steam-engine boilers. It is expected, that a higher temperature than 220° will be productive of a proportionably increased effect. But this is a subject of experiment. It is supposed that this improve-

ment will accomplish a saving in the cost of the iron in Great Britain to the amount of at least 200,000*l.* per year.—*Jameson's Journal, Jan.* 1830.

10. *Preservation of Corn in Siloes.*—An account has been read to the Society of Agriculture in France, of the opening of a silo, in which the corn was found in an unexpected state. The place consisted of an ice-house, and the grain, when put in, was of the finest appearance, perfectly dried, and in excellent condition. The door had been hermetically sealed, and yet, when opened, a considerable thickness of the mass of corn was found destroyed by weevils, the latter being in such quantity as to occasion an elevated temperature. As part of the same corn had been perfectly well preserved in other siloes, the cause of this deterioration was sought for, and a hole was found in the lower part, which had been made by mice, and which, by admitting air in sufficient quantity, had allowed the weevils originally in the corn to live and increase their numbers to the degree mentioned. After some observation upon experiments, which shewed that insects could live for a very long time in vitiated air, a committee was named to ascertain the requisite state of the air, and the circumstances connected in the inquiry, with the preservation of grain in these repositories.

At another meeting of the society, M. Hachette described the method proposed by M. Clement to prevent the destruction of corn by weevils. It is founded upon a fact observed by him, that these insects cannot live in an atmosphere which contains less than a certain proportion of moisture. He therefore proposes that the corn shall be subject to a continued ventilation of air, dried by passing over quick lime or chloride of calcium. All the weevils originally in the corn would thus be quickly destroyed.—*Recueil Industriel,* xii. 73. 208.

§ II. Chemical Science.

1. *Preparation of Bromine and its Hydrate.*—The mother liquors containing bromine are to be evaporated to a fourth of their volume in iron pans, and then left for several days, in which time the largest part of the chloride of calcium crystallizes. The supernatant liquor, being diluted with water, is to be mixed with sulphuric acid as long as a precipitate is formed. The liquid portion being separated, and the solid residue pressed, all the fluid is to be mingled and evaporated to dryness, and then re-dissolved, that a certain quantity of sulphate of lime may be removed. On acting upon the solution by sulphuric acid and peroxide of manganese and then distilling, bromine is obtained.

Hydrate of Bromine.—This compound is easily formed at a temperature of from 4° to 6° (39° to 43° F.), by making the vapour of bromine pass into a tube moistened with water; in about a quarter of an hour the tube is filled with solid hydrate.—*Annalen der Physik,* xiv. 485.

2. *Detection of Iodine.*—M. Balard's process for the detection of iodine, which consists in mixing the fluid to be examined with starch, sulphuric acid and chlorine, is the most delicate that is known. M. Casaseca has remarked that when the quantity of hydriodate is very small, the blue indicating ring cannot be seen at the part where the solution of chlorine is in contact with the water containing the starch and acid; then the whole should be strongly agitated and left for a while, when the starch acquires a distinct violet colour. One part of hydriodate of potash was dissolved in 2 parts of distilled water; a drop weighing 0.0455 grammes was put into 14 litres of water, to which were added 2 grammes of starch, a little sulphuric acid, and 8 drops of a solution of chlorine: in 14 hours the starch became slightly coloured, in 24 hours strongly tinted of an amethystine or violet hue. Hence it appears the test thus applied is able to detect 0.0000008 of a part of iodine in solution.—*Jour. de Pharmacie.*

3. *Preparation of Hydriodic Ether.*—A method proposed by M. Serullas for obtaining this compound is as follows: Put 40 parts of iodine and 100 parts of alcohol into a tubulated retort, and add $2\frac{1}{2}$ parts of phosphorus in fragments, at the same time shaking the retort; distil nearly to dryness, then add from 25 to 30 parts more of alcohol, continue the distillation, and cease at the same point as before. Add water to the product, the ether will immediately separate and sink; it is to be washed in the usual way, and rectified from off some fragments of chloride of calcium.—*Ann. de Chimie,* xlii. 119.

4. *Chloride of Phosphorus and Sulphur.* Serullas.—By passing a moderate current of dry sulphuretted hydrogen into a vessel containing perchloride of phosphorus, muriatic acid was evolved and a liquid compound produced; the latter, being collected and rectified by distillation in a small retort, was obtained as a limpid colourless liquid, heavier than water, with a sharp aromatic odour, mingled with that of sulphuretted hydrogen; producing vapours in the air, and boiling at 257° F.

This substance was found to contain nothing but chlorine, phosphorus, and sulphur, in the following proportions:

3 Proportionals	Chlorine . . .	10.194
1 Ditto . .	Phosphorus . .	3.010
1 Ditto . .	Sulphur . . .	3.080
		16.284

Ann. de Chimie, xlii. 25.

5. *On the Effect of Ammonical Gas upon Heated Metals.*—It is

well known that both M. Despretz * and M. Savart * have published some account of the peculiar action which takes place when ammonia is passed over heated metals. M. Despretz has more recently read a memoir on the subject, from which we extract the following details.

The specific gravity of copper has been reduced in the operation from 8.9 to 5.5, without sensible augmentation of weight. In other experiments considerable diminution of specific gravity has frequently been obtained, when the increase in weight has been less than $\frac{1}{300}$. Having submitted the same metal to the repeated action of ammonical gas, 100 parts of iron have been increased to 111.5 parts by weight. The following are some of several other results.

9.427	parts of iron became	10.102	or 100 became	107.162
6.587	. . . . . .	7.095	. 100 . .	107.728
29.960	. . . . . .	31.472	. 100 . .	105.046
7.955	. . . . . .	8.553	. 100 . .	107.517

That the increase of weight thus obtained is not due to oxidation from air, water, or carbonic acid, is thus shewn. Air was excluded by passing the ammonical gas for some time before heat was applied, and also after the whole was cold again. Water was excluded by passing the gas through a tube 40 inches long filled with chloride of lime. Carbonic acid gas, by washing the ammoniacal gas in a solution of potash. The small quantity of water left in the gas by the chloride of lime is considered as unable to cause oxidation, whilst such enormous excess of ammonia as the experiment required is constantly present. The experiment usually lasted 6, 7, or 8 hours.

If the iron presented the least blueness upon the surface it was returned to the tube and again heated in ammonia. It was only considered good when the iron had the whiteness of unpolished platina. In this state it was brittle and even friable; light, by comparison with common iron, having frequently a specific gravity of 5. only, and also less changeable in air or water: it was still magnetic and readily soluble in acids.

M. Despretz thinks that many of the changes in the iron are produced not by a permanent, but by a momentary state of combination of one of the elements of the ammonia, and thinks this sufficient to account for the alteration in properties both in it and copper, when the actual weight was not increased $\frac{1}{1000}$dth part. To illustrate effects of this kind, he oxidized red-hot iron by the vapour of water, and then reduced it again perfectly by hydrogen gas; the cohesion of the iron was very much diminished, and the specific gravity lowered from 7.79 to 6.18.

The next point was to ascertain whether it was oxygen, or carbon, or ammonia, or one of its elements, that by a temporary

* Quarterly Journal of Science, N. S., vol. iii. 476, iv. 205, vi. 175.

combination produced these effects. 7.094 parts of ammoniated iron were heated in a tube, and hydrogen gas passed over them; the gas was dried before coming into contact with the metal, and again afterwards by passing through a tube containing chloride of calcium, and carefully weighed; the iron was reduced to 6.585 parts in a pure state, so that it had lost 0.509, and the chloride of calcium tube had increased in weight only 0.05. Oxygen, therefore, could not have made more than $\frac{1}{10}$th of the matter which had been fixed in the iron. Other experiments gave a smaller quantity of water. The water through which the gas passed, after leaving the iron and drying tube, rendered syrup of violets green, and contained ammonia. Hence there is reason to believe that nitrogen is the substance absorbed by the iron.

Ammoniated iron, dissolved in dilute sulphuric acid, gave hydrogen gas mingled with nitrogen, the latter being occasionally as much as 6 per cent. of the whole quantity. All the specimens of iron, which had increased in weight, gave nitrogen gas when dissolved in acids. The volume of mixed gases evolved is less than the volume of pure hydrogen produced by an equal weight of pure iron, in consequence of the formation of a little ammonia, which may be found by evaporating the acid liquor and mixing quick lime with it; much ammonia is set free.

The substance absorbed by the iron cannot be either hydrogen or ammonical gas; for in the first case the volume of the gas would be greater than with pure iron, and in the second equal *, whilst on experiment it has been found less. Nor does it appear to be carbon.

Copper is more altered than iron in these kind of experiments; its specific gravity is frequently lowered $\frac{1}{3}$, being 5.5. It becomes scaly, porous, and coloured variously, grey, yellow, green, orange, red, purple, &c.; it always has a nacreous crystalline aspect: but it retains a very little extraneous matter. It is, therefore, supposed that the copper combines with a considerable quantity of azote; but immediately gives off this substance again, for M. Despretz thinks the experiments fully establish the fact that the alteration in the properties of the metals is due to a durable or a momentary combination of them with nitrogen.

No conclusion is drawn respecting the nature of ammonia from these experiments, but further results are promised.—*Ann. de Chimie*, xlii. 122.

6. *Fluid in the Cavities of Rock Salt.*—Dr. Nicol has examined certain samples of rock salt, which being clear, colourless, and transparent, exhibited small cavities in innumerable quantities, some of which contained a fluid, and others fluid with a bubble of air. Upon examining the fluid, it was found to differ from satu-

* Not equal; $\frac{1}{10}$th less, for instance, if only $\frac{9}{10}$ths were iron and the rest the peculiar substance.—*Ed.*

rated solution of salt, and in fact to consist of a saturated solution of muriate of magnesia, mixed with a little muriate of lime; the salt which contains these cavities and fluids being itself perfectly free from magnesia and from chloride of sodium.—*N. Edin. Phil. Jour.*

7. *On the Formation of Steel by means of Silica.*—It is well known to chemists, that silicum combines freely with iron, and also, that certain persons have considered silica a substance which possesses in common with carbon, and perhaps boron, the power of converting iron into steel. M. Boussangault, if we mistake not, put forth some views, in which he considered silicon as the constant agent in the production of steel.

The Editor of the *Recueil Industriel* has described several cases, in which the presence of silicon appears to have an important influence of this kind. Some time since, Dr. Eynard of Lyons, having put some files into a mixture of five parts of water with one of sulphuric acid, was astonished when he took them out, to find, at the bottom of the glass vessel in which they had been placed, a greyish-white and glairy substance. This, being collected and dried, proved to be silica.

In 1732, MM. Perru of Neufchatel established a manufactory of steel cylinders at Lyons. These cylinders, which were highly polished, and so hard as to be untouched by the file, had been fused, it is said, with *silica*. They have never been imitated. A pair of these cylinders, 5 inches in diameter, sell at this time for 2400 francs.

Some years since, a person of the name of Ranquet, of Lyons, formed vessels of white cast-iron of extreme hardness. M. Culhot manufactured cylinders from the fragments of these vessels, and the cylinders were of such hardness, it was impossible to shape them by cutting instruments, so that they could be worked only by emery, and similar substances, occupying months for the purpose. This person made a secret of his process, but it is said, used no charcoal in his process.

M. Eynard found silica in cast iron, but not in forged iron; its occurrence in the former, or rather the presence of silicon, is a well-known circumstance.

Upon these facts, the Editor founds some earnest recommendations to those who have the opportunity of ascertaining whether the cementation of iron without charcoal, but with silica alone, would produce steel, and in what proportions the silica ought to be mixed, to obtain a very perfect and hard steel; there are also other points urged, which are either already upon the minds of metallurgists, or are naturally suggested by the two above. —xi. p. 311.

8. *Analyses of various Cast Irons and Steels.*—These analyses have been made by MM. Gay-Lussac and Wilson, at the works of Charenton. They are appended to a work on chemistry by M. Lassaigne.

	Quantities per 1.00000 *parts of*					
	Carbon.	Silicium.	Phosphorus.	Manganese.	Iron.	Obtained by
Welch grey pig	0.02450	0.01620	0.00780	traces	0.95150	coke.
Do.	0.02550	0.01200	0.00440	„	0.95310	coke.
Do.	0.01666	0.03000	0.00492	„	0.94842	coke.
Franche Comté do.	0.02800	0.01160	0.00351	„	0.95689	coke.
Creusot do.	0.02021	0.03490	0.00604	„	0.93385	coke.
Champagne do.	0.02100	0.01060	0.00869	„	0.95971	coke.
Berry do.	0.02319	0.01920	0.00188	„	0.95573	wood & coke.
Nivernais do.	0.02254	0.01030	0.01043	„	0.95673	charcoal.
Champagne white pig	0.02324	0.00840	0.00703	„	0.96133	wood.
Isere do.	0.02636	0.00260	0.00280	0.02137	0.94687	wood.
German do. (Siegen)	0.02690	0.00230	0.00162	0.02590	0.94338	wood.
Do. (Coblentz)	0.02441	0.00230	0.00185	0.02490	0.94654	wood.

Table of the Composition of certain Irons of Commerce.

	Carbon.	Silicium.	Phosphorus.	Manganese.
Swedish, best quality	0.00293	traces	0.00077	traces.
Do.	0.00240	0.00025	traces	traces.
Creusot iron	0.00159	traces	0.00412	traces.
Champagne do.	0.00193	0.00015	0.00210	traces.
Scrap iron of Paris	0.00245	0.00020	0.00160	traces.
Berry iron	0.00162	traces	0.00177	traces.
Brittle Moselle iron	0.00144	0.00070	0.00510	traces.

Table of the Composition of certain kinds of Steel.

	Carbon.	Silicium.	Manganese.	Made of.
Best fused English steel	0.00625	0.00030	traces	Swedish iron.
Fused steel of Isere	0.00651	traces	traces	Isere iron.
Do. 1st quality	0.00654	0.00040	traces	French iron.
Do. 2nd quality	0.00936	0.00080	traces	French iron.

—*Bull. Univ.* F. xiii. p. 238.

9. *On Artificial Crystals of Oxide of Iron.*—M. Mitscherlich has examined certain crystals of oxide of iron, found in a pottery furnace at Oranienbourg. These were in very obtuse rhomboids, and resembled the specular iron of volcanoes, having the same brilliancy, hardness, scratch, and other properties. The smaller crystals, composing extremely thin plates, were transparent and red, like micaceous iron. The faces were brilliant, the angles could be measured, and were the same with those of the natural crystal. So greatly do these resemble the crystals of volcanoes, that the same theory of formation may be applied to both. The first are formed in a pottery furnace, in which the vessels, when baked, are glazed by means of common salt. The clay used consists principally of silica, alumina, and a little oxide of iron. The salt is volatilized, and with water coming in contact with the surface of the vessels, new compounds are produced, water is

decomposed, muriatic acid is formed, and the soda produced unites with the silica to make the necessary glaze. As to the oxide of iron, its history will be best understood by an experiment or two. If a mixture of salt, oxide of iron, and silica, be heated to redness in a tube, and water in vapour be passed over it, much muriatic acid is formed, but very little chloride of iron, and crystallized oxide of iron will be found in the mass: but if muriatic acid be brought in contact with ignited oxide of iron, water and chloride of iron are formed, and sublime; if the chloride of iron come in contact with more water, muriatic acid is first developed, then chloride of iron, and a residue of crystallized oxide of iron remains. The formation of chloride of iron by the action of muriatic acid upon oxide of iron appears, therefore, to depend upon the proportion of water present. M. Mitscherlich applies these experiments and principles in explanation of the manner in which volcanic crystallized oxide of iron is formed—all the conditions necessary, according to the above view, being present in those cases, where heretofore it had been supposed the oxide of iron, as such, had been actually sublimed. —*Annalen der Physik.*—*Bull. Univ.* B. xix. 245.

10. *M. Becquerel on Metallic Sulphurets, Iodides and Bromides.*—The researches of M. Becquerel into the nature of minutely exciting causes of electrical action, and the effects of electricities of feeble intensity, are well known, both for the ingenuity and the industry with which they have been carried on. Lately, he has been led to conceive, that many of the minerals in the rocks forming the surface of our globe may have been the result of the influence of feeble electric powers acting upon ordinary matter. He puts a question thus: "Have the mineral substances in veins been originally dissolved in a liquid, which, by slow dissipation, has allowed the molecules to come together, under a crystalline arrangement? or, have they resulted from the slow decomposition of certain combinations but little soluble into which they entered, as constituent parts?" and believes that the facts he afterwards quotes throw light upon it.

The apparatus used consisted of two small tubes, *a* and *b*, open at both extremities, placed upright, side by side, and filled in the lower part with very fine clay, slightly moistened with a liquid capable of conducting electricity. Such liquids are to be poured into the upper parts of the two tubes, as are competent, under the circumstances, of acting upon the metal experimented with. The metal may be a single plate, bent so as to be immersed in the fluid of both tubes, and connect them together; or it may be a compound-metallic communication. The two tubes are placed in a third, large enough for the purpose, which is to be partly filled with a fluid, intended to complete the electric communication through the apparatus. Thus arranged, the clay retards the mixture of the fluid in the two smaller tubes, and allows of the time necessary for the formation of the new compounds.

M. Becquerel mentions the natural sulphurets of zinc, iron, manganese, tin, arsenic, molybdena, antimony, bismuth, copper, lead, silver, and cobalt, as compounds perfectly crystallized, whilst the corresponding artificial substances can be obtained only in the amorphous state*. The sulphurets have no action on dry oxygen gas; but those containing very oxidable metals act on the moist gas, and become sulphates or sulphites. When a metal can decompose water at common temperatures, so can its sulphuret. Metals acted upon, when free, by nitric acid, are also acted upon when existing in sulphurets, with the exception of very few cases. Such are the general properties principally connected with the present question.

By putting into the tube *a* a saturated solution of nitrate of silver, into the tube *b* a solution of hydrosulphuret of potassa, and connecting the two solutions by a plate of silver, there is formed after some time a deposit of metallic silver on the end of the plate in *a*, and a double sulphuret of silver and potassium on the end in *b*. The latter is in fine prismatic crystals, which are gradually decomposed by the nitric acid, attracted in a later stage of the electro-chemical action, and there ultimately remains a sulphuret of silver. During the action a part of the liquid evaporates, and there remains at last above the clay a pasty substance, in which lies the sulphuret of silver in fine octoëdral crystals, not only upon the silver plate, but also on the sides of the tube.

These crystals have the same appearance, malleability, colour, and other properties of native sulphuret of silver in crystals, and cannot be distinguished from them. The crystallization is the result of the slow decomposition of the double sulphuret, the particles having time allowed them for that degree of motion necessary to bring their homologous faces parallel to each other.

By a similar process, crystallized sulphuret of copper was formed, having a grey metallic lustre, with a shade of blue, and yielding a black powder. They dissolved in ammonia, rendering it blue. They had, in all respects, the properties of the native sulphuret of copper.

Leaving all things in the last experiment unchanged, except that, instead of a single copper connecting plate, it was conjoined with a piece of antimony, so that the antimony end should be in the sulphuret, there was formed in the course of time little red octoëdral and lamellar crystals. These were soluble in neutral hydro-sulphuret of potassa, and gave sulphuretted hydrogen when acted upon by muriatic acid. Alkalis rendered them yellow.

* There is evidently some mistake here. Bright yellow sulphuret of iron has been obtained artificially from solutions by time. Probably, many of the others might be crystallized artificially; and as to antimony, it would be a difficult thing to prevent crystallization.—*Ed.*

The substance, therefore, was kermes, and was thus obtained for the first time in a crystalline form.

By similar processes, cubical crystals of sulphuret of tin were obtained, having a brilliant white and metallic appearance. With the additional circumstance of excluding the air from the tube *b*, and substituting the proper substances, sulphuret of iron in brilliant yellow cubical crystals was obtained.

According to these results, M. Becquerel thinks it may be permitted us to believe that nature may have followed a similar course in the production of such sulphurets as are found in metallic veins. Thus, sulphuret of silver is found combined with the sulphuret of antimony, arsenic, or lead; and these combinations, unaffected by air, have remained in the state they assumed at their first formation; but the case is not the same with the double sulphuret of silver with potassium or sodium, which no doub twas formed at the time of the consolidation of the great terrestrial masses: for then the alkali now existing in vegetables probably existed in numerous compounds containing also sulphur. A slow decomposition commenced, and a crystallized sulphuret of silver has been the consequence The planes and appearances of the metallic sulphurets actually formed as above described are so like those of the natural compound, in form, grouping of the crystals, colour, and general characters, as to give the stronges treason for supposing this has been an active natural power.

From these facts it would also appear, that to obtain an insoluble substance in a crystallized state, it is only necessary to make it enter into combination with another which is soluble, and then to decompose the compound very slowly. In proof of this opinion comes the following experiment:—Finely divided clay being moistened with a solution of arseniate of potash was put into a glass tube, and a solution of nitrate of copper poured upon it. After some time there were seen, in certain hollow parts of the clay, crystals exactly resembling those of arseniate of copper. Perhaps many crystals which line mineral cavities may have a similar kind of origin.

Upon extending the experiments to insoluble iodides, substituting hydriodate of potassa for the hydro-sulphuret of potassa, and using a connecting plate of metallic lead, a double iodide of lead and potassium is first formed, which is gradually resolved into regular octoëdral crystals of iodide of lead, of a golden yellow colour and brilliant aspect. This substance, which is insoluble, is the iodide of lead, and this is the first time that it has been obtained crystallized*.

By following up this train of reasoning and experiment, it is very probable that other metals could be formed into crystallized iodides, &c., and that the bromides and seleniurets might likewise be

* This is a mistake. The iodide of lead is not insoluble, although it is but slightly soluble. It may be obtained crystallized at pleasure, by allowing a saturated hot solution to cool slowly.—*Ed.*

procured in a definite and crystalline condition.—*Ann. de Chimie*, xlii. 225.

11. *Preparation of Pure Oxide of Cobalt.*—I treat the ore of cobalt (unroasted) with nitric acid, evaporate to dryness, and redissolve in water. I precipitate with carbonate of potash, until I perceive that arseniate of cobalt begins to fall. I separate the arseniate of iron by a filter, and pour an acid oxalate of potash into the solution. In the course of some hours, all the oxalate of cobalt precipitates; the iron, arsenic, and nearly all the nickel remaining in solution. The precipitate, well washed, is then to be treated with ammonia, according to M. Laugier's process, and it will be sufficient to use but little ammonia with heat, which will then first dissolve the oxalate of nickel. If this be not thought necessary, (and the quantity of nickel present is very small,) nothing remains but to decompose the oxalate by heat, in an open vessel. The oxide of cobalt thus obtained will contain no iron or arsenic, and only minute traces of nickel.—Quesneville.—*Journ. de Pharmacie*, xv.

12. *Properties of Cobalt.*—The following are, according to Lampadius, the properties of pure cobalt, obtained with great difficulty in pieces the size of a pea. Colour greyish-white, between steel and silver; specific gravity 8.71; lustre considerable, strongly reflecting light, and being unaffected by the air; hardness moderate, not more to the file than that of copper; malleability moderate: the metal bears a few blows with a hammer, and then gives way in scales; when heated, the effect is the same; fracture fine and granular; fusibility between nickel and platina; magnetism, the magnetic force (of iron?) being taken as unity, that of cobalt is 0.701.—*Bull. Univ.* A. xii. 455.

13. *Preparation and Properties of the Bi-iodide of Mercury.*—The following details are extracted from M. A. Hayes's account. Boil a mixture of 125 parts of iodine, 250 parts of clear iron filings, and 1000 parts of distilled water in a flask. When the colour from brown has become light green, decant the clear fluid, wash the residue, and add the washings to the former liquor; a solution of 272 parts of corrosive sublimate, in 2000 of warm water, is to be added, and the resulting precipitate washed and collected.

This salt, either in crystals or in powder, presents two distinct and beautiful colours. If the precipitate be heated in a small subliming apparatus, or in a glass tube, it melts and sublimes copiously, and the vapour is condensed in large transparent rhombic tables, of a fine sulphur yellow colour. These crystals are permanent in the air, and unaltered by the direct solar rays; but the slightest friction, or the contact of a fine point, is sufficient to alter their interior arrangement. The point of contact instantly becomes of a *rich scarlet*, and the same colour spreads over the whole surface of a single crystal, and extends to the most remote angle if a group of crystals be the subject of experiment. This change of colour is accompanied by a sensible mechanical action, so that a small heap of the crystals appear as if animated. An ordinary electroscope does not indicate the development of any electricity, nor is there any considerable elevation of temperature during the change.

By gently warming the crystals, supported on paper over the flame of a lamp, the original yellow coloured salt is obtained, and the same experiment may be often repeated : affording an elegant illustration of the connexion between colours and the mechanical structure of bodies. Transparent, but minute rhombic prisms of this salt may be obtained by allowing a hot solution of it, in a solution of corrosive sublimate, to cool very gradually.—*Silliman's Journal.*

14. *On a new Compound of Mercury*, by Mr. Phillips.—On adding carbonate of lime to a solution of corrosive sublimate, and heating the mixture, a dark coloured precipitate was formed, crystalline, very heavy, and the larger crystals nearly black ; very slightly soluble in boiling water and scarcely at all in cold. It dissolved in acids, and gave peroxide of mercury by potash, and chloride of silver by nitrate of silver.

Upon trial it was found that no excess of carbonate of lime rendered the change of the corrosive sublimate complete, the muriate of lime formed at the same time producing a counteracting effect. When hard carbonate of lime, as calcareous spar, was put into the corrosive sublimate solution, months were required to obtain only a few crystals of salt. Powdered and precipitated carbonate of lime acted more readily.

The following process gave the new compound quite pure: 272 grains or 1 atom of corrosive sublimate was dissolved in water, and boiled with 648 grains or 3 atoms of peroxide of mercury ; a dark precipitate soon began to form, and eventually nearly the whole became the particular substance. Corrosive sublimate, equivalent to 6 grains of peroxide, only remained undecomposed, and of course some peroxide remained diffused through the precipitate ; this may be removed by boiling the whole with about 100 grains of muriatic acid diluted with water ; the salt is then perfectly black, dense, crystalline, and frequently very brilliant.

Mr. Phillips calls this substance a dipermuriate of mercury ; considering it not as a compound of oxide and chloride of mercury, with water of crystallization, but of muriatic acid and oxide of mercury. That it contains no water of crystallization is considered as proved, because, when the crystals are put into a glass tube, and the latter heated in a salt water bath, no moisture rises from them, and the crystals undergo no change. At a higher temperature, water, corrosive sublimate, mercury, and calomel, are produced.

Considered as a dipermuriate of mercury, the composition of this substance is as follows:

Muriatic acid . .	1 atom .	37	or	7.8
Peroxide of mercury,	2 atoms .	432	.	92.2
		469		100.

Phil. Mag. N. S., vii. 129.

15. *Reduction of Nitrate of Silver.*—M. Charles de Filiere prepared a large quantity of nitrate of silver in 1826. The finest crystals were put into unsized paper, and then put hastily into a card-board box, in such a manner, however, as to prevent any contact of them with the bodies held in suspension in the atmosphere.

This packet having been refound in the month of November, 1829, was examined. The paper had acquired the usual deep violet tint; but when opened, the crystals were unchanged in form, but perfectly reduced, and now consisted of very malleable metallic silver.—*Ann. de Chimie,* xlii. 335.

16. *On some Properties of Silver.*—M. Weslar states, that chloride of silver, blackened by light, is not a mixture of reduced silver and undecomposed chloride, but a sub-chloride of silver unattackable by nitric acid, though resolved by ammonia and a solution of common salt into metallic silver and chloride. It cannot be obtained pure by exposure of the chloride to light, but may by allowing silver to remain in a solution of muriate of copper or iron. When silver containing copper is blackened by solution of muriate of ammonia, it is in consequence of the formation of this chloride.

Silver left for a long time in strong solution of common salt is attacked; the liquid becomes weakly alkaline, and on evaporation yields crystals consisting of the combined chlorides of sodium and silver.

It is known that a hot solution of sulphate of iron dissolves silver, and that silver falls as the solution cools: but the metal is not totally precipitated, a part remains dissolved at common temperatures, and this is in greater proportion as the solution is more acid. Dilute sulphuric acid does not act on silver at common temperatures, but it is only necessary to add a drop of solution of sulphate of iron to make it do so. It appears that the oxygen of the air is conveyed to the silver by means of the solution of iron, the iron, after giving oxygen to the silver, retaking more from the air.

Solution of chloride of silver in common salt is not decomposed by potash, probably in consequence of the great affinity of the chlorine for silver, and the potassium for oxygen. It is owing to the same affinities that the complete decomposition of the chloride of sodium by oxide of silver can be effected.—*Phil. Mag. N. S.* vii. 58.

17. *Purple Precipitate of Silver.*—M. Frick states, that if a dilute solution of tin in nitric acid, prepared without heat, be mixed with a dilute solution of nitrate of silver, the solution, after some minutes, becomes yellow, afterwards brown, and eventually of a deep purple colour : if dilute sulphuric acid be added to it, a deep purple precipitate will be obtained, but which, although it otherwise resemble the purple precipitate of gold, does not possess the property of colouring glass.—*Phil. Mag. N. S.* vii. 58.

18. *On the Action of Alkalies on Organic Bodies.*—We gave M. Gay Lussac's highly interesting paper on this subject in a former number of our Journal *. On the 16th of November, M. Gay-Lussac stated to the Academy of Sciences, that since his first observations on the formation of oxalic acid, he had also ascertained, that acetic acid and water were very generally produced when the caustic alkali was made to act either upon animal or vegetable substances, in the manner before described.

19. *Preparation of Formic Acid.*—Dobereiner's process for preparing this curious acid has been given in a former volume of this Journal †. M. Wohler recommends the following process for the same purpose :—A mixture of starch and peroxide of manganese is to be put into a retort, sulphuric acid poured upon it, and the whole subjected to distillation ; much carbonic acid gas is evolved, and an acid liquor passes over at the same time, which has a very penetrating smell, and strongly irritates the eyes : this is formic acid, rendered impure by a volatile matter, to which its strong odour is to be attributed. When the liquor is saturated with an oxide the smell ceases, and the solution has a yellow tint. It is only necessary to distil the formiate thus obtained with sulphuric acid to obtain pure formic acid without any smell but that which it naturally has. The salts which even the impure acid forms with baryta, lime, and lead, have precisely the same crystalline form as the same salts prepared with the nitric acid. The salt of lead was analysed : its acid was found to contain the same constituents as common formic acid.—*Hensmann's Repertoire*—*Phil. Mag. N. S.* vii. 60.

20. *Pelletier on a new Vegeto-alkali.*—M. Pelletier, in a letter to M. Gay-Lussac, remarks, that a particular bark is resorted to in Peru for the purpose of mixing with the bark of cinchona (quinquina cassaya), as an adulteration. The bark has no medical properties ; has not yet been botanically distinguished ; is still under chemical investigation, but has been found to contain a new vegeto-alkali. It may be distinguished from tonic bark (quinquina cassaya) by a drop of nitric acid, which renders the former of a deep green, and the latter of a red-brown.

* See vol. vi. p. 414. † Quarterly Journal of Science, xiv. 232.

If this bark be acted upon, as pale bark is, for cinchonia, the new substance will be obtained. At first it resembles cinchonia, being white, transparent, and crystalline; but after being fused by heat, it does not volatilize as cinchonia does. It is soluble in alcohol and ether, but not in water. It has no taste at first, but gradually a sense of heat, bitterness, and constriction is felt. Acids develope its taste.

In combination with acids it is easily distinguished. The sulphate of cinchonia is crystalline. The sulphate of the new alkali does not crystallize, from its aqueous solution; but if, in proper proportion, the solution on cooling become gelatinous and white, like blanc-mange; if then dried, it becomes horny, and the dry mass in boiling water becomes gelatinous.

The sulphate dissolved in hot alcohol crystallizes in silky needles as the temperature falls; these are soluble in water, producing, if in proper quantity, the gelatinizing solution. Unlike the alkaline crystals, they are not soluble in ether.

Nitric acid has a very peculiar action on the new vegeto-alkali. If strong, it produces an intense green colour; if weak, it dissolves the substance without producing alteration of tint. In the first case there is destruction in the substance; in the second, mere combination.

A correct analysis of this bark is to be given hereafter.—*Ann. de Chimie,* xlii. 331.

21. *Buccina: new principle in Box-wood.*—An apothecary of Bordeaux announced to the Pharmaceutical Society of Paris, at its last sitting, that he had discovered in the wood, and particularly in the bark, of the box-tree, an alkaline principle to which he gives the name of buccina. It is in the form of powder, and neutralizes acids, forming uncrystallizable salts: it has a very strong sudorific action and bitter taste. M. Dupetit Thouars, in making this statement at the Philomathic Society, remarked that buccina might perhaps be advantageously used in the manufacture of beer, "for there is more box-wood than hops employed in making almost all the beer brewed in Paris."—*N. M. Mag.* xxx. 112.

22. *On certain Double Compounds of the Muriates of the Vegeto-alkalies.*—This investigation has been carried on by M. Caillot through several classes of substances. He first considered the compounds of the muriates of the vegeto-alkalies with the perchloride of mercury. If into any of those muriates a diluted solution of corrosive sublimate be poured, there is an abundant white precipitate produced: it is slightly soluble in water and alcohol, and unaltered by the air; its taste partakes of that of the substances employed. Heat first fuses it, and then causes decomposition.

The compound produced by the muriate of cinchonia is the only one that has been analyzed: 100 parts contained 39.57 corrosive sublimate.

In the course of these experiments, it was remarked that morphia, brucia, and other vegeto-alkalies, combined with corrosive sublimate to form triple salts, and that narcotine and cantharadine had the same property, although not considered as vegeto-alkalies. Being led by this to suspect that, under the influence of corrosive sublimate, they might be made to neutralize muriatic acid, a weak solution of the mercurial chloride was added, drop by drop, to a solution of narcotine in muriatic acid; a pulverulent white precipitate was formed, and the liquor, at first acid, became actually neutral: thus proving the alkaline tendency of narcotine, under particular circumstances.

For want of cantharadine, the proof was not so strong; but still the alkaline tendency was evident.

On experimenting with the hydriodates of the vegeto-alkalies, in conjunction with the iodide of mercury, the compounds formed appeared to be very definite and constant; and whenever a vegeto-alkali combined with acetic or hydriodic acid occurs, the double iodide of mercury and potassium occasions the precipitation of a double salt, consisting of the iodide of mercury and the hydriodate of the vegeto-alkali. This precipitate is always white in the state of hydrate, but becomes yellow by heat. Generally it is uncrystallizable, nearly insoluble in water and alcohol, and but slightly sapid. The compound containing cinchonia is the only one analyzed, and was found to contain, per cent., 42.67 iodide of mercury.

Attention was then given to the double compounds formed by the union of the hydriodates of the vegeto-alkali with corrosive sublimate, that containing cinchonia being used. No production of iodide of mercury took place, as would have happened with the hydriodate of potassa; but the hydriodate and the chloride combined together to form a stable double compound. The solutions should be mixed, well agitated, and then the precipitate filtered out and washed well; it is white, but little soluble in water or alcohol, uncrystallizable, and unaltered in the air: it contains 34.91 perchloride of mercury per cent.

The hydriodates and hydrobromates of the vegetable alkalies also combine in a similar manner with the cyanide of mercury. The compounds are white, light, uncrystallizable, unaltered by air, and only slightly soluble in water or alcohol: the compound of the hydriodate of cinchonia contains 33.94 per cent. of cyanide of mercury.—*Ann. de Chimie,* xlii. 263.

§ III. Natural History.

1. *Method of obtaining the Skeletons of small Fishes.*—Some time since I was employed in making observations on the produce of some of the ponds in the neighbourhood of London, and I discovered that the tadpole was a very serviceable animal in anatomizing the very small fishes, as well as some of the larger sorts, generally found in such places—the tadpole acting in the same manner as

the ant. I have tried the experiments several times, and on various sorts of fishes, and was always successful, particularly with that very little one, called by children stickle-back: even in these the skeleton was at all times perfect. My method is this: I suspend the fish by threads, attached to the head and tail, in a horizontal position, in a jar of water, such as is found in the pond, and change it often till the tadpoles have finished their work—which, if two or three tadpoles are allowed to work on so small a fish as the species just mentioned, they will complete in twenty-four hours. I always select the smallest sort of tadpoles, as they can insinuate themselves between the smallest bones without destroying their articulation.—T. Bluett.—*Phil. Mag. N. S.* vii. 151.

2. *Physiological Phenomenon produced by Electricity.*—The following singular results are by Professor Marianini of Venice. He has stated, in a memoir published some time since *, that a difference existed in the contractions of a frog when the electricity acted immediately upon the muscles, and when it acted upon the nerves which presided over the muscular motions: the former were called *idiopathic convulsions,* and the latter *sympathic convulsions.* The difference consists in this, that the former contractions occur in whatever directions the current of electricity traverses the muscles, whilst the latter take place only when the current which traverses the nerves proceeds in the direction of their ramification.

From this it follows that, when a current traverses a limb in the direction of the nerves, the two shocks should occur together; but, when it proceeds in the contrary direction, only the idiopathic convulsion should be produced. In the first case, therefore, the contraction should be stronger than in the second.

If the right hand be in contact with the positive pole of a voltaic battery, and the left hand equally in contact with the negative pole, a contraction is felt in both arms every time the circuit is completed, but stronger in the left arm than in the right. If the direction of the current be inverted, the right arm feels a more powerful convulsion than the left.

If a hand be in contact with the positive pole, and a foot in contact with the negative pole, the circuit will be in the direction of the nerves in the leg, but not in the arm; and the contraction is much stronger in the leg, where the two effects are simultaneous, than in the arm, where the idiopathic effect only is felt. The same effect takes place if the electricity is passed from the shoulder to the hand, from one foot to another, from the knee to the foot, &c. &c.

This striking difference varies in different persons, especially in those who are paralytic. The current, from eighty pairs of plates, being passed from the hand to the shoulder of a person struck with hemiplegia, the muscles of the arm were scarcely contracted

* Quarterly Journal of Science, N. S. vol. v. p. 406.

at the same place, where the convulsion was very strong, when the current was passed from the shoulder to the hand.

Sometimes this difference existed only in one limb.—A woman, who had lost the use of the lower limbs in consequence of an inflammation of the spinal marrow, felt the left foot contract with most force when it touched the negative pole of the pile; the right foot contracted with equal force, whether it was in contact with the positive or negative pole. This effect appeared to be due to a loss of nervous sensibility in the right foot, so that they had become indifferent to the direction of the electric current.

If a finger be immersed, as far as the second joint, in water, connected with the positive pole of a battery containing twenty-five or thirty pairs of plates, and the circuit be completed by touching the negative pole with a metallic cylinder, held in the other hand wetted, a shock is felt in the finger, not extending beyond the second joint; if the direction of the current be reversed, the shock is felt in the third joint. Upon giving attention, it will be found that the first shock is more external, and accompanied by a distressing sensation; whilst the second shock is more deeply felt, and is accompanied by no particular sensation at the place where the finger touches the water. The effects are accounted for by the supposition that, when the finger touches the negative pole, the simultaneous occurrence of the two convulsions causes the stronger contraction; and that, when the finger touches the positive pole, the electricity which traverses the nerves proceeds in a direction contrary to their ramification, and, in place of producing a shock, occasions the peculiar sensation.

If two metallic cylinders be covered with wet cloth, and one being held in each hand, contact is then made with thirty or forty pairs of plates, moderately active, besides the shocks which are felt each time the communication is completed, there is a particular sensation in the palm of the hand, communicating with the positive pole. This sensation has been very distinct in some persons sensible to the effect of electricity; and they compared it to that pricking sensation often felt in the hands or feet when the nerves have been compressed for some time together.—*Bib. Univ.* xlii. 287.

3. *Ossified Brain.*—M. C. Matteucci having examined a brain which he discovered in an old anatomical collection, found it to exhibit a singular case of ossification throughout its whole substance. When heated, it burnt, evolving ammonia, leaving a bulky charcoal. When examined by chemical agents, carbonate of lime was found in small quantity, and phosphate of lime in much larger, but the principal part of the mass was animal substance, closely allied to *osmazome.*

The composition of the concretions that are sometimes found in the pineal gland are, according to Fourcroy, of the same nature.—*Ann. de Chimie*, xlii. 335.

4. *New Medicinal Substance.*—This substance has been obtained by MM. Caventou and François, from the root of a Brazilian tree of the family of the rubiacies, called the caïnca, (*chiococca racemosa*,) and in Bahia by the name of the *rair-prela* or black root. The peculiar chemical principle which gives character to the extract of this root has tonic without exciting properties. It slightly purges and is very diuretic. Its mode of action on the urinary passages is remarkable; the first day it very little augments the quantity of urine evacuated, but on continuing to administer it, the effect is progressively increased from day to day. As the augmentation of action is slow, no undue irritation is produced; and in consequence, the administration of this body as a medicine has never produced more favourable effects than in those cases where the urine, red and irritating, has been rendered with much pain.

The extract of the root being tonic, purgative and diuretic, it is evidently applicable in cases of dropsy, and very favourable results have been obtained in several instances of this kind.—*Bull. Univ.* C. xix. 338.

5. *Population of Wales:*—

Shires.	1700	1750	1801	1811	1821
Cardigan .	25,300	32,000	44,000	52,000	59,000
Brecknock .	27,200	29,400	32,700	39,000	44,500
Carmarthen	49,700	62,000	69,600	79,800	92,000
Carnarvon	24,800	36,200	43,000	51,000	59,100
Denbigh .	39,700	46,900	62,400	66,400	78,000
Flint . .	19,500	29,700	41,000	48,100	54,000
Radnor .	15,300	19,200	19,700	21,600	23,500
Merioneth .	23,800	30,900	30,500	32,000	35,100
Anglesea .	22,800	26,900	35,000	38,300	46,000
Glamorgan	49,700	55,200	74,000	88,000	103,800
Pembroke .	41,300	44,800	58,200	62,700	75,500
Montgomery	27,400	37,000	49,300	53,700	61,100
Total .	366,500	450,200	559,500	632,600	732,500

Y. Cymro.

6. *Use of the Chlorides of Lime and Soda in cases of Plague.*—A medical expedition to Egypt and Syria is under the direction of M. Parisot. The following are extracts from a letter written by him. "Tripoli, Syria, 28th June, 1829. You have not forgotten that our mission had two objects,—to ascertain the cause of plague, and to ascertain the effect of chlorides on the infectious matter and pestilential miasmata. For the first object we proceeded to Egypt, a country always considered as the original source of plague. Our conjectures have been confirmed by an examination of the localities, and we expect, on our return to France, that no doubt will rest on this point.

"For the second object, it was necessary to meet with the plague; we expected it in Egypt, but it was in Syria. We left for that country, and after many delays arrived at Tripoli on the 30th of May; on the 31st we took up our abode in the house of the French consul. When there, we requested six sets of dresses, (six shirts and six pair of drawers,) in which persons had recently died of the plague; these, some of silk and some of cotton, were bought on the 2nd, and put into the garden of the consul's house on the 3rd of June. On the 4th, the state of the clothes was examined; they were foul with diseased matter, and of a detestable odour. A woman (infected with the plague) steeped them in mere water to remove the excess of dirt, after which they were passed into a vessel containing a solution made by M. D'Arcet of 3 pounds of the chlorides in 50 lb. of water, and there they remained 16 hours.

"On the 5th, in the morning, MM. D'Arcet and Guilhau withdrew them from the solution, wrung them out and exposed them to the sun. The stains were weakened, but still very evident. At mid-day they were dry, each of us (MM. Dumont, Guilhau, Lagasque, D'Arcet, Rose, and myself) took two pieces of the clothing and put them on in contact with the skin. We put off this clothing on the 6th, after having worn it for 18 hours. No one suffered; since then, 22 days have passed, and our health is the same.

"The consequence of all this is, that we possess a means of, i. disinfecting quickly and cheaply goods and clothing, without the least injury to them. ii. Of reducing the disease to its own case, and preventing it from producing a second or third case, as here, or a fourth or fifth case, as seen elsewhere; and that by destroying the venom left in the first case, which otherwise would perpetuate the evil: and not only can this be done with the plague, but with variola, rubeola, typhus, and even yellow fever: for I shall die with the conviction that these fevers are contagious in Europe and every where.

"If this simple means be associated with better police regulations respecting interments in Egypt, and other parts of the Turkish empire, (for the sepulchres are here in a deplorable state,) it is as clear as day that plague may be eradicated from the world.

"Since the 11th June, we have seen and touched many patients, but no degree of plague has been contracted by us, the chloride having preserved us.

"Abdalla Pacha, who governs western Syria, writes to us, that the plague is at Acre, and begs for chloride. Many great Turkish personages at Tripoli have also requested them. Patience—good is done, but slowly, yet it is done: evil only is done quickly.

"Oh, that I could distinctly understand the judgment of the Gibraltar junta on the character of the yellow fever!"—*Bull. Univ.* C. xix. 233.

To these details a letter by M. D'Arcet adds other circumstances:

he says that when they wore the clothes next their skin they covered themselves up and took much exercise to excite perspiration. At this time from 12 to 25 persons were dying per day. On the whole, the chloride of lime appears better than the soda preparation, because it attacks the fabrics less strongly. The chloride produced no effects when administered as medicine to those having the plague: they were neither better nor worse for it. Two hours after death, a corpse was opened and examined; it was first washed with chloride of lime and the hand kept continually bathed in the solution; the viscera were still warm. No injury to the examiners followed.—*Revue Ency.* xliv. 223.

7. *Fecundity of the Viper.*—M. Robineau Desvoidy stated at the Academy of Sciences, that on opening a viper (one of those usually called red snakes) he found in the uterus more than 3000 little ones in different states.

8. *Use of Sulphate of Soda instead of Salt for Sheep and Cattle.*—Numerous experiments, made since those performed and recommended by the Marquis de Sainte-Père, have demonstrated that coloured sulphate of soda may be substituted for common salt for the use of sheep and cattle. It is a weak purgative, which moderately irritates the intestines, and gives tone to the stomach. It appears to unite the double advantage of smaller price (?) and greater energy.—*Bull. Univ.* D. xiii. 245.

9. *Alimentary Tubercle of Van Diemen's Land.*—A singular substance has been found at the depth of a foot or a foot and a half in the earth of that country. It has not yet been described, but is called *indigenous bread.* It is covered with a thin skin, has a rounded form, like a potatoe or yam, and is sometimes as large as a man's head. When cut, it appears as if composed of a solid spongy mass, containing a considerable quantity of alimentary matter. No root or fibre has been found adhering to it, so that sometimes it has been thought to be a sort of terrestrial polypus, possessing a principle of animal life. The only indication of its presence which the natives have is the occurrence of an exceedingly small leaf, which rises from the earth, and is connected with it by very thin and delicate fibres, which break whenever the tubercle is raised.—*Asiatic Journal.*

10. *Effects of Light on Vegetation*—Saussure and others have said that seeds exposed to light during germination were developed and grew equally with those in the dark. The following experiment on this point is by M. Boitard. Three garden pots filled with willow earth were placed, on the 1st of August, under a shelter towards the north upon the surface of the earth; in each was sown the same quantity of auricula seeds. These seeds are

delicate and difficult to raise. The first was covered with a clear transparent glass shade; the second with a roughened glass shade; and the third with a similar cover enveloped entirely in black cloths. The garden pots were placed in vessels of water, so that the surface of the earth should be constantly humid. On the 9th of the same month, the seeds, deprived of light, began to grow; on the 12th, the radicles of those under the ground glass just began to appear; and on the 15th, none of the seeds under the clear glass cover, had exhibited the smallest signs of vegetation; in most of them, the embryo appeared to have resisted the action of moisture, and not even to have swelled under its envelopes.—*Bull. Univ.* D. xiii. 310.

11. *Luminous Points in the Horizon.*—The following is part of a letter to the Baron de Zach. " Permit me to communicate to you a discovery which I made about six years since, but which I did not make known to you before, until I had assured myself of its reality; I beg you to make it known, if possible by some of the journals. For these last six years, I have been occupied in making observations on the clearness of the atmosphere by day and by night. This gave me the opportunity of remarking a singular light towards the north-east and the south-west. It very much resembles the zodaical light, but it is always observed in the magnetic meridian. It is more luminous towards the north-west, than in the south-east: even when the sky is covered with clouds, if they be equally diffused, this magnetic light may be seen towards the north west, insomuch, that I have sometimes been in doubt whether it was not a distant fire. To be convinced of the truth of this appearance, it is only necessary that the observer should have a free horizon, and that he attend continually to the clearness of the air. It is requisite that the eyes should be accustomed for a while to this kind of observation, that they may be enabled clearly to see the phenomenon."—*Bib. Univ.* xlii. 272.

METEOROLOGICAL DIARY for the Months of December, 1829, January and February, 1830, 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 DECEMBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Tuesday ...	1	35	42	29.67	29.67	E	SE
Wednesday.	2	35	43	29.61	29.60	SE	SE
Thursday ..	3	37	44	29.58	29.61	SE	SE
Friday	4	38	46	29.61	29.61	E	SE
Saturday...	5	40	50	30.00	30.20	SW	SW
Sunday	6	32	46	30.30	30.30	SE	SE
Monday....	7	25	35	30.30	30.18	E	ESE
Tuesday ...	8	29	35	30.15	30.22	SW	WbN
Wednesday.	9	27	37½	30.24	30.18	NE	ENE
Thursday ..	10	31	38	30.02	30.00	NE	SW
Friday.....	11	24½	39	30.00	29.98	SSW	S
Saturday...	12	35	47	29.93	30.00	SW	SW
Sunday	13	39	48	30.00	30.00	SSW	S
Monday....	14	28½	42	30.17	30.20	SW	SSE
Tuesday ...	15	26	43	30.22	30.23	SW	NNE
Wednesday.	16	26	41	30.20	30.10	NNE	WNW
Thursday ..	17	29	37	29.90	29.90	W	W
Friday	18	27	37	29.53	29.65	NE	NNE
Saturday...	19	26½	38	29.78	29.78	N	NbW
Sunday	20	29	34	29.75	29.80	NE	NW
Monday ...	21	20	37	29.80	29.70	S	W
Tuesday ...	22	21½	36	29.60	29.68	NE	SE
Wednesday.	23	27	33	29.68	29.68	NNE	NNE
Thursday ..	24	24	33	29.68	29.81	NE	NE
Friday.....	25	26	34	30.00	30.10	NNE	NE
Saturday...	26	23½	32	30.26	30.28	NE	NE
Sunday	27	23	32	30.30	30.31	NNE	NW
Monday ...	28	13	30	30.24	30.20	W	W
Tuesday ...	29	10½	31	30.20	30.24	NW	NE
Wednesday.	30	25	32½	30.30	30.38	EbN	ENE
Thursday ..	31	27	34	30.39	30.48	NE	ENE

For JANUARY, 1830.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Friday	1	27	37	30.48	30.50	NE	NE
Saturday...	2	25	36	30.40	30.38	NE	WbN
Sunday	3	31	39	30.38	30.33	NW	NW
Monday....	4	23½	41	30.21	30.17	WbN	NNE
Tuesday ...	5	33	42	30.17	30.18	W	NW
Wednesday.	6	27	41	30.20	30.03	W	WSW
Thursday ..	7	36	44	29.70	29.80	W	NW
Friday.....	8	29	38	29.98	30.00	NNW	NW
Saturday...	9	25½	39	30.07	30.00	W	NW
Sunday	10	31	39	29.73	29.73	NW	NW
Monday....	11	31	40	29.68	29.57	NW	NNW
Tuesday ...	12	28½	36	29.70	29.90	NNE	NNE
Wednesday.	13	27	35	29.87	29.72	NbE	NE
Thursday ..	14	22	32	29.80	29.80	ENE	ENE
Friday.....	15	27	37	29.92	29.82	NE	NE
Saturday...	16	30½	36	29.84	29.84	NE	NE
Sunday	17	20½	31	29.80	29.80	ESE	SE
Monday....	18	15	29	29.83	29.83	SE	NW
Tuesday ...	19	6½	27	29.63	29.47	NE	E
Wednesday.	20	21	36	28.80	28.73	EbS	NW
Thursday ..	21	26½	40	29.18	29.31	W	ESE
Friday	22	29	38	29.43	29.60	EbS	EbN
Saturday...	23	27	35½	29.80	29.80	NE	NE
Sunday	24	31	39	29.78	29.83	E	E
Monday....	25	29	40½	30.10	30.21	WbN	NbE
Tuesday ...	26	32	39	30.21	30.10	E	ESE
Wednesday.	27	31½	40	29.78	29.71	SSE	SSW
Thursday ..	28	32	39	29.98	30.09	N	NbE
Friday.....	29	30½	40	30.10	30.10	NE	N
Saturday...	30	26	41	29.87	29.82	NW	NW
Sunday	31	19	25	30.01	30.00	E	NW

For FEBRUARY, 1830.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Monday....	1	10	29	29.87	29.80	W	N
Tuesday ...	2	18	25	29.74	29.76	NE	NE
Wednesday.	3	17½	27	29.76	29.84	NE	NE
Thursday ..	4	17	35	29.84	29.73	NE	N
Friday	5	13	28	29.70	29.69	NbW	NE
Saturday...	6	3	23	29.53	29.53	E	SE
Sunday	7	15	41	29.30	29.22	SW	SW
Monday ...	8	38	46	29.20	29.43	W	W
Tuesday ...	9	33	43	29.30	29.32	NE	WbS
Wednesday.	10	30	41	29.61	29.83	W	NW
Thursday ..	11	24	43	30.00	30.00	W	SbW
Friday.....	12	28	44	29.92	29.92	S	SE
Saturday...	13	32	41½	29.94	29.97	SE	SW
Sunday	14	28	44	29.97	29.99	WbS	WbS
Monday ..	15	23	39	30.12	30.18	SE	NE
Tuesday ...	16	26	37	30.18	30.12	W	W
Wednesday.	17	26½	40	29.88	29.70	W	WNW
Thursday ..	18	29	37	29.55	29.58	NE	NW
Friday	19	25	38	29.58	29.60	WbN	WbN
Saturday...	20	22	40	29.62	29.62	W	W
Sunday	21	25	38	29.47	29.30	SW	SW
Monday ...	22	26½	41	29.47	29.52	W	WbS
Tuesday ...	23	31	52	29.43	29.60	W	NE
Wednesday.	24	39	57	29.70	29.83	SW	WbS
Thursday ..	25	44	55	29.90	29.89	SW	SW½
Friday	26	43	55	29.81	29.82	SW	SW
Saturday...	27	42	55	29.80	29.87	SW	WbS
Sunday	28	42	55	29.85	29.90	W	WSW

THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

On some Points connected with the Analysis and Structure of the Greek Tongue, by William Sankey, A. M., *of the University of Dublin, and Extraordinary Member of the Royal Medical Society of Edinburgh.*

In directing our attention to the structure of languages, as compared among themselves, we are at once struck with the great variety that is found to prevail amongst them, in respect to what are called the inflections of speech, so that some languages express almost all the circumstances which modify the leading ideas, as of nouns or verbs, by means of appropriate inflections, whilst other languages, using for the most part distinct words to express these modifications, are little, or not at all, inflected. The inflections, therefore, are obviously none other than distinct words, or particles of words, which, by constant use, have come to be coalesced with the word expressing the leading idea, which, therefore, may be considered as the radix of the combined word. The permanence of these combinations, or, in other words, the inflecting of any language, will, therefore, not have been the effect of a high degree of refinement, as is so often mistakenly asserted by eulogists of the Greek tongue, but of directly the reverse, as arising rather out of a rapid and indistinct mode of pronunciation, such as is found generally to prevail among an unlettered people. This much, indeed, may be safely maintained, that all these inflections must have entered into, and taken root in the language, prior to its becoming a written tongue, as the effect of committing it to writing would have been constantly to correct those aberrations from the simplicity of the original speech, which a faulty pronunciation was continually tending to introduce. We may exemplify this by our own language, wherein we find throughout its dialectic varieties, as of Cumberland, Yorkshire, Somersetshire, &c., several inflections which, however commonly used in those counties, have never yet been able to make their way into our written speech. Thus, in the north-west of England, it is not unusual to coalesce the definite article *the* with the noun, whenever this latter commences with a vocalic sound: thus, instead of The egg, they say, T'egg, instead of The ham, T'am, instead of The wine, T'wine; which inflection (as it really is) might well be considered, by the grammatical distributors of the parts of speech, as "a definite case of the noun." In like manner, even in ordinary conversation, inflections are frequently found to prevail, which yet are not tolerated in the written language; for instance, can't, sha'n't, wouldn't, couldn't, shouldn't, don't, &c., which, had they not been rejected from the propriety of good writing, might fairly claim a place in the grammatical arrangement of the English tongue, as constituting what might be called a distinct *negative mood*. Now, it is obvious that such as these, and the dialectic inflections above noticed, have not been the result of a high degree of refinement, but are rather justly considered as barbarisms.

Seeing, therefore, that the inflecting of speech arises (as is well known to philologists) from the combination, or, as it were, synthesis of words, or particles of words, it is obvious that direct analysis may decompose, to use an expression borrowed from the chemist, the combined word again into its constituent parts. It is manifest, however, that when we have resolved any word into its more immediate constituents, we are not, therefore, to consider ourselves as having arrived at its most elementary forms, but, by the re-application of the principle of analysis to the constituent parts themselves, we are to ascertain whether they may not be further resolved into parts still more elementary; following herein, also, the example of the chemist, who, when he has decomposed a salt, for example, into its base and salifiable principle, does not thereupon suspend his researches on the same, as though he had arrived at its elements, but, by a further application of the analysis, endeavours to resolve both the base and the salifiable principle into parts still more simple. To illustrate this by a direct application of the analysis, let us take the words, in Greek, λεγω and λογος. These are each easily resolved into its radix, and that part which properly and solely inflects the word; λεγ-ω, for instance, into λεγ and ω, λογ-ος into λογ and ος. It is manifest, however, that the radices λεγ and λογ will bear a still further comparison with one another, as being closely allied in external characters, as well as in radical meaning. The result, then, of this comparison will be, to shew that the difference between the peculiar modifying ideas contained in these particles, as connected with their distinct oral or written characters, will lie in the ε or ο, inserted between their radical letters λ-γ. Now, it is obvious, that as λεγω signifies *I speak*, and λογος a word, or that which is spoken, therefore the difference between the force of the ε and ο inserted will consist in this,—that while the former is used in expressing simply the action denoted by the root, the latter serves to mark the result of the action as carried into effect. We shall be confirmed in this view by further considering that λογ is not only found as a constituent of λογ-ὸς, but also of λε-λογ-α, the second perfect, or perfect middle, where, obviously, the force of the ο inserted will be connected with the action of the verb considered as past.

In like manner, analysing μεν-ος, μεν-ω, μον-ος, με-μον-α, we arrive at the particles μεν and μον, the former of which, indeed, is still found as a distinct word in the Greek tongue, and bearing a significancy perfectly agreeable to the radical meaning attached to it in its compounded state, while μον clearly denotes the result of the same, it being the necessary consequence of *remaining* behind all others, that we shall find ourselves *alone*, as expressed by the word μον-ὸς.

Here it may be observed, that by taking this analytic view of the peculiar distinguishing characteristics of the constituent particles presented to us in their uncombined state, we shall free ourselves from that artificial system of derivation which is founded *solely* on a *mechanical* addition or subtraction of letters.

For example, it is usual to consider a large class of nouns as derived from the middle perfect, cumbered though almost every part of it must be with the different modifications of mood, tense, number, and person; whereas a just analysis will give us that part of the noun which connects it with the middle perfect, as a constituent part of both, and, consequently, as formed in both, on the very same principles, directly from the same original radix.

Proceeding in this way upon any inflected language, it is obvious that we shall approximate, at least, to a knowledge of its roots, or most elementary parts, from which again, by synthesis, according to the analogy of the language, the words may be recombined into their present inflected forms. The particles of the language, such as *μεν*, &c., it is obvious will, for the most part, be found partaking of the simplest forms, as not being subject to any modifying inflection; hence, they will at once be recognized as roots, and not abbreviations, according to the erroneous hypothesis of the ingenious Horne Tooke:—Abbreviation acted not so much in breaking down words as in coalescing many into one, and so inflecting the language.

If, now, we suppose the analysis carried into all languages, it is clear we shall thus arrive at the roots or elementary parts of each distinct language. Comparing, then, these elements with one another, we shall be enabled to simplify them still further, whilst very many of the elements of one language will be found to be exactly the same as those of another. Often, indeed, we shall find that which is only an element in one tongue, occupying a place as a distinct and significant word in another; thus, for example, analysis gives us *er*, as an elementary and constituent part in the English language, as in the word *great-er*, while we have it a distinct word in the German tongue. In this way, it is obvious that the aggregate sum of the elements to be found throughout all languages will be much diminished—the great families of languages as they may be called, such as the Aramean, Greek, Teutonic, &c., consisting, for the most part, of the same elements. Thus, then, we might be enabled to construct a universal key to all languages, by simply arranging these elements in alphabetic order, from the recombining of which, according to the analogies of each particular tongue, the languages themselves will be again re-constructed.

In comparing the elements of different languages with one another, we shall be much assisted by a tabular arrangement which shall present, as at one view, those elements which are nearly allied, both in radical meaning and sensible or external character. For example, to take the roots in five different languages, Hebrew, Greek, Latin, German, and English, of the words signifying to *bear*, a *sack*, *wine*, we shall arrange them in parallel rows, corresponding to each particular language, so that still the radices shall stand in each row directly under the word at the top of the table, which denotes the significancy of the roots. In this way, then, the roots themselves will be arranged beneath one another in perpendicular columns, thus :—

	Bear.	*Sack.*	*Wine.*
Hebrew .	פר, ph . r.	שק, s . q.	וין, y.in
Greek .	φ . ρ, ph . r.	σ . κ.	οιν, oin.
Latin . .	f . r.	s . c.	vin.
German .	f . hr.	s . ck.	wein.
English .	b . r.	s . k.	win.

From looking at this table, we see at once the necessity of attending, in the analysis, to the affinities of the letters, as *p. b. f.* &c. as grounded on a natural affinity connected with the organs of speech employed to give them utterance. In this view, then, it is well to be acquainted with the powers of the letters, for example, to keep in mind, that as *f* is equivalent to *ph*, so *v* is equivalent to *bh*, &c. This is most important in analysing, or even rightly understanding, the Greek tongue, in which we have so many characters, both consonantal and vocalic, which have a compound power. Such is the case of φ, χ, ϑ, which may be considered as the resultants of the combination of their corresponding middle, or tenues, with an aspiration. In like manner, the double letters, as they are called, ξ, ψ, may be counted as resultants, ξ of γ, κ or χ and ς; and ψ of ϐ, π, or φ, and ς; ζ also as compounded of σδ, or δς, and η as a resultant of εα, or, in some instances, of εε; for example, when it is found as the *temporal* augment of verbs commencing with ε. An attention to this we shall find of the greatest use in reducing many apparent anomalies in the Greek tongue, which are even still considered as such by the grammarians, into their proper place, as being altogether agreeable to the analogy of the language, although now disguised by an interchange of the literal characters. In this way, what at first view seems to be an irregularity, may be found most conformable to rule.

Having made these few remarks upon the analysis of languages in general, exemplifying them by a reference more especially to the Greek tongue, I have thought it might be interesting to notice some points with respect to that language, in which, it appears to me, correcter views might be taken than are usually to be met with in the writings of the grammarians. The observations I have to offer on this head will, I trust, be found no less valuable in an analytical than in a practical point of view, as tending to facilitate the acquisition of the language.

The first practical error, then, which I shall notice, is with regard to what is called the theme, or foundation, whether of the noun or verb. This, in nouns, is usually, I might say universally, considered by grammarians to be the nominative case singular, and, in verbs, to be the first person singular, present tense active. Now, this is altogether a mistake, these parts being no less inflected parts of their appropriate noun or verb than the other parts, and by no means the simplest parts of the same. It is, in fact, to the vocative of nouns, and the imperative, second person singular present active, we are to look as the simplest parts of their corresponding nouns and verbs, they being the very root itself, or else the root with an additional breathing ε suffixed, as in λογ-ε, λεγ-ε, the same being the natural effect, as it were, of impassioned utterance in personally addressing any one as present, such as is the force of the vocative case and imperative mood. *A priori* reasoning itself would, without much difficulty, lead us to this conclusion, inasmuch as the simplicity of a direct address, or of a positive command given to an individual, will introduce few or no modifying ideas, and consequently will give rise to scarcely any inflection. Now, applying this to the vocative, the first error we would correct is in the mode of translating it, which is constantly done by prefixing O, as if O were a necessary and constant characteristic of this case. Now, it is obvious from what I have said, that except in nouns masculine and feminine of the second declension, where it is inflected by a breathing ε postfixed, analysis will give us no part in this case which can be properly rendered by the English O: for example, *Σωκρατες* is properly rendered simply by Socrates, as *Ακουε μϐ, Σωκρατες*, is, Hear me, Socrates, just as, in ordinary conversation, we say, William, hear me, not O William, hear me. This is the more evident, from considering, that the Greeks themselves did not conceive this O to reside in their vocative, inasmuch as they often prefixed it to that case, as *ω Σωκρατες*, which does indeed signify O Socrates.

The next error I would point out as having arisen from not attending to the circumstance that the vocative is more properly the theme of the noun than the nominative, is to be found in the mode in which grammarians usually account for some datives plural of the third declension; for example, they say that nouns ending in *ευς* form their datives plural from the nominative singular, by adding *ι*. Now, a little attention will shew us that this is altogether a mistaken view of the subject, being grounded solely on the mechanical addition of the letter *ι*, which, however, being properly and always the characteristic of the dative case, cannot by itself communicate any thing of a plural signification to the nominative singular. The true view is this: these datives, as *βασιλευ-σ-ι*, are formed directly from the vocative, *βασιλευ*, by postfixing *σ* and *ι*, the *σ* being the characteristic of the plural in all the cases of this declension, except the genitive, and the *ι* being the constant characteristic of the dative case, so that the dative *βασιλευ-σ-ι* is thus naturally accounted for, as also, in like manner, *βϐ-σ-ι* from the vocative *βϐ*.

In truth, many of such errors would be avoided by first analytically ascertaining the radix, and then synthetically forming from thence the several inflections, according to the analogy of the several declensions. Of course, the radix will be analytically arrived at by taking that part of the noun which is common to all the cases, that which is peculiar to each being the proper characteristic of the case and number. To apply this, then, to the several declensions, we shall first take the article as an exemplar of the second and first declensions. Now, I would here observe, that the masculine and feminine had better have been considered as distinct articles, connected as they are with, and evidently derived from, the Hebrew masculine and feminine third person pronouns הוא and היא, which are always treated of by Hebrew grammarians as if totally distinct.

To commence, then, with the masculine article, and comparing the different cases together, we readily perceive that ο is the radix, the ῾ or Η in the nominative being equivalent to the prefix τ of the other cases, which, indeed, was formerly found in the nominative itself, as τ-ο-ς. Looking now to the dative as the next simplest form, we find it written of old, τοι, from whence τῷ. Hence we infer that ι, even though here subscribed, is the proper characteristic of the dative case, and carries into it a signification somewhat the same as our preposition *to*, so that τῷ will be, analytically, "to him," or "to it." Now, postfixing to the primitive dative τοι the pronoun ὃ, *which*, we have το-ι-ο, the Ionic genitive, which, therefore, will signify "which to him" or "it," and this gives the force of the genitive, the ellipsis being mentally supplied. In confirmation of the correctness of this view, we may remark, that the datives and genitives of the Hebrew pronouns are formed from the radix on these very principles: thus, for example, from the radix ו of the pronoun הוא, the dative is formed by prefixing the preposition ל, signifying *to*, and from this again the genitive is formed by prefixing ש, signifying *which*, so that here the Greek and Hebrew analogy differ only in this, that the Greek postfixes these characteristics of the case, while the Hebrew prefixes them. So that, in a tabular view, we may present them thus—

Dat.	לו	to him.	το-ι	Dat.
Gen.	שלו	which to him.	το-ι-ο	Gen.

From this τοιο, still retained amongst the Ionics, came the common του, the υ being probably here pronounced at first like our *u*, with a diphthongal sound, as εε, οο, which, probably, was not far removed from the sound of ιο in τοιο. So that, possibly, the difference between τοιο and του originally consisted in little more than the mode of writing them. Whilst upon the subject of the datives and genitives, considered in connection with one another, I may observe, that it is quite a mistake to consider the use of the dative for the genitive a Græcism, it being as properly a Hebraism. But, in truth, it is also very remarkably a Latinism, the genitives of the first declension being, in fact, none other than datives, as, for example, *musæ*, common to both cases, is properly only a dative, being from the Greek Μουσῃ, the Doric or Æolic α being used in the Latin instead of the common η. In this way also we account for there being but one form common to both cases in the dual. Coming now to the accusative, we find it formed from the radix, with τ prefixed, by suffixing ν thus, τ.ο.ν. The force of this ν seems to denote, that the action expressed by the governing word, whether verb or preposition, has arrived at and rests *in* the radix, so that ν will possibly be taken from the preposition εν, *in*. This seems to be a just view of a *distinct* accusative: for, in truth, in expressing any action as resting on a noun, there is no absolute necessity for introducing any new modifying idea, and consequently we need not necessarily expect any characteristic inflection which may distinguish it from the nominative. Were it so, we should be sure to find always a corresponding particle or preposition to express this characteristic in languages where the cases are uninflective, which, however, we know does not take place, as in the sentences—Samuel saw John, and John saw Samuel, nothing of this kind is to be found. That there is no *necessity* for a *distinct* accusative, is further manifest from there being none such in neuter nouns or in the dual number.

It is not my intention, neither would time permit me, to carry the analysis into every part of the article. I would observe, however, that the nominative plural οἱ or τοι, seems closely allied to the ancient dative singular τοι. Now, at first view, it may seem difficult to account for this, although these cases are even exactly the same in the first declension of the Latins. When, however, we consider how nearly the dative singular is connected with the genitive singular, and that this latter is very generally inflected in Latin and English, and in the third declension of Greek, somewhat similar to the nominative plural, we may perhaps be enabled to trace the origin of the connection between these two cases of the singular with the nominative plural. It is obvious, then, that the idea common to the genitive singular and nominative plural must be that of *belonging to* in the genitive singular, implying *belonging to* in the way of possession, in the nominative plural implying *belonging to*, as to a class of objects denoted by the noun, which is manifestly the distinguishing characteristic idea, which we mentally attach to the plural, as, for example, to *the sheep*, when by the context we are led to look upon the expression as plural rather than singular. Hence, then, we see that the ι in the nominative plural, as well as in the dative singular, denotes *to*, and is probably connected with, or a fragment of εις, *to*, as the nominative plural termination ες of the third declension is none other than the poetic form of εις, while the ς, which we find terminating genitives singular of the feminine article, and those of the first declension which follow its analogy, as also all those of the third declension, seems to be another fragment of εις, its import being very nearly the same as that of ι, and, like it, it is no less a characteristic of plural number, occurring as such, as we shall see, *always* in the datives and accusatives plural. Thus, by postfixing ς to the old dative singular τοι, we have the dative plural naturally formed, by simply adding to the singular the characteristic of the plural. The further addition of ι final, as in use among the poets, seems to have arisen solely from the ear being accustomed to a final ι in datives plural of the third declension. In like manner the accusative τους is formed by adding the plural characteristic ς to the accusative singular, thus, τον, τον.ς. The Greek ear, however, not bearing the collision of ν and ς, the former was changed into α, according to a very common analogy of this tongue (as τιθεαται Ionic for τιθενται), thus giving us τοας, which regularly contracts into τους. Hence, we shall see that the invariable rule for forming the accusative plural is by postfixing ς as the characteristic of the number.

With respect to the feminine article ἡ, what we have observed on the masculine will in general suffice. It is necessary, however, to say a word or two on its genitive singular, which does not follow the analogy of the masculine, but is formed by postfixing ς; now this being, as I remarked above, equivalent in import to ι of the dative, it will follow that this genitive is in truth analytically another dative, which is, however, *constantly* used *elliptically* as a genitive. The genitives of the third declension, on the other hand, we shall find by the insertion of ο before the ς, are truly and analytically genitives.

It is further worthy of observation respecting this article, that while its radix in the singular number is of the common dialect, as η, in the dual or plural it is of the Doric. This is most important to be attended to, as the anomaly at the first view throws a difficulty in the way of the analysis. For example, I said that all accusatives plural are constantly formed from the accusative singular, by postfixing the plural characteristic ς. Now, in applying this rule to accusatives of the feminine article, and nouns which follow its analogy, it will be necessary to take into consideration that, with a view to the synthesis, we must first take the accusative singular as of the Doric dialect, as ταν, then suffixing ς, we have ταν-ς, changing ν into α, on account of euphony, we have ταας, which contracted is τᾶς.

With respect to the neuter article, I may observe, that properly speaking, there is no such *distinct* article, except in the nominatives singular and plural, there being, as I said before, no distinct accusatives or vocatives in neuter nouns.

Further, it is remarkable that, while the neuter singular το follows the masculine, being indeed but another and apparently older form of it, the neuter plural follows the feminine, being, in fact, the same as the nominative dual of the feminine article. We need not wonder at this, when we consider that, in our own language, the third person pronoun *they* belongs to all the genders, shewing us that there is no natural and necessary distinction between the same.

Having taken this view of the inflections of the article, it is obvious, that little need be said upon the second and first declensions which for the most part follow their analogy. It will, however, be necessary to make a few observations upon the genitives of masculine nouns of the first declension, as they are not formed after the feminines, but terminate in ου instead of ης. This anomaly, however, perhaps may be thus accounted for. It is obvious, that analysis will give us the proper theme, terminating in η or α, as τελωνη, Αινεια, from τελωνη-ς, Αινεια-ς. The nominative, therefore, being itself formed by postfixing ς to the theme, as is not unusual in nouns not only of this, and the second declension, but also in the third, it is manifest that if the genitive singular were formed by the same suffix, there would be no difference between these cases. The genitive, therefore, here follows the analogy of the masculine and postfixes ο to the dative. In this way we shall account not only for the common form of these genitives, but even still more immediately for the unusual forms in εω and αο. In order to exemplify this, let us take the word νεφεληγερετης: its theme is obviously νεφεληγερετη or νεφεληγερετα, which last is indeed found in the vocative and in Attic nominatives—adding, therefore, ο to the dative νεφεληγερετῃ or νεφεληγερετᾳ, we have two forms of genitive, νεφεληγερετηο and νεφεληγερεταο, so well known as used by the poets. From νεφεληγερετηο again, by throwing the quantity upon the last syllable, according to the poetic rhythm of the Greek tongue, we have νεφεληγερετεω, or even in shortening the η before the ο, according to the prosody, simply their common conversation, we obtain νεφεληγερετεο, from which, regularly contracted, we have the common form νεφεληλερετου.

We now come to the third declension, and here, more than in any other, it is necessary to look for the real theme, keeping in mind, that here the nominative singular is, in general, no less an inflected case than any other. We are also to observe, that nouns of this declension frequently take τ, δ, or θ, between the root and the postfixed characteristic of the case. Further, as the root is often disguised in the nominative, in consequence of the suffixing ς producing a change in the characters, whether oral or written, we shall, therefore, find it necessary first to obtain the root analytically from the oblique cases, if not already prepared for us in the vocative, and then synthetically to recompound the cases.

To exemplify this, let us take οδους. Here, as we shall see, the radix is in truth disguised; analysing, however, the genitive case οδον-τ-ος, we find it manifestly composed of οδον the radix, and ος the characteristic of the case, with τ inserted. From this radix, then, recompounding the cases, and adding to the nominative its very common characteristic ς, we have first οδους, or changing ν into α on account of Greek euphony, οδοας, and contracting, οδους. The genitive is formed by postfixing ος, and in this noun inserting τ between it and the radix. Now the characteristic ος seems to be composed of ο, signifying *which*, and ς, equivalent in its import to ι of the dative, and consequently denoting *to*; so that this termination ος of the genitive will have the same force as the Ionic termination ιο of the second declension. Hence, therefore, we have the genitive οδον-τ-ος. The dative is regularly formed by suffixing ι, and in certain nouns, such as this before us, also by inserting τ, thus giving us οδον-τ-ι. The accusative is regularly formed by postfixing ν. Now, it is obvious, that ν cannot be pronounced directly after an inserted consonant, or a consonant in the radix immediately preceding it. In this case, therefore, ν is changed into α, according to the analogy of the language, so that the accusative in this noun will be οδοντα. When the radix terminates in a vowel, the accusative may be formed directly by suffixing ν, as from ερι, εριν. The other oblique cases, however, taking δ inserted, we have also, by adding ν and changing it into α, εριδα. The nominative plural we have seen is formed by postfixing ες; the dative plural is formed by adding to the radix ς, the characteristic of the number, and ι the characteristic of the case, inserting when requisite τ, δ, or ϑ; we, therefore, have here οδον-τ-ς-ι, or, throwing out τ on account of euphony, οδον-ς-ι, and changing ν into α, οδοασι, which contracts regularly into the form in use, οδουσι. Sometimes, indeed, the datives plural are formed directly from the nominatives *plural*, by suffixing ι, the characteristic of the case: thus οδοντες, teeth, οδοντες-ι, to teeth, which poetically, for the sake of quantity, doubles the ς, making it οδοντεσσι. The accusative plural is formed according to the general rule, by adding ς, the plural characteristic to the accusative singular, thus making οδοντα-ς.

Another instance, wherein we shall find the advantage of attending to the real theme or root, with a view to correcting the erroneous rules laid down by grammarians, is to be found in respect to the rule they give us for the formation of datives plural, whose nominative singular terminates in ξ. In these instances, they *mechanically* form the former case, by adding to the latter. We, however, shall take a correcter view, in considering that the double letter ξ disguises both the last consonant of the radix, whether γ, κ, or χ, and ς, here a characteristic of the nominative singular, so that to take *αρπαξ*, and comparing it with the other cases, as the dative singular *αρπαγ-ι*, we find the root is *αρπαγ*, which, suffixing in the nominative singular, ς, becomes *αρπαξ*. Now, the dative plural is formed from the same radix *αρπαγ*, by postfixing ς and ι, the characteristics of the case and number, thus giving us *αρπαξ-ι*. What in truth leads to the idea of the dative plural depending for its formation on the nominative singular, more immediately than the other cases, is the simple circumstance of the double letter ξ occurring in them both and in them only. In this way also, we account for θριξι, the dative plural of a nominative singular θριξ. Here, analysis gives us τριχ as the radix, which, suffixing ς for the nominative singular, and transferring the aspiration thus lost, from the final consonant χ to the initial τ, we have θριξ. Again, postfixing to the root τριχ, ς and ι, the characteristics of the dative case and of the plural number, and bringing forward the aspiration, we obtain θριξ-ι.

When the radix terminates in αν, as Αιαν, then suffixing ς in the nominative singular, we have Αιαας, and contracting Αιας, the ν again appearing in the oblique case, as Αιαν-τος. So likewise the adjective πας was originally πανς, or changing ν into α, παας, contracted into πας. And here I may observe, that in both adjectives and participles which follow this declension, the neuter is properly the radix, just as here παν is the root.

If the radix terminates in εν as τυφθεν, then the nominative singular taking ς after it, becomes τυφθενς, or changing ν into α, τυφθεας, which, according to the analogy of the tongue, contracts into τυφθεις, for though εα not followed by ς contracts into η, yet with ς after it regularly is contracted into ει, as αληθεας, αληθεις.

Here I would remark, that the formation of the feminine of the participles active, as the present, &c., such as λεγουσα, is thus accounted for: originally, like the Latin, there was but one gender in these participles, which was formed from the radix by suffixing ς, thus giving us, for example, from λεγον λεγονς, as we find in the Latin *legens stans*, &c., of all genders: postfixing, however, to λεγους, α, the sign of the feminine gender, as borrowed from the article in the Doric dialect, we have λεγους-α, or changing ν into α, λεγοασα, and contracting λεγουσα.

From the view which we have just taken of the inflections of nouns, it is obvious that three things are to be attended to in the analysis: 1. The radix. 2. The characteristic of the case. 3. The characteristic of the number, as these will synthetically again recompound the word. And here we would observe, that ι, whether subscribed as an actual constituent of the word, is always the characteristic of the dative, while ν or its substitute α, is that of the accusative.

We would also remark, that ι subscribed is not confined to datives of the first and second declension, but is also found in some of the contracted forms of the third declension, as in κερᾳ; for here the root being κερα, by postfixing ι gives κεραι, which, by subscribing ι, becomes κερᾳ, or inserting τ, κερα-τ-ι.

With respect to the verbs, I feel that few observations will suffice, inasmuch as the attention of the analysts of language having been very generally directed to this part of the Greek tongue, its analysis has been pretty fully completed. I would observe, that it is time now for grammarians to lay aside many of their rules for the formation of tenses, as being grounded solely on a mechanical addition, subtraction, or interchange of letters—for example, what can be more absurd than to derive the first indefinite passive from the third person of the perfect passive, by changing ται into θην, thus connecting the third person of one tense with the first person of another? Even where the connection between the tenses is apparently more natural, as in the first future, and first indefinite, which seem to partake of the same characteristics, it is well to consider whether they are or can be at all allied in force, and consequently, whether the similarity of their characteristics may not have arisen out of different circumstances in each, independent one of another; thus the proper characteristic of the first future is ς, disguised, however, sometimes in ψ or ξ, and on account of euphony, necessarily rejected from roots terminating in the liquids, λ, μ, ν, ρ. Now, this characteristic ς is taken from ες, signifying *to*, and thus denoting something future, which ες, indeed, was itself formerly found in all futures active, as τυπ-ες-ω, which, by leaving out the ε, became the form of the first future τυπ-ς-ω, or as written, τυψω; or leaving out the ς, τυπεω, and contracting τυπῶ, second future. Hence, also, we account for the two futures of the passive: from the simple radix τυπ, we obtain the second future τυπ-η-σομαι, or inserting and *aspirating* τ, τυπ-θ-ησομαι, while the middle futures follow, in their formation, the analogy of active futures. Now, the first indefinite does also take ς, and reject it in the very same circumstances, in which this takes place in the future. Yet considering the force of this tense, it does not seem in significancy to partake any thing of a future, while the prefix augment and the termination α ally it rather with past tenses, and especially the perfect; ς, therefore, seems rather to arise from a sibilation before the termination α, instead of an aspiration, just as the Latins, from the Greek ἑξ, form *sex*, by sibilating instead of aspirating. It is obvious, then, if this view be correct, that the same verbs which for euphony reject ς in the future, must also reject this sibilation from the aorist; hence, then, the aorist seems to adopt the characteristic of the future, whereas it more properly takes the peculiar characteristic α of the perfect, whilst its prefix augment shews that it is included in the class of past tenses in general. The disadvantage of thus, as it were, mechanically connecting two tenses together, without considering whether there be really any natural connection between them as to power, is, that it not only throws difficulties in the way of the analysis, but, still more, it leads to vague ideas as to the value of one or other of them, whilst we expect to find, for example, something of a future character in that which is really and analytically a perfect; hence, then, the very unsatisfactory name of indefinite or aorist, when it should be more properly a perfect. In this way, then, we see the Greeks have three perfects active, the proper and distinctive characteristic of them all being the α after the root, the force of which is equivalent to our auxiliary *have*. And it is remarkable, we find *a* retaining this meaning as a distinct auxiliary in the third person singular present indicative, or root, of the French verb *avoir*. Hence then, synthetically, we form the three perfects active; thus, suffixing α to the root, prefixing the augment, and reduplicating the initial consonant, we have one form commonly called middle perfect, as *τ-ε-τυπ-α, τ-ε-τι-α*; following the same process and inserting either the aspirate or *κ*, before the characteristic *α*, we have another form or perfect active, as *τ-ε-τυπ-ἁ*, or as written, *τ-ε-τυφα, τ-ε-τι-κ-α*. Lastly, without reduplicating but sibilating the *α*, we have a third form, or that called first indefinite, as *ε-τυπ-σα*, or written *ε-τυψα, ε-τι-σα*. Analytically then, the force of these forms will be expressed by *have been*, the *ε* of the augment, whether disguised as in the temporal, or absolutely expressed as in the syllabic augment, being the radix of *ε-ω* or *ειμι* be so, that *τ-ε-τυπ-α*, means accurately *be, strike have I*, or *I have been striking*.

It is obvious from what we have occasionally noticed as we proceeded in this analysis, that the verb *τυπτω* has been very ill chosen as a paradigm or exemplar verb. In the first place, from the Greeks combining *πς*, or *ps*, into a distinct single character, *ψ*, and *p* aspirated, or *π*ʽ, also into a distinct character, *φ*, it is clear that whenever these combinations occur as both the radical letter *π*, and the characteristic *ς*, or the aspirate, are disguised; the same disadvantage also occurs from aspirating the *τ*, as in *ετυφθη*; secondly, the root is properly only *τυπ*, from the Hebrew פת, not *τυπτ*, as is manifest from the analysis of all the tenses, the *τ* inserted before the inflections, being found, whether simply or aspirated, only in the present and imperfect tenses, and in the first future and first indefinite, as they are called, passive. Hence it is that for so long a time the second indefinite was classed as a distinct tense *sui generis;* neither have grammarians yet altogether agreed to abandon this distribution; whereas it is obvious that *ε-τυπ-ον* is as regularly formed an imperfect from the radix *τυπ*, as *ετυπτ-ον* is from *τυπτ*. In this view, then, we should consider all the moods of this indefinite, except the indicative, as being also of a present signification, inasmuch as the augment is dropped and they retain no distinguishing mark of past time besides.

In this way, also, we see that the first and second indefinites passive are both of different forms of the same part of the verb, the one being formed from the simple root *τυπ*, as *ε-τυπ-ην*; the other from *τυπτ*, aspirating the *π* and *τ*, as *ετυπ*ʽ*τ*ʽ*ην*, or as written, *ετυφθην*. To put the correctness of this view beyond the shadow of a doubt, we have only to take for a paradigm any verb whose radix is such as does not admit of any distinct second indefinite; for example, *τι-ω*, which does and can give us only ε-τι-ον, of the form of imperfect active. The fact is this: whenever grammarians meet with an anomalous imperfect, as being either not properly connected with the verb itself, with which, however, it is erroneously classed, as ηλθον, from ελευθω, and not ερχομαι, or else a duplicate form, in consequence of the nature of the root admitting both of them, immediately they have recourse to the second indefinite, which, however, in this latter case, will be found to be only a more colloquial, and, therefore, shortened form of the imperfect; for example, while the regular imperfect of λειπω is ελειπον, this conversational form is ελιπον. In contracted verbs, we have further to notice, that what is classed as the second indefinite, is in reality nothing more than the regular imperfect of the contracted present: thus we have φιλεω, and, contracted, φιλῶ; the former gives us the imperfect εφιλεον, contracted εφιλουν; the latter gives us simply εφιλον.

To present them in a tabular arrangement:

	1st Person.			2d Person.			
Pres.	φιλε-ω		φιλω	φιλε-εις		φιλ-εις	&c.
Imp.	εφιλεω	εφιλουν	εφιλον	εφιλεες	εφιλεις	εφιλες	

I have found it necessary, in order to be understood, to adopt the usual expressions of the first and second future, &c.; it would be better, however, could I find some other mode of distinguishing such duplicate forms, as the terms *first* and *second* are apt to mislead the tyro, and might induce him to imagine, that one future, or perfect, was *more intensely future*, or perfect, than the other.

Whilst making these cursory remarks upon the verbs, I would observe, that it is a mistake of some grammarians to trace the common form of the second person of the present tense indicative, for example τυπτῃ, through an older form terminating in ει, as τυπτει, in deducing it from the most ancient form, τυπτεσαι. Τυπτεσαι, by omitting the σ, gives evidently the Ionic τυπτεαι, from which immediately and analogically, without the necessity of any intermediate step, comes τυπτῃ, the εα of τυπτεαι being contracted into η and the ι subscribed.

Another point, of importance to notice, is a mistake in the power of the optative mood, as if it was precatory, and not merely votive, giving rise to the idea, that it must always be rendered as a prayer to the Deity, and so introducing His name (of the Deity) into the translation, as for example, turning μη γενοιτο by "God forbid." Now, an accurate analysis shews us that this mood contains not, in its peculiar characteristic, or indeed in any part, anything which can in the least be considered as denoting Deity; for instance, in the active voice optative, τυπτ-οιμι, as the radix resides in τυπτ, the power of the optative must be connected with οιμι, which is obviously a distinct, though unused verb in μι, being the active of οιμαι, *to think*, and also derived from the verb οιω, *to carry*, which shews that, analytically, this mood denotes being *mentally carried* towards any object, which proves a votive, but not precatory power. In like manner, in the passive voice, analysing the optative τυπτ-οιμην, we arrive at οιμην, as that part which is connected with, and expressive of, the power of the mood: now, οιμην is manifestly the imperfect of the verb οιμαι, as is clear from this mood adopting the rule of imperfect tense, in terminating its third person dual in ην, and not in ον, like the second person. Now, the force of οιμαι and οιμην is obviously not *precatory*, but denotes the mind being carried to think with desire or inclination in regard to any object. What is here said about the optatives, as formed in οιμην, applies, if possible, still more strongly to those in ειην, which seem, indeed, analytically to partake more of the subjunctive than even *optative* character.

I would here remark, that it appears to me that the grammars are defective in not giving to the imperative mood a first person plural in Greek as well as in Latin, for though the *form* may be the same as that of the first person plural of the subjunctive, yet in the mode of application they are essentially different. Thus we have αγωμεν, let us go, αποθανωμεν, let us die, &c. The form being the same is no more a reason for omitting it in the imperative, than it would be a reason for rejecting the second person plural of the present from the same mood, inasmuch as it is found also in the indicative mood.

Having thus had occasion to notice the unused verb οιω, I am induced, in connexion with it, to remark upon another error, in calling in parts of one verb to supply those of another, not merely as adopted substitutes recognized as such, but as if they were the legitimate offspring of the root; thus we have a strange commixture of tenses gathered out of different verbs, in conjugating the verb φερω, as thus, φερω, οισω, ηνεγκα. We might just as well say that *carried*, in English, was the past tense of *bear*. I admit English grammarians are not without their faults also in this particular: thus, it is usually said that *went* is the past tense of *go*, whereas it is the past tense of the old verb *wend*, whilst *go*, or rather *gae*, its allied form in the Scottish dialect, has its own past tense *gaed*. Similar to this error is that common one in most languages, of connecting comparatives and superlatives with positives, with which, as perhaps neither with one another, they are no way allied in literal or sensible characters, such as good, better, best.

To return, however, to the verbs, I shall make but one further observation upon them, and it is this, that in analysing the verb we are to attend to six different circumstances: viz. 1. the radix; 2. the characteristic of the voice; 3. the characteristic of the mood; 4. the characteristic of the tense; 5. of the number; and 6. of the person. We should observe, however, that the simpler the form the fewer the characteristics: for example, there is no distinguishing characteristic of the active voice, or present tense, whereas we find αι or ην characteristics of the passive voice, and οι or ει of the optative mood.

Connected with the analysis, it may not be altogether irrelevant to notice here the remarkable anomaly in the Greek tongue of neuters plural, for the most entering into construction with singular verbs. Now it is manifest, that analytically there is no reason why the third person singular should not be connected with plural nouns as well as singular, as it contains no distinctive characteristic of the number to limit it to the same, just as we find, in the English tongue, the same form answer for the first person singular, all the persons of the plural number, inasmuch as it contains no distinctive personal characteristic, and is, therefore, equally applicable to all. In a language, however, so minutely inflected as the Greek, and which has a distinctive third person plural in its verbs, it can scarcely be accounted for on this principle, but rather seems to have arisen from the circumstance that these neuters plural, always terminating in α, and this being a common termination of feminine nouns, more especially in the Doric dialect, the ear, which must have been the chief guide to the concords among the primitive unlettered Greeks, early introduced this anomaly and solecism amongst them, which afterwards, by use, continued to hold its place in the language as a beauty and propriety of the tongue.

I shall now close with observing, that it is obvious, from what I have said above, that the nominative case singular of substantives, the masculine of adjectives, and the first person present singular, not any of them being necessarily the radix of their corresponding inflected parts, there is no occasion why they should be specially selected to be, as it were, the representatives of the other parts in the arrangement of dictionaries. It is clear, that arriving at the root by the analysis, it would be far better to construct the dictionaries on this principle, of putting down the roots only in alphabetical order. This, in nouns, for example, will assist immediately the student in seven or eight cases out of ten—as this root is generally found, in its simplicity, occupying the first part of the word in the oblique, and not consequently most numerous cases, whereas it is commonly disguised in the nominative, and consequently requires a somewhat tolerable acquaintance with the language to be able even to make use of the dictionary. The same holds good also in the verbs. It is obvious that the object of a translating dictionary is to present the meaning of the words of one language in those of another, which is supposed to be understood by him that is to use the dictionary. Now it is clear, that this intention could only be completely fulfilled by inserting in the dictionary all the words of the language, including therein every inflected part; as, however, this would render the dictionaries too bulky and cumbersome, as well as expensive, and as these inflections were connected together according to a general rule, it was found to be a more commodious way to put down only the leading words, from which, as it was supposed, the others were formed according to an established analogy, the rules of which it is the business of the grammar to teach, so that, in fact, the introduction of grammar, properly so called to distinguish it from the syntax, arose out of the necessity of abbreviating the dictionary.

In proposing such a dictionary of roots, it is obvious I by no means have in view any such plan as that followed by Scapula, which, so far from assisting a beginner, requires a pretty extensive knowledge of the language to use it with comfort and advantage.

Not having here in view an etymological, but a translating dictionary, the arrangement I would propose is to give the roots *only* of the inflected parts, still retaining the derivatives, as well simple as compound, in their proper alphabetical order. I would also observe, that with a view to clearness, simplicity, and brevity, I would even recommend this, or any other dictionary, to be conducted on the principle of giving to each word or root that meaning, which, as a leading idea, may comprehend under it all the apparently various significations, which, according to the context, it may seem to bear in different passages; and here I would remark, that there cannot be a worse mode of interpretation than that of applying generally to words a forced meaning, derived from the context in particular passages, in which, perhaps, the real meaning may be modified by some other word of the sentence, in which it may be intended all this peculiarity of meaning may reside. This mode of interpretation may display a show of learning, but manifests a real want of critical acumen, at the same time that it gives a great space to mistranslations, in consequence of prejudice.

There are many other points connected with the analysis which it might be interesting to notice; but this article having already run to a greater length than I expected, I must, therefore, now conclude these observations with expressing my wish that the mere mechanical system of grammatical arrangements may be speedily exchanged for one which, as founded on a juster basis, shall prove more intelligible to the pupil, and less embarrassing, by wrong associations to the analogist.

Some Remarks on the Reciprocal Action of Indigo and the Fixed Oils. By Charles H. Weston, Esq.

[Communicated by the Author.]

Of the many substances which the experiments of chemists have led us to consider, as primary or simple, there is none of more pervading importance than oxygen; none capable of accomplishing a greater variety of changes by its combination with other bodies.

Some of the many and dissimilar effects of oxygen are happily exemplified by the metallic oxides, and all their shades of colour—by oxides of the same metal varying according to the proportion of oxygen absorbed—and by the conversion of many substances into the family of the acids. Among organic bodies, however, indigo stands pre-eminently conspicuous, for beautifully illustrating the diversified consequences of the same agent. These, indeed, in a general way, are known to all persons in the least conversant with the nature and properties of this valuable dye. Yet, a more minute knowledge not only discovers that there is no period, in which indigo from first to last (short of decomposition) is not influenced by oxygen, but that the changes resulting from a combination with this gas, vary according to the *quantity* thus chemically absorbed.

In a paper connected with the subject of indigo, in the Quarterly Journal of Science of June last, I remarked that a certain proportion of oxygen renders the dyeing substance, before it is separated from the leaves of the plant, soluble in water; that a greater proportion renders it insoluble *, and that a still further accession again renders it soluble. Change of colour, corresponding with these increments of oxygen, invariably follows, which, commencing with a yellowish green,

* Hence it follows, that, in proportion as the indigo of commerce imparts to boiling water more or less of the *greenish* yellow, in the same proportion has the manufacture of it been imperfect. The immediate cause, however, of the presence of this deoxygenized indigo can only be known to the manufacturer, because it may have resulted either from mismanagement in the beating vat—or, if this part of the process had been properly conducted, from the imperfect desiccation of the leaves, or from want of sufficient sunshine during the growth of the plant.

and passing through the different shades of light green—greenish blue—blue and blackish blue, terminates in black.

These facts, as they shew us upon what the solubility and insolubility of indigo depends, enable us fully to understand how that, if we wished to make soluble the insoluble *blue* indigo, which occupies the middle state, we must deprive it of a certain quantity of oxygen, and so again render it green. On these principles it would be legitimate to infer, that all substances which have a greater affinity for oxygen than indigo, and simultaneously afford a menstruum for solution, must be capable of dissolving indigo. This influence led me to consider that, as oils possessed such qualifications, particularly when heated, it was highly} probable, that in oils *alone* a solution might be accomplished. I accordingly mixed pounded indigo with cocoa-nut oil *, in a Wedgewood's evaporating dish, and by an Argand's lamp gradually raised its temperature to the boiling point. The mixture became at first *green*, and passing rapidly to *blue*, and reddish blue, assumed a *fine crimson*.

These changes were as beautiful as they were unexpected; and on this interesting compound I instituted various experiments, many of which it would be superfluous to mention, as I have, since my return to England, found myself in a great measure anticipated by Mr. Crum, in his elaborate paper on Indigo, in the Annals of Philosophy for 1823. I shall, consequently, after first making a few remarks on the substance dissolved, pass on and direct my attention to a subject not touched upon by Mr. C.; namely, the *immediate cause* of these changes.

Indigo, in its marketable state, is, as is well known, a compound colour. It first gives to boiling water a Madeira colour with greenish tinge; then to boiling diluted muriatic acid a light brownish yellow; and, lastly, to boiling alcohol a fine bright carmine. As I had used the indigo in this impure state, it was necessary first to see how these extraneous colours influenced the colour under consideration. The indigo was, therefore, purified by water, the acetic and muriatic acids, and

* Animal and vegetable oils are equally effectual, as one might have supposed, from their containing the same ultimate products, and the same, or nearly the same, proportion of them.

alcohol; yet the indigo, so purified, when mixed with oil and boiled, was found capable of imparting the crimson colour. I again purified indigo in the dry way, by sublimation, and boiled the crystals* in alcohol, to which they imparted no tinge; but these, when boiled in oil, produced a similar colour. Indigo, then, is the *sole source* of this crimson colour.

My next object naturally was to investigate the immediate *cause* of the changes; and, for this purpose, I submitted the crimson colour, made as before stated, and a *cold* solution of oil and indigo, to the following experiments. Both solutions were subjected to the *same* experiments, that a comparison of the effects produced might lead one to discover what alteration had resulted from the application of heat.

Hot Solution.	*Cold Solution.*
No. 1. Became first *green*, and rapidly passing to *blue* and *reddish blue*, ended in *crimson*†.	No. 1. Immediately becomes *green*, which remains *unchanged* when *out of contact* of the atmosphere. When exposed, the *surface* first becomes purplish, which gradually pervades the solution. If a limited quantity of air be admitted, the change of colour is also limited.
Muriate of Tin.	
2. Immediately lessens, and finally removes colour.	2. Lessens and removes colour.
Alcohol.	
3. Removes colour.	3. Removes colour‡.

* These crystals, obtained from indigo *once* sublimed, were viewed under a lens as powerful as was consistent with a proper quantity of light. Their form appeared to be that of a four-sided prism. They were opaque, and varied in colour according to the angle of light in which they were seen, changing from a light copper to a reddish metallic appearance, and presenting, in different positions, lilac, lightish green, pink, and pinkish white. When viewed under the *direct* rays of the sun, they exhibited a brilliancy approaching to combustion.

† Care must be taken not to decompose the crimson by continuing the application of the heat too long.

‡ This solution must be in its *minimum* state of oxygenation, otherwise all the colour will not be removed.

Hot Solution.	*Cold Solution.*
Sulphuret of Potash.	
4. Lessens colour.	4. Lessens colour.
Liquid Chlorine.	
5. Removes colour, by *first* bringing it back to *blue.*	5. Removes colour.
Oxygen Gas.	
6. Passed through the solution did not increase the colour.	6. Surface rendered *reddish blue.* The whole a lighter green, with a precipitate of what appeared to be indigo.

We see, in No. 1, that the indigo is first deoxygenized, and rendered *green*, and that it then regains its oxygen, and becomes *blue*, unless the atmosphere be excluded; and, therefore, it follows, the same laws which govern its solution in water, and *so far*, it confessedly changes its colour by the *absorption* of *oxygen.*

Again, we see that the crimson is affected by the deoxygenizing tests equally with the cold solution, and consequently the substance contained in both these solutions is, I conclude, analogous in its nature.

We also see by No. 6, that oxygen, without any elevation of the temperature of the menstruum, can produce the *reddish* blue; and lastly, by No. 5, we see that chlorine, in combining with the oxygen, and abstracting it from the indigo in solution, makes the compound, as it were, to retrace its steps, and reduces it *from* the crimson *to* the blue. The collective evidence, therefore, of these experiments seems to prove, that no decomposition of the indigo has taken place by the application of heat, and that the carmine, or crimson colour, has resulted from the *extraordinary quantity of oxygen* which the favourable circumstances attending the solution has enabled it to receive. One remarkable circumstance, suggested by experiment No. 3, must here be noticed. It is there seen, that alcohol removed both the crimson and the green colour, and yet alcohol has no influence whatever on the *blue* indigo or its crystals. The capability, therefore, of alcohol as a solvent of indigo, when combined with either a *less* or a *greater* quantity of oxygen than the blue, is precisely similar to that of water, to which I have before alluded *.

But the absorption of oxygen by the indigo is not the only effect produced in this compound. The menstruum has, likewise, suffered a very important change. The vegetable oil, which was the subject of the experiments, was converted into a *drying oil*, and, on being mixed with carbonate of lime, made good putty. Now, we know that vegetable oils become drying oils by an absorption of oxygen †, and we saw how instantaneously the oxygen was abstracted from the indigo by its reconversion to its *green* state from its *blue*. This was the first step, but the indigo could not long remain so deprived of its oxygen. In its turn, it decomposed the atmosphere, and regained its blue, and then again came into action the superior affinities of the oil. Thus, therefore, this play of elective attractions had been going forward, the oil abstracting oxygen from the indigo, and the indigo from the atmosphere; and both gradually progressing, till oil and indigo had received as much oxygen as the temperature permitted, which was limited only by the non-decomposition of the indigo.

I would remark, in conclusion, that this reciprocal action of the indigo upon the oil gives us a clear view of the extensiveness of the operation of oxygen throughout the process detailed in this paper, and forcibly corroborates the inferences I have ventured to draw from the experiments above enumerated.

* See also the analysis of Guatemala indigo by M. Chevreuil, in Ure's Chem. Dictionary, Art. Indigo.

† Vide Brande's Manual of Chemistry, third edit., vol. ii., p. 482, and Ure's Chem. Dict., Art. Humidity.

Commentary on a Paper in the Philosophical Transactions of the Royal Society for 1829, p. 9, *entitled* "A Description of a Microscopic Doublet, *by* W. H. Wollaston, M.D., F.R.S., &c." *By* C. R. Goring, M.D., &c.

[Communicated by the Author.]

Commentaries afford a very useful method of communicating information, for they give us an opportunity of telling the public *what is wrong* as well as *what is right.* I have ever found, from experience, that he who instructs only on *one* of these points, does his work by halves: moreover, commentaries on the writings of distinguished persons (if ably executed) frequently attain a consideration which the matter contained in them would not receive in another form, purely on account of the reputation of the writer commented upon; accordingly, most of the ancients of note, and many moderns, have had commentators upon their works: several of our most valuable law books have been given to the world in the form of comments on those of other lawyers. No apology, therefore, can be necessary for my present illustration of Dr. Wollaston's paper. I hope that the same indulgence which Dr. Wollaston seems to petition for, on the score of his health, will be conceded to me also, for I am equally entitled to it; and I seize this opportunity of apologizing for my productions *in general,* of which I am heartily ashamed, for almost all I have given to the public has been *dictated,* and few can dictate as accurately as they can write. I am distracted, stupified, and confused by a constitutional determination of blood to the head, which rarely permits me to write myself, or indeed to read much, without a dangerous aggravation of the distemper: moreover, I have frequently been unable to correct my printed *proofs,* and my papers are thus given to the world at the mercy of the printer, or of some friend ignorant of the subject, with all their sins and blotches about them; nevertheless, I know, that if a man, sick or well, commits a *faux pas* in print, he will be laughed at, for *what is written is written,* as the Turks very justly observe—such allowances, however, as the public can, in conscience, make for me, on account of my infirmities, I shall be grateful for. I shall do by Wollaston as I would be done by myself, and shall proceed to a rectification of divers matters discussed by him, with precisely the same delicacy and respect to his dignity and genius which I should have observed had he been still living, (though, strictly speaking, such men as Wollaston never die.)

Dr. Wollaston's paper being *hastily thrown* together in *paragraphs*, I shall number them, for the sake of convenient reference; in the original, the text contains nineteen, and the Appendix three more. Dr. Wollaston seems to have been animated by a most laudable ambition of surpassing every thing which has yet been done in the way of improving microscopes, and of presenting to opticians not only a *better* article than has yet been produced, but a *cheaper* one also (Par. 6): in this most praiseworthy design, he assures us he has thoroughly succeeded (Par. 4 and 18), and, moreover, condensed his instrument into a box four inches square (App. Par. 2.)

As there is not much connection between the different passages of his paper, I shall notice them pretty much in the order in which they are written. Touching the instrument itself, it may be divided, like all other microscopes, into 1st, its optical part; 2nd, its illuminative apparatus; 3rd, its mechanical arrangement for applying the object, and adjusting the focus, &c.

Par. 2. "In the illumination of microscopic objects, whatever light is collected and brought to the eye beyond that which is fully commanded by the object glasses, tends rather to impede than to assist distinct vision."

Par. 3. "My endeavour has been to collect as much of the admitted light as can be done, by simple means, to a focus in the same plane as the object to be examined."

The principle laid down in Par. 2 is undoubtedly correct, at least if taken in a certain sense, and unconnected with the following Par. 3. It is well known, that the aberration of all optical instruments is most sensible with brilliant objects, or with others rendered so artificially. Thus, it may be said, that a telescope which is unable to command or define a large star, will nevertheless reduce a small faint one to a brilliant dot, and an outsized reflector, which cannot, perhaps, give any definite figure whatever to Saturn, will yet be able to draw the light emitted by some distant cluster of stars to a well determined focus, and shew each distinct and separate from the rest, though as close as regular double stars, which it would be utterly incompetent to separate. For the same reasons, it will shew faint nebulæ nearly as well as if it was a more perfect instrument—upon the same principle, microscopes which, with a faint illumination, exhibit no traces of aberration, shew it strongly when a vivid light is thrown upon the object, whether diaphanous or opaque. It is probably on this account that deep object-glasses and lenses with large angles of aperture, perform better than equally perfect shallow ones of the same penetrating power, which admit a larger portion of light, and thus render their defects more sensible.

Now, this is what I should suppose Wollaston alludes to in Par. 2, did he not, in the next, speak *of collecting as much of the admitted light as can be done, by simple means, to a focus in the same plane as the object to be examined*, (of course, a transparent one is here meant.)

The practice of *condensing* light on a transparent object is that which of all others most tends to aggravate the imperfections and aberration of a microscope; as far as my own experience goes, I have never found it advantageous *with any object whatever.* The light of the atmosphere procured by directing the body of a microscope against the sky, without the aid of mirrors or lenses, is quite sufficient to any object with which I am acquainted, with by far the greater number, more than sufficient; even artificial light, if the construction of the mounting permits it to approach near enough to the object, needs no condensation.

If, moreover, a microscope has *no aberration whatever about it*, still there is a certain degree of intensity in the light intercepted, which is best adapted for exhibiting objects to advantage, and this is almost constantly *below the standard of bright daylight*, which requires qualification by reflection from mirrors, or by other means, to produce the maximum of distinctness of vision. I have perpetually preached against the practice of condensing light on the plane of the object; for it is observable, that all microscopes possessing concave mirrors, or condensing lenses, under the stage, are capable of producing the effect recommended by Dr. Wollaston, much in the same manner as his own illuminating apparatus, used *in his own way*, as directed in App., par. 3, does; for it is of little consequence to what space the illumination is confined, whether by the image of a perforation, or by a stop placed close behind the object; because no microscope can collect or bring rays to the eye, which are without the pale of its field of view, the field-bar, or aperture of the lens (if a simple microscope) excluding all the extraneous light.

Par. 4. " With respect to the apparatus for magnifying, notwithstanding the great improvements lately made in the construction of microscopes, by the introduction of achromatic object-glasses, and the manifest superiority they possess over any single microscope, in *the greater extent of field* they present to view at once, whereby they *are admirably adapted to make an entertaining exhibition of known objects. Hardly any one* of the compound microscopes which I have yet seen is capable of exhibiting minute bodies with that extreme distinctness which is to be obtained by more simple means, and which is absolutely necessary for *an original examination of unknown objects.*"

I have often thought it a great pity that the new microscopes were not introduced into use by some of those *magnificos* of science who are, *ex officio,* the lights of the world, and whose geese are all swans, as, no doubt, they would have taken care that due justice should be done unto them. It is, however, refreshing to hear such an unquestionable authority as Wollaston assert, that compound microscopes (or engyscopes as I call them) are, by the introduction of achromatic object-glasses, at length capable of making *an entertaining exhibition of known objects*—(sufficiently perfect, I suppose, to amuse women and children).

Surely, if a microscope can be proved to possess the power of shewing all the difficult *known test objects*, both opaque and transparent, it may be safely trusted in an original examination of *unknown ones.* Where is the object which the best achromatics, and the Amician reflector (an instrument which the Doctor does not notice, but which is, undoubtedly, capable of greater perfection than the achromatics) do not shew in the most satisfactory manner?

How are we to judge of the fitness of any optical instrument whatever, to examine *unknown* objects, save by its performance on *known* ones? Many a good microscope has been made, though its maker never, perhaps, saw any object with it but a bit of dial plate, or, at best, an artificial star; yet, by these objects alone, he would be able perfectly to ascertain its performance on anything else: the distinction of known and unknown objects appears to me puerile.

When some objects, or some class of objects, are discovered (as may one day by possibility happen) which simple microscopes can shew, but which the engyscopes cannot, *then, and not till then*, will it be just to assert, that they are unworthy of confidence upon unknown objects.

I really would advise observers (for the present at least) to be very cautious how they see objects with the former instruments, which they cannot see with the latter. *A great deal too much* has been seen with simple microscopes from the days of Leeuenhoek downwards: if *by their fruits alone* we are to know them, they seem less to be depended on than the old compounds; for, with the help of weak eyes and a strong imagination, there is no end to what can be shewn by them. Engyscopes, unfortunately, do not strain the eyes sufficiently to enable us to penetrate very far into the invisible world; besides, people always entertain a little wholesome distrust of them, so that we cannot hatch wonders with them with half the effect produced by employing a simple lens.

I have observed, that all hypothetical theories, and preconceived notions, are somehow always best confirmed by deep single lenses; and a degree of scandal and ridicule has thus been brought upon microscopists and microscopic science, which really makes me ashamed of my trade. If I mistake not, the witty author of the "Miseries of Human Life" has set down in his catalogue of human sufferings, that of reading an impudent, lying, pompous account of some pretended microscopic discovery, made under a tremendous high power.

I have, in my paper on the "Comparative Merits of the new Microscopes," formerly published in this Journal, and also in a Note to Mr. Pritchard's paper on Diamond Lenses, expressed my own views of the relative powers of simple microscopes and engyscopes (every man is, of course, welcome to his own on these subjects). I shall here propose some fresh considerations of the same nature. I have heard it asserted, that, under certain circumstances, telescopes shew us objects better than we could see them with our naked eyes under the same visual angle. If they do, I do not see why engyscopes should not shew us objects *as well* as simple microscopes. *I think* Sir W. Herschel somewhere says, that one of the effects of the enormous light and penetrating power of his reflectors, was to shew him what o'clock it was at a distant church, in a night so dark that he could not have told the hour with his unassisted eyesight, by going up to the building—a circumstance very conceivable, if the intrinsic brightness of the visual pencil of the telescope exceeded that of the cylinder of rays taken in by the iris proceeding from the clock, at the distance at which it could be conveniently viewed.

I have often amused myself by placing some object, on a clear day, *so near me, that there should be no sensible quantity of atmosphere to look through* (about 30 or 40 feet distant), and viewing it with a fine telescope, charged with an *eye-glass of moderate depth*, and afterwards going up and looking at it with a pair of spectacles, under the same angle at which I viewed its *image* in the telescope, and have been astonished at the perfect resemblance of the copy to the original, and this I conceive to be analogous to comparing the action of a simple microscope, against an engyscope of the same power. I think any unprejudiced person making the same experiment would be compelled to admit, that he saw *as well* with the telescope as with the spectacles, *provided always, that the image of the telescope was but moderately amplified.* The degree of amplification, which the images of *small* perfect telescopes will bear, is often very great. I have seen a Gregorian of 4 inches focus, and 2 inches aperture, and another of 5 inches focus, and 3 inches aperture, by Cuthbert, both of which bore a power of 120 with absolute distinctness *, though, of course,

* The image given of a star of the first magnitude was quite round and very small, with a few faint rings about it—(the result no doubt of the immense angle of aperture: for Mr. C. says, that if he had made the telescopes of the same aperture and a longer focus, he should have been utterly unable to preserve the small round artificial disc)—I may remark, that the power of 120 was that to which the metals were worked *perfect*, for a Gregorian can only be perfect with one power.

some more light would have been exceedingly acceptable. Now in the Gregorian construction, the primary image is magnified by means of a reflecting engyscope, so that, in fact, we see only the image of an image. The power of the said engyscope was of course equal to that of a lens of $\frac{1}{30}$ of an inch focus, in the 4 inch telescope, and $\frac{1}{20}$ in the other, for such would have been the focus of the magnifiers necessary to give the said power of 120, had the telescopes been of the Newtonian form. Now, if we would get on at this rate with the larger telescopes, there would be no end to their magnifying power, but unfortunately *the larger they get, the worse they get.* It has not yet fallen to *my* lot to see a Newtonian of 7 feet focus bear a lens of less than $\frac{1}{6}$ of an inch focus (giving a power of 504), with the same distinctness with which the small Gregorians bore a $\frac{1}{30}$ and $\frac{1}{20}$. It is, however, truly wonderful that human art should indirectly be able to form a picture of such extreme beauty and truth, as to bear the dilatation of a lens of $\frac{1}{6}$ of an inch, or 48 times its natural magnitude, without betraying coarseness and imperfection; other pictures and works of art, produced by *the direct operation of the human fingers,* seldom bear to be looked at with a lens of less than 1 inch focus: of this nature are some highly finished Dutch pictures, engravings on seals, &c. I have, however, seen the Lord's prayer in a circle $\frac{1}{8}$ of an inch diameter, which bore a lens of $\frac{1}{2}$ an inch focus pretty well.

Now, I heartily wish I could say that the images formed by the metals of the Amician, or the object-glasses of the aplanatic engyscopes, would bear as much amplification as those of small telescopes, but I am afraid, that (supposing their bodies to be from 6 to 9 inches long) *they will not carry a magnifier deeper than $\frac{1}{4}$ of an inch, without becoming inferior to simple microscopes in the strength and correctness of their vision;* the greater the power of their objective part, the less occasion is there for a deep eye-glass to assist it—and the farther we are enabled to advance in the range of power, *without losing the more delicate touches* of the picture; still, however, I am disposed to think that those yet made, are at their maximum of effectiveness, with a power about equal to that of $\frac{1}{30}$ of an inch (240), and if made to exceed it on such object as the scales of the Podura, for example, decidedly suffer in their performance. I have, therefore, always expressed my conviction, that simple microscopes with large apertures and good figures, of the $\frac{1}{40}$, $\frac{1}{50}$, $\frac{1}{60}$, and $\frac{1}{80}$, &c., of an inch focus, beat engyscopes of equal power *. Such being the result of my experience, I must confess, that when I hear opticians speaking with triumph and exultation of having made an engyscope magnify some 7000 times, I lose all patience—what good or useful purpose such preposterous dilatation and dilution of a mere picture can serve, I am wholly at a loss to guess, unless that of exciting the wonder and admiration of the stupid vulgar, at the expense of provoking the ridicule and derision of those who understand the quackery of such practices, be esteemed such.

The old chromatic telescopes were capable of shewing a very great deal; and I consider the old compound microscopes, if constructed in the best manner, to be fully adequate to the task of making *an entertaining exhibition of that numerous class of objects, which is visible with small angles of aperture;* but the makers of them seem determined to evince as hearty a contempt of the science of optics as possible, and consider a large flat field of view as the only circumstance necessary to be attended to in their construction. It is rare to see any but equi-convex glasses employed in them, not one of which ought by right to be used, either for the object or eye-glasses, but the said equi-convex lenses are the *cheapest,* and of course the best to get a profit upon.

In a paper I formerly published in the old series of this Journal, on False Light (vol. xvii. p. 202), I described some object-glasses for microscopes, which consist of a plano-convex lens of considerable aperture, with its plane side next the

* It is not my wish to represent the engyscopes as any better than they really are, and I shall venture to assert that it would be necessary to have a perfect aplanatic object-glass of $\frac{1}{20}$ of an inch focus, and the same aperture, in order to obtain a power from it equal to that of $\frac{1}{60}$ of an inch, which should surpass the performance of a good equivalent simple microscope (I should not be surprised if the double object-glasses of Chevalier should one day be made of this depth, by combining two or three together). I state it as my own private notion, that if two object-glasses are equal in point of aperture and perfection, that the force and decision with which they will exhibit proof objects, is nearly in the direct ratio of their intrinsic power, that is, one of half an inch focus will perform nearly twice as well as one of an inch focus, &c.

object, and a stop in its focus to determine the aperture, with another plano lens immediately behind the said stop, the plane side of which glass should also be next the radiant *, *the whole being on the principle of the erecting eye-piece of an achromatic telescope*—such object-glasses are neither expensive nor difficult to make, and I think might be adopted by such as cannot get or make aplanatics. Most of *the easier transparent test-objects* are shewn by the old compounds, provided we use an object-glass, which *has by itself the power of shewing them as a simple microscope*—such as a lens of $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{10}$, $\frac{1}{15}$, or $\frac{1}{20}$ of an inch focus, as *I have* frequently remarked in my writings. The only consequence is, that when the power of such lenses has been *quadrupled* by the action of the compound body attached to them, the object is seen *infinitely worse than by the simple glass*—still, however, *it is seen*—and for many individuals this is quite enough †. It is astonishing how coarse an organ the eye is in the majority of mankind, and how easy it is to deceive them in such things as microscopes. I would recommend every man who wishes to see any microscope as it ought to be seen, to *go to the maker of it*—he will be sure to make it put forth its mettle—for it is to be observed, that there is but *one way* of shewing a microscope *as it ought to be seen,* but *fifty others*—nothing is so easy as to make a good instrument appear a bad one, by injudicious or knavish management.

* The forms here recommended are *good enough,* but not the best which might be employed: the fact is, that the apertures which *object* lenses and doublets of the most perfect figures will bear *without exhibiting so much chromatic aberration as would destroy all distinctness,* is so very inconsiderable, that it is not worth while to correct the spherical aberration to any great extent, for it produces no sensible benefit. Before I caused the achromatic object-glasses to be made, I tried, I think, every possible construction to procure achromatism without them, but so strictly do engyscopes follow the law of telescopes, that unless both the objective and ocular parts are achromatic *in themselves,* the whole will not be so—if two Huygenian eye-pieces are employed, one for the object-glass, and the other for the eye-glass, here will be the *double compensation,* and they will make an achromatic but very indistinct erecting eye-piece for a telescope: it cannot, however, be used as a *microscope upon a real object,* because *the focus is between the two objective glasses;* nor can this defect be altered or distinctness attained without destroying the achromatism.

† To shew how completely achromatic glasses are thrown away on some, I shall give the following anecdote:—A certain worthy personage had a very good achromatic object-glass, with achromatic Huygenian eye-pieces: he was not content with the field of view given by these, but got an eye-piece made of four or five glasses on the good old plan, to give a swinging large expanse of field—of course this complicated composition would not be rendered achromatic;—what was to be done next? why the object-glass—which wanted no reduction of its aperture, was cut off, till the faintness of its light rendered the aberration of the eye-piece less sensible—the whole was considered a vast improvement!

Par. 5. "My experience has led me to prefer a lens of a plano-convex form, even when made of glass; but the sapphire lens of this form, recently introduced into use by Mr. Pritchard, has a decided superiority over every single lens hitherto employed."

As experience is but a blind guide in the science of optics, it would be much more satisfactory if Dr. Wollaston would condescend to assign some *reason* for what he asserts, if not to prove his propositions. Mr. Herschel, in his treatise on light, has been so obliging as to favour us with the reason why a plano-convex lens of sapphire, or other substance of equally refractive power, is the best figure for bringing parallel rays to a focus, when its convex side is opposed to them, or, by consequence, for causing diverging rays impinging upon its plane surface to emerge parallel *. Had Dr. Wollaston chosen to prove mathematically the superiority of a sapphire lens over a glass one, the same reasoning would also have served to prove the equal superiority of a diamond one, of proper figure, over the sapphire, in point of lower spherical aberration †, though it is certainly true, that the dispersive power of sapphire is lower than that of diamond. No substances are presented to us by nature crystallized in such a variety of forms as diamonds, by far the greater number of which are unfit for optical purposes, having two axes of polarization: some stones are, however, occasionally met with, totally free from all traces of double refraction; some, again, have a double refraction, but this can be got the better of, as in the case of the sapphire, by causing the axis of the lens to coincide with that of the stone.

Mr. Cornelius Varley has a partly polished plano-convex diamond lens in his possession, which has no double refraction about it. Mr. P. has a perfect double convex, which has been

* Encyclopædia Metropolitana, pl. 19, p. 388. Article, Light.
306. If $\mu = 1.6861$, as is nearly the case with several of the precious stones, and the more refractive glasses, $R'' = o$; and the most advantageous figure for collecting all the light in one plane, is plano-convex, having its convex side turned to the incident rays.

† I take the liberty of extracting from Mr. Coddington's invaluable work a note to page 110, to shew how very rapidly aberration decreases with an increase of refractive power, at least with a lens of the best figure: the refractive index of sapphire is 1.764, that of diamond as high as 2.755.

"The following are values of the complete coefficient of y^2, which we may re-"present by ω for different values of μ. I have also inserted the best ratio of the advertised for exhibition to the whole world, and has been examined, and highly approved of, by many sufficient judges; he has, moreover, a diamond now in his possession which he intends to form into a lens, which, in its flat state, shewed no traces of double refraction or polarization. Before I caused the first diamond lens to be worked, Messrs. Rundell and Bridge were so polite as to allow me to examine several of their Asiatic *laske* or plate diamonds, one of which I saw through as if it had been a piece of common glass.

It is much to be regretted that the difficulty of procuring rough diamonds in this country, together with the vast expense connected with all operations on them, has not yet permitted us to ascertain that particular form or forms of crystallization in the diamond, which denote its fitness for optical purposes; this, however, will no doubt be discovered hereafter. Now, *if* it is admitted that *some* diamonds can be procured free from *flaws, crystallized texture, and polarization, or that the double refraction of others can be subdued*, it seems to me impossible to deny, that of all the substances furnished either by nature or art, adamant is, upon the whole, that best adapted for making small lenses, on account of its freedom

" radii for parallel rays, and the numerical coefficient of the least aberration, ap-
" proximately in some cases, but with sufficient accuracy for most purposes."

μ	ω	$r : s$	$-d_y f : \frac{y^2}{f}$
1,4	$\frac{25}{112} \frac{1}{f3} \left\{ \frac{17}{2} x^2 + \frac{48}{5} a x + \frac{62}{25} a^2 + \frac{343}{50} \right\}$	$1 : 3\frac{2}{3}$	$\frac{12}{11}$
1,5	$\frac{1}{6} \frac{1}{f3} \left\{ 7 x^2 + 10 a x + \frac{13}{4} a^2 + \frac{27}{4} \right\}$	$1 : 6$	$\frac{15}{14}$
1,6	$\frac{25}{192} \frac{1}{f3} \left\{ 6 x^2 + \frac{52}{5} a x + \frac{102}{25} a^2 + \frac{512}{75} \right\}$	$1 : 14$	$\frac{14}{15}$
1,7	$\frac{25}{238} \frac{1}{f3} \left\{ \frac{37}{7} x^2 + \frac{54}{5} a x + \frac{497}{100} a^2 + \frac{4913}{700} \right\}$	$1 : -93$	$\frac{2}{3}$
1,8	$\frac{25}{288} \frac{1}{f3} \left\{ \frac{19}{4} x^2 + \frac{56}{5} a x + \frac{148}{25} a^2 + \frac{729}{100} \right\}$	$1 : -12$	$\frac{5}{14}$
1,9	$\frac{25}{342} \frac{1}{f3} \left\{ \frac{39}{9} x^2 + \frac{58}{5} a x + \frac{693}{100} a^2 + \frac{6859}{900} \right\}$	$1 : -7$	$\frac{1}{6}$
2	$\frac{1}{16} \frac{1}{f3} \left\{ 4 x^2 + 12 a x + 8 a^2 + 8 \right\}$	$1 : -5$	$\frac{1}{16}$

from colour, and uniting an enormous refractive power with a low dispersive one. The garnet (was it not for its deep red colour, even when worked into very small lenses) would also be a material admirably calculated for making lenses, for it has not a trace of double refraction about it, and its refractive power is intermediate between sapphire and diamond, while its dispersion is little superior to that of glass: the first lenses formed of precious stones were made many years ago of this substance by Dr. Brewster, and he considers them superior to sapphires. I have not seen any myself, but, doubtless, they would be so, did not their darkness disqualify them for every practical purpose, save that of making lenses for viewing the sun: two of them formed into an Huygenian or Herschelian eye-piece would, I think, be found to stop enough light to enable the eye to bear the lustre of that body. Dr. Wollaston probably never saw a garnet lens, and I know that he never saw a diamond one, for he utterly disdained to inspect that which was presented to him by Mr. Pritchard for examination. His want of inquisitiveness in this respect was very remarkable, inasmuch as a diamond lens, supposing it to be good for nothing, is still a *curiosity*, for many have disbelieved, and still disbelieve to this day, the possibility of working a diamond into a spherical curve.

I conceive that the superiority of sapphire lenses is established on a solid basis, and needs not the support of any man's authority; they will always (when properly made) be able to stand on their own bottom, and maintain their reputation now and hereafter. As an instance of Dr. W.'s taste in them, I shall relate the following anecdote, which I think too good to be lost. The facts I allude to are fresh, and may be easily verified. Professor Shumacher wished to have a sapphire lens; Dr. Wollaston, being a man who knew dirt from other things, was deputed to choose one for him, which he did, out of a considerable number, presented to him by Mr. Pritchard for that purpose. Having selected one, he marked his initials upon the edge of it with a diamond, in order that this identical lens, and no other, might be sent to Professor Schumacher. It was sent accordingly, and the Professor returned it as a *bad* one; and bad enough it was allowed to be by every other person here who inspected it. I may observe, that Professor Schumacher is not more difficult to please in these matters than other men, and has now several sapphire lenses of *Mr. Pritchard's own recommendation*, which he considers very fine. I shall only say, that Dr. Wollaston acted with admirable consistency in spurning a good diamond lens, and selecting a *bad* sapphire *par excellence*. It is no discredit to Mr. Pritchard, or to any man, to make bad lenses of the precious stones, for they are subject to the defect of flaws, imperfect consolidation, and crystallized texture, which unfits them for optical purposes (like glass, when caused to cool very slowly, and thus to assume a crystallized form). If three parallel plates of sapphire are cut truly in the axis of the stone, the middle one, perhaps, will be good for nothing, and the other two will make excellent lenses: it is impossible to know, *à priori*, what part of the stone is good or bad, except by polishing and trying the laminæ*, or making them into lenses outright, which latter method is generally preferred by Mr. Pritchard. The hardest stones make the best lenses; for these reasons, I am afraid it will be for ever impossible to make lenses of any *considerable size* of any of the precious stones. However, a lens of sapphire, about an eighth of an inch diameter, can be procured free from all defects. In order that *jewelled* microscopes may not lose their reputation, I would counsel all those who choose lenses of any of the precious stones, to *try them as object-glasses*, as it will be utterly impossible that any defect, either with regard to their giving double images, or that muddiness or indistinctness arising from a crystallized texture, can fail to be visible in this way. It has been supposed by some, that though a lens has, properly speaking, only one axis, it may be considered to have a great number of eccentrical or oblique pencils, which must necessarily be *inclined to the axis of single refraction*, and thus inevitably cause double images, however truly the *axis* of the lens may coincide with the axis of single refraction. I have nothing to object to this in point of theory, but in practice it certainly does not occur: for instance, if we cause a sapphire lens to

* The crystallization may generally be seen with a good engyscope of considerable power, by using *oblique candle-light* to illuminate the stone.

form an image, *every part of the lens must necessarily co-operate in forming the image.* If there is any double refraction given by any part of the stone, it must infallibly make itself felt, which is not the case, as a good sapphire lens forms a better image than a glass one. It is scarcely possible to miss seeing a double image, where there really is one, if the lens is only used as a simple microscope to view a bat's hair, or some similar object, having intercepted light strongly condensed upon it. The best way to make a comparison, is to get a lens which *really has a double image*, and just to see what a precious mess it makes of every thing viewed with it as a *mere magnifier; (for, used as an object-glass it can hardly be said to form an image at all.)* The following are the characteristics of a diamond lens, having a triple refraction, used as a simple microscope. It has no determinate focus at all, not one of the lined objects can be seen with it; a bat's hair, seen with direct light, shews three images, overlapping each other; nor can the *outline* of any object whatever be discriminated with it.

Another circumstance has been supposed to indicate double refraction in the sapphire, namely, *overlapping images.* Now, all microscopes and engyscopes, of what nature or kind soever, give overlapping images, either when *out of adjustment*, or when *oblique light is thrown through them, however rigid their adjustment may be*, as is well known to every optician. This is rendered peculiarly sensible with the class of *lined objects* seen by artificial light, a complete penumbra, or double image, of which may be seen to cross the true picture, when the object is put out of focus; moreover, in all microscopes, &c., the lines on the scales of butterflies' wings, &c., may be seen double, when the object is put a very little *within the focus.* I mention these circumstances, that inexperienced persons may not be led astray by them into a belief of things that are not.

Par. 6. "The cost, however, of such a lens, in comparison with glass, as well as the readiness with which any number and variety of the latter kind can be procured, led me to consider what simple combinations of them might, perhaps, equal the sapphire lens in performance, without great cost, or difficulty of construction; and though both Mr. Herschel and Professor Airy have recently applied their superior talents to the analytical investigation of this subject, it seemed not impossible that the more *humble* efforts of a mere experimentalist might be rewarded by some useful results."

It is all vastly proper for a mere tradesman to determine, first of all, the cost of his microscope, and next, the size of the box which is to hold it, having these two points given to find the third, namely, the optical and mechanical construction of his instrument: I have constantly found the spirit of trade not only indifferent, but utterly hostile to all sorts of innovations and improvements; for why should a sh-Optician make them, when they only create trouble without increasing his profits? A gentleman and a man of science should disclaim all considerations connected with expense, unless he really means to get up a cheap shop article. I can safely say myself, that I never could have caused any real improvement to have been made in microscopes, unless I had been wholly regardless of expense*.

A perfect eye is, by a good education and much practice, rendered capable of making very nice distinctions in the performance of optical instruments. First-rate opticians will, by merely looking at a brick-wall *which they have been accustomed to view*, in about a quarter of a minute, be able to look into all the imperfections or merits of a telescope; yet even they, probably, could not say with *certainty*, that a lens whose curvatures are as 2 to 5, had the same aberration as a plano-convex, or that one whose radii were as 1 to 6, had less (judging only from their *performance* as *single* object-glasses or magnifiers). The evidence, therefore, of mathematical demonstration is vastly superior to the criterion of the eye, and experiment is only to be resorted to, when there is no theory to guide us; as was the case, for example, with the triple aplanatic object-glasses for engyscopes †. It is surely no small merit in Mr. W. Tulley, to have discovered effective curves for these *by trial, assisted, however, by the theory of those of telescopes.* Since I published an account of them in Vol. xxii., p. 265, of this Journal, he has discovered others considerably better; but it s not probable that he ever will discover, *by experiment, the best*

* The *two first effective* achromatics made cost me 80*l*.

† I was not aware that there was any regular theory on the subject when these were first made; and, at the time this paper was sent to the editor, supposed that the theory of Euler was for double object-glasses like Chevalier's.

possible system of curves for them: for it is the peculiar glory and privilege of science to do this, and it is to be hoped that their highnesses, the mathematicians, will one day cast a spell for that purpose. It is curious to see how they have, with the exception of Euler, *blinked* the subject hitherto. As to microscopic doublets, there are many theories on the subject, and I suspect that a man who sets up a candle in the blaze of the meridian sun does as wise and as useful an action as he who experimentally treats a subject which has been analytically examined by Messrs. Herschel and Airy. We shall see, by and by, whether Dr. Wollaston's labours have been rewarded by any useful result or not.

Par. 7. "The consideration of that form of eye-piece for astronomical telescopes, called Huygenian, suggested the probability that a similar combination should have a similar advantage, of correcting both chromatic and spherical aberration, *if employed in an opposite direction as a microscope!*"

It is very possible that Dr. Wollaston may here have risen a full octave above my dwarfish comprehension, for my education has been sadly neglected in the exact sciences. The paragraph quoted, contains the theory of his microscopic doublet, or what he at least considers such. What is the Huygenian eye-piece but a *microscope for viewing an image?* Its property of correcting chromatic aberration depends upon its field-glass being negative, and detracting from the magnifying power of the other, as a concave lens might do.

The forms of its component lenses, considered apart, are such as give a handsome field of view, rather than correct spherical aberration, though they certainly give its emergent pencils parallel, *when operating with an object-glass**. As to *reversing or inverting it, its optical constitution is utterly subverted and destroyed by this operation;* in fact, it never can be employed to magnify anything but an *image, its focus being between the glasses.*

* I observe that all those individuals who have given the construction of erecting eye-pieces of four glasses to effect *the greatest possible reduction of the spherical aberration*, always select forms similar to those of the Huygenian eye-piece, as the *best to correct, along with the other two* (which perform the part of an object-glass, and cause the rays to impinge on the ocular ones, much in the same state as if they had proceeded from the objective of a telescope;) it is probable, therefore, that an Huygenian eye-piece, *acting as a part of a telescope or engyscope*, occasions no spherical aberration.

Par. 9. "As far as my trials have hitherto gone, I am led to consider the proportion of three to one as nearly the *best* for the relation of the foci of these lenses, and their joint performance to be the most perfect, when the distance between their plane surfaces is *about* 1 4-10ths of the shorter focus. But as all the lenses I possess are not similar segments of spheres, or of the same relative thickness, I could not expect exact uniformity in the results." Again, par 12. "The compound magnifier, too, consists, as before mentioned, of two plano-convex lenses; the proportion of the foci of these lenses being about as 3 to 1. They are fixed in their cells, having their plane sides next to the object to be viewed, their *plane surfaces* being distant from each other *about* 1 4-10ths, or $1\frac{1}{2}$, of the length of the shorter focus. This distance should be varied, by trial, until the utmost possible degree of distinctness has been attained, not only in the centre, but *throughout the whole field of view.*"

Microscopic doublets are of considerable antiquity; there are many better, as well as worse, than Dr. Wollaston's.

Mr. B. Martin, in chapters 2 and 3, article 96 to 125, has given a complete theory of distinct vision, by that combination of glasses selected by Dr. Wollaston. His computations are for the *object-glasses* of telescopes, *but these being reversed, or used in the opposite direction*, of course make microscopic doublets; because, diverging rays issuing from the focus of the glasses will travel backwards by the same route in which the parallel rays arrived at it; the result of Mr. Martin's analysis is as follows: if two plano-convex lenses have their convex surfaces opposed to parallel rays, and their foci equal 3, and 2, respectively, the lens of the longer focus being placed first, with an interval between them equal to 1, so that their foci *shall coincide on the same point*, their spherical aberration will be reduced to one quarter of that of an equi-convex lens of the same focus and aperture. Moreover, if the lens 3 is supposed to be constant, and the lens 2 variable, it matters not what its focus is, (*if it does not exceed that of the lens 3, provided always that it is so posited, that its focus shall coincide with that of the other, which is its best position.*) *The magnifying power and distinctness of the combination always remaining the same*, an infinity of different proportions may be selected, all equally good; it follows, therefore, that two plano-convexes of the *same focus* may be placed *in contact*, and that their aberration will also be equal to one quarter of that of the equi-convex lens. In Mr. Coddington's treatise on the reflection and refraction of light (which contains the substance of Professor Airy's papers on the achromatism, and on the spherical aberration of eye-pieces), article 117, page 156, is to be found this combination, which is the best for a microscopic doublet on Martin's principle, because the lenses being in contact, *the full power of each tells*, only as the thickness of microscopic doublets must be very considerable, on account of the enormous angle of aperture they require, it will be necessary to make the focus of the second lens shorter than that of the first in the ratio of its thickness, otherwise the rays cannot draw to the same point.

It is evident, therefore, that the distance selected by Dr. Wollaston for the distance between his lenses, namely $1\frac{1}{10}$ or $1\frac{1}{2}$ of the length of the shorter focus is wrong, and does not give the minimum of aberration, it should have been 2, more especially if the distance is measured from the plane surfaces. I here give a table of the foci and distances of plano-convex lenses for doublets, in which the unit may be supposed to represent the one-hundredth part of an inch, the constant lens, therefore, is one-tenth of an inch focus.

Con.	10,	10,	10,	10,	10,	10,	10,	10,	10,	10
Var.	10,	9,	8,	7,	6,	5,	4,	3,	2,	1
Dis.	0,	1,	2,	3,	4,	5,	6,	7,	8,	9

On trying any of these doublets by the well known rule, "*multiply the foci together, and divide the product by their sum minus their distance, the quotient will be the focus of a lens of the same power with the combinations*," which are all equal to 5 or one $\frac{1}{20}$ of an inch; their spherical aberration is also equal to $\frac{5}{12}$ of the thickness of an equi-convex lens of the same power and aperture. The only mistake which can well be made in this sort of doublet is, to place the lenses at *wrong distances*; for any two plano-convex lenses may be combined together in their *best position*, (if I understand Martin rightly,) and their aberration will always be equal to one quarter of that of an equi-convex lens of the same focus. As I look at every thing in a practical point of view, I consider all those combinations of glasses, whatever their merit may be, *which are not in contact*, as unfit for microscopic purposes, for it must be evident, that the greater the distance between the lenses is, the nearer will the external focus approximate to the surface of the glass next the object, so that before the combination arrives at any considerable power, it is impossible to view opaque objects with it, and a little further on in the scale, it becomes impracticable to use it on transparent ones also: thus Dr. Wollaston's combination, before it reaches at the power of one-twentieth of an inch, is unable to view the objects in a common microscopic slider, having its inferior side where there are no rings, presented to it: in fact, *it can only look through the thickness of a piece of talc;* and I have before observed, that it is necessary for simple microscopes greatly to exceed the power of one-twentieth of an inch, to enable them really to beat the new engyscopes.

Mr. Herschel, in a memorable paper in the Transactions of the Royal Society for 1821, has been so kind as to favour us with *the best possible way* of making microscopic doublets both with plano-convex and other lenses. He selects two plano-convex lenses, having their foci in the ratio of 1 to 2.3, a proportion not very different from Wollaston's, but he does not place them with an interval of 1.3 between them, (by which he would only have reduced the aberration to one quarter of that of an equi-convex,) but *in contact with their convex surfaces towards each other*, the weaker lens being next the eye—having discovered that by so doing he reduces the aberration to less than one quarter of that of a crossed lens of the least possible aberration, or 0.2481, and diminishes the distortion and prismatic dispersion into the bargain! His combination could also be executed *cheaper* than Wollaston's, for there would be no trouble in measuring the distance between the lenses. Not content with this, Mr. Herschel has given us two other combinations of crossed lenses, with menisci, which destroy the spherical aberration in the axis altogether! What can we have more? Is it not *too bad* to go on experimenting, as if this colossus of science had done nothing? I cannot conceive it possible that *any* person taking up the subject after Mr. Herschel, can produce better combinations, the subject is therefore exhausted. It would surely be the most insufferable presumption in any experimentalist to expect to arrive at such curves. If it is admitted that he might, then may analysis be considered mere useless subtlety and refinement, like the antique practice of shoeing horses by witchcraft. Some years ago, I caused Mr. Herschel's combination, Fig. 5, to be executed to a focus of one-*sixth* of an inch, which I employed as the eye-piece of a seven foot Newtonian telescope; but, as the power it gave was considerable, and the adjustments of my instrument were not good, I could not keep the object truly in the axis of the lenses where the main distinctness lies; therefore, I did not feel the peculiar efficacy of the combination, so I made a *microscope* of it, and in this way it shewed very great penetrating power, for I saw with it many lined objects which I never saw *with so low a power before:* I tried it against a triple aplanatic of one-*fifth* of an inch focus, and hoped that the latter, having its spherical aberration completely destroyed, and its chromatic corrected also, would be able to shew the objects exhibited by Mr. Herschel's doublet, but found, very much against my will, that it would not. Now, I conceive, and such is the result of my experience, that there is a fair way of proving microscopes *by trying which can shew a given object with the lowest power,* for very great distinctness will, *to a certain extent,* compensate for magnifying power.

Thus, suppose some telescope gives a remarkably small round image of a star, free from all irradiation, might it not be proved mathematically, that it must separate a double one with a lower power than another telescope which gives a large irregular image confused by aberrant light, whereby the images of the two stars are so blended together, so that it must require a high power to force them apart, if indeed it can do more than shew the star elongated? Now, what close double stars are in a telescope, the lined objects are in a microscope, for they can be only seen with the *low powers,* when their image is uncommonly sharp, fine, and distinct, and the space between them clear and transparent. I shall here give a slight sketch of the *best* sorts of simple microscopes: first, come the aplanatics, in which both the chromatic and spherical aberration is destroyed by concaves of flint-glass *. These, however,

* As the eye-piece of a telescope is nothing but a microscope for examining its

proceed not far in the scale of power: the deepest which has yet been made, is one by Mr. W. Tulley of $\frac{1}{10}$ of an inch focus, and another by Messrs. Chevalier, composed of two double ones, each of two French lines focus. It is true, that these being intended for the object-glasses of engyscopes, are not right, in point of figure, as mere magnifiers, in which way they shew a want of aberration in the convex lenses, when tried upon an artificial star put out of focus—a certain reduction of the aperture, however, makes them right. In default of these as well as for higher powers, come the doublets of Mr. Herschel, which, I think, might be executed as far as the one-twentieth of an inch focus. I am afraid it would be very difficult to work a concave surface in sapphire, but a doublet of that substance, of proper curves, would be an improvement on those made of glass, on account of the low dispersive power of the sapphire: then come simple plano-convexes (or, if possible, menisci) of sapphire and diamond, of which latter substance a plano-convex of the one-eightieth of an inch might be executed.

As no plano-convex lens of the one-eightieth of an inch focus could be formed from any other substance, such a lens would, in my idea, constitute the ultimatum of the perfection of the single microscope. A plano-lens of glass could not be well made deeper than one-fortieth of an inch, and its aberration would greatly surpass that of diamond; if powers higher than one-eightieth of an inch are wanted; the forms even of diamond lenses must necessarily be equi-convex, from the *impossibility of executing* others.

I now proceed to the consideration of the illuminative part of Dr. Wollaston's instrument.

From the commencement of the period in which I first observed the property of the *lined objects* as *tests* *, that is to say, of *being only visible by microscopes, having large angles of aperture, free to a certain extent from aberration*, I remarked also, that the more difficult ones are only visible

image, it will be found out one day, that these lenses, properly figured, will make the best sort of *positive* eye-piece for the telescopes of astronomical instruments, in which it is no less important to correct the *chromatic* than the spherical aberration, for no other kind of *positive* combination can be rendered achromatic.

* Vide vol. xxi. p. 34, and xxii. p. 265, of the old series of this Journal; as *mere microscopic objects*, they were first noticed by Leeuenhoek.

with an oblique illumination in a certain state of intensity; even the easiest of the class are very ill brought out with direct light, which, if condensed upon them, seems to drown and confuse the more minute markings altogether. I am here speaking of them as transparent objects, but the easier ones are also visible as opaque bodies, provided they are illuminated on the same principle. Now Dr. Wollaston has selected precisely that kind of illumination for them which is least favourable to delicate perspicuity, by condensing the direct light of a diaphragm of three-tenths of an inch aperture, into the space of one-twentieth of an inch, which of course renders it far more intense than day-light. I can only say, for my own part, that I have not the grace to see the lines on the *podura, brassica,* and other objects of equal difficulty, even with a one-sixtieth of an inch lens without *oblique* light.

Notwithstanding what I have said concerning Dr. Wollaston's illuminative apparatus, I shall now shew that it *may,* by *proper management,* be made to hide aberration, shew off bad object-glasses to advantage, and to yield a most useful and effective illumination, (*when used with bright day-light,*) affording easy and certain means of developing the tissue of the lined objects, and improving the distinctness of vision in almost all others. All that is necessary for this purpose is merely to cause it to act with *diverging rays** instead of converging ones, and to make the diaphragm *eccentric* † instead of centrical, at least for the *lined* objects, (a mode of using it exactly opposite to that recommended by Dr. Wollaston,) without changing any thing in the *construction.* I shall hereafter suggest a modification of the Doctor's apparatus, which precludes the necessity of employing a *long tube,* and is therefore more *convenient,* though I do not pretend that it performs a whit better than his; my improvement will adapt itself readily to any microscope or engyscope.

Lambeth, Feb. 1, 1830.

* By removing the illuminating lens to a sufficient distance from the object, to allow the rays to cross and diverge before they arrive at the object; this distance, and with it the strength of the light, must be varied to suit the power, object, and state of the atmosphere: the apparatus is, I think, useless with artificial light.

† This gives oblique light in the direction of a line drawn from the centre of the visual pencil, to the centre of the image of the diaphragm in it.

On Proper Names.

[Read before the Literary and Philosophical Society of Liverpool, by Mr. MERRITT.]

THE subject of proper names has engaged the attention of etymologists, antiquarians, and philologists, less perhaps than any other department of the researches connected with language. It has commonly, indeed, been ranked among those "*difficiles nugæ*," which are more curious than useful, and which are better adapted to fill up the occasional vacancies of literary leisure, than to be classed amongst the serious objects of useful study. Under this conception, it has been so much neglected, that except in those curious and ingenious fragments of Camden, commonly called his "Remains," I am not aware of any author possessing the requisite talents who has written on it expressly, or systematically*. I may, however, venture to affirm, that the subject is neither so frivolous nor so useless as it may at first sight appear. An inquiry into the origin, varieties, and classification of proper names is calculated to throw considerable light on the history of the human mind, on the character of nations, and on the process of civilization. In this view, it may claim a superiority over many other pursuits, which serve to exercise the ingenuity of the studious, and to swell the list of liberal exercitations, without making any very important additions to the stock of useful knowledge. At all events, the subject in question has the merit of being one of those *qui omnium pariter intersunt hominum*, and which equally concerns, in however slight a degree, each individual of the human race. Every man has a name, and every man, if his attention should happen to be turned in that direction, must feel some curiosity to know of what that name is significant, and how it originated. But although in the daily intercourse of men, the subject meets our eyes and ears more than almost any other, yet very few have thought it worth their while to make it the object of deliberate reflection, and still fewer of premeditated investigation. The

* Of late years some slight attempts at investigating the formation of British proper names have appeared in some of our literary journals, but all that I have seen have been quite superficial and indigested, loosely compiled from insignificant sources, and produced visibly by persons destitute of most of the requisite qualifications for such an undertaking.

present essay is very far from making any pretensions to supply this striking desideratum, in a manner adapted to satisfy the learned and persevering inquirer*. But it may afford some useful entertainment to those who have not inclination or opportunity to examine the subject for themselves, and may serve to direct the attention and guide the inquiries of such as may think it worth their while to enter more minutely into its details.

I take it for granted that all names, at their original imposition, were, in some way or other, significant. It is not, I think, to be supposed, that in the task of affixing a name to a child or a stranger, any man, or society of men, ever deliberately sat down to contrive new combinations of letters or syllables with this object. It may safely be taken for granted, even if the supposition was not confirmed by actual research, that few persons would choose to take the trouble of such elaborate fabrications; unless, perhaps, in instances like those of Gulliver's Travels, when Swift took the pains of forming such words as Lilliput, Brobdignag, and Houyhnhnms, in accordance, as he supposed, with the extravagant fictions he was about to compose. Whenever a new name was wanted, there is reason to believe some term or combination of terms already in use was universally adopted, though the selection was made from motives which cannot always be traced, and on principles which the lapse of ages has often buried in irretrievable oblivion. The choice was, no doubt, principally influenced by habit and usage, but these usages varied essentially amongst different nations and at different periods.

In the early periods of society, what we call † surnames, or sirnames, would, of course, be unknown. The members of a family or a tribe would not be so numerous, that any individual should require more to distinguish him from others, than a

* A complete investigation of this branch of knowledge would demand not only a very exact acquaintance with all the Celtic and Teutonic dialects which now remain, but a careful research into ancient charters, family records, and original registers. The most complete success would hardly compensate such various and complicated labours.

† These two appellations differ from each other (though often confounded) as genus and species. A *surname* (*sur*) is an additional appellation of *any* kind added to the name. *Sirname* (*quasi sirename*) is an additional appellation derived from the father.

single appellation. These *mononymes* (if the term be allowed) were originally not very numerous, and were commonly of that species called by the Romans "*nomina fausta*," or "*nomina boni ominis*," such as *Joseph*, *Eumenes*, *Felix*. But as it would frequently happen, that different families would fix on the same term, the necessity would, in no very long time, become apparent, of devising some means of distinction between individuals of the same name. The first and most obvious expedient for this purpose would be to superadd the name of his father or his family to the individual whom it was meant to distinguish. This practice appears to have obtained universally amongst the Hebrews, the most ancient people of whom any records are now extant. Thus, "Esau the son of Isaac," "Joshua the son of Nun," "Caleb the son of Jephunna," was uniformly the manner in which individuals were then distinguished. Amongst the Syrians, Chaldeans, and other Oriental nations, the same practice appears to have prevailed from the earliest periods of time.

The names in use amongst the Hebrews indicate strongly their natural theocracy and peculiar characteristics. As Gabriel, "the strength of God;" John, "the grace of God;" Joseph, "he shall increase," and a multitude of others. In process of time, however, it became necessary, as the nation grew more populous, to adopt, like other countries, the use of sirnames; as Simon Peter, Judas Maccabæus, Judas Iscariot. These sirnames, like those in use amongst other nations, were sometimes patronymic; occasionally significant of some real or imputed attribute of mind, body, or condition, and sometimes purely capricious or accidental.

It is not necessary to take further notice of the progress of proper names amongst the early nations of the East. The Hebrew was the great prototype of all the oriental tongues, in all their principal attributes, and this branch of the subject does not demand any more minute investigation.

Of all the nations that have ever existed on the earth, the Greeks must be allowed to have eminently excelled the rest in the euphony, variety, and significancy of their proper names. In that point, as in many others equally frivolous, that vivacious, vain, and ingenious people, betrayed their predominant national characteristics, their haughty assumptions of superiority, and their real pre-eminence, in whatever marked refinement in taste or elevation of sentiment. How inexpressibly must a language be enriched by the contrivance of making every mention of a proper name not only convey a pleasing sensation to the ear, but raise a grateful image in the imagination, or awaken a train of ideas in the understanding! The following are a slight specimen.

Themistocles,* . . .	renowned for justice.
Philippus, ...	a lover of horses.
Alexander, ...	a power auxiliary.
Thrasybulus, ...	daring in counsel.
Theophilus, ...	a lover of God.
Aristobulus, ...	an able counsellor.
Sophronia, ...	the prudent.
Alithea, ...	the true.
Euphemia, ...	of fair fame.
Philadelphia, ...	brotherly love.
Euphrasia, ...	of fair speech.
Theodora, ...	the gift of God.

In adopting sirnames, the Greeks chiefly used patronymics, and in this, as in all similar cases, the great object was, to render this process an instrument of making their language more euphonous, picturesque, and poetical. They frequently converted the patronymic into an epithet as Πελειαδεος Αχιλλευς; Achilles the son of Peleus; Ατρειδης Αγαμεμνων, Agamemnon the son of Atreus. Sometimes to render the phrase more poetical, they introduced also the term of relationship as Νεστορ υιος Νελειος; Nestor the Nelein son. In process of time, however, they were compelled, like other nations, to adopt the usual mode of affixing surnames. Sometimes the place of birth or of abode supplied the cognomen, or Θοας Αιτολος, Thoas the Ætolian, and sometimes a quality, profession or art, as Μεριονες Φερεκλον, the artificer Phereklon, and innumerable others.

The Roman proper names are far inferior to the Greek in variety and beauty, and especially in significance. Several of their names, indeed, convey no meaning which can be dis-

* In the citation of these names, I have used the Roman orthography, as being more familiar to the general reader.

tinctly traced, but this may probably arise from their having belonged to families of the aboriginal or surrounding inhabitants, where a different language was spoken. In the course of time they increased the number of appellatives which composed each name; many of them comprising three or four separate words. The usual distinction of these separate terms was into *prænomen*, *nomen*, and *agnomen* or *cognomen*, for these two words seem to be nearly synonymous. Thus Marcus, Tullius Cicero; Publius Æmilius Paulus; here Publius is the prænomen, Æmilius the nomen, and Paulus the cognomen. The last of these was commonly indicative of some attribute, peculiarity, or achievement, as Paulus, very little; Scipio, a staff; Africanus, the conqueror of Africa; Cato, inferior. The few following Roman cognomina, agnomina, or surnames, taken promiscuously from the Roman history, will shew how inferior they are to those of the Greek in melody, magnificence, and especially in what may be termed poetical or picturesque significance.

Cicero	a vetch or lentil.
Naso	a nose.
Scipio	a staff.
Flaccus	flap-eared.
Paulus	small.
Brutus	brutish or stupid.
Crassus	fat or gross.
Priscus	old, ancient.
Claudius	from claudus, lame.
Faliscus	a black pudding.
Macerinus	lean.
Cossus	a worm.
Cassius	from cassus, vain.
Figulus	a potter.

Most of these names, it is apparent, must have been conferred in caprice, derision, or from some frivolous circumstance or casualty not calculated to confer any dignity on the person named, or to raise any very pleasing or impressive association on the mind of the hearer. The case is very different with those magnificent and grandiloquent names conferred in consequence of some great conquest or illustrious transaction, as Africanus, Britannicus, Poplicola, Capitolinus. When only one name was mentioned, it was understood to have been for the sake of brevity, as Cicero remarks; *nomen cum dicemus, cognomen quoque et agnomen intelligantur oportet.* Many of their surnames were evidently taken from the Greek, as Cato, Priscus, Coccus, &c.

I shall not say much respecting the proper names of the nations of modern Europe, because what I have to remark concerning English names will comprise nearly all that is essential to the subject. It is observable, that the French, even more than the English, confine their sirnames principally to the names of places: in general, the individual's place of birth, residence, property, or patrimony. They have very few patronymics, (in the sense in which that word has been used in this essay,) but a considerable number of such as may be called capricious, attributive, or, in English, nick-names. The great mass, however, as before observed, are derived from names of places, as

Jean du Vivier . .	John of the fish-pond.
Guillaume de Rouen .	William of Rouen.
Matilde de le Bastide .	Matilda of the cottage.
Pierre de la Metairie .	Peter of the farm.
Louise du Mont . .	Louisa of the mountain.
Lisette du Tertre . .	Lisette of the little hill.

The capricious or attributive names are much less frequent, as

Jean Jacques Rousseau	John James the red-haired.
Robert de l'oiseau . .	Robert of the bird.
Henri le gros . . .	Henry the fat.
Baptiste le petit . .	Baptiste the little.
Marie la bonne . . .	Mary the good.
Jeannette le sage . .	Jane the wise.

We come now to British sirnames, and this branch of the subject, it must be confessed, is abundantly copious, from the variety of *stirpes*, or national mixtures, and foreign insertions, from which our actual population is derived. I shall, however, attempt no more than a very concise sketch, merely in the hope of imparting to such as have never paid particular attention to the subject, some information on a matter concerning which it does not become any one to remain in absolute ignorance *.

The ancient inhabitants of this country are generally admitted to have been a branch of that very extensive race known by the names of Gael, Gauls, or Celtes; which, before the Christian era, had overspread a great part of the middle and southern districts of Europe. Several dialects of the language of this ancient people still remain; in the north of Scotland, the west of Ireland, in Wales, Cornwall, the north-east of France, and, according to some accounts, certain mountainous parts of Spain and Italy. Few Celtic proper names, however, can now be traced in this country, except in the districts where the language is still extant, a melancholy proof how completely the native tribes were extirpated or expatriated by their barbarous invaders. A great part of those which still remain are monosyllabic, as Caird, a pedlar or tinker; Cairn, a sepultural hill; Benn, a promontory; Glynn, a valley; Linn, a mountain-stream; Gillies, a servant; Braithwaite, a steep inclosure, &c. Many of the Saxon names, however, have a Celtic termination or adjunct.

With respect to the Roman conquest †, as that all-conquer-

* The helps to be derived, in this inquiry, from English literature, are few and scanty. Camden, as before remarked, has treated more expressly on the subject than any other author. The voluminous writings of Sir Edward Coke contain many valuable but detached notices respecting names, a matter which seems to have occupied much of his attention. From the old glossaries and books of heraldry something is to be learned; and, lastly, the posthumous work of that eminent linguist, the late Dr. Murray, of Edinburgh, on the history of the European languages, though, in my opinion, abounding in fanciful and strained theories, is, perhaps, the most important help to the etymological student which has appeared since the "Epea Pteroenta." In fact, Murray is very superior to Horne Tooke as a linguist, though greatly inferior to him as an acute and philosophical philologist. Indeed, it was the great fault of Tooke, that by constantly aiming at being philosophical, he often becomes too refined and subtleized. He had always his thoughts on metaphysics and politics, in hopes of seizing on some passing auxiliary to his prevalent opinions. Murray, though far below him in genius and vigour, had great advantages over Tooke in his intimate acquaintance with the oriental languages, without which no etymological researches can be successfully pursued. Camden is a most fair and candid inquirer, and, on the whole, a very accomplished scholar, but appears to have had a very superficial acquaintance with the Celtic dialects, a deficiency which, as I have found by experience, is a perpetual hinderance to the British etymologist, and especially to the investigator of names. If Dr. Murray had happened to have turned his attention particularly to this branch of his subject, he would have accomplished all that is practicable.

† Camden is decidedly of opinion that sirnames, used as family names, and perpetuated through a course of generations, were unknown amongst the ancients,

ing people had probably never much more than a sort of military possession of the country, it is not very likely that they left behind them many of their family names. I have, indeed, met with a few English sirnames of Roman formation, chiefly in the northern part of the kingdom, such as Faustus, Felix, Cornelius, Carus, Sartorius, mostly Latin prænomina —but these are, in all probability, of late introduction.

The universal spread of the names, language, manners, and institutions of the Saxons, who, in the fifth and subsequent centuries, obtained forcible possession of the country, indicates a more sudden and entire change of inhabitants, than ever before, perhaps, took place in any nation. The extirpation or expatriation of the Britons must have been nearly universal, to account for so radical a mutation. The Danes, who afterwards obtained a partial footing, left but few traces of their names, except in particular districts, chiefly along the eastern coast; and the Normans, who, at a subsequent period, seized the government, diffused their names, manners, and language chiefly amongst the higher classes of society. The country remained substantially Saxon, in all its general features: in addition to this, it may be remarked, that the Saxon, Danish, and Norman languages possessed many points of identity, especially the two former. The language of the Normans, though, like the others, a Teutonic race, had become mixed with the French, and indeed assimilated to it by their long proximity. Our present list of English sirnames, therefore, is principally Saxon or Teutonic, with some British, partly in a simple, and partly in a compounded state—a few French, and a few foreign names, imported by occasional settlers.

The Saxons appear to have derived and conferred their sirnames principally from the five following sources:—

1. From the place of a man's birth, residence, or property, as *Wales, Cheshire, Ireland, French, Leyland, Worth, Lee, Clough*, &c.

except to the Romans, and that only after the league of union with the Sabines. These were afterwards called *nomina gentilitia*, and *cognomina*, as the others were called *prænomina*. This opinion has been controverted, but probably without foundation, as it appears certain that the sirnames imposed by the Hebrews and Greeks, though various in kind and origin, were affixed to individuals, and not to families, and were altered with each successive race.

2. From his trade or occupation, as *Smith, Wright, Webster, Baker, Leach, Kemp, Clerk*, &c.

3. From his father or mother, as *Jackson, Dickson, Robertson, Mollison, Bettison, Megson*, &c.

4. From some quality or attribute, inherent or imputed, as *Hardy, Bold, Wise, Long, Short, Grey, Green, Cruickshanks, Longshanks, Littlejohn*, &c.

5. From natural objects or productions, chiefly animals or vegetables, as *Lyon, Lamb, Hawk, Salmon, Rose, Hawthorn, Lilly*, &c.

These five classes comprise about ninety-five per cent. of all English sirnames, and a few words only will be required on each of them, in the way of explanation. The small remainder are of different species, mostly monosyllabic, and are often very difficult to trace. When successfully investigated, they are often found to belong to one or other of the five classes above enumerated.

The first division, or the sirnames derived from towns, districts, or habitations, is by far the most numerous, as it comprehends nearly one-half of the whole. This large class may be best subdivided by its termination, of which the most frequent are the following:

1. *Ton*, a town, as *Flixton* (or *Felixtown*) the happy or pleasant town; *Newton*, the new town; *Preston*, the sheriff town; *Norton*, the north town; *Sutton*, the south town, *Langton*, the long town, &c.

2. Wood, as *Firwood*, the wood of fir; *Norwood*, the north wood; *Haslewood*, the wood of hazles; *Elmwood*, the wood of elms.

3. Ley, lea, or lee; a pasture, as *Broadley*, the broad field, *Langley*, the long field, or pasture; *Ousley*, the field of the Ouse; *Hanley*, the field of the haven; *Netherley*, the lower field; *Beverley*, the beaver field.

4. Hill, as *Churchill*, the hill of the church; *Bethell* (a word compounded of a Celtic and a Saxon monosyllable), the bed of the hill; *Wardell*, the hill of the ward; *Farnell*, the hill of the fern.

5. Ing, a swampy bottom, and sometimes an appropriated part of a common field, as *Withering*, the hill of withers or

willows; *Wilding,* the uncultivated ing; *Deeping,* the deep ing.

6. Thorp, a village, as *Hanthorp,* the village of the haven; *Hillthorp,* the village of the hill; *Winthorp,* the village of furze.

7. Shaw, a small wood, as *Cockshaw,* the wood of the hill; *Fernshaw,* the shaw of fern; *Kilshaw,* or *Kilnshaw,* the wood of the kiln.

8. Cock, an abrupt hillock or knoll, as *Pitcock,* the hillock of the pit; *Hancock,* the knoll of the harbour; *Wilcock,* the hill of William.

9. Ham, a hamlet, as *Durham, Middleham, Farnham:* of which the compounds are too obvious to need explanation.

10. Holm, a rising ground, mostly planted; as *Waxholm,* the hill of wax or bees: *Burnholm,* the hill of the rivulet: *Dunholm,* the hill of the fortress.

11. Coates, a fold, a hut, as *Hancoates, Westcoats, Sutcoats.*

12. Dover, a ferry, as *Andover, Wendover, Hardover.*

13. Wold, a stony ridge, as *Coxwold, Easingwold, Westwold.*

14. Stead, a home or residence, as *Winstead, Bowstead, Brickstead.*

15. Wick, or Wich, a town at the mouth of a river: or sometimes merely a town, as *Nantwich,* the town of the valley; *Hardwich,* the stony town; *Sandwich,* the town on the sand.

16. Den, Dale, Don, or Dell*, a small and deep valley, as *Warden, Huntingdon, Handell, Farden, Dovedale.*

17. By, the Danish term for a habitation, or place of abode, as *Appleby, Gatesby, Hillsby, Burnby.* These chiefly prevail in the eastern parts of the kingdom.

18. Stow, a place or seat, as *Bradstow,* the broad place; *Chepstow,* the cheap place; *Winstow, Barnstow.*

19. Comb, a valley, as *Witcombe, Hanscomb, Bradscomb.*

20. Garth, a small inclosure, as *Haygarth, Wingarth, Hogarth.*

* Several of these compounded monosyllables, it will be observed, are Celtic; a few Danish, &c. It is probable, however, that most of them have been adopted and appropriated by the Saxons.

21. Bert, bright or famous, as *Gilbert, Albert, Hilbert, Herbert.*

22. Hurst, a meadow or pasture, as *Deerhurst, Barnhurst, Nairhurst.*

23. Graves, a little ward, as *Hargraves*, the high ward: *Cotgraves*, the ward of the cottage.

24. Druf, a thicket, as *Wardruff, Woodruff, Lendruff.*

25. Steth, the bank of a river, as *Bickersteth, Toxteth, Barnsteth.*

26. Thwaite *, a pasture, as *Postlethwaite, Burnthwaite, Branthwaite.*

These are some of the principal terminations under this class, but many others might be enumerated. A great number are too well known to need explanation, as Field, Land, Hill, Dale, Wood, Water, Croft, Lin; whence, *Fairfield, Buckland, Barnhill, Littledale, Blackwood, Drinkwater, Bearcroft, Blacklin.*

The names of many of our nobility and higher gentry are purely Norman French, but they are mostly of this class. They are generally compounded in the same manner, but their combinations are not so obvious, though most of them have been traced by the French antiquaries and genealogists.

The SECOND of the five great sources of English sirnames, are Trades, Occupations, Professions, or Employments: such as *Smith, Wright, Baker, Tailor.* Almost every village, it may be observed, however small or insignificant, contains a Smith, or worker in iron; a Wright, or worker in wood; and a Tailor. Accordingly, the names of *John* the *smith, Thomas* the *wright, William* the *tailor*, are the first that would be appropriated, and they are accordingly amongst the most common of all our sirnames. When the village becomes a little larger, it will contain a *Fisher*, a *Weaver* or *Webster*, a *Miller* or *Milner*, a *Fletcher*, or maker of arrows, a *Kemp* or *Soldier*, a *Mason*, a *Porter* or *Carrier*, a *Baker*, &c.; and these also

* This termination, so common in the north of England, has given rise to much dispute. Camden, following the Saxon etymologists, derives it from the Dutch *Heoit*, a pasture; but Lord Coke, whose knowledge of the Celtic dialects is far more extensive, says it signifies a wood grubbed up and turned to arable; and as this name is chiefly found in woody and mountainous districts, we have no reason to doubt his correctness.

are very usual names, though not so frequent as the others. The arts or trades of later introduction, it may be remarked, are never found amongst sirnames. Thus we never hear of *Printer, Jeweller, Engraver, Milliner, Engineer*. The term *Spinner*, at a time when nearly every family supplied its own garments, would not have furnished a designation sufficiently distinctive, and does not occur, though we find *Spinster*. It appears somewhat unaccountable that we find no sirname expressive of the trade of shoemaker, though that handicraft is found in every village. It may be remarked, however, that the *shoe*, or covering for the foot, was, some centuries ago, of so rude a form, and of such inartificial construction, especially amongst the lower classes, that it was commonly manufactured at home, and did not furnish a distinct occupation. It is observable, moreover, that the name of *Cordener*, visibly from the French *Cordonnier*, is found in the south and east of England; and the name of *Sutor*, from the Latin, occurs in the north; but these are probably of recent introduction. It has also been remarked, as a singularity, that though we have *Clerk* and *Leach*, to designate two of the three learned professions, we have none to express Lawyer. But the word *Clerk* was abundantly employed, especially in the north, to express Lawyer as well as Priest, and this may account for the extreme frequency of that sirname.

The THIRD principal class of English names consists of what may be termed patronymics, or names derived from the parent. The most common of these are taken from the contractions, diminutives, or familiar appellatives of Christian names, with the addition of *son:* as *Jackson, Thomson, Wilson, Dickson, Betson, Mollison, Megson*, and a multitude of others. There is also a large class taken from regular or written Christian names, as *Richardson, Williamson, Robertson, Johnson, Thomason, Allanson.* To many of the Christian names is subjoined the Saxon epithet, *kin* or *kins*, a diminutive expressive of littleness or childhood: thus we have *Wilkins*, young or little *Will; Dickins*, or little *Dick; Jenkins*, or little *John; Hodgkins*, or little *Hodge:* and the appellative was again transmitted to the next generation, as *Wilkinson*, the son of little *Will; Dickinson, Watkinson*, &c. We have also a considerable class of Gaelic and French patronymics, chiefly in Scotland, Ireland, and Wales, as *Fitz*, *O*, *Mac*, and *Ap*: whence *Fitzhenry*, the son of Henry; *O'Donnell*, the son of Donnell; *Mc Iver*, the son of Iver; *Ap rui*, contracted into Prui; *Ap Howell* into Powell. In Wales, since its conquest, and also in some parts of England, the custom has prevailed of simply adopting the father's Christian name for the name of the son, with an *s* annexed, when the name does not end in *s*, but not otherwise. Thus we have *Roberts*, *Jones*, *Hughes*, *Williams*, *Evans*, *Griffiths*, with *s* subfixed, and *Thomas*, *Morris*, &c., without it. Sometimes the contracted or diminutive Christian name is taken, with the addition of *s*, as *Jacks*, *Toms*, *Harris* or *Harry's*, *Davy's* or *Davis*. Sometimes, also, the female diminutive, or contracted appellative is adopted without addition, as *Jenny*, *Molloy* or *Molly*, *Beatty* or *Betty*, *Nanney* or *Nanny*. The Jews, it may be remarked, take the prænomen without the *s*, as *Joseph*, *Samuel*, *Solomon*, *Levi*. It sometimes happens, though not very frequently, that the SIRNAME of the parent, with the subjunction of son, has been adopted for a new name, as *Brownson*, *Grayson*, *Scotson*, *Wrightson*.

The next, or FOURTH, of the five principal divisions, above enumerated, consists of names derived from some supposed quality, attribute, or accident. The three classes already considered, comprise such names as were conferred involuntarily, or according to a fixed and positive rule. The class we are now upon, and the next which follows, are much more difficult to reduce to any arrangement, as they appear to have been bestowed capriciously, or at least arbitrarily: such are the names *Good*, *Wise*, *Strong*, *Hardy*, *Bold*, *Fair*, *Worthy*, and a great many more. Sometimes they seem to have been given from a sentiment of respect, like those above mentioned, and sometimes of derision, as *Cruickshanks*, *Longshanks*, *Whitelegs**, &c. Some of them indicate a peculiarity of habit or character, as *Gotobed*, *Younghusband*, *Wellbeloved*. How far these names were affixed accidentally, or imposed by any authority, paternal, magisterial, or official, I have not been

* Camden thinks the former were generally self-imposed, and the latter given by others.

able to find; nor can I discover any traces of the history or the origin of this species of appellation. Some of them, I know from personal observation, must have been mere nick-names, first given in caprice, and afterwards, from time, ignorance, and the general want of written documents, permanently appropriated. In this way, Long John would become *John Long;* Brown Bess would grow into *Betty Brown;* Wise Thomas would become *Thomas Wise**.

This class of names, in the opinion of some etymologists, is the most ancient of all others, and Dr. Murray is decidedly of opinion that, amongst all early tribes or nations, the more obvious qualities of mind or body would universally supply the first names. This may possibly be correct in the main, but it is also certain that names derived from the parents, or from the place of birth or residence, would follow so immediately as to be nearly connate with the others. Names would frequently be conferred on children before any distinctive quality of mind or body could have time to develope itself.

The FIFTH, and last, of the five general divisions which I have laid down, is that class of names derived from natural objects or productions, chiefly animals and vegetables, as *Lyon, Tiger, Bird, Swallow, Hawk, Salmon, Rose,* &c. Dr. Murray and others suppose that these were conferred generally from some imaginary analogy or similarity between the individual designated and the object which supplied the name. This, in some cases, may very probably have happened; but many names of this class are so insignificant, as applicable to a human being, that it is difficult to imagine any intelligible analogy. There is, it must be confessed, as much difficulty in tracing the origin, motives, and history, of this class of names, as in those of the fourth division. One branch of them, derived from dignified titles, such as *King, Prince, Lord, Duke, Marquis, Bishop, Earl,* &c., were probably given to persons who belonged to the family or household of these high personages,

* Some years ago, a woman named Ann Wilson, in a Yorkshire village, became so renowned for fluency of speech, that she acquired the name of *Nanny Clack*, which was so completely affixed, that her real name was only known to a few of the older inhabitants. An instance occurred, a few years since, at Lancaster assizes, of a man who did not know his own name, and was recognized only by one of those nicknames, or appellations, which people so often acquire in the villages of the north.

as William of the Earl's, would become *William Earle;* John of the Bishop's would be abridged into *John Bishop,* &c.; for it is not likely that the dignitaries themselves would be thus designated, being always distinguished by their several titles. Camden says that many names of this class were taken from the device in the armorial bearings, or the family crest.

These five classes, or divisions, comprise, as I have already 95 observed, about 96 per cent. of the whole catalogue of English sirnames. Of the small remainder, a considerable part are monosyllables, of which I have not been able to find the etymology, partly from the words having become so far obsolete as to escape the efforts of ordinary research, and partly from that change or corruption in orthography, which always prevails in early civilization *. Dr. Johnson well observes, that "as all language was, at its beginning, merely oral, all words of necessary or common use were spoken before they were written, and, while they were unfixed by any visible signs, must have been spoken with great diversity, as we now observe those who cannot read to catch sounds imperfectly, and utter them negligently. When this wild and barbarous jargon was first reduced to an alphabet, every penman endeavoured to express, as he could, the sounds which he was accustomed to pronounce, or to receive, and vitiated, in writing, such words as were already vitiated in speech. The powers of the letters, when they were applied to a new language, must have been vague and unsettled, and, therefore, different hands would exhibit the same sound by different combinations."

As respects proper names, we can trace this varied or corrupted spelling in peculiar abundance, because in such cases there was no general standard, and every thing was determined by individual caprice, error, or stupidity. Thus Crookshanks, was changed into *Cruickshanks;* Peasecod into *Peasegood;* Bloomfield into *Blomfield;* Highergraves into *Hargraves;*

* I must beg leave to repeat, that to pursue this inquiry into its minute ramifications with full success, would require a very nice acquaintance with the various Celtic and Teutonic dialects, and a collection of Glossaries, Lexicons, and Vocabularies, much more extensive than I have had the means of procuring or perusing. The utmost that I can pretend to, is, to exhibit, in the way of synthesis, a succinct view of the general subject under its leading subdivisions; and even this is more than has hitherto been accomplished in any systematic manner, as far as I am acquainted with English literature.

and so in innumerable instances. This often arises from provincial accent or pronunciation, by which local and colloquial errors are perpetuated in writing. For example, the word *Gates*, in many of our northern provinces, is pronounced *Yates*; Pert is pronounced *Pritt*, and hence arise the two sirnames of *Yates* and *Pritt*, which, without a knowledge of this mode of corruption, could never be traced. Instances of this kind are abundant, and add greatly to the difficulty of classifying our catalogue of sirnames.

On analysing a list of 100 names taken promiscuously from a General Directory, the proportion appears as follows :—

Names of countries, towns, or residences, .	48
Attributes, qualities, or nicknames, . .	19
Trades or professions, ...	14
Patronymics, ...	9
Natural objects or productions, . . .	7
Not comprized in any of the above, . .	3
	100

Two or three general corollaries may be deduced from this slight survey, and will conclude the subject.

In the first place, it may be remarked, that a large part of our great families bear the name of some town, village, or district, in * Normandy or France, as *Beaufort*, *Russell* or Rousselle, *Nugent*, *Montague*; a few are from the French designations of trades or *occupations*, as *Grosvenor*, *Molyneux*, *Butler*. Many of them, however, still bear Saxon names, which shews that, even after the conquest, some of the old families retained their dignity, and that some were ennobled, as *Stanley*, *Howard*, *Pelham*, *Parker*, *Wentworth*.

In the second place, we find scarcely any of the patronymics, any of the trades or professions, and, indeed, very few of any of the five general classes, except the first, amongst our great families; a proof that, in the earlier periods of our history, persons of the lower orders were very seldom advanced to the higher classes. Before the general spread of trade, commerce, and manufactures, opportunities of such advancement rarely

* Camden remarks that there is scarcely a village in Normandy that has not given its name to some of our great families; a proof that the spoliation of property at the conquest was very general.

presented themselves. But the case is now very different, not only from the causes just mentioned, but from the more general diffusion of education, and the more liberal elevation of merit wherever it is found. Thus we have had, of late years, the sirnames of *Smith, Johnson, Robinson, Jenkinson,* and many other plebeian names, raised to the rank of nobility.

Thirdly. It is remarkable that no trace can be found of the period when the apposition of sirnames ceased in this country. For some reasons already hinted at, I am induced to believe, that the practice has not existed (except accidentally) for two or three centuries at least. It may be remarked, however, that from the vast increase of population, the extinction of some names, and the creation of none others in their place, certain sirnames have become so common, that, to avoid confusion, a new coinage will, in no long period, be found necessary. It is certainly a perverse incident in the history of modern nations, that during the periods of scanty and stationary population, new names were bestowed in profusion, and at present, when population is rapidly on the increase, and the list of names continually diminishing by the extinction of families, no means are thought of for supplying the deficiency.

Perhaps the easiest remedy would be to authorize every individual of a certain prescribed list of names, such as *Smith, Wright, Robinson, Jones, Johnson, Wilson,* and about one hundred others, to take a new name on their marriage, out of a certain prescribed list prepared for that purpose. Any confusion as respects property, rights, or liabilities, arising from this proceeding, might easily be obviated by a public registry of the change, which could be accessible to all.

These inquiries might have been pursued much further; but I am apprehensive that most persons will imagine I have already taken more pains than the subject deserves, and that the time devoted to it might have been much better employed. To those who think thus, I can only say, that, in my opinion, these detached and insulated portions of science; these small *offsets* (if the expression may be allowed) from the great tree of knowledge, are peculiarly adapted to the purposes of a Literary and Philosophical Society. The broad highways of science and letters are already so much occupied by the mighty masters of every department, that little is left in their beaten tracks to be gleaned by the author of a short and incidental essay. The present subject is one of those which, though constantly before us, is little attended to—very familiar, and yet, in many points, very recondite. The few individuals who feel an interest in it will think, perhaps, that it has been carelessly and cursorily treated, whilst others will wonder how it ever came to be thought worth the pains of investigation.]

Remarks on the Composition of the Fin Rays and certain other Parts in the Anatomy of Fishes, by Dr. J. Hancock, Corr. Memb. of the Zool. Soc., &c. &c.

On a former occasion (p. 136 of the last number of this Journal) I have alluded to the importance of the character derived from the *number* of the rays in the fins of fishes, as furnishing the best diagnostic marks for discriminating the species; the vast variety and the different combinations arising from these numbers, seldom, if ever, failing to afford distinctions, even in genera composed of the most numerous and nearly allied species, and in which all other characters will be found to fail of affording marks sufficiently clear and obvious.

In a character of such value, therefore, it is proper to point out the fallacies which from inadvertence may arise in its application.

It is a circumstance which has not hitherto been duly noticed, if at all, or which I had never before observed, that the posterior or soft rays of the fins are generally *double* or *twin*, consisting of two bones so accurately joined as to appear but one; they are easily separable however, so that the anatomist, on dissecting, might in some cases almost double their number. They are placed (the two pieces) laterally with each other, or side by side—never fore and aft. At their apophyses or base, each twin ray sends off a projection sideways, to which is attached a lateral tendon,—and below they are connected with another series of similar bones, which proceed inwards, in a contrary direction, corresponding, as it were, to the *root*, or, as Linnæus has designated it, *descending caudex* in vegetables. The anterior rays, in some cases, are strong, spinose, and sharp pointed; they consist of one piece: and a peculiarity in them is a perforation, or a round aperture at the base, through which a longitudinal cord or ligament passes—which cord, like the lateral attachment just noticed in the double ones, serves to confine the bony ray in its place. These spinose single rays are chiefly found in the anterior part of the fins. The same circumstances are found to obtain, more or less, in the fin rays of most of the bony fishes.

We may easily account, then, for the discrepancies in different authors, and the frequent assertion, that the characters derived from the number of rays are found to be variable: for it is plain how the same fish, examined by two different persons, or even by the same person at different times, may give results very different in this respect. The most certain way of avoiding such mistakes is also the easiest, and consists of counting the rays, without using any violence, taking their numbers as they appear *near the base* of the fin, or near to the body of the fish: by such precautions the most *constant*, *convenient*, and *certain* characters will be elicited.

When the fish are boiled, their muscular attachments become very slight, and more liable to the deception here noticed; and it is not unusual for the naturalist to embrace the opportunity at table of examining a fish for such information; thus, for example, as I have experienced, the five rays (six, Linn.) of the ventral fin in a boiled mackerel, being but slightly handled or rubbed between the fingers, will fall into eight or ten. I have only examined the queriman (*mugil littoralis*) of Guiana, and mackerel, for this elucidation; and I find the rays of all the fins in the latter to multiply under the fingers in the manner just stated (*i. e.* in a boiled fish), with exception of the anterior dorsal fin, which consists of sharp pointed spines, with perforated apophyses *.

I am doubtful as to the method observed by Linnæus, in

* I have, since the above was written, examined several other fishes, gadi and pleuronectes, as the whiting, haddock and cod, sole and flounder; and I find the same circumstances to prevail in all of them; it may, therefore, most probably, be considered as the general structure.

numbering the rays of the caudal fin; in most of his Sparii, for instance, we find he has reckoned about 17 or 18, and at most 22 rays, which is in his *Sparus vergatus.* Now this species has 18 long rays, besides the shorter ones above and below, which amount to twelve more, making 30 in all; and this I find to be constant, from examination of several individuals: they are not all visible, however, without dissection, or till, by boiling or otherwise, they are denuded. It would seem from this, that he neglected to enumerate these shorter rays in the caudal fin; but we can hardly suppose he would purposely omit them, because some of them are as long, and quite as obvious, as the middle ones*.

On recurring to those before mentioned, I found that every one of these radii are twin or double—even the very outermost or shortest, which are so small as to require a lens to observe them. Being composed of *jointed half cylinders,* applied in such close contact as to appear quite entire, and forming tubes which are occupied by a gelatinous substance, or albumen perhaps, as it seems to coagulate or harden with heat—these bones branch off about their middle, with two radii, and are thence subdivided into eight. When the investing membrane is removed, and the gluten about them dissolved by boiling or maceration, the two pieces will separate spontaneously in drying.

No other structure could at once afford such strength, lightness, and elasticity, as that which we thus observe in the fins of fishes. We see that all those fins which require a power of strong lateral exertion, are chiefly composed, as above noticed, of two semicylindrical portions, laid in contact, admitting of a small sliding motion. They stand separate, and somewhat distinct at the base (especially in the caudal fin), which gives them a degree of firmness or a bracing power; and a projection of each at the base gives insertion or attachment to strong lateral tendons, which serve to pull them to either side. To augment their elasticity and easy motion, they abound with short joints, especially towards their extremities: finally, to

* We must consider, however, that many, or doubtless the far greater part of the descriptions of Linné are derived from various other authors, in which he found the species and genera confounded, and only doubts and obscurity prevailing. His system, therefore, should be regarded as only an approximation, or rather as the foundation of a method (and that the best of all, so far as it goes) for reducing this chaos to some order. The misfortune is, it has been considered as too great an advance towards perfection.

perfect their pliancy, they are *subdivided* into several branches, fore and aft, not laterally; and each branchlet, however minute, may be observed, with a lens, to be composed precisely as just noticed of the larger stems. Their jointed and channeled structure reminds one of the culm or stem of a rush when split open *.

This wonderful organization is, more or less, observable in the fin rays of all the bony fishes,—it is in general most notable in the tail fin, which may be considered as the *main-spring* in propelling the animal forward in opposition to the watery medium; the pectoral and ventral conduce to the same end, as well as facilitating various other evolutions in the water,—whilst the anal serves, in the manner of a rudder, to *steer* or direct its motion through the element. The lateral action being particularly required in this fin, we find it generally to consist of the soft pliant structure; whilst those of the back, the office of which is less apparent, are more frequently composed of the *rigid* kind, or firm *entire* spinose rays, which possess a much less perfect *side motion.* It is plain, however, when we consider of it, that this fin (the dorsal) serves to give a steady motion; it may usually be observed elevated when the fish is swimming with any velocity, and contributes to maintain its position in the water, or to prevent its falling on either side.

In many individuals of the goldfish (*cyprinus auratus*), which, for the splendour of its colours, is kept in glass jars, we observe the lower half of the caudal fin to be double, so that the tail appears trifid;—this, perhaps, may be regarded as one of those curious anomalies resulting from transmigration (being indigenous to Japan). The bones composing the rays being, as in

* Linnæus in his Syst. Nat., tom. i., p. 1127, Lipsiæ, 1788—*Pisces*—observes, "Spinæ a radiis geminis coadunatis ortæ, nec ut muticæ radiis dichotomis." This is a very singular error of that great naturalist; and it has probably given rise to a similar remark, which I observe in the Règne Animal of M. Cuvier, vol. ii. p. 123 of the new edition, where he observes, that "the rays are of two kinds: one consisting of a single bony piece, commonly hard and pointed, somewhat flexible and elastic, *divided* longitudinally—these are called *spinose rays;* the others are composed of a great number of small articulations, and are usually branched at their extremities: these are called soft rays, articulated or branchy (branchus?)" Now the spinous rays never rise in twins joined together (geminis coadunatis), as asserted by Linnæus, or as stated by Cuvier, *divisée longitudinalement,*—nor ever separated at either extremity, or in any manner whatever. We see by this, however, that naturalists have entertained some idea, though a very confused and erroneous one, regarding the *non integral* structure of certain radii in the fins of fishes. A similar statement occurs in some of the older writers. It may, therefore, be considered, perhaps, as successively copied by different authors from those of a more remote antiquity.

other fishes, naturally double, probably diverge, from some casual defect of the cohesive substance and investing membrane.

It is probable that the *rays* in these bifid portions will be found to be *single;* this, however, I offer only as a conjecture, never having examined one of them *.

This composition, or double structure of the fins of fishes, is peculiarly obvious in the tail fin; when denuded of its membranes, these bones will be seen separated some distance at the base; as I have lately observed in the tail-fin of a cyprinus, the skeleton of which I had suspended to dry two months since, and from which the soft parts were found to have been entirely removed by flies, or other insects. This specimen exhibits the rays of the tail-fin, spread on *each side* of the expanded fan-shaped bones which terminate the last piece of the vertebral column; and thus displays, without being boiled or dissected, most clearly, the twin or double layers of the rays.

It also exemplifies the great power of the tail, or caudal fin, in propelling the fish with rapidity through the watery element; for each of these bones we find to have strong muscular attachments laterally, enabling it to ply like an oar in the water, and giving that powerful elasticity and flexion from side to side.

I have been, it may be thought, rather tedious and minute on this subject; but, from the vast accession of new species which have been derived from the discoveries of later naturalists and travellers, it has been found in many cases, especially in respect to fishes, that the characters usually employed for discriminating the species have become extremely defective. It is this deficiency in the external characters which has induced some of the most able naturalists of modern times to direct their attention to comparative anatomy, for more certain means of discrimination, as well as for the elucidation of the general structure and functions of animals.

I need say no more, however, on this subject, but refer to an examination of the *structure of the fins* of fishes, in proof of the facts here stated.

* Another singularity observable in this fish, is, in numerous instances, a privation of the posterior part of the dorsal fin: it is never observed to sleep; and Mr. Butler, of Covent Garden, asserts, that they thrive best without being fed. Many *plants* flourish in water—some in air alone: it is rare, however, to find an animal nourished without something more substantial.

Ossicula on the Brain.

I also alluded, in the last number of this Journal (p. 134), to those petrous or bony appendages found within the cavity of the cranium, and in contact with the brain, in the gilbagre and other fishes—the siluri, more especially, of the Guiana coast.

These petrous bodies, from their situation, I had denominated brain-bones, being uncertain whether or not they might pertain to the auditory organ. After having, in Guiana, consulted the writings of Buffon and Daubenton, the only ones which presented on the subject, I perceived nothing to identify them with the organ of hearing. The observations of Blumenbach, which I have subsequently had access to in London, have very little contributed to remove the obscurity.

I shall here transcribe the only passage that appears at all to bear on the present subject, from Blumenbach's Manual of Comparative Anatomy, translated by Laurence, revised and corrected by Coulson, 1827:—"It is only in some genera of cartilaginous fishes, viz., the skate, shark, and lampreys, that a *tubular* appendix of the vestibulum is continued backwards and outwards, so as to represent a rudiment of a tympanum."

"Much light has lately been thrown on the organ of hearing in bony fishes, by Professor Weber. They possess *near the anterior cervical vertebræ* considerable *ossicula*, which may be compared to the malleus, incus, and stapes; and, in those which are provided with a swimming-bladder, these bones are so connected with that organ as to render it probable that it is auxiliary to the sense of hearing?"

"Their internal ear consists of three large *canals*, which are *generally* seen to project into the *cavity of the cranium*. Opposite to the termination of the auditory nerves *on the vestibulum*, one, two, or three, neatly-formed stones are found. These are white as porcelain, particularly in several of the bony fishes, and very *dry* and *brittle* in their texture."—P. 285.

Here is nothing said respecting bones posited in the cavity of the skull, and in contact with the hemispheres of the brain;

but "ossicula *near the anterior cervical vertebræ*:" then—"*on the vestibulum* are neatly-formed stones," &c. From this, it would seem as if there were two different sets of ossicula belonging to the organ of hearing.

I should scarcely conceive that the remarks just cited from Blumenbach bore any allusion to the bones found upon the brain of the Guiana fishes.

Cuvier's delineations and descriptions differ much from what I have observed in the fishes of Guiana, both in respect to the inclosure and situation of those ossicula; yet, as affording points of resemblance, they serve, perhaps, to identify those encephalic bones with the *ossicula auditus* as they are called, which, in the northern or European fishes, it seems, are placed in a lateral cavity, or rather a lateral extension of the cerebral cavity, much exceeding the volume of the brain, and containing the labyrinth, &c. "Cet appareil entier (the semicircular canals, &c., in fishes) est situé dans les *côtés* de la cavité du crâne, et s'y trouve fixé par du tissu cellulaire, des vaisseaux et des brides osseuses ou cartillagineuses *."

It appears that in the fishes of the North these ossicula vary in different species from one to two or three in number (*i. e.* on each side), and are enveloped in the sac of the labyrinth along with the common pulp.

In the fishes of Guiana, the brain of which I have examined, I have found but two of these petrous bones, each separately inclosed in its proper tunic, which is exceedingly thin and pellucid. Being suspended by slender cords attached to the sac, they seem in a manner to float, in a gelatinous liquor, in the cranial cavity, in contact with the brain—not as in the cod and others, in which the sac is attached, with some degree of firmness, to the base and sides of the cranium.

If these ossicula are, in the fishes of Guiana, fewer in number, they are more developed or larger than in the northern fishes. One of these bones, in a gilbagre of three feet in length, weighs upwards of seventy grains, whilst in a full-grown cod (of the same size) it does not exceed twenty grains. Yet M. Cuvier observes, they are larger in proportion in the cod than in any other fishes (l. c., p. 458.)

* Cuvier, Anat. Comp., tome ii., p. 455.

In respect to their extreme density and hardness they also show a very great disparity, but this need not here be insisted on.

That such are the positions of the organs of hearing in fishes, *i. e.* within the cavity of the cranium (along with the brain), there can scarcely remain a doubt, although it appears to have been disputed by no less a naturalist than M. de St. Hilaire. Of this I have been informed by Mr. Yarrell, who will, ere long, it is to be hoped, instruct us with the result of his more exact investigations on the mysterious points of comparative anatomy here referred to.

The bones found within the cavity of the cranium of the manati are undoubtedly the same as those alluded to by Mr. Gray in the first edition of his Pharmacology. He observes, p. 105, "Manati stone, *lapis manati.* The teeth of the sea-cow;" and Blumenbach doubtless alludes to this bone also in his Manual, p. 283, where he observes, that "the hard bony substance which was formerly very erroneously called *lapis manati*, or *tiburonis*, is merely the *tympanum* and *bulla ossea* of the whale."

It would hence appear, that this learned naturalist had no knowledge of, or had mistaken, this stone or bone of peculiar hardness and density, possessing the highest celebrity among the Indians and natives over all Guiana and the West India islands, for their reputed virtues in convulsions, colic, disuria, &c.

Air-Bladder.

The term air-bladder seems very proper, but sound, on the contrary, if signifying any thing connected with the bottom, is surely the reverse of propriety.

It is certain that the air-bladder of fishes assists them in rising in the water, and hence ought to be regarded as, in some measure, auxiliary to motion; but the idea of its contributing to progression through this element is contrary to reason, for there can be no necessity for it in a horizontal movement, the great power of the tail and fins in fishes, and their bodies being naturally little, if anything, more than the specific gravity of water, an increase of bulk could only retard, instead of facilitating their progress through that element;—indeed, it must be, on the contrary, an increase of specific gravity which should accelerate the motion in an animal provided with powerful external organs for propelling itself through the water. This organ (or gasometer, if it might be compared with an instrument of modern mechanics) appears to be of much less importance to the swimming of fishes than is generally supposed; and it is probable that its paramount utility will be discovered in some other function, when the touchstone of natural philosophy, by actual experiment, shall be duly applied to the subject.

Men are prone to form hypotheses to suit appearances and their own convenience; they observe that the lampreys, flounders, &c., affect the bottom, and their air-bladder, being less obvious, they place the cause for the effect, and assert they reside at the bottom, because they are not provided with a swimming-bladder; whilst, if they would take the trouble of searching and judging for themselves, they would find this organ, or something analogous, I presume, in all fishes*. That this organ is smaller in those fishes is true, for the obvious reason that their habits and economy render it less necessary for them to raise themselves in the water: the bladder, therefore, less frequently called into use, is, for the same reason, smaller than in most other fishes, as we constantly find the development of the muscles and organs of animals to bear a near relation to the frequency with which they are called into action. Of this there are abundant examples amongst men: mountaineers, or those accustomed to much walking, have stout and strong legs; whilst sailors and blacksmiths excel in the size and strength of the arm, and the pectoral muscles.

One remarkable fact respecting the float or air-bladder in fishes is, that it is sometimes double, and at other times single, in individuals of the same species. This has been observed by Blumenbach, and I have found it to prevail in the cuirass of

* I find it exists in all the species of pleuronectes and petromyzons, which I have had the opportunity to examine, and even in that extraordinary animal the hag-fish (*gastrobranchus cæcus*, or *myxine glutinosa*, Lin.), although this fish, being *supposed* destitute of vision, would seldom venture to quit the bottom.

Guiana, as noticed in certain observations which I had made on the silurii of Essequibo in 1825, and exhibited to those excellent naturalists of the Zoological Society, Messrs. Vigors, Bennett, and Yarrell, as well as an extraordinary and unique species of articulation in the armature of the gilbagre, a near-allied species of the same genus, at the museum of that society, and to which, to save repetition, I beg leave to refer the reader for a further explanation.

We find the diversities in the structure of the air-bladder to be very remarkable, even in closely-allied species. In the herring, for instance, it is a detached cylindrical tube. In the shad it is of the fixed kind, or a membrane stretched across, as in the mackerel, mullet, &c.

We find this organ most fully developed in the flying-fish of the tropics (*exocœtus volitans*). One of these flew, in 1804, on board of a vessel I was in, about the latitude of Barbadoes, which afforded me the opportunity of dissecting a fresh specimen.

Since the above investigations were made, I have noticed some very interesting remarks on the subject in the Anat. Comp. of M. Cuvier, tome i., p. 501; and v., p. 270—"*De la vessie natatoire des poissons*;" and to which I beg leave to refer the reader. I will only observe here, that what that sagacious naturalist has there stated is sufficient to put authors upon their guard how far they ought to repose confidence in various authorities and commonplace reports.

We may say, perhaps, with the utmost fairness, that in general, the more important, solid, and valuable improvements in the arts and sciences either originate in Britain, or are there developed in their greatest perfection. At the same time, the cultivators of certain of the physical sciences are, most incontrovertibly, far behind their continental brethren; and it must be owing to the extremely loose and vague manner of employing the distinctive characters in fishes, that such great confusion and contradictions prevail as were lately pointed out by Mr. Yarrell, in the fourteenth number of the Zoological Journal, regarding the differences existing between the shad and whitebait, which, it appears, had been confounded by one or two of the later British naturalists; notwithstanding which, the disparity of the two fishes is so great, that even the common fishermen and fishmongers ridicule the idea of their being identical.

It is rather surprising, that those authors who have had the best or most frequent opportunities for observing the two subjects in question should be the only naturalists who have gone into this error; for we do not find it in Linnæus, nor in Cuvier, Bloch, Lacepede, or any other of the continental writers, so far as I have observed, and the naturalists just named have not even hinted at such an identity, in respect to these two species. By Linnæus they are even placed in different *genera*, and by Cuvier in distinct *subgenera!*

It is to be regretted that Mr. Yarrell should not rather have given the actual characters from the results of his own examination (for it appears he had numerous specimens before him at various times), instead of quoting the confused and discordant accounts of other authors. But he compares the number of fin rays in the white-bait, as accurately numbered by himself, with those in the shad as stated by Donovan, and which differ extravagantly from those observed by Linnæus. The probability is, they are two distinct species confounded by Donovan, and admitted by one or two British writers without examination. I will only observe further, that the reputed identity of the shad and white-bait appears to be merely an incidental error of some later writers, although Mr. Yarrell mentions it as an opinion long since settled or established.

If the knowledge of natural history in general be defective, it is more especially so in respect to ichthyology. Many of the British species, or those fishes which annually visit the British coast, are either nondescripts or mistaken, and falsely identified with the Linnæan species: in some instances the fresh-water species are, likewise, thus confounded.

It would seem as though the writers on British fishes have considered that every species has been sufficiently determined, and that nothing remains to be done but to repeat, and continually copy, from each other. It is not a little strange, that a fish supposed to be so well known as the shad, should still be mistaken *; either the *Clupea ficta Lacepede* (*Finta*, Cuv.) or a nondescript species, is taken for the *clupea alosa* of Linnæus, and referred to as identical in Turton's translation of the Syst. Nat.—a work, by-the-by, most preposterously shorn of the more essential parts of its original, especially in respect to synonyms, and references to figures, and more complete descriptions; at least, this is the case in those parts of the work embracing the *fishes*, *quadrupeds*, and all the more important animals, in which not a single reference is given, whilst the most lengthy, tiresome, and minute lists of references and synonyms are appended to the less important department of natural history, as, to many of the species of *shells*, *worms*, *insects*, *moluscæ*, &c.! No wonder that ignorance should hold the reins, where such bad judgment is manifested by the heads and pretended magnates of the science.

It appears rather singular that the celebrated French naturalist, who quotes all standard authors, should constantly abstain from any mention of the work on the "British fishes," which is considered the chief authority here, although he frequently cites the common English, or what are termed the vulgar names of species. I presume, he regards the British fishes as partly nondescripts, or as imperfectly known, and as being, in numerous instances, falsely assigned to the Linnæan.

It was, doubtless, a sense of this depressed state of the science which induced a few enlightened and enterprising na-

* Mr. Yarrell states that the shad has strong teeth in both jaws: but I have not found any rudiments of teeth in those I have examined. The vertebræ I find to be 55, counting the last piece, or that which ends in the fan-shaped expansion of bone forming the extremity of the spinal column. Is this included by Mr. Yarrell in counting the vertebræ? That examined by him, compared with white-bait, is, doubtless, what usually obtains the name of shad here, but appears not to be the *clupea alosa*, certainly not that of Cuvier, which he (Cuvier) says is distinguished by the absence of teeth, and a single black spot behind the gills—"*une tache noire*, derrière les ouïes!" Règ. An., vol. ii., p. 319; and that Bloch, pl. 30, has given, under the name *alosa*, only a *Finte*, *Clupea finta* of Cuvier, with well-marked teeth in both jaws, five or six black spots along the side! and a body more oblong than in the *alosa*.—*Synonym*, *agone de Lombardie*. This may have been the species compared with white-bait by Mr. Yarrell. For a figure of the true shad (*clupea alosa*, Linn.) he refers to Duham., sect. III., pl. i., fig. 1.

Are the *alosa* of Linnæus, Bloch, and Donovan, all different species, whilst intended for one and the same? If so, it is quite sufficient to account for the reported variation in the number of the fin-rays. It is not Nature, however, but our mistakes, I presume, in confounding different species, which chiefly give rise to this supposed want of constancy.

turalists to unite their powers in the formation of a society for the purpose of congregating living animals, as well as the preservation of specimens in every branch of zoology; and which, in the extraordinary short period of four years, has already arrived to a condition which rivals even the oldest establishments of the kind in Europe, or in the whole world.—The museum and gardens of the Zoological Society, being in a rapid state of improvement, have already diffused a taste for one of the most rational and interesting species of intellectual enjoyment. It is a "Noah's ark" realized, in which one may, as it were, see at a glance all over the animal world.

This exhibition will, of course, become more interesting in proportion to the accessions made in point of number, and to the accommodation afforded the animals,—especially as their natural habits become better known. On this point, for instance, I should beg leave to recommend, that the two beavers in the gardens be furnished with means for a display of the extraordinary powers and economy of this sagacious, cleanly, and industrious animal, in cutting timber and constructing their own houses, instead of one formed for them of stones—a domicile they are not accustomed to in their own country: besides, their most congenial food is derived from the bark and tender twigs of trees which they fell, cut up in junks, and carry for building and constructing dams. This is the more desirable, as many people are unable to give credit to such reports respecting the natural habits of this animal; nor could I myself, perhaps, had I not witnessed it.

Amongst those trees of which the beaver is most fond, are the black birch (*betula nigra*), and common ash (*fraxinus juglandifolia*), of North America.

J. H.

Stafford-place, Pimlico.

On the Systems of Numerical Signs used by different Nations, and on the Origin of the Expression of Value by Position in the Indian Numbers. *By* Alexander von Humboldt.

[Read in a Class-Session of the Royal Academy of Sciences, in Berlin, the 2d of March, 1829.]

Translated from the German, and communicated by J. W.

In our researches upon numerical figures (the only hieroglyphics which, among the nations of the old continent, have been preserved, besides the alphabetical figures used to express the different sounds of spoken language,) our attention has, hitherto, rather been directed to the characteristic physiognomy of the figures and their peculiar formation, than to the spirit of the methods by which human sagacity has succeeded in expressing quantities with a greater or less degree of simplicity. These researches have been entered into with views as narrow and as contracted as those made on languages. The latter have, for a length of time, been compared rather according to the frequency of certain sounds and terminations, or to the form of their roots, than to the organic formation of their grammars. For many years I have been occupied constantly, and with particular predilection, in endeavouring to bring under a general view the different systems of numerical figures used by the different nations of ancient and modern times, and in this way I have succeeded in throwing some light on the origin of what is called the *Arabic numerical system.* Many circumstances concurred to enable me to effect it. I myself have acquired, on my travels, a knowledge of the numerical systems of the Aztekes (Mexicans), and of the Muyscas* (the inhabitants of the elevated plain of Cundinamarca); Thomas Young made the discovery of the Egyptian numerical figures, which (as is now known) do not, all of them, express the *multipla* of the groups by juxtaposition; the Arabic Gobar, or Dust-figures, were discovered by Silvestre de Sacy, in a

* On the opinion that the numerical figures of the Muyscas (which at the same time are the hieroglyphics of the moon-days of the increasing age of the moon) have some connexion with the face of the moon, increasing by degrees according to its different phases, see Humboldt, Vues des Cord. et Monumens des Peuples indigènes de l'Amérique, t. ii., p. 237—243. Pl. XLIV.

Parisian manuscript; the peculiar nature of this system induced me to compare it with the numerical figures of the Mexicans and Chinese; by the publication of a great number of Indian grammars we have lately obtained the certainty, that in India within the Ganges, as well as without that river, not only different figures and alphabetical signs are used in expressing numbers, but also that the systems themselves of computation differ from one another,—in some of them value being expressed by position, in others not; lastly, a peculiar Indian method, till now quite unknown, has been preserved and discovered in a scholion of the Greek monk Neophytus. In an Essay composed by me, and read in the Session of the Académie des Inscriptions et Belles Lettres, in 1819, I have already tried to shew, that some nations shorten the unartificial method of juxtaposition by writing exponents, or indicators over the figures, (as it is used by the Mexicans, in the ligatures of four times 13, or 52 years, by the Chinese, the Japanese, and the Hindoos, speaking the Tamul language,) and also the manner in which, by means of these indicators, and the suppression of the group-signs written horizontally or vertically, the excellent Indian system, expressing value by position, might have been arrived at. The spreading of this system must considerably have been favoured by the very ancient use of a particular contrivance, *the strings of reminiscence and calculation.* These are either loose cords, as the quippos of the Tartars, Chinese, Egyptians, Peruvians*, and Mexicans, which have been transformed into Christian beads, or ritual calculation-machines †; or they are extended and fixed in a frame. In the last form they are found in the *Suanpan*, used in all the internal countries of Asia, in the *abacus* of the Romans and ancient Tuscans ‡, and in the instruments of the palpable arithmetic used by the Slavonian tribes §.

The files of cords or wires in the very simple Asiatic

* On the quippos used for counting the sins in the confessional, see Acosta, Hist. Natural de las Indias, lib. vi., cap. 8. El Inca Garcilaso, lib. vi., cap. 9. Freret, Mem. de l'Acad. t. vi., p. 109.

† Klaproth's Asiat. Magaz., t. ii., p. 78.

‡ Otfried Müller's Etrusker, t. ii., p. 318.

§ In the Russian language *chotki* signifies the beads, and *choty* the calculation-board, with its fixed cords.

Suanpan represent the higher or lower groups of a numeral system, whether tenths, hundredths, and thousandths, or, in the sexagesimal division, degrees, minutes, and seconds. The manner of calculating is always the same. The pearls or beads again are the indicators of the groups, and an empty file indicates a cypher, consequently the empty *sunya* (sansu), *sifr*, or properly *sifron*, *sihron* (Arabic, according to *Meninski prorsus vacuum*).

I am not able to prove in a strict historical manner, that the Indian system of nine figures, indicating different values by the places they occupy, has been invented in the way I have indicated, but I believe I have discovered the way in which this invention may have been gradually brought about. That, perhaps, is as much as can be done: for the history of the first steps in the development of the mental faculties, and of the scale of civilization, is involved in darkness; and commonly imparts to us nothing more than the knowledge of possibilities. But it is for that very reason that this part of history is so interesting.

Only a short extract of the Essay read by me in the Académie des Inscriptions has been published, and that in a place where it hardly will be sought for*. The manuscript itself is in the hands of M. Champollion, who intended to publish it, with some new and important facts respecting the different methods of the Egyptian figures discovered by him in Turin. Since that time I have continued, from time to time, to render my first work more complete; but as I have no hope left to find the necessary leisure for publishing it in its whole extent, I intend to put together in this essay the most important results. Perhaps this is also a favourable time for doing it; for, now that the researches respecting languages and monuments have taken a different and more useful direction, and that our intercourse with the nations inhabiting the southern and eastern countries of Asia is on the increase, it may be of some utility to bring into discussion problems, which are in close connexion with the intellectual march of the human mind, and

* Gay Lussac et Arago, Annales de Chimie et de Physique, t. xii., p. 93, among the Monthly Notices of the Transactions of the Institute. Humboldt, Essais Pol. sur la Nouvelle Espagne (2d. edit.), t. iii., p. 122—124.

(by the latest comparisons of numerical hieroglyphics, and the simple graphic method,) with our glorious progress in the mathematical sciences. One of the greatest mathematicians of our times and of all times, the author of the "Mécanique Céleste*," says, "The idea of expressing all quantities by nine figures, whereby is imparted to them both an absolute value, and one by position, is so simple, that this very simplicity is the reason for our not being sufficiently aware, how much admiration it deserves. But it is this simplicity, and the facility which calculations acquire by it, that raises the arithmetical system of the Indians to the rank of the most useful inventions. How difficult it was to discover such a method may be inferred from the circumstance, that it has escaped the talents of Archimedes and of Apollonius of Perga, two of the most profound geniuses of antiquity." The following observations, I hope, will shew, that the Indian method of calculation may very possibly have arisen by degrees out of others, which were used earlier, and even now continue to be used, in the eastern countries of Asia.

As language, in general, affects the manner of writing, and writing again reacts on language, but only under certain conditions which have been inquired into by Silvestre de Sacy, and my brother, so also the different modes of computation used by different nations, and their numerical hieroglyphics, reciprocally act upon one another. Yet no very great consequence is to be attached to this alternate action. Numerical figures do not always follow the same groups of unities like languages; and moreover in languages we do not always discover the same resting-points (the same quinary intermediate stops) as in numerical signs. But if we bring together under one view, the language (names of numbers) and the numerical figures used in the remotest parts of the earth, as the common product of human intelligence applied to quantitative relations, we discover in the written numbers of one race, the isolated seeming peculiarities of language of another race; we may even add, that

* Laplace, Exposé du système du Monde (5th edit.), p. 325. The assertion of Delambre, (Histoire de l'Astron. Ancienne, t. i., p. 543,) in his contest respecting the merit of the ancient Indian arithmetic, as it is explained in *Bhascara Acharya's Lilawati,* is in very strange opposition to the opinion of Laplace. But language alone can hardly prompt me to suppress the figures of the groups.

a certain awkwardness in the numerical command of language and writing is a very false standard of what is called the state of cultivation of mankind. Here the same complicated and contrasted relations to each other take place, as with nations, of which some possess an alphabet, or mere ideographical signs, some have the most luxuriant abundance of grammatical forms and flexions, rising out of the root in systematical progression, whilst others use languages destitute of forms and flexions, as if benumbed in their very birth; and all this in the most different gradations of intellectual culture and political institutions. So the one race of mankind finds itself driven in the most opposite directions by the alternate action of the internal and external world, (an alternate action whose first decisive efforts are wrapped up in the mythological darkness of the remotest antiquity); keeping most stedfastly to its old nature, even when great revolutions of the world bring geographically near to each other races the most heterogeneous in language: whilst the similarity of the sounds which re-echo from the remotest zones, in grammatical forms of language, and graphic attempts to express large numbers, proves the unity of the old stock—the ascendancy of that which springs out of the internal intelligence, out of the common organization of mankind.

Travellers observing that some nations formed heaps of five or twenty pebbles or grains of seed, when they were about to make a computation, asserted that these nations were not capable to count farther than till five or twenty*.

As well may it be asserted that the Europeans are not capable of counting farther than ten, because seventeen is compounded of seven and ten. In the language of the most cultivated nations of the west, for instance in those of the Greeks and Romans, some expressions are yet preserved, which refer evidently to such heaps or groups, ψεφίζειν, *ponere calculum, calculum detrahere*. Groups of unities procure resting-places in counting; and as in the different nations the members of the body are similarly formed (the four extremities are divided fivefoldly) they stop either after having counted the fingers of

* Pauw, Recherches Philos. sur les Américains, t. ii., p. 162. (Humboldt, Monumens Américains, t. ii., p. 232—237.)

one hand, or those of both, or they add also the two feet to the hands. This difference in proceeding produces different resting-places, and thus are formed groups of 5, 10, or 20. It is, however, worth observing, that among the nations of the new continent, as among the Mandingas in Africa, the Biscayans, and the Gaelic tribes of the old continent, groups of twenty are in prevailing use *.

In the Chibcha language of the Muyscas, (who, like the inhabitants of Japan and of Thibet had an ecclesiastical and a laical chief; and whose method of intercalating the 37th month like the inhabitants of North India, has been published and explained by me †), 11, 12, 13, are called foot one (quihieha ata), foot two (quihieha bosa), foot three (quihieha mica), from quihieha or quhieha (foot), and the first three unities, *ata*, *bozha* or *bosa*, and *mica*. The arithmetical signification of *foot* is ten, because the foot begins to be taken into account, when both hands are passed through. To express twenty, the Muyscas use in their arithmetical language the expression *foot ten*, or *the small house* (gueta), perhaps because they used, in counting, grains of maize, and such a heap of maize reminded them of the barn, where maize was laid up. By means of the expression *small house* (or barn), and twenty (both feet and hands) they formed the expressions for 30, 40, 80, by joining them together, as, twenty plus ten; twice twenty; four times twenty. Quite similar are the Celtic expressions which have passed into the languages of Roman origin, as, quatre vingt, and quinze vingt, or those more rarely met with, as six vingt, sept vingt, huit vingt. Deux vingt, and trois vingt are not used in French; but in the Gaelic or Celtic dialect of West Britany, through which I passed a few years ago, twenty is called *ugent*, forty *daou-ugent*, or two twenty; sixty *tri-ugent*, or three twenty. It is even said *deh ha nao ugent*, or ten over nine twenty = 190 ‡.

* Instances of such numeral groups of twenty unities are found in America among the *Muyscas*, the Otomites, the Aztekes, the Cora Indians, &c.

† Monum. Américains, t. ii., p. 250—253. The Muyscas had some stones covered with arithmetical hieroglyphs, which, by being placed in a certain order, facilitated to the priests (xeques) the intercalation of the ritual year. The figure of a stone which served for that purpose may be seen in my work, Tab. XLIV.

‡ *Davies's Celtic Researches*, 1804. p. 321. Legodinec Grammaire Celto-bretonne,

It is by no means a difficult task for me to give a still greater number of remarkable instances of the analogy existing between language and numerical hieroglyphics in juxtaposition, in the subtraction of unities by prefixing them, in writing, before the group, and in the intermediate landing-places of 5 and 15 in those nations who have adopted groups of 10 or 20. In the languages of some very rude American tribes, for instance in those of the *Guarinis* and *Lulos*, *six*, *seven*, and *eight* are expressed by *four with two*, *four with three*, *five with three*. The more civilized *Muyscas* say *twenty* (or house) *with ten*, instead of thirty. The Cymri, in Wales, use in such a case *deg* (ten), or *ugain* (with twenty); as the French use *soixante et dix*, for seventy. We find addition effected by juxtaposition, chiefly among the old Tuscans, Romans, Mexicans, and Egyptians; subtractive or diminishing expressions among the Indians in the Sanscrit *, where 19 is called *unavinsati*, 99 *unasata;* among the Romans in *undeviginti* (*unus de viginti*), for 19; *undeoctoginta*, for 79; *duodequadraginta*, for 38: among the Greeks in εἴκοσι δεόντα ἕνος, 19; and πεντεκόντα δυοῖν δεοντοῖν, 48; that is, *wanting two to make up fifty*. Such subtractive expressions have passed into the writing of numbers, and in such a case the figures expressing unities to be subtracted are prefixed to the signs of the groups of five and ten, and even to their multiplicates, for instance, to 50 or 100 (IV, and XI, and XL, and XT, for four-and-forty among the Romans and ancient Tuscans †; though in the last nation the numerical figures probably were entirely derived from the alphabet, according to the researches lately made by Otfried Müller). In some rare Roman inscriptions

1807, p. 55. In the Celtic or Kymrich dialect of Wales, 5 is called *pump;* 10 *deg;* 20 *ugain;* 30 *deg ar ugain;* 40 *deugain;* 60 *trigain*. (William Owen's Dictionary of the Welsh Language, vol. i., p. 137.) The same system of twenty unities is used in the language of the Biscayans: *bi* is 2; *lau*, 4; *amar*, 10; *oguai*, 20; *birroguai*, 40; *lauroguai*, 80; *berrgoiuai-tamar*, 50, namely forty and ten (amar). *Larramendi*, urte della Lengua Bascongada, 1729, p. 38. (The numeral figures of the Biscayan and Gaelic languages are not mixed together in my Monum. vol. ii., p. 237, but they are placed near one another to facilitate the comparison; by an error of print, however, *les premiers* is said instead of *les deux* or *les uns et les autres*.)

* M. Bopp quotes even 95 (or hundred —5) in the words pantschonam satam —contracted from *pantcsha* (five) and *una* (less).

† Otfried Müller, Etrusker, ii., p. 317—320.

collected by Marini *, even four unities are found before ten, for instance, IIIIX=6. We shall soon see, that among some tribes inhabiting the East Indies, methods are found in which the joining of figures, which among the Romans and ancient Tuscans indicated only addition or subtraction, expresses addition or multiplication, according to the place or the direction of the signs. In these Indian systems, (to use Roman figures,) IIX is twenty, and XII is twelve.

In many languages the groups of 5, 10, and 20, are called a hand, two hands, and hand and foot (in the language of the Guaranis *mbombiade*). When the fingers of both extremities, viz., fingers and toes, are gone through in counting, then it is considered that the number comprehends the whole body, thus: the word *man* becomes the symbol for twenty. Therefore, in the language of the Yaruros (of which tribe I found populous villages, erected by the missionaries on the river Apure, which falls into the Orinoco) forty are called two men, *noeni pume*, from *no eni* two, and *pume* man. It is known, that in the Persian language the word *pencha* signifies *fist*, and *pendj* five, derived from the Sanscrit word *pancha*. "The last is (according to the ingenious observation of M. Bopp) to be considered as the original of the Roman *quinque*, as the Indian *chatur* of quatuor. The plural of *chatur* is chatvaras, and approaches very near the Doric-Æolian *τέτταρες*. The Indian *ch* is pronounced as the English in words of Saxon origin, but in Greek it becomes a *t*; therefore *chatvaras* is changed into *tatvaras*, as *pancha* into *panta* (the Greek *πέντε*, Æolice *πέμπε*, whence *πεμπάζειν* to count by fingers, or up to five). But in the Latin language, the Indian *ch* is expressed by *qu* and thus *chatur* is changed into *quatuor*, and *pancha* into *quinque*. *Pancha* itself never signifies in India *hand*, but only the number five. But panchasakha is a descriptive expression for hand, as a limb with five branches†."

As in languages, and with peculiar naïveté in those of

* Iscrizioni della Villa di Albano, p. 193. Hervas Aritmetica delle Nazioni, 1786, p. 11, 16.

† On the numerals in Sanscrit, compared with those of the Greek, Latin and Gothic languages, a very interesting essay, in manuscript, was communicated to me by M. Bopp, in Paris, in 1820; and it was composed by him for the purpose of being inserted in my work, *On the Numerical Signs of Nations.*

South America, the groups of five, ten, and twenty, are distinguished by peculiar expressions, so, in the same manner, these groups are easily to be recognized in numeral hieroglyphics. The Romans and Tuscans* had single figures to express 5, 50, and 500. In the language of the Aztekes (natives of Mexico), we find not only the group-signs of a flag for 20, *a quill filled with grains of gold* (which in some districts of Mexico were used as money) for the quadrate of 20 or 400, and a bag (xiquipilli) with 8000 cocoa nuts (which likewise served as a medium of barter) for the cube of 20 or 8000, but even (in the instances where the flag is divided into four equal parts, and the half or three quarters of it are coloured) numerical signs for *half-twenty* or 10, and for $\frac{3}{4}$ twenty or 15, two hands and a foot, as it were†.

But the most remarkable of all proofs of the alternate effect between numerical language and numerical figures, is to be met with in India. Here the value of the unities expressed by their position has, in Sanscrit, been even transplanted into the language. Thus the Indians have a figurative method of expressing numbers by the names of objects, of which a certain number is known. For instance, *surya* (the sun) expresses 12, because, according to the mythology of the Hindoos, there were twelve suns in the order of the twelve months. The two *Aswinas* (Castor and Pollux), who also are met with in the Nakchatras, are used to express the number two. *Manu* signifies fourteen, taken from the *menus* of their mythology. Now it will become intelligible, how the word *surymanu*, in which the symbols of twelve and fourteen are combined, signifies the year 1214. These facts I owe to the kind communication of the learned Colebrooke. Probably, according to these principles, 1412 will be expressed by *Manusurya*, and 314 by *Aswinimanu*. We find besides so complete a numeration in Sanscrit, that a single word, *koti*, is used to express 10 millions. In the *qquichau* language of Peru, which does not count by groups of twenty, a single word stands for a million (hunu).

If it be true that we count by decimals, *quia tot digiti, per*

* Respecting the Tuscan figure for 500, see Otfried Müller, Etrusker, sec. iv., fig. 2.

† Humboldt, Monum. Améric., tom. i., p. 309.

quos numerare solemus, as Ovid says, we probably should have adopted a duodenary scale*, if our extremities had been divided sixfoldly. Such a scale has the great advantage, that its groups can be divided by 2, 3, 4 and 6 without leaving a fraction, and for that reason it has been adopted by the Chinese for their measures and weights, from the earliest times.

The preceding observations regard the relation between language and writing, between the numerals and their figures. I shall proceed to consider the latter by themselves. I remind the reader that this essay is only an extract from my larger and unfinished work, and that I shall not speak of the heterogeneous forms of the simple elements (numerical) figures, but of the spirit of the different methods of expressing numeral quantities, which are adopted by different nations. I, therefore, shall only take notice of the figure or form of the numerical signs, whenever it affects my conclusions respecting the identity or heterogeneousness of the methods themselves. The mode of proceeding adopted for the purpose of expressing the pure or mixed *multipla* of the denary fundamental groups n (for instance $4n$, $4n^2$, or $4n+7$, $4n^2+6n$, $4n^2+6n+5$) is very different in different nations. Sometimes it is effected by forming a row (that is by giving different value to the figures according to their position), after the manner of some nations of the Hindoos; sometimes by mere juxtaposition, as among the ancient Tuscans, the Romans, Mexicans, and Egyptians. Some nations use for that purpose coefficients, as that tribe of the Hindoos, inhabiting India within the Ganges, which uses the Tamul language; others use exponents or indicators, placed over the figures of the groups, as the Chinese, Japanese, and also the Greek in employing myriads; others again, use an inverted method. These place over the nine numerical figures a number of cyphers or points, to indicate the relative value of them, and in this system, the cyphers or points are the signs of the groups placed over the unities. The last method is found in the Arabic Gobar writings, and in an Indian numerical system, preserved and explained in a scho-

* Debrosses, vol. ii., p. 158.

lion of the Greek monk Neophytus. These five different methods are in no way dependent on the forms of the numerical figures, and to prove this fact more evidently, I shall, in this treatise, employ no other signs than the arithmetical and algebraical, which are in common use. In this way the attention is more directed to the essential point, the spirit of the method. I already have used such a mode of pasigraphic notations in another work, treating of a very heterogeneous subject, the regular stratification and often periodical filation of the minerals (in the appendix to the Essai Géognostique sur le Gisement des Roches) *, and there I have tried to shew, how by this method our abstract views on an object can be rendered more general. Proceeding in this way, all observations respecting the form and composition of individuals, which in themselves may be very useful and true, but would turn off the attention, are suppressed, in order to place in a purer and clearer light a phenomenon, which we wish to investigate in preference to others. This is an advantage which in some way may be justified by the chilly insipidity of such abstractions.

As regards the manner of writing numbers adopted by different nations, it is usual to distinguish the *figures not depending on alphabetical* letters, from the alphabetical letters which indicate numerical value, either by forming a certain row, or by short lines or points added to them; or lastly, by the initials of the numerals (in reference to language) †.

It is beyond all doubt, that the nations of Hellenic, Semitic, and Aramaic origin (among the last even the Arabs, before they received the numerical figures from the Persians, in the fifth century of the Hegira ‡), used the same signs to express

* Ed. 1823, p. 364—375.

† The Arabic figures called *Diwani*, are compounded of mere monograms or abbreviations of the initial letters of the numerals, and are the most complicated instance of such initials. Whether the C and M, used by the ancient Tuscans and Romans, are, in fact, initials taken from the Tuscan and Roman languages, is much more subject to doubt than is commonly believed. (Leslie, Philos. of Arith., p. 7—9, 211. Debrosses, t. i., p. 436. Hervas, p. 32—35. Otfried Müller, Etrusker, p. 304—318.) The cross at right angles, used by the Greek, and quite similar to the Chinese figure of ten, signifies in the most ancient inscriptions *a thousand* (Boeckh, Corpus Inscript. Graec., vol. i., p. 23), and is only the oldest form of *ch.* (Nouveau Traité de Diplom. par deux Religieux de St. Maur, vol. i., p. 678.)

‡ Silvestre de Sacy, Gramm. Arabe, 1810, t. i., p. 74, Note 6.

letters and numerals, even in the period of their ripe civilization. On the other hand we find, in the new continent, at least two nations, the Aztekes and the Muyscas, who used numerical figures without having letters. Among the Egyptians, the numerical hieroglyphics commonly used to express units, tenths, hundreds, and thousands, seem likewise to have had no relation to the phonetic hieroglyphics. Quite different from the alphabet are likewise the ancient Persian figures of the *Pehlwi*, for the first nine unities, as those of the ancient Tuscans, the Romans, and even the Greeks in the most ancient times. Anquetil has already observed *, that the alphabet of the Zend language, which, being composed of 48 elements, could have facilitated the expressing of numbers, by alphabetical letters, never uses them as numerical figures, and that in the books written in the Zend language, numbers are always expressed by the figures of the Pehlwi, and at the same time with the numerals of the Zend language. Should, by further researches, this want of numerical figures in the Zend language be confirmed, it must induce us to suppose that the Zend nation, which, in its language, discovers a very intimate affinity to the Sanscrit, must have been separated from the Hindoos, before the last had arrived at expressing value by position. But, the nine unities excepted, the figures of the groups of ten, hundred, and thousand are derived from letters in the Pehlwi language. *Dal* is 10; *re* and *za*, connected together, 100; *re* and *ghain*, connected together, 1000. When we take together, in one view, all that is known to us of the numerical figures used by the different nations, little as it is, it seems sufficient to oblige us to confess, that the distinction of alphabetical numerals and of numerical figures, independent of the alphabet, is as uncertain and useless as that of the languages in such as are composed of monosyllables, and in those using polysyllabic words, a distinction long ago abandoned by truly philosophical investigators of languages. The numerical figures used by the inhabitants of some southern districts of India within the Ganges, who speak the Tamul language, do not express value by position, and

* Mém. de l'Acad. des Belles Lettres, t. xxxi., p. 357.

are quite different from those used in Sanscrit manuscripts, if the figure of 2 is excepted. Who can say whether these figures are not to be derived from the Tamul letters? It is true the figure for the group of a hundred is not to be found among these letters, but the sign of the group of ten is to be recognized in the letter *ya*, and the *two* in the letter *u*. The numerical figures in the *Teloogoa* language, likewise spoken in the southern districts of India within the Ganges*, which indicate value by position, are in a strange manner different from all other Indian figures, as far as they are known, in the signs for 1, 8, and 9, whilst they agree with them in the figures of 2, 3, 4, and 6. The necessity of expressing quantities by writing has probably been felt sooner than that of writing words, and therefore we may consider numerical figures as the most ancient written characters.

The instruments of *palpable arithmetic*, as they are called in an ingenious work, the Philosophy of Arithmetic, by Mr. Leslie, (1817,) in opposition to the *figurative* or *graphic*, are both hands, heaps of pebbles (calculi, ψῆφοι), grains of seed, loose strings with knots (arithmetical strings, the quippos of the Tartars and Peruvians), the suanpan put in a frame, the table of the abacus, and the calculating machine of the Slavonian nations with balls or grains of seed in files. All these instruments furnish to the eye the first graphic notations of groups of a different degree. One hand, or a string with knots or moveable balls, indicates the unities up to 5, or 10, or 20. How often, by shutting of the single fingers, one hand is gone through (πεμπάζεσθαι), is indicated by the other hand, of which every finger, that is, every unit, expresses a group of five. Two loose strings with knots stand in the same relation to one another. The calculation-strings, with moveable balls, extended and fixed in a frame, or the ancient Asiatic suanpan, which, in very ancient times, (perhaps by the Egyptians at the time of the Pythagorean league,) was brought to the

* Campbell's Grammar of the Teloogoa language, (Madras,) 1816. p. 4. 208. This is what formerly, but falsely, was called the Gentoo language. By the natives it is named *Trilinga*, or *Telenga*. With the table of the numerical figures in Campbell's grammar, other varieties of Indian numerical characters to be found in Wahl's General History of the Oriental Languages, 1784, Tab. I, may be compared.

nations of the west as abax or tabula logistica, are used in the same manner, only that the strings indicate groups of second, third, and fourth order upwards and downwards. The *kouas*, which are more ancient than the characters used at present by the Chinese, and even the magic drawings (ralm) of central Asia and Mexico, which exhibit knotty parallels, often broken off, almost in the manner of musical notes, seem only to be graphic projections of these calculation and reflection-strings *.

In the Asiatic suanpan, and in the abacus, which was much more used by the Romans, on account of the inconvenient figures adopted by them, than by the Greeks, who had been much more successful in their mode of writing numbers †, the quinary rows are preserved together with the denary ones, forming geometrical progressions upwards and downwards. Outside of every calculation-string, indicating a group or order, (n, n^2, n^3,) a shorter string was placed, on which every ball expressed the amount of five balls of the longer string. By this contrivance the number of the unities was diminished to such a degree, that the principal string needed only to contain four balls, and the accessary only one ‡.

It seems that among the Chinese, from the most ancient times, the custom had prevailed to consider arbitrarily any one of the parallel strings as containing the units, and that thus they obtained, upwards and downwards, decimal fractions, entire numbers, and powers of ten §. How late (in the

* In the East, *ralm* is called the *negromantic art of the sand.* Uninterrupted lines, and others broken off, present the elements of it, and direct the negromant. (Richardson and Wilkins, Diction. Persian and Arabic. 1806. t. i., p. 482.) In Dresden a remarkable manuscript is preserved. It was brought from Mexico, and exhibits nothing but figures like musical notes. I have published it in my Monumens Améric. Pl. xliv. When at Paris, I was visited by a learned Persian, who, at the first view, recognized in it an oriental *ralm*. Quite similar, and truly American kouas and linear drawings, I discovered after that time in some Aztetic hieroglyphic manuscripts, and on the sculptures of Palenque, in the republic of Guatimala. In the ancient Chinese numerical figures, the sign of the group of ten is a pearl on a string, and evidently an imitative drawing of the quippu.

† Nicomachus, in Ast. Theologumena Arith. 1817, p. 96. In the financial system of the middle age, the account table (abax) became the exchequer.

‡ So the Roman abacus was contrived. In China five balls were placed on the first, and two balls on the last. The balls, which were not employed in a calculation, were pushed aside.

§ On the first attempts to establish the decimal system, made by Michael *Stifelius* of Eslingen, *Stevenus* of Brugge, and Bombelli of Bologna, see Leslie, Philos. of Arithmetic, p. 134.

beginning of the sixteenth century ?) has the knowledge of the decimal fractions been introduced into the countries of the West, which the nations of the East had been taught long ago by their palpable arithmetic ! The descending scale from the unit downwards, was known to the Greeks only in the sexagesimal system for degrees, minutes, and seconds ; but as they had not $n-1$, that is, 59 numerical figures, the value by position could only be expressed by layers of two figures.

If we direct our attention to the origin of numbers, we find that they could be written and read with great exactness, when they were indicated by heaps of pebbles, or by the balls on the strings of the calculation-machines. The impression which these proceedings left behind on the mind, has everywhere affected the manner of writing numbers. In the historical, ritual, and negromantic hieroglyphics of the Mexicans, published by me, the units up to nineteen (the first simple figure for a group is twenty) are exhibited as great round coloured grains, and, what deserves to be mentioned, they are counted from the right to the left hand, like the Semitic writings. This is evident in 12, 15, 17, where the first row contains ten, and the second is not quite filled up. In the most ancient Greek monuments, and in the Tuscan sepulchral inscriptions, the units are expressed by vertical lines ; the same custom prevailed among the Romans and Egyptians (which, respecting the last, has been proved by Thomas Young, Jomard, and Champollion). The Chinese use horizontal lines up to the number four ; and such lines are likewise found in some Phœnician coins described by Eckhel (t. iii., p. 410). The Romans sometimes omitted the quinary figure in inscriptions, and, therefore, we find even eight lines as unities placed together. Many instances of this kind are collected by Marini, in his *Monumenti dei Fratelli Arvali**, a work which deserves attention. The heads of the nails, which anciently were employed by the Romans to indicate the years (annales antea in clavis fuerunt, quos ex lege vetusta figebat prætor maximus, says the elder Pliny, vii. 40.) could have led them to the unity-points of the Mexicans, and, in fact, we find such points

* T. i., p. 31. ; t. ix., 675—for instance in Octumvir.

used in the subdivision of ounces and feet, together with the horizontal lines (used by the Chinese and Phœnicians*). These points and lines, nine or nineteen in number, in the denary or vicesimal scale of the old and new continent, are the most simple of all notations in the system of juxtaposition. Here the unities are properly more counted than read. The separate existence, the individuality, if I may say so, of the numerical figures as signs of numbers, is first to be recognized in the numerical letters of the Semitic and Hellenic tribes, and among the inhabitants of Thibet, and the Indian tribes, who express 1, 2, 3, 4, by ideographic, distinct figures. In the ancient Persian Pehlwi a remarkable transition is to be observed from the inartificial method of expressing numbers by the repetition of the figure of the unity, to that of using compound ideographic hieroglyphics, in numbers greater than the unity. There the first nine figures are evidently formed by as many notches or teeth as they contain unities; five and nine are even merely the numbers 2, 3, and 4 twisted together, without the repetition of the figure of *one*. In the system of the *Devanagari*, which is truly of Indian origin, in the Persian and Arabic-European figures, we are only able to discover a contraction of 2 and 3 units, in the figures of 2 and 3 †, certainly not in the higher figures, which in India within the Ganges are written very differently from one another.

As I mention here the *Indian* numerical figures, and shall be obliged to do it frequently in this essay, I feel myself bound to make some observations on this expression. At the same time I shall take the opportunity of declaring myself against the old prejudices, that in India only one set of numerical figures are employed in expressing numbers, and never alphabetical letters in their place, and that in every district of that extensive country, a knowledge of a system, a different value to the different position of the figures is met with, likewise that there never are peculiar figures used to indicate the groups. As, according to what has been repeatedly said by my brother William von Humboldt, the Sanscrit is not

* Marini, t. i., p. 228.

† Abel Remusat Langues Tartares, p. 30. On the strange numerical figures used in Java, see Crawfurd, vol. ii., p. 263.

well distinguished by the name of "Indian and ancient-Indian language," because in that country many more very ancient languages are found which do not derive their origin from the Sanscrit; so likewise the expression, "Indian and ancient-Indian figures," is too indefinite, not only as far as it regards the form of the figures, but also respecting the spirit of the methods. For in India the principal groups of n, n^2, n^3, and their *multipla*, $2n$, $3n$ . . . . . are sometimes expressed by juxtaposition, sometimes by coefficients, and sometimes merely by the place of the figures. Even the existence of a distinct figure for the cypher, is, in the Indian system, no necessary condition of the method of expressing value by position, as it is proved by the scholion of the monk Neophytus. In India within the Ganges the most extended languages are the Tamul and the Teloogou. The tribes who speak the first use figures different from their alphabet, of which only two, the two and the eight, exhibit a slight similarity with the Indian (Devanagari) figures of two and five*. Much more different from the Indian figures are those of the Cingalese†. In both the Tamul and the Cingalese languages the different value of the figures is not indicated by position, they have also no distinct figure for the cypher, but distinct hieroglyphics for the groups n, n^2, n^3. . . . The Cingalese make use of juxtaposition, the Tamuls of coefficients. In India without the Ganges, in the empire of the Burmese, we find the value expressed by position, and a distinct figure for the cypher, but the figures used by them do not resemble the Arabic, Persian, and Devanagari Indian figures‡. The Persian figures, used also by the Arabs, are all of them quite different from the Devanagari figures§; 7 is like a Roman 5; 8 like a Tuscan 5. Among the figures which we call Arabic, only 1, 2, 3, resemble the figures of the Devanagari of the same value; the 4 of the Devanagari is our 8; our 9 is the 7 of the Devanagari. Our 7 is the Persian 6.

* Robert Anderson, Rudiments of Tamul Grammar, 1821, p. 135.

† James Chater, Grammar of the Cingalese Language. Colombo, 1815, p. 135.

‡ Carey, Grammar of the Burman Language, 1814, p. 196. Only the Burmese figures of 3, 4, and 7, resemble in some manner those of 2, 5, and 7.

§ Compare John Shakespear, Grammar of the Hindustani Language, 1813, p. 95, and Pl. I. William Jones, Grammar of the Persian Language, 1809, p. 93. Silvestre de Sacy, Grammaire Arabe. Pl. VIII.

In Bengali the 5 is expressed by the figure of a crescent, and 2, 5, 6, 8, and 9 are quite different from the Devanagari figures *. The numerical figures of Guzerath are only distorted Indian Devanagari figures †.

I shall make no observation on the influence of the earliest numerical figures on the form of the alphabetical letters, nor of the distortion of the letters purposely introduced in order to distingush them from the numerical signs, nor even on the difference of the place which a figure used in both respects occupies sometimes (as in the aboudjet of the Semitic tribes in Asia and Arica) ‡. Such observations do not belong to the subject of this essay, and have been the origin of many groundless hypotheses in comparing the alphabetical letters with the numerical hieroglyphics. I myself was once of the opinion that the Indian figures, notwithstanding the form of two and three, were the letters of an obsolete alphabet, of which yet some traces are found among the Phœnician, Samaritan, Palmyrian, and Egyptian characters, (the last on the mummies). Even the old Persian monuments of *Nakshi Rustan* seem to exhibit them §. How many characters in these inscriptions resemble in a striking manner the numerical figures known under the name of Indian! Many other scholars have likewise asserted, that the numerical figures called the Indian, are derived from the Phœnician alphabet ||, and the sagacious Eckhel has already observed that the similarity between the letters of the Phœnicians and the numerical figures was so great, that the word *Abdera* is expressed by 19990, and by 15550 ¶. But the origin of the numerical figures, as well as that of the alphabetical letters, is enveloped in an obscurity, to dissipate which, by a philological investigation, founded on historical facts, is rendered impossible by

* Graves Chamney Houghton, Rud. of Bengali Grammar, 1821, p. 133.

† Robert Drummond, Illustrations of the Grammatical Parts of the Guzerath and Mahratt Language, 1818, p. 25.

‡ Silvestre de Sacy, t. i., p. 10.

§ Silvestre de Sacy, Antiquités de la Perse, Pl. I., n. 1. Compare the numerical inscriptions on Mount Sinai, in Description de l'Egypte, vol. v. Pl. LVII.

|| Guyot de la Marne in Mem. de Trevoux, 1736, p. 160; 1740. Mars, p. 269. John, Bibl. Archeol. b. i., p. 479. Büttner, vergl. Tafeln, 1769, St. i., p. 13. Eichhorn, Einleitung in der alte Testam. b. i., p. 197. Wahl, Geschichte der Morgenl. Litteratur, p. 601, 630. Fundgruben des Orients, b. iii., p. 87.

¶ Doctrina Numerorum veterum, 1794, t. iii. p. 396—404, 421, 494.

the scarcity of the materials, if we do not wish to content ourselves with a few negative results.

We have seen that some nations, in expressing numbers by writing, mix together alphabetical letters and ideographic figures arbitrarily chosen. Likewise we find, that respecting the mode of expressing the *multipla* of the fundamental groups the most heterogeneous methods are used. We discover even that one system completely developes what, in another, is only slightly indicated. The same incongruity obtains in languages. In one language, some grammatical forms do appear only in a few instances, and are slightly expressed, whilst another has developed them with a peculiar predilection, and with every effort of mental power. Should I, therefore, explain the numerical systems singly, as they are used by different nations, the similarity of their methods would be rendered obscure, and we should lose the track on which the human mind proceeding, at last arrived to discover the masterpiece of the Indian arithmetic, in which every figure has a double value, an absolute and a relative, of which the last is increasing, in a geometrical progression, from the right to the left. In my following observations I shall therefore abandon the ethnographical order, and only consider the different means employed by nations to express in writing the groups of the units.

First method. *Juxtaposition* is effected by simple addition in numerical figures as well as in alphabetical signs. It was in use among the ancient Tuscans, the Romans, among the Greeks only up to a myriad, among the Semitic tribes, the Mexicans, and also in the greatest part of the Pehlwi calculations. This method renders the computation extremely difficult, when the *multipla* of the groups ($2n$, $3n$, $2n^2$ . . . .) are not expressed by distinct signs. The Tuscans and the Romans repeated the figure of ten as far as fifty; the Mexicans, whose first figure of a group was 20 (a flag), repeated this hieroglyphic up to 400. The Greeks, however, have in the rows of the tenths and hundreds, which begin with *iota* and *rho*, distinct figures for 20, 30, 400 and 600. The three *episemes* (letters of an obsolete alphabet), *bau*, *koppa*, and *sanpi*, serve to express 6, 90, and 900. The two last terminate the rows of the tenths and the hundreds, and in this manner the numerical value of the Greek alphabetical figures approaches a little nearer to the semitic *aboudjed* *. M. Böckh, in his learned observations on the digamma, has shewn that *bau* is the *wau* of the Semites, (the F of the Latins,) *koppa*, the Semitic *koph* (9), and *sanpi* the Semitic *shin* †. The row of the unities beginning with *alpha*, and ending with *theta*, forms in the Greek system the oeot-numbers (πυθμένες), and Apollonius had invented a contrivance ‡, by the help of which they were reduced, in the last results, to the corresponding members of the second and third row (the analogues).

SECOND METHOD. *Multiplication or diminution of the value by signs placed over or under the figures.* In the fourth row of the Greek notations, the *pythmenes* return by analogy, but increased a thousand times by a line placed under the figures. In this way the Greeks arrived, in their numerical system, at a myriad,—they wrote every number up to 9999. Had they adopted this notation with a line for all the groups, and suppressed all the figures after *theta* (9), the letter β with one, two or three lines would have expressed 20, 200, and 2000, and thus the Greek system would have approached, as we shall see afterwards, the system of the Arabic *Gobar* figures, which is very little known, and at the same time the system expressing value by position. But, unhappily, the Greeks did not adopt this notation for the tenths and hundreds, applied it only for the thousands, and did not try to employ it in higher groups.

As a line added under the figures increases their value a thousand times, thus a vertical line, in the Greek system, added over the figure, indicates a fraction, whose numerator is the unit, and whose denominator is expressed by the figure itself. Thus in Diophantus, $\gamma' = \frac{1}{3}$, $\delta' = \frac{1}{4}$; but if the numerator is greater than the unity, it is expressed by the principal figure, and the denominator is written like an exponent $\gamma^{\delta} = \frac{3}{4}$ §. In

* Hervas, Aritmetica delle Nazioni, p. 78. On the ancient order of the letters in the Semitic alphabet see Description de l'Egypte moderne, t. ii. P. ii. p. 208.

† Staatshaushaltung der Athener. B. II. p. 385.

‡ Delambre, Hist. de l'Astronomie Ancienne, t. ii. p. 10.

§ Delambre, t. ii. p. 11. The line added over the alphabetical letters to indicate that they are used as numerical figures, ought not to be confounded with the sign of fractions. The first is also never vertical in the oldest manuscripts of mathethe Roman inscriptions, a horizontal line added over them, increases their value a thousand times, and here it may be considered only as a means of abbreviating and of saving space.

The method of *Eutocius* for expressing myriads is more important. In it we find among the Greeks the first trace of the system of exponents, or rather indicators, which rose to such a degree of importance in the East. M^{α}, M^{β}, M^{γ}, indicate 10,000, 20,000, 30,000. Here we find these indicators used only with the myriads. But the Chinese and the Japonese, which last received their civilization from the first about 200 years before our æra, both use them for the *multipla* of all groups. Three horizontal lines under the figure of ten, signify thirteen, but if they are placed over it they express thirty. According to this method, 3456 is written, (I use the Roman figures as signs of groups, and the Indian as exponents)

$$M^{3}$$
$$C^{4}$$
$$X^{5}$$
$$I^{6}$$

Among the Egyptians the same kind of indicators are found. Two or four unities placed over a curved line, which denotes a thousand, are used to express 2000 or 4000*. Among the Aztekes, or Mexicans, I found for 312 years the sign of the *vinculum*, with six unities as exponent $(6\times52=312)$, and I have published it in my work of the American Monuments. Among the Chinese, Aztekės, and Egyptians, the signs of groups are always under the exponent, as if $\overset{5}{X}$ were written instead of 50; but in the Arabic *Gobar* figures the signs of the groups are placed over the indicators. For in the Gobar system the signs of the groups are points, consequently cyphers; for in India, in Thibet, and in Persia, cyphers and points are identical. These Gobar signs, which, since 1818, have attracted

matical works, but horizontal, and thus the mistaking of it for the sign of fraction is obviated. Bast. de Usu Litterarum ad Numeros indicandos, in Gregorii Corinthii Liber de Dialectis Linguæ Græcæ. 1811. p. 850.

* Kosegarten, de hierogl. Aegypt. p. 54. The assertion of Gatterer adopted by him from Bianchini (Dec. 1, cap. iii., p. 3), from Goquet (vol. i. p. 226), and from Debrosses (vol. i. p. 432), that among the Egyptians the figures received value by their position in a perpendicular row, has not been confirmed by modern researches. Gatterer, Weltgeschichte bis Cyrus, p. 555, 586.

my peculiar attention, were discovered in a manuscript in the library of the old Abbey of St. Germain du Près, by my friend and instructor, M. Silvestre de Sacy. This great orientalist says, "Le *Gobar* a un rapport avec le chiffre indien, mais il n'a pas de zero*."

To me, however, it seems, that the figure of the cypher there is found; it is, however, not placed aside the figures, but over them, as in the scholion of Neophytus. It is, indeed, the signs of the cypher or the points, which have caused to be given to these characters, the strange name of *gobar* or *dust* characters. He who sees them for the first time is doubtful whether they represent a transition from figures to letters, or not. It is only with pain, that the Indian 3, 4, 5 and 9 can be distinguished. *Dal* and *ha* are, perhaps, the Indian figures of 6 and 2 distorted.

The indication by means of points is thus effected:—

3 instead of 30,
4.. instead of 400,
6∴ instead of 6000.

These points recall to our memory a mode of notation used by the Greeks †, but not frequently met with, and beginning only with the myriad. Here, $\alpha^{\cdot\cdot}$ is used for 10,000, and β:: for 200 millions. One point, which, however, is never employed, serves to express 100 in this system of geometrical progressions. In Diophantus and Pappus, a point is found between the alphabetical figures, instead of the initial Mυ (Myriad). In this method, therefore, a point multiplies to figures to the left 10,000 times. It would seem that some obscure ideas of notation, by points and cypher, had been brought from the East into Europe by the Alexandrines. The figure of the cypher is, indeed, used by Ptolomæus, and even as an indication of something that is wanting. He employs it in the descending sexagesimal scale to indicate the wanting degrees, minutes, and seconds. Delambre even pretends to have found the figure of the cypher in the manuscripts of *Theon*, in his commentary on the Syntaxis of Ptolomæus‡. The cypher,

* See Gramm. Arabe, p. 76, and the observations added to pl. viii.

† Ducange, Paleogra, p. xii.

‡ Histoire de l'Astronomie Ancienne, t. i., p. 547; t. ii., p. 10. The passage of Theon is not to be found in his printed works. Delambre is inclined to attribute the origin of the Greek figure of the cypher sometimes to an abbreviation of οὐδὲν

consequently, was known in the west long before the invasion of the Arabs *.

We have seen, that the Chinese, by placing indicators perpendicularly over or under the groups, indicate the difference between $\underset{2}{X}=12$, and $\overset{2}{X}=20$. The same effect is produced among the Greeks, Armenians, and those tribes of the Hindoos, who speak the Tamul language, by adding figures in horizontal direction. Diophantus and Pappus wrote $\beta M \upsilon$ for twice ten thousands, whilst $\alpha M \upsilon \beta$ (where β is placed to the right of the initials of the myriad) signifies ten thousands, plus two or 10,002. In the same manner, the Tamul figures are used, as, for instance: 4X=40, and X4=14. In the ancient Persian Pehlwi, according to Anquetil, and among the Armenians, according to Cerbied †, multiplicators employed to express the *multipla* of the hundred, are found placed to the left. To these examples may yet be added, the above-mentioned point of Diophantus, which is used instead of $M \upsilon$, and multiplies the figures to the left a thousand times. For, as far as regards the method, it belongs to this class ‡.

Fourth Method. — *Multiplication and diminution, in ascending and descending direction, brought about by dividing the figures in layers, of which the value decreases in geometrical proportion.*

Both Archimedes in the Octades, and Apollonius in the Tetrades, make use of this notation, but only in numbers above $(10{,}000)^2$, and in a hundred of millions, or a myriad of myriads ‡. Here, as well as in the descending sexagesimal scale

sometimes to a peculiar relation in which the letter *omicron* stands to the sexagesimal system, l. c., t. ii., p. 14, and Journal des Savans, 1817, p. 539. It is strange, that in the old Indian arithmetic of the Lilawati, the figure of the cypher, placed aside a number, indicates, that such a number is to be subtracted. Delambre, vol. i., p. 540. What does the Ling (the true cypher) signify, which in Chinese figures is written under 12, 13, 22, 132? In Greek inscriptions the signs of cyphers indicate oboles. (Böckh, Staatshaushaltung der Athener, b. ii., p. 379.)

* Planudes, Treatise on the *Arithmoi Indikoi, third method, Multiplication of the Value by Coefficients.*

† Grammaire Arménienne, 1825, p. 25.

‡ This mode of separating numbers by points, though it is otherwise used in a very inconsistent manner, expresses properly the value by position. It is also used in three passages of the elder Pliny (vi., 24, 33; xxx., 3), which have given rise to many controversies.

§ Delambre, Hist. de l'Astron. Ancienne, t. i., p 105; t. ii., p. 9.

of the astronomer of Alexandria, where degrees, minutes, and seconds are indicated, the value is evidently expressed by the position of the figures. They follow one another in different layers, and thus, they express an absolute and a relative value. But, as in the last mentioned scale, every layer is composed of two figures (for want of $n-1$ or 59 figures); the value expressed by position does here not procure the advantages accruing from the Indian figures. When the three hundred and sixty parts of a circle are considered as so many entires, the minutes are sixtieths of them, the seconds are sixtieths of the minutes, &c. Considering them as fractions, Ptolomæus distinguished them by the sign of fraction, a line placed above, and, in order to express their descending progression, by which every layer of two figures has sixty times less value than the preceding, the fraction-signs were increased in number from layer to layer. According to these principles, the minutes are indicated by one line, the common designation of fractions (the numerator of which is the unit), the seconds by two such lines, the terces by three; but, the degrees themselves, as being entires, were not distinguished by a line, but, perhaps, by nought (οὐδέν), or a cypher *—I say, perhaps, for in the writings of Ptolomæus and Theon, the figure of the cypher is not yet used to indicate degrees.

The simple enumeration of the methods used by those nations who did not know the Indian system of position, in expressing the *multipla* of the fundamental groups, shews, in my opinion at least, the way in which the Indian system probably has been invented by degrees. When the number 3568 is written either in a perpendicular or in a horizontal direction, by means of indicators: $\overset{3}{\mathrm{M}}\,\overset{5}{\mathrm{C}}\,\overset{6}{\mathrm{X}}\,\overset{8}{\mathrm{I}}$, it is evident that the figures of the groups M C, &c., may be omitted. For our Indian figures are only the multiplicators of the different groups. This mode

* On the use of the cypher, *see* Leslie, p. 12, 135; Germanen und Griechen Hist., v. ii., p. 2—33; Ducange, Glossar. Mediæ Græcitatis, t. ii., p. 572. Mannert de Nummorum quos Arabicos vocant Origine Pythagor. p 17. In the Greek arithmetic M_0 signifies the unity; *μόνας*, as a delta (Δ), when a cypher (properly omicron) is placed over it, signifies *tetartos*. Bast, Gregor., Corinth. p. 851. Thus, we find in Diophantus, $M^{o}\kappa\alpha = 21$. The Indian grammatical sign, *anuswara*, has indeed the figure of the Indian cypher. It indicates, however, nothing but a modification in the accentuation of the vowel placed nearest to it, and is in no way connected with the *sunya*.

of expressing every number only by unities (multiplicators) is moreover suggested by the *suanpan*, of which the strings indicated the thousands, hundreds, tenths and unities in fixed order. To express the above-mentioned number the strings contained 3, 5, 6 and 8 balls. No sign of groups is here to be observed. The places themselves supply the signs of groups, and these places (the strings) are filled up with the unities (multiplicators). Thus the Indian system may have been invented in either way, by the *figurative* as well as by the *palpable* arithmetic. If a string was empty, or in writing a layer was not occupied by a figure, if consequently a group (a member of the progression) was wanting, the empty place in writing was filled up by the hieroglyphic of emptiness, a circle open in the middle, sunya, sifron, cyphra*.

That the notation of numerical quantities has been improved and brought to perfection in India only by degrees, is proved by the numerical figures of the Tamul language, in which every numerical quantity is expressed by the nine figures of the unities, by distinct figures for the groups of 10, 100, and 1000, and by multiplicators added to the left hand. The same is also proved by the remarkable *arithmoi indikoi*, in the scholion of the monk, Neophytus, which is preserved in the library of Paris (Cod. Reg. fol. 15), and was communicated to me by the kindness of Professor Brandes. The nine figures of Neophytus are, the four excepted, all of them like those of the Persians. The figures of 1, 2, 3, and 9 are even found in some Egyptian inscriptions containing numbers †. The unities are multiplicated, a ten, a hundred, and a thousand times, by writing over them one, two, or three cyphers, as $\overset{\circ}{2}=20$, $\overset{\circ}{2}4=240$, $\overset{\circ\circ}{5}=500$, $\overset{\circ}{\overset{\circ\circ}{6}}=6000$. If, instead of cyphers, *points* are used, the Arabic *Gobar* figures are obtained. I shall give here a literal translation in Latin of the scholion itself, and observe only, that the monk erroneously calls the expression tzüphron an Indian word.

* In English, the expression cypher for nullity has been preserved; whilst, in the other languages of the west, this word is used to indicate the numerical figures in general; in them the cypher is called zéro (sifron, siron). According to Wilson any numerical quantity is called in Sanscrit *sambhara*.

† Kosegarten, p. 54.

Tzyphra est et vocatur id, quod cuivis litteræ inde a decade et insequentibus numeris quasi ōμικρὸν inscribitur. Significat autem hac Indica voce tale analogiam numerorum. Ubi igitur scriptum est simile primæ litteræ ἄλφα, pro unitate scriptæ, atque superimpositum habet vel punctum, vel quasi ōμικρὸν, addita altera figura litteræ Indicæ, differentiam et augmentum numerorum declarat. E. g. pro primo Græco numero, ᾱ scripto, apud Indos | sive linea recta perpendicularis, quando non habet superimpositum punctum vel ōμικρὸν, ipsum hoc denotat unitatem, ubi vero superimpositum sit punctum atque altera littera adscripta sit, figura quidem similis priori, significat XI, propter additamentum similis litteræ atque superimpositum unum punctum. Similiter etiam in reliquis litteris, quemadmodum adspectus docet. Si vero plura habet puncta, plura denotat. Quod intelligas, lector, et supputes unumquidque.

In this system we do not find that value is expressed by position any more than in the system of the *Gobar* figures. The number 3006 was written $\overset{\substack{\circ\\\circ\circ}}{3}6$. But in using it, it must soon have been observed, that the same figures often expressed different value, and that (when all the groups were filled up) in $\overset{\substack{\circ\\\circ\circ}}{3}\ \overset{\circ\circ}{4}\ \overset{\circ}{6}\ 7$, the points or cyphers, by decreasing regularly in number, became superfluous. The cyphers, as it were, served only to facilitate the pronouncing of the number. If we now suppose that the custom of writing the cyphers *aside* the figures instead of placing them *above*, became prevalent, the Indian notation as used at present, was introduced for the unmixed groups, as $\overset{\substack{\circ\\\circ\circ}}{3}=3000$. If further, to $\overset{\substack{\circ\\\circ\circ}}{3}=3000$ were to be added $\overset{\circ}{4}=40$, that place of the cypher was filled up which was assigned to 40 by the exponent indicating the group. Thus 3040 was obtained, and two of the three cyphers, which were required to express the thousands, and which had previously been placed on *one* line with the unities, remained there to indicate the empty places. According to the scholion of Neophytus, therefore, the figures of the cypher are (like the points over the *Gobar* figures) indicators for the notation of the ascending groups. From the observations made on this system it is easy to perceive, how the cyphers have been placed in the row of the figures, and have preserved that place, when the value by position was adopted.

In reviewing once more the different methods used by the different nations of both continents in computing numbers, which till now have been in part so little known, we find, *firstly*, in some only a small number of figures indicating groups, and those almost only for n^2, n^3, n^4 .... not for $2n$, $3n$, and $2n^2$, $2n^3$ .... as among the Romans and ancient Tuscans *, X, C, M, and, therefore all the intermediate groups, for instance, $2n$ or $2n^2$, are to be expressed by juxtaposition, as in XX or CCCC; we find further, in others, a great number of figures of groups, not only to express n, n^2, (*iota* and *rho* among the Greek alphabetic figures) but also to express $3n$ or $4n^2$ (in λ and υ), by whose application a great heterogeneity of the elements is produced in expressing $2+2n+2n^2$ (for instance, σκβ for 222); we find lastly, that the multipla of the fundamental groups and their powers ($2n$, $3n$, $4n^2$, $5n^2$,) are, by others, expressed either by the addition of indicators over or under the figures of the groups (by the Chinese $\overset{2}{\text{X}}$, $\overset{3}{\text{X}}$, $\overset{4}{\text{C}}$, $\overset{5}{\text{O}}$, by the Hindoos speaking the Tamul language 2X, 3X, 4C, 5C,) or by placing over the figures of the first nine unities a progressive number of points, that is $\dot{\alpha}=10$, $\dot{\beta}=20$, $\ddot{\alpha}=100$, $\overset{\therefore}{\alpha}=1000$, $\overset{::}{\delta}=40,000$, as in the *Gobar*-figures, in the scholion of Neophytus, and in the descending sexagesimal scale of the astronomers of Alexandria, for $\frac{1}{60}$, $\frac{1}{60^2}$, $\frac{1}{60^3}$, in $1° \, 37' \, 37'' \, 37'''$ .... We have seen in what manner the indicators (multiplicators) used by the nations of Eastern Asia, and by the inhabitants of the southern districts of India within the Ganges, or, where originally figures of groups did not exist, in what manner the placing of points over the *pythmenes* in the *Gobar*-system and in the scholion of Neophytus; and lastly, in what manner even the strings of the suanpan, in which different value is expressed by the

* For brevity's sake I here take no notice of the figures of the groups of the *quinary* system (V, L, D...) which form intermediate sections.

relative position of the strings, could lead men to invent that system, in which value is expressed by position.

Whether the simple Indian system, expressing value by position, was brought into the west by the learned astronomer, Rihan Muhammed eben Ahmet Albiruni*, who remained a long time in India, or by Moorish custom-house officers in the ports of North Africa, and their intercourse with the Italian merchants, I do not presume to decide. Further, though mental culture was doubtless very early disseminated in India, it remains doubtful whether the numerical system expressing value by position, which has so powerfully affected the progress of the mathematical sciences, had already been invented and adopted by that nation, when the Macedonian conqueror invaded their country. In how different a condition, in how much more perfect a state would the mathematical sciences have been transmitted to the learned epoch of the Hashimides by Archimedes, Apollonius of Perga and Diophantus, if the western countries of the old continent had received the Indian numerical system twelve or thirteen centuries sooner, at the time of Alexander's expedition. But that part of upper India, which was then overrun by the Greeks, the Penjab, as far as Palibothra, was, according to the learned researches of M. Lassen, inhabited by nations very little advanced in civilization. Those who lived farther to the east, called them even barbarians. Seleucus Nicator was the first who passed the river Sarasvatis, and by doing so, the limits that separated the civilized and uncivilized tribes; and then he advanced towards the Ganges†.

The old Indian numerical figures of the Tamul language, which express the quantities $2n$, $3n^2$ . . . . by the addition of multiplicators, and consequently besides the figures for the first nine unities, have distinct ones for n, n^2, n^3, ... prove evidently that, in India, besides that system which exclusively has obtained the name of Indian (or Arabic) figures, and in which value is expressed by position, there yet, at the same time,

* According to an observation of the orientalist Sedillot, not less acquainted with the Greek, than with the Arabian astronomy.

† Lassen, Comment. Geog. de Pentap., p. 58.

existed others, which did not express value by the same method. Now it may be the case, that Alexander and his successors in Bactria, in their temporary incursions, did not have intercourse with any tribes, among whom the knowledge of the system expressing value by position had then become prevalent.

I could wish that the traces of what is still to be discovered (and that is yet very much), might soon be pursued with increasing zeal, by philologists, who have opportunity of examining either Greek, Persian, or Arabian manuscripts*. The manner in which old manuscripts of the Sanscrit literature are paged, can sometimes bring us to important observations and discoveries. To give an instance, hardly any person would have expected to find in India, besides the decimal system with position, a sedecimal system without position. It seems, however, that some Indian tribes had adopted in their calculations groups of sixteen, as the natives of America, and the Gauls and Biscayans, those of twenty. For such a remarkable numeration has been discovered, more than ten years ago, by M. Bopp, in a manuscript of the old Indian poem, Mahabharata (Cod. Reg., Paris, p. 178). He had the kindness to communicate it to me for publication, when I laid before the *Académie des Inscriptions et Belles Lettres* my first essay on the numerical signs of the different nations. The first sixty-five pages are paged with Indian alphabetic letters, but only the consonants of the Sanscrit alphabet are used (*k* for 1, *kh* for 2....). This refutes the opinion till now generally prevailing, that the Hindoos always used ideographic figures to express numbers, and never alphabetical letters, as the Semitic tribes and the Greeks†.

On the sixtieth page begins the extraordinary sedecimal

* Among the Arabian manuscripts, those especially are to be recommended to peculiar attention, which treat of custom-house or financial affairs, or of arithmetic in general; as, for instance, Abu Jose Alchindus *de Arithmetica Indica;* Abdelhamid Ben Vasee Abulphadl *de Numerorum Proprietatibus;* Amad Ben Omar Alkarabisi *Liber de Indica Numerondi Ratione;* the Indian Algebra by Katka; Mohammed Ben Lara *de Numerorum Disciplina* (Casari Biblioth. Arabico-Hispana, t. i. p. 353, 405, 410, 426, 433.)

† Si l'arithmétique de position n'est pas originaire de l'Inde, elle doit au moins y avoir existé de temps immémorial; car on ne trouve chez les Indiens aucune trace d'une notation alphabétique telle que la notation des Hebreux, des Grecs et des Arabes.—(Delambre, Histoire de l'Astronomie Ancienne, t. i., p. 543.)

notation. Of the *pythmenes*, fifteen in number, hardly two figures are found among those we know. (The aspirated *t* of the Sanscrit alphabetical letters is used for 3 and *d* for 12.) They are likewise quite different from the Indian (Arabic) figures. It deserves to be noticed, that the figure of 1, with a cypher added to it, signifies four, as that figure doubled (two vertical lines), with a cypher added to them, signifies eight. They form, as it were, resting-places, intermediate landings, of the sedecimal system for $\frac{1}{4}$ and $\frac{1}{2}$ n. But, $\frac{3}{4}$ of n (12), is not indicated by a cypher, but by a peculiar hieroglyph, similar to the Arabic four. To express the principal fundamental group itself (16), and the *multipla* of it ($2\,n, 3\,n \ldots.$), the known Bengali figures are used; so that the Bengali 1, preceded by a curved line, signifies 16; the Bengali 2 is used for 32; the 3 for 48. The multipla of n are consequently indicated only as groups of first, second, third, ... order. The numbers $2\,n + 4$, or $3\,n + 6$, (that is 36 and 54 in the sedecimal system,) are expressed by the Bengali figure of 2, and the added Mahabharatan figure of 4, or by the Bengali figure of 3, and the added Mahabharatan figure of 6 *.

It is, indeed, a very regular, but at the same time a very inconvenient and complicated mode of counting, the origin of which is the more difficult to guess, as it presupposes the knowledge of the Bengali figures.

* I use here the expression Mahabharatan figure only for the purpose of indicating, with a proper word, the numerical system discovered in the manuscript of that poem.

Remarks on Snake-Poisons and their Remedies, by Dr. J. Hancock, *Corr. Memb. Zool. Soc., &c. &c.*

It is with the bites of serpents, as with that of the mad dog: hundreds of things are cried up as antidotes or remedies, because not one bite in ten takes effect, at least to prove mortal, and whatever is applied obtains the name of an antidote.

It has been determined, to a sufficient degree of certainty, that not more than one case of hydrophobia occurs in twenty instances, of the bites of rabid animals (see Oxley's Medical Journal, 1805, p. 33); and this refers to those only which have been proved to be really mad, independently of the vast numbers falsely represented as such.

Many harmless serpents are reported venomous; for instance, the parrot-snake (*Coluber viridissimus* of Lin.), because it resembles the poisonous Iguana serpent in its green colour; and also a small, but harmless snake, which is common at Demerara, called Labaria, and thought to be exceedingly malignant, being confounded with one of the same name which is truly so.

The coral snake, a species of amphisbæna, so called, is regarded by the Spaniards of the Orinoque as a serpent of deadly venom. I had one of these, four or five years ago, in my house. Finding it destitute of fangs, I allowed it to bite my hand, and it drew blood. Had it been a Venezuelean bitten, he would have resorted to the guaco; and after that, he would make the "sign of the cross," and swear, on every occasion, that he owed his life to *the guaco*, after being bitten by "el *corāl*— un serpiente mas venenosoa que hai en el mundo." I afterwards gave it to my children as a toy, and they carried it about coiled round their arms and necks, when it had the appearance of a bright coral necklace, intermingled with black bands.

Regarding this species of serpent, I may, by-the-by, refer to the Monthly Magazine for August, 1826, where the guaco is set forth by a traveller in Mexico, as "the best remedy for the bite of the coral-snake, which is considered mortal if not immediately cured"!

The reader will excuse these desultory observations. I allude to such misconceptions, from a persuasion that a love of truth will make him desirous of seeing such vulgar and pernicious errors exploded, as they tend continually to mislead and to chain us in error.

When a person is bitten by a venomous serpent, the first step should be to apply the mouth and suck the wound, and that immediately, without the least delay, which may be done by the patient or a companion; next, to open the wound freely, with a knife or lancet, by several cross cuts, and instantly suck it again as powerfully as possible, pinching up the skin and flesh deeply by the thumb and fingers of both hands, applied all round the wound, so as to impede the circulation through the part; then to fill the wound with common salt (*mur. soda,*) or with *sal nitre*, and the juice of any of the aristolochias or other stimulant substance at hand; but the suction ought to be continued for a considerable time, and a ligature should be applied above the wound.

This method is actually practised with the most decisive success amongst the Parinagotos, Macosies, and other tribes inhabiting the mountains of Sibaroni and Parime; and that, not only against the bite of the crotalus or rattle-snake, but that of the Quaima* and the Haimararia, which are considered still more deadly than the crotalus. These are almost the only Indians who will undertake to cure the bite of these serpents. It is certainly the most rational, and indeed the only method which can prove successful, as it usually is, when promptly applied, unless any of the larger vessels be punctured. In this case the venom may so speedily mix with the vital fluid as to preclude all remedies.

This operation, then, if promptly executed, will be sufficient;

* This is the most lethal, perhaps, of all known serpents, and the largest of the venomous tribe. It grows to ten or twelve feet in length, and has large poisonous fangs, *seven in number,* on each side the upper jaw. This serpent, when enraged, has the power of contracting its body to about one half its length, and swelling proportionately, which enables it to make a spring, or project itself suddenly forward: whilst in this posture, it presents one of the most horrid figures imaginable, with its enormous gape and projected fangs. It is the Crotalus mutus of Linné, mistaken by Spix, badly figured by Seba; and, although but vaguely known to the Swedish naturalist, is yet better characterized by him than any subsequent writer. It is called by the Arowak Indians of South America, Koonukusi, *i. e.*, bush-master, or master of the forest.

Two perfect specimens of this serpent may be seen at the Museum of the Zoological Society in Bruton Street.

but if some time has elapsed, we must, withal, strive to counteract the effects of absorption: to this end it is best to cause the patient to swallow a large dose of opium, together with wine or other alcoholic stimuli, and the juice of the buhyari, a bitter, odorous species of Aristolochia (Abuta amara of Aublet), as also, the trilobata, or Aristol (Abuta) or the infusion of other aromatic plants*. The warm bath is a potent auxiliary; and if sweat is not soon produced, we should bleed the patient whilst in the bath. If a warm perspiration comes on, the patient may, in general, be considered as out of danger. This method, which is partly deduced from personal experience, and partly from the usage of the natives, would seldom fail of success if promptly pursued, even in the worst cases, from the bites of the rattle-snake and *bush master*.

There can be no doubt that *hydrophobia* might, by the same means, be prevented with equal safety and success.

Many persons, unacquainted with the subject, and confounding all poisons, have expressed horror at the idea of applying one's mouth to such a dreadful poison as that of the bush-master. It would be folly to enter into a refutation of such an idle fancy. I will only observe, that although it could act on the mucous membrane of the mouth and fauces like arsenic, and with many times its force, still the small portion left in the wound, diluted with the blood, and with the saliva of the mouth, and spit out, could produce no sensible constitutional effects; but instead of acting as a caustic like arsenic, corrosive sublimate, &c., it exerts no action whatever on the mucous membrane.

I have often tried, on the tongue, the effect of snake-poisons, as well as that of the Worary †. They have, neither of them, any sensible acrimony, and may be swallowed with impunity. They prove lethal by inoculation only, or by immediate contact with the system of nerves and blood-vessels, through the medium of a wound.

* The siruba, or native oil of laurel, is considered by others of the native tribes as one of the most powerful alexipharmics taken inwardly, and applied to the wound.

† For some account of this poison, see page 50 of this Journal for September last, as respecting its proposed uses in hydrophobia and other convulsive disorders.

The application of cupping-glasses to poisoned wounds was proposed long ago, by Mr. Hume, of London. They would, doubtless, be very effectual, yet less so, perhaps, than the united action of the mouth and fingers, exerted instantly; and the apparatus can seldom or never be had at the moment required. The same mode of cure was directed by Celsus, and others of the ancients.

Since these observations were made, I have observed, with some surprise, this method again spoken of as a new discovery by an English gentleman in Paris, and in some of the Parisian and English journals, intermingled with some doubtful theories respecting the influence of the atmospheric pressure on the circulation of the blood.

On the subject of alexipharmics, I may observe, that those plants which are regarded as alexiterial, as antidotes or counterpoisons, are chiefly those eminently bitter, aromatic, and piquant, being the most powerful warm sudorifics. It is worthy of remark, and no slight proof of the ascribed virtues in such plants, that in all ages and countries, amongst civilized and savage people, we find similar virtues ascribed to plants of the above description. We know that the corymbiferæ afford many examples of this sort; but the guaco, although of this natural order, is almost destitute of the forenamed sensible properties.

When we consider the perverse bent, the erroneous tendency of the human mind, and the sources of fallacy and deception just hinted at, it will not appear strange, that, amongst a deluded, superstitious, and credulous people, the plant should obtain such unrivalled fame and be admitted as a sovereign remedy, after being so extravagantly lauded by so influential, so celebrated, and infallible a man as Don Celestino Mutis, at once a divine, a philosopher, and a botanist! I pretend not to any positive proofs against the antidotal powers of the guaco; but I assert that our evidences in favour of this reputed sovereign remedy are of a most suspicious character, and before it is worthy of implicit confidence, its powers should be duly investigated by direct experiments made for the purpose *.

* This, and several paragraphs here, are taken verbatim from a letter which I

Mutis, from whom Humboldt's account is chiefly taken, and almost copied verbatim (for I have the MS. of Mutis, or a copy of it), asserts indeed, that, by inoculation with the guaco, the system is rendered secure or unobnoxious to snake-poisons, for any length of time afterwards! but this exquisite item, we may imagine, the learned Baron thought rather *too good to be true;* for he has suppressed, or made no allusion to it.

Another thing worthy of notice here is, that the name of guaco, or bejuco de guaco, in the parts of Spanish America where I have sojourned, is almost exclusively applied to different species of aristolochia, and not to a composite or corymbiferous plant like the mikania guaco of Mutis. May not Mutis have mistaken this composite for the guaco of the people? If not, they have since, most of them, mistaken him! At all events, it is chiefly amongst the different species of *aristolochia* that the natives seek their antidotes.

I have just observed another account of the guaco (all from the same source, however), in the *Lancet* of the 10th of April—it is complete nonsense, although read before the College of Physicians.

The plant called raiz de mato is an aromatic bitter species of aristolochia. This plant is esteemed as a most certain remedy in the Orinoque and Venezuela, for the bites of the mapanary, the rattle-snake, and other venomous serpents. (See the *Lancet* of the 6th March for further particulars, and where I have related a case which fell under my own observation.)

The efficacy of this method, by suction and scarifying, and the inutility of all others usually resorted to, will, I hope, be considered a sufficient apology for brief repetitions which I have made on several occasions.

The maporiti chequita, also, is an herb, exhaling an odour

addressed, in 1827, to the Rev. Stephen Isaacson, A.M., Secretary to the Phil. Soc. of British Guiana; but, in January following, we had the most direct and convincing proofs of the total inefficacy of the guaco; the fresh herb was tried by several gentlemen of that Society, on fowls, dogs, and several other animals, bitten by the rattle-snake and labaria, in George Town. Not long after this an occasion presented for proving it on the human subject:—A servant of Captain Warren, of George Town, was bitten, whilst in the act of giving water or food to the reptile (*crotallus durissus*). The guaco was instantly procured, was given in large doses, and applied to the wound, but without the smallest effect. The man died in a few hours after the accident.—The results, therefore, have fully confirmed my anticipations respecting the value of the guaco as an antidote.

similar to that of the *maporiti* (polecat), warm and biting to the taste, famed against spasms, or cold and spasmodic disorders, and against the bites of snakes especially.

I may here observe, that the rattle-snake, it would seem, now constitutes one of the numerous articles of commerce from North America*. I read lately, in an American paper, a notice of an agreement for the supply of a certain number of *good merchantable live rattle-snakes!* I am told, that there are, or were, not long since, not less than one hundred in the Tower. What they are to do with such a number of these vile reptiles, whose bite is present death, I know not. I should think it not at all improbable that, in such a traffic, in moving them from the vessel and shifting them from one place to another, some of them might by chance get loose, and disseminate their species here †, the climate being perfectly congenial. They are found, I presume, as far north as Canada. I know they inhabit New Hampshire; a rocky elevation, near the town of Romney, in that State, is called Rattle-snake Mount, from the number of these reptiles found there, where the mean temperature is nearly the same as it is in England. The species found there is, I believe, the crotalus horridus, thought to be the most venomous of all.

On coming from Wiveliscombe, in October 1828, being with several gentlemen on the outside of the coach, and passing a little to the eastward of Taunton, I observed a gentleman's seat on the north side of the road, with a very neat and verdant plat of grass, and a pond of water. One of the passengers remarked, that near this pool, he had, some time previously, assisted in killing a *rattle-snake.* I thought this extraordinary, and inquired what kind of a snake it was. His description corresponded precisely with that of the *rattle-snake;* it had five or six joints or rattles at the tail, as it were, he said, *strung close together!* I regretted afterwards not having got his name or

* I think the traffic merits the attention of the Legislature, either to prohibit or impose a heavy duty upon it.

† From the carelessness of the sailors, the reptiles might be dropped overboard on the coast, or ascending the Thames, the Mersey, or the Severn, and find their way ashore; the serpent tribe, in general, being amphibious and good swimmers. I may notice, that the Colucunaru of the Arowaks (Boa Constrictor) is never found in the water; but if caught and thrown in, it swims with wonderful celerity.

other particulars, for, on mentioning it to some of the *knowing ones*, in London, they ridiculed the idea, as altogether fabulous, making me ashamed of my credulity; yet I see no impossibility in the story.

J. H.

Stafford-place, Pimlico.

Observations on the Relations which exist between the Force, Construction, and Sailing Qualities of Ships of the Line.

[Continued from p. 233 of the Quar. Jour., Part II., for 1829.]

As the comparative statements given in Tables I. and II. exhibit themselves sufficiently to the eye of the scientific constructor, we shall abstain from entering into any thing like an amplification of their contents; but in deriving Table III. from Table II. we must remark, that perhaps its most important result is the developement of the effects of large and small dimensions, on a ship of the line of a given force. It will be perceived, that in the 80-gun ship of Chapman, although its hull is less in weight than the hull of the French 80-gun ship by 182 tons, and carries 193 tons less ballast, it can only get the midship lower port 5.03 feet above the plane of floatation, with an English armament and equipment amounting to 1891 tons. On the other hand, the 110-gun ship of Chapman is a palpable instance of the advantageous operation of superior dimensions, together with a comparatively light hull: with a real tonnage, equal to that required even by the English 120-gun ship, it will carry the midship lower port 2.13 feet higher out of the water than the latter. The French 120 shows a similar advantage over the English 120 from similar causes; but even allowing it to have a hull of the same weight as that of the latter, and thus requiring an equal displacement with an English armament and equipment, it will carry the lower midship port 6.44 feet above the water, or 1.78 feet more than the English ship.

The English 84-gun ship, which is the same as the French 80*, is illustrative of the pernicious effects of increasing the

* The four additional guns are mounted in the captain's cabin, in which two ports on each side were generally left without guns by the French until lately.

weight of the hull without making alterations in the design, proper to meet the excess, granting it to be necessary. In fact, it cannot be too strongly impressed on the minds of those who make any material innovation in the *practical carpentry* of an excellent ship, that without a proper reference to the *theoretical design*, they will certainly neutralize all its former good qualities*. To make this more plainly appear, we may notice, that this class of ships, when constructed in the French dock-yards, will carry the lower port 6.46 feet above the water with an English armament and equipment, amounting to 1891 tons, whilst the same dimensioned and modelled ship, when built in our own dock-yards, can only carry the lower port 5.3 feet above the surface of the water, with the same real tonnage.

The Swedish 74 shews the good results of a light hull on a ship of nearly the same dimensions as those of the English 74, whose hull weighs 322 tons more than that of the Swedish ship.

We have said, that no ship of the line should have the port-sills of the lower battery a less height above water than 5.75 or 6 feet, we shall now state our reasons for such an opinion.—If a ship of the line be supposed to incline no more than 7° † with a wind whose force is two pounds avoirdupois on the square foot, and carrying top-sails, top-gallant-sails, driver, fore-top-mast stay-sail, and jib, we shall find that, in a first rate, whose breadth is 54 feet from out to out, the portion of the lee side submerged by the inclination will be about 3.25 feet. In the 84-gun ship, whose breadth is 52.5 feet, the part of the side submerged by the same degree of heeling will be about 3.15 feet; and in the 74-gun ship, with a breadth of 49.5 feet, the side will be submerged about 3 feet. Hence, by referring to the second table, the heights of the lee midship port of the lower battery will be nearly as follows, in the different con-

* The French naval architects have been so carefully observant in this particular, that the ships of the French navy built at Toulon are allowed somewhat greater dimensions than those of the same force built in the other dock-yards, in order to meet the circumstances attending the use of the timber of Provence and Italy, which weighs eighteen or nineteen pounds more in the cubic foot than that used at Brest, &c.—Vide " Instruction Elémentaire et Raisonnée sur la Construction Pratique des Vaisseaux, par M. De Duranti de Lerincourt: à Paris, 1771."

† We say, " no more," because, if a ship heels over more than 7°, she cannot make use of the windward guns with facility, and indeed cannot depress them to point blank.

struction of ships we have noticed, when armed and equipped for our service, and heeled over to an angle of 7°.

TABLE IV.

Of what Construction.	Height of Lee Portsill of the Lower Battery when heeled over 7°.		
	Three-Decker of 120 guns.	Two-Decker of 84 guns.	Two-Decker of 74 guns.
English . .	1.36 feet.	2.15 feet.	2.14 feet.
French . .	3.97 ..	3.30 ..	4.01 ..
Swedish . .	3.58 ..	1.88 ..	3.38 ..

And, if we suppose the foreign ships to have their original armaments, &c., the Table above will be altered to the following:

TABLE V.

Of what Construction.	Height of Lee Portsill of the Lower Battery when heeled over 7°.		
	Three-Decker of 120 and 110 guns.	Two-Decker of 84 and 80 guns.	Two-Decker of 74 guns.
English . .	1.36 feet.	2.15 feet.	2.14 feet.
French . .	2.53 ..	2.71 ..	2.86 ..
Swedish . .	3.06 ..	3.59 ..	3.65 ..

Now if the sea were perfectly smooth, in all these cases, no inconvenience would arise, even in the ship which has the *lowest* battery; but a top-sail gale will cause a considerable undulation, and in the British 120 we perceive, from the last table, that a wave of three feet high would be above the port-sills in midship, whilst the French 120 would allow of a wave of five feet, and the Swedish 110 a wave of six feet, without such an effect being produced. A similar advantage will be found to obtain, in the French and Swedish two-decked ships of the line, over those of our construction.

The preceding deductions are made on the supposition, that there is no lateral oscillation or rolling to affect the heights of the ports out of water when heeled over; but when there is such a motion, in addition to an agitated sea, the disadvantages of the ships with the lowest batteries will, of course, be aggravated. Hence we may form a pretty accurate idea of the difficulties which will obstruct, if not remedied, the path of victory to those placed in an English fleet of English constructed ships, when opposed to a French line of battle to leeward, in rough weather *.

As it is of great importance for a constructor to know how a variation in the armament of a ship may affect the weights of the ordnance equipment, not only in quantity but in position, and as this seems to be the natural place to introduce the means of acquiring such information, we have inserted the two following tables, which have been drawn up with much care, and may be relied on for as close an approximation to rigid accuracy as the necessity of taking the average weights of heavy ordnance will allow. The total weights are those incidental to the foreign service war-proportions of shot, &c. as the tables express; but if we wish to ascertain the total weights incurred by each calibre with the foreign service peace-equipment for ships of the line and frigates †, it may be very nearly approximated to by subtracting the weight of the gun and carriage from the total weight, in each case, and reducing the said total weight by one-fourth of the remainder so obtained, when the gun or carronade is mounted on the lower deck of a ship of the line; and by one-fifth the same quantity for all other guns and carronades used in these ships and in frigates.

* James, in his Naval History, relates the inconvenience suffered by the Queen Charlotte, of 100 guns, in the action of the 29th of May, 1794, her lower deck ports, being only 4.5 feet above the water, let in the water in such quantities as to keep the pumps at work during the greater part of the ensuing night. The Victory, of 100 guns, built in 1786, although formerly much esteemed in the English navy for sailing qualities, was, however, miserably deficient in this most vital particular; for even when equipped for home service, the midship-port of the lower battery was only 4.25 feet out of water. This ship, in consequence of her now admitted inferiority to the larger three-deckers of the present day, has been very recently ordered to be *razéed* to a two-decker—a very doubtful alternative; but a similar experiment inevitably awaits all ships of such contracted dimensions if retained in the service.

† The number of round shot per gun is in this case reduced from 80 to 60 for the lower deck ordnance of ships of the line; and from 100 to 80 per gun for all other ordnance mounted on ships of the line and frigates.

TABLE VI. *Weights incurred with each Calibre of English naval long Guns at present used, when equipped, in time of war, for foreign service.*

Deck, mounted on	Lower.			Main of Frigates.	Upper, and Quarter, and Forecastle.	Middle.	Main of Frigates.	Upper of Ships of the Line, and Main of Frigates.				Quarter and Forecastle.			
Calibre, in pounds avoir.	89	32	32	32	32	24	24	24	24 Congreve's	18	18	12	12	9	6
Length of gun, in feet	$6\frac{2}{3}$	$9\frac{1}{2}$	$9\frac{1}{2}$	$9\frac{1}{2}$	8	$9\frac{1}{2}$	$9\frac{1}{2}$	9	$7\frac{1}{2}$	9	8	9	$7\frac{1}{2}$	$7\frac{1}{2}$	6
Weight of ditto, in tons	2.5	3.2	2.8	2.8	2.375	2.5	2.5	2.4	2.0	2.06	1.9	1.69	1.44	1.29	1.08
Weight of gun and carriage, in tons	3.18	3.57	3.17	3.17	2.70	2.88	2.88	2.77	2.38	2.34	2.19	1.92	1.68	1.48	1.27
Total weight of gun, carriage, furniture complete; 80 round, and 3 grape, shot; proportion of powder, wads, and ordnance stores, in tons	6.66														
Total weight of gun, carriage, furniture complete; 80 round, 3 double-headed, and 3 grape, shot; proportion of powder, wads, and ordnance stores, in tons	....	5.51	5.11												
Total weight of gun, carriage, furniture complete; 100 round, 3 double-headed, and 4 grape, shot; proportion of powder, wads, and ordnance stores, in tons	....	....	....	....	....	4.63									
Total weight of gun, carriage, furniture complete; 100 round, 3 double-headed, 4 grape, and 4 case, shot; proportion of powder, wads, and ordnance stores, in tons	....	....	5.40	5.53	5.17	....	4.69	4.61	4.20	3.71	3.64	2.86	2.61	2.14	1.77

TABLE VII. *Weights incurred with each Calibre of English Ship Carronades at present used, when equipped, in time of war, for foreign service.*

Deck, mounted on	Lower.	Quarter and Forecas. of the largest Frigates.	Quarter and Forecastle of Ships of the Line and Frigates.		Main of small Frigates	Flush of Corvettes and large Brigs.	Flush, of small Brigs.		Poop of Ships of the Line.	Cutters, &c.
Calibre, in pounds avoir.	68	42	32	32	32	32	24	18	18	12
Length of carronade, in feet	$5\frac{1}{4}$	$4\frac{1}{3}$	4	4	4	4	$3\frac{2}{3}$	$3\frac{1}{3}$	$3\frac{1}{3}$	$2\frac{2}{3}$
Weight of carronade, in tons	1.8	1.12	.89	.89	.89	.89	.67	.52	.52	.31
Ditto of ditto, carriage and slide, in tons	2.3	1.5	1.23	1.23	1.23	1.23	.95	.73	.73	.46
Total weight of carronade, carriage, and slide, furniture complete; 80 round, and 3 grape, shot; proportion of powder, wads, and ordnance stores, in tons	5.77									
Total weight of carronade, carriage, and slide, furniture complete; 100 round, 5 grape, and 5 case, shot; proportion of powder, wads, and ordnance stores, in tons	....	4.04	3.58	3.58	3.58	....	....	....	1.87	
Total weight of carronade, carriage, and slide, furniture complete; 60 round, 6 grape, and 6 case, shot; proportion of powder, wads, and ordnance stores, in tons	....	....	....	....	....	2.99	2.01	1.54	....	1.04

By ascertaining the weights of the hulls, we are in possession of the average weight of hull per foot of the length; which, in the English 120, 84, and 74-guns, is respectively 12.3, 9.77, and 9.6 tons. Hence, from knowing these facts, and having the data furnished in Table II., we may venture with a considerable degree of certainty on the design of new classes of ships, similar in form and practical construction, but carrying armaments different, both in force and adjustment, to those at present used. Suppose it to be required to assign the displacement for a ship of 98 guns on *two* decks instead of three; viz.

On the lower deck . .	Thirty-two 32-pounder long guns. Two 68-pounder carronades.
„ upper deck . .	Thirty-six 24-pounder long guns.
„ quarter-deck and forecastle .	Eighteen 32-pounder carronades. Ten 24-pounder Congreve guns.

As the armament here proposed is similar to that of the English 84-gun ship, (Table II.) we shall have the real tonnage per gun, (exclusive of ballast,) 19.65 tons; ballast, per gun, 3 tons nearly; and the weight of hull, per foot of the length, 9.77 tons*. Now the length required to put 17 guns into battery on each side, besides the bow-port, will be 217 feet; hence,

$217 \times 9.77 = 2120.09$ tons for weight of hull.
$98 \times 3 \quad = \ \ 294.00$ tons for weight of ballast.
$98 \times 19.65 = 1925.70$ tons for real tonnage, exclusive of ballast.

Sum 4339.79 tons for whole displacement.

But if the new 32-pounders, of 8 feet long, and of the same weight as the 24-pounders, be substituted for the latter on the upper deck, the increase of weight incurred thereby will be at the rate of .56 tons per gun†, so substituted, which, in 36 guns, will amount to 20.16 tons, and will increase the displacement to $4339.79 + 20.16 = 4359.95$ tons. And if the 32-pounders, of 56 cwt., were mounted on the upper deck, there would re-

* We presume the scantlings of the timbers, &c. to be the same in the two cases; and there can be no doubt but that 9.77 tons per foot of the length is quite sufficient to ensure a proper degree of strength to the proposed ship. Chapman allows only 8.96 tons in his 110 gun ship.

† Vide Table VI.

sult a further excess of .36 tons in each gun on that deck, which, in 36 guns, would occasion an increase of weight to that last mentioned, of 12.96 tons. Lastly, if the very recently constructed 32-pounders of 64 cwt. be at the same time the arm of the lower deck, it would also add a weight of 8 cwt. per gun on that deck, which, in 34 guns, would amount to 13.6 tons. Hence, if 32-pounder guns, of 64 cwt., be mounted on the lower deck of this ship; 32-pounders of 56 cwt. on the upper deck; and 32-pound carronades on the quarter-deck and forecastle, we shall have to provide displacement for 46.72 tons more than our original calculation, and therefore our proposed ship of 98 guns would require a total displacement equivalent to 4386.51 tons*.

We have been the more solicitous to explain our views on this important question, because so little care has been taken, as far as we are aware, to elucidate it, and to render the practical determination of the displacement of a ship-of-war an easy and straightforward process. It is hardly necessary to say, that in this country the most vexatious and mortifying failures have repeatedly taken place from a neglect or ignorance of what has been here advanced. It must be confessed, however, that in the almost total absence of the necessary data which has prevailed, the risk of *one* failure in each class of ships must have been run by the most skilful and scientific constructor; but the result of his experiment, even had it proved bad, would have provided him with facts amply sufficient to prevent its recurrence, and thereby make some return for such an expensive mode (though the only certain one) of procuring given quantities in the theory of naval architecture.

The mere arithmetical operations, whose results, under the heads of displacement and weight of hull, furnish Tables II. and III. with their most important information, although

* If we pursue a similar calculation with reference to the 92-gun ships on *two decks* now building in some of our dock-yards; making an allowance (for the increase of scantling given to them over the 84-gun ship) equal to the difference between the weights of hull, per foot of the length of the 74 and 84, or .17 ton; we may, with some confidence, expect that the weight of hull will be about 2047 tons, and the load displacement for foreign service, in time of war, 4207 tons, including 276 tons of ballast. A height of battery of 6.79 feet will result with this displacement.

founded on principles which cost the intellect of Newton one of its happiest efforts to discover, are so very simple and uniform, that the merest schoolboy may perform them with ease; and hence it may not be unlikely, that persons may avail themselves of the very plain rules for calculating the contents of areas and solids, lately published, without the slightest knowledge beyond that of being able to perform the simplest operations in decimal arithmetic. Such may be able to put forth the displacement, &c. of a ship; but we must warn them, that without an intimate acquaintance with mathematical science, both pure and mixed, it is in vain for them to endeavour to advance in the path of improvement. The very data they may have collected are most likely to turn out useless, because, ignorant of their specific connections with the various qualities of a ship, and their mutual relations, they know not how to institute comparisons between them, much less to derive maxims, which are the very primary objects of analysis and inquiry. They may, in a small degree, tell us what a ship *is*, but cannot inform us *what it ought to be*, or depart a step out of the beaten track without floundering amongst the pitfalls that beset the footsteps of ignorance, and which the light of science alone can discover, and teach them to avoid. In no branch of the arts is a *little* learning, in the hands of an over-confident and presumptuous man, more likely to produce irreparable mischief than in that of which we are treating.

The next important property of a ship of the line, which is affected by the force, is the *stability*, or the hydrostatic effort by which the inclining power of the wind on the sails is counterbalanced. If a ship of the line, of two or three decks, do not possess this property in a sufficient degree, it is liable to lose the service of its lower tier of cannon, although the ports may be sufficiently high when it is upright; and even if fighting to windward (as a depression of not more than 7° can be obtained with a long gun) it would waste, in close action, nearly the whole of its force on that side, excepting what might be expended against the enemy's masts and rigging.

It has been already mentioned, that the construction of ships, with three or more batteries, becomes a task of no small difficulty; and this principally arises from a complication of disadvantageous effects, produced on the stability by the great increase of moment incurred by every additional tier of guns, not only by their simple weight, but also by their greater height above the water's surface, and also by the weight of an additional deck and height of topside. The inclining power of the wind on the additional height of topside in a three-decker being greater than that, on an equal surface, of a two-decker of the same force; and the quantity of sail of the former being elevated by a whole interval between deck and deck, constitute additional reasons why a three-decked ship requires a greater quantity of stability for the same surface of sail than a two-decked one. One obvious method of counteracting this tendency to instability, and that most usually resorted to, is to add *weight* below the water plane; but the use of ballast as a *corrective* is to be avoided; for, in a given ship, unless some of the stores are discharged, the vessel must be more submerged, and the lower ports brought nearer to the water than before*, thus producing the long train of evils attendant on such a circumstance. The only certain way of ensuring success in this particular, is to give a suitable increase to the dimensions of the ship, and a proper conformation to the sides between wind and water; and happily the means by which we can obtain a good height of battery are capable, in the hands of a skilful constructor, of being made conducive to the acquirement of a proper and permanent stability. It is very common to hear persons ignorant of the theory of naval architecture assert, that the mere breadth of beam is sufficient to make a stable ship; this is, however, but a very loose maxim. It is partially true, but not generally so:—it is possible for a ship with less breadth of beam than another, but having all other circumstances the same, to possess a greater stability than the latter. The truth is, that the *area* of the plane of floatation, and the *conformation of the sides between wind and water*†, are the dominant elements in the determination of this property,

* It should be an object to have as little ballast as possible, and to make the heavy stores in the hold act as much as we can in that capacity. The introduction of iron tanks for the water, and iron chains instead of hempen cables, are very favourable to such a disposition; the shot, too, ought to be stowed so as to effect the same purpose.

† Atwood's Disquisition on the Stability of Ships. Philosophical Transactions for 1798, part 2d.

so important to a ship-of-war, and, indeed, to vessels of every description.

It is unfortunately out of our power to give any comparative statement with respect to the stabilities of the ships we have been analysing; and for this reason, that the position of the centre of gravity is only given in the Swedish ships. We cannot regret too much, that, for want of knowing the position of this point, the stability of a British ship of war has never yet been ascertained with anything like precision, notwithstanding a scientific experiment of the simplest description would put every information necessary to its correct determination into our hands*. At a period when the failures of empiricism shew so plainly that naval construction is not a mere matter of fancy, it is surprising that so little anxiety should be manifested in the collection of experimental data. A simple order from the *proper Authorities* directing the performance of the experiment alluded to, whenever a ship leaves port for a cruize, would complete the practical application of the mathematical theory of the stability of floating bodies to the ships of the British navy, which we should no longer see undergoing the clumsy and discreditable correction of *doubling*, after having been built and found deficient in such an essential quality†.

We cannot quit this part of our subject without further remarking, although at the hazard of being thought digressive, that there are two methods of ascertaining the stability of a ship; viz. the French or metacentric method, published in the Traité du Navire of Bouguer in 1746, and the more recent and general one of Atwood, of which Bouguer's is a particular case, but which some are contented, for the sake of saving trouble, to adopt as a *general* method of calculating the stability of a ship, instead of Atwood's. We cannot, however, concur in recommending this unwarranted extension in the use of the

* In addition to the necessity of knowing the position of the centre of gravity for the determination of the stability, we may observe that we should be enabled to obtain other important results connected indirectly with its situation.

† The experiment for determining the height of the centre of gravity of a ship, first very obscurely and imperfectly hinted at by P. Hoste in 1696, was fully described by Don Juan D'Ulloa, in his "Examen Maritime," in 1771. Chapman in 1787 published a similar mode of operation in the Transactions of the Swedish Academy of Sciences. A modification of the same may also be seen in the first volume of Morgan and Creuze's papers on Naval Architecture, and in the Annals of Philosophy for November, 1825.

metacentric formula to any person careful of his reputation as a naval constructor. The *fact* related by Romme in his *L'Art de la Marine*, proves the *possibility of its leading to failure;* besides, accuracy ought never to be sacrificed to an indolence most strongly to be condemned where such an enormous stake as the success of a ship of war depends on a little extra exertion. Many writers (and even Atwood) have strangely misrepresented and exaggerated Bouguer's estimation of the method he discovered. Clairbois, in particular, by an erroneous discussion of the subject, has contributed to involve the question in confusion*. The inventor of the metacentric method himself unequivocally says, that it requires a modification, in order that it may give correct results at finite angles of heeling; a modification which he points out, and which is as much founded on the true principles of the hydrostatic stability of floating bodies, as the more general method of our countryman. Being aware of these circumstances, and that the displacement must be ascertained for both the methods, and moreover that if the calculator were deprived of a table of cubes, he would not have to boast much either in elegance or facility by pursuing the metacentric method; we must condemn such a lax mode of proceeding†. The most objectionable part of Atwood's process consists, we apprehend, in the unavoidable incorrectness, in a practical sense, of his geometrical construction for determining the horizontal moments of the areas immersed and emersed in the vertical sections of a ship, when deflected from the upright position. The following simple method is therefore here subjoined to render more direct this part of the operation, and which although a particular case of a principle of investigation, giving results for any angle of inclination, with trouble increasing as the angle increases, yet is short and simple for angles not exceeding 10°, and avoids the errors inevitably attendant on a complex geometrical construction with mathematical instruments.

Let γ, γ', and γ'' be three radial ordinates or half breadths, of which γ corresponds with the upright, and γ'' with that of

* Vide "Essai Géométrique sur l'Architecture Navale" of this author.

† We intend at some future period to enter more fully into this question.

the given inclined position of the ship; γ' bisecting the angle made by γ and γ'', thus dividing the area, included by γ' and γ'' and the side of the vessel, into two triangles whose sides opposite the radial point coincide almost rigorously with the sides. Putting the angle of heeling $= 2\,\Delta$ and rad. $= 1$, we shall have the sum of the moments of the two triangles with regard to the radial point, and in direction of γ'' equal to

$$\left\{(\gamma'' + \gamma' \cos. \Delta)\,\gamma'' + (\gamma' \cos. \Delta + \gamma \cos. 2\Delta)\,\gamma\right\}\frac{\gamma'}{6}\sin. \Delta.$$

and supposing $\Delta = 3\tfrac{1}{2}°$, we have this expression very nearly approximated to by

$$\left\{(\gamma'' + \gamma')\,\gamma'' + (\gamma' + \gamma)\,\gamma\right\}\frac{\gamma'}{3}\cdot 0305^{*}$$

a very symmetrical expression for the horizontal moment of the areas immersed and emerged at an angle of 7°, requiring only the simple measurement of the radial ordinates, and but a very moderate degree of trouble in the arithmetical computation of its value †.

If the sides between the limits of emersion and immersion be straight in a vertical sense, the above expressions will become still more simple for ascertaining the moments of the areas emerged and immersed, for the formula

$$(\gamma'' + \gamma \cos. 2\,\Delta)\frac{\gamma\,\gamma'' \sin. 2\,\Delta}{6}$$

will shorten the arithmetical operations in those portions of the sides which are plane surfaces. At an angle of heeling of 7° it becomes very nearly

$$(\gamma'' + \gamma)\,\gamma\,\gamma'' \times \cdot 0203.$$

The expressions just given are only particular results, as we have before remarked, of a very general principle of calcu-

* In using this formula, it is evident the operation will be further simplified by the application of logarithms.

† The simple areas of immersion and emersion will be expressed by

$$\frac{(\gamma + \gamma'')\,\gamma'}{2}\sin. \Delta;$$

a formula in every way calculated to save trouble in finding the solids immersed and emersed, and the positions of their centres of gravity with regard to the length whereby we may determine whether the longitudinal axis of rotation remains in the diametral section as the ship heels.

lation for any number of radial ordinates, the choice of which must be left to the computor, and the degree of inclination he intends to calculate the stability for. We have made choice of 7°, because no ship of war should incline more in action; but from knowing the stability at this inclination, and the form of the ship's sides, we may with confidence predict whether it will be an increasing or decreasing quantity at other finite angles.

Fragments on Egyptian Literature.

V. *Egyptian word for "Temple."* In M. Champollion's general table, No. 291, he gives a group, which he interprets "Rpe, erpe, *temple, hieron,* groupe symbolique." Dr. Young had previously ascertained this to be the signification of the group, and had published it as such in No. 87 of his article "Egypt." I do not intend to dispute the correctness of this interpretation, but I maintain that the groupe is *phonetic;* the characters of which it consists are S and E; and there are, I think, evident traces to be found of the ancient Egyptian word SE, *a temple,* in proper names, although it is a word unknown to the modern Coptic. One compound name, of which it is an element, is that of the city, from which the Sebennytic mouth of the Nile was denominated Sebennu, that is, "the temple of Bennu," a god, who was represented in the form of a water-fowl, and whose phonetic name is of frequent occurrence. Other names, in which this element appears to occur, though corrupted (as it was natural to expect that it would be, during the lapse of so many ages), are Si-wah, "the temple-Oasis," visited by Browne; and Galab-she, Gir-she, and Se-boua—temples in Nubia. I am aware of the *supposed* Arabic derivations of some of these words, but I put no faith in them. I believe them to be relics of the ancient Egyptian names, though, doubtless, corrupted. We have in Ireland many names of undoubted Celtic origin, for which very *plausible* English derivations may be imagined, and are actually given. An ancient name, like an ancient landmark, will often survive a shock by which tower and town may be destroyed. After above 3000 years, the same name of Accho is at this day given to a seaport in Palestine, which it bore in the time of the Judges of Israel (Judges i. 31); while that of Ptolemais, imposed by its Egyptian lord, has perished, like the edifices which he erected in it. Burckhardt found many towns and villages beyond the Jordan retaining the names by which they were known to Moses. And as for *some* of the names of Nubian temples, we have demonstrative evidence that they are not *Arabic*. Tafa and Korti are the very names, the terminations being slightly changed, by which the same places were known to the Romans; and, as these names are certainly not of Roman origin, it is probable that *they*, at least, are Egyptian; and if *they* be so, we need not wonder at *others* being so too. The Arabs may have altered names, as they did that of Syene, which, by prefixing their article, and rejecting the last vowel, (if this, indeed, were a part of the *Egyptian* name, and not a *Greek termination* merely,) they made "As-souan;" but I think it likely that it was not their general practice to substitute new ones for the old.

VI. *Hieroglyphical Tablet from Saccara.* In the 77th and 78th plates of the Collection of Hieroglyphics, edited by the lamented Dr. Young, we have a copy of the inscription on a broken tablet of chalk. It contains two female names, inclosed in ovals of royalty; each of them being twice written, horizontally and vertically. The two names were read by the learned editor, "ARSNE or Arsinoe, and PHILPATRA for Philopator." He supposed them to be the name and surname of "the queen, whose *priestess* is mentioned in the inscription of Rosetta," (*Quart. Jour., N. S.*, I., 408.) An examination of the hieroglyphical context has convinced me, that in drawing this inference he was too hasty. I find that the names are not a name and surname of the same female, but the names of two distinct females; and I read them, Arsinoe and Philotera. The latter is mentioned by Strabo, as a daughter of Ptolemy Soter; and the Arsinoe of this inscription is her sister, the "Arsinoe Philadelphe," whose *basket-bearer* is mentioned in the Rosetta inscription. This is quite evident from the titles given to the two princesses in this tablet. Philotera (there is certainly no P in the middle of the hieroglyphical name) is called "the king's daughter, the king's sister;" and Arsinoe is called "the king's daughter, the king's sister, the king's wife," and again, "the goddess brother-loving." The two princesses are only incidentally mentioned in this inscription. We learn, however, from it, that they had *priests*, and of course were both regarded as deities. In the first horizontal line, mention is made of the year 23 (of Philadelphus), that is, the year 62 Phil. or 26½ B. C. This may have been the date of the tablet; at any rate, it is a limit, which this date was not anterior to.

The title "Philadelphus," given to the second Ptolemy, is commonly translated "brother-loving;" and was given to him, it is generally said, because he *quarrelled* with his brother! But surely it is more natural to supposed that it signified "sister-loving," and was given him, or assumed by him, on the occasion of his marriage with Arsinoe, his sister. A union of this kind, being similar to that of Osiris and Isis, was regarded by the Egyptians as sacred; and the young Epiphanes is declared, in the decree preserved on the Rosetta stone, to be "a god, son of a god, *and goddess*, as was Horus, the son of Isis and Osiris." He was the first of the Ptolemies who sprung from such an union; for Philadelphus had no children by his sister Arsinoe; they were all by his first wife, the daughter of Lysimachus; and Evergetes married a Cyrenian princess, his cousin. He was, therefore, the first Har, or Horus, among the Lagidæ; and it was for this reason he was styled Epiphanes, which does not signify "illustrious," as some have translated it, but "manifested," or "incarnate," and which was peculiarly applied to the *Hori*.

VII. *Double dates of some of the Lagidæ.* It is well known that, in some of the inscriptions and papyri that have of late been partially deciphered, we meet with double dates. For example, the Berlin papyrus, No. 40, is dated in "the 14th year, which is the 11th year" of Cleopatra and Ptolemy Alexander. The Enchorial tablet, Hierogl. Pl. 51, is dated in the 19th year, which is the 5th year (or, according to Dr. Young, the 4th year) of Cleopatra and Ptolemy Cæsar. It has been supposed that these dates refer to different epochs from which the reign of a sovereign might be counted; but I am of opinion that they refer to the different commencements of the reigns of *two* sovereigns, who reigned jointly. I have never known the double date to occur, where only *one* sovereign was named. There is a second date used in the monuments of the reign of Physcon; his years are reckoned *only* from his brother's *capture* by Antiochus, when he was first elevated to the throne by the Alexandrians; there is no *second* year from his brother's *death* ever recorded. Yet, if a double date regarded the different epochs, from which a sovereign's reign might be said to commence, we can scarcely conceive a case in which it would be more likely to be employed that in that of Physcon. I would, therefore, explain the date last mentioned, "the 19th year of Cleopatra, which is the 5th of Ptolemy Cæsar;" not "the 19th year of Cleopatra, counting from her father's death, but the 5th from her brother's death."

It is strange that Dr. Young should have supposed the last number to be 4 instead of 5. It is the figure which Champollion gives for 5, in Kosegarten's work; and it is that which stands for *five* in the 29th line of the Rosetta inscription; though Dr. Young has here unfortunately attached to it an upright line belonging to the following word, thus making the Enchorial "5" to resemble a Hebrew "He" instead of a "Resh." This mistake would have been of less consequence, if it had not led to others; but in deciphering, one error is almost sure to induce several. In the 75th and 76th plates of hieroglyphics is another date, which Dr. Young first explained as "the *seventh* year" of Cleopatra and Ptolemy Cæsar (*Quar. Journ. N.S.* II., 315,) and afterwards in his posthumous work, as "the year VI." with a mark of doubt, and an observation that "we should *most naturally* read 'year VIII.,' which *would be* the year after Cleopatra's death." (*Ench. Dict.* p. 37.) Cleopatra certainly perished in her 22nd year; consequently, if her son's 4th year was her 19th, his 8th would be beyond the limits of her reign. The figure, nevertheless, is a *most distinctly formed* "eight;" and this is only to be accounted for by making the 5th, and not the 4th, of the young king to coincide with the 19th of his mother. His 8th would then be her 22nd. We may, therefore, fairly conclude that the first year of Ptolemy Cæsar was the 15th of Cleopatra, that is, 287 Phil. or $3\frac{8}{7}$ B.C. Their 22nd and 8th year was $3\frac{1}{0}$ B.C., in the beginning of which the battle of Actium was fought, and in the latter end of which Egypt submitted to the conqueror. By the bye, an important epoch in the submission appears to be recorded in "hieroglyphics," Pl. 74, A 5 b. The characters which Dr. Young supposed to signify "the munificent," (*Quar. Journ. N.S.* II., 314,) are certainly a *date;* Pharmutic, or Mesore, 12; that is May 9, or August 7, in the year 30 B.C.

VIII. *Astrological MS. from Saccara—Correction of the received Chronology of the Roman Emperors.* In the 52nd plate of Dr. Young's collection of "Hieroglyphics," is the facsimile of a Greek MS., found in a mummy-pit, the contents of which are far more important than at first sight they appear to be. It is a table of the position of the heavens, at the birth of some unknown *native,* and "in the *first* year of Antoninus, the 8th of the month *Adrian,* according to the *Greeks,* but the 18th of Tybi, according to the *Egyptians,*" or "according to the *ancient system,*" as it is expressed in another place; for the date is written *twice,* and very distinctly. This day was the 4th December, 137. I have computed accurately the positions of the sun, moon, and Venus, for that day, which was the 18th of Tybi, in the 885th year of Nabonassar; and I find them closely to approximate with those given in the MS., which are evidently not *observed,* but *computed* places. It is manifest that no nearer approximation can take place in any other year. The sun's longitude would be a quarter of a degree *farther* from the place assigned in the MS., on the same day in the following year; and the moon would be near four signs from the place assigned to it, on that day in any of the four circumjacent years. As I only consider the matter in a chronological point of view, I have not thought it necessary to compute the places of the other heavenly bodies, except in a very rude manner, by which I find that they must have been, on the day above-mentioned, in the quarters of the heavens assigned to them in the MS. In the upper of the two following lines are given the positions of the sun, moon, and Venus, as assigned by the MS. for the 18th Tybi, in the 1st of Antoninus, "in the first hour of the day," that is, the first from sunrise; and in the lower line are the positions of the same bodies, as computed from the tables in Vince's Astronomy, for the 3d December, 137; $17^h\ 30'$ P.M. Greenwich time, or $19^h\ 35'$ P.M. Saccara time, which must have been about the time of day referred to in the MS.

☉	☾	♀
$8^s\ 13^\circ\ 23'$.	$10^s\ 3^\circ\ 6'$.	$8^s\ 9^\circ\ 54'$.
8 11 48.	10 1 13.	8 11 45.

The coincidence is so close as to leave no room for doubt, that Antoninus Pius was Emperor in December, 137; and as it is well known that Adrian died on the 10th of July, having adopted Antoninus on the 25th February preceding it, we must refer these events to the year 137; not to 138, as is generally done on the authority of Dion, nor yet to 139, with those chronologers who follow Spartianus. For the *earliest*—and, as it now indisputably appears, the *correct*—date, we have the single authority of Ptolemy's canon; and on the faith of that canon, thus confirmed in a case where it was generally believed to have been in error, I cannot hesitate to fix the death of Trajan in 116, instead of 117.

We further learn from this MS. that the 4th of December was the 8th of *a* month of the fixed Alexandrian year. Some doubt has existed as to the precise day when this year began, but it is agreed that it was about the end of August. Each month being of thirty days, the month which was called "Adrian," and which, it appears, was the fourth in the year, must have commenced on the 27th November; and the year on the 29th August. The preponderance of evidence has certainly been in favour of this day; yet there are conflicting authorities, so as to render this confirmation of the received epoch by no means superfluous.

E—— H——.

K——h, 22d May, 1830.

Illustrations of the Cetetheræ, including the Loripeda, Semipeda, and Pinnipeda, or Loripeds, Semipeds, and Pinnipeds: being the arrangement of the Seals, Dugongs, Whales, and their Allies, indicated in Outline.

Wal-fisch, or *Wallow*-fish, whence our modern *Whale*, like the κήτος and cete of ancient naturalists, although restricted in their present meaning, would seem to have been once very general terms, indifferently and loosely applied to indicate many marine animals of uncouth form and exceeding size, without reference to their structural affinities; thus were they equally applied to the porpoise and the shark: indeed, so little were anatomical peculiarities once valued, that Aristotle's proposal to separate the Cete from fish, and to consider them as a different genus, observing that there might be established *the genus of Birds, that of Fishes, that of Cetacea and others*, was neglected for upwards of 2000 years; nor was it till the time of Linnæus, that the whales were esteemed other than warm-blooded fish. Even our celebrated Ray, who spurned the idea of considering the bat a bird, because it is viviparous, lactescent, and destitute of feathers, although, like birds, it has a double heart, warm blood, and breathes by lungs—still succumbed to popular prejudice with regard to the dolphin and the whale, by classing them with fish: while the distinctions are much greater between the true pisces and his "Pisces cetacei seu belluæ marinæ," than between the very properly-separated bats and birds. The watery habitat of the whale, it doubtless was, that so long cherished the vulgar error to which its external ichthyoid-form at first had given rise; and to suit this prejudice, the old definition of fish was in such manner framed, that some of the most essential and distinctive characters were of necessity omitted. Thus the cetacea have all a double heart and warm blood, while fish have cold blood and a single heart; these breathe by gills, those by lungs; these are oviparous and destitute of breasts, those viviparous and suckle their young, &c. &c. But what is the definition given in the Ichthyologies of Willughby and Ray?—"Animal aquatile sanguineum, pedibus carens, pinnis natans, vel squamis, vel cute nuda, glabra, et depili contectum, in aquis perpetuò degens, nec unquam sponte in siccum exiens." To Linnæus belongs the credit of disuniting for ever the whales from the proper fish, and of establishing the connexion which naturally exists between them and the other mammalious beasts: on this subject he most justly writes, "Hos a piscibus divulsos jussi mammalibus associari ob cor biloculare calidum, pulmones respirantes, palpebras mobiles, aures cavas, penem intrantem fœminam mammis lactantem, idque ex lege naturæ jure meritoque."

But the Linnæan dental system of subordinate arrangement caused him still to keep them widely separated from the Dugongs, the Walruses, and Seals, their natural allies; and which are surely the ascending grades by which the pisciform mammalia become connected with the truly four-footed beasts. Save Pennant, whose term Quadrupeda of course excluded whales, all modern zoologists of note since Willughby and Ray, have maintained the Linnæan doctrine, and classed the cetacea with the other mammalious beasts, yet none have ventured sufficiently to insist on their close affinity with the Seal and Morse, although the Dugong, which forms the connecting link, has been by some allied with the Cetacea, and by some with the Trichecus. The old Celtic nomenclature would seem, in part, to adumbrate this natural connexion between the *wal*-rus, nar-*whal*, *whale*, and others; and Blumenbach partly favoured such an association, by subdividing his digitate and palmate beasts into three sections each, viz., Glires, Feræ, Bruta; and placing the latter next his ninth order, containing the dolphins, the narwhal and the whales: but the distribution was marred by associating the ornithorhynchus and the beaver, with the walrus and the seal; and this much more closely than either of these latter with the dolphin and their natural allies; these being in a different order, while those are in the same. Illiger also placed his *fin-footed* beasts, the seals and morses, next to his swimming beasts, *i. e.*, the dugong, porpoise, whale, &c., in like manner as Lamarck and Carus have made their orders Cetacea and Amphibia contiguous; in which schemes, although imperfect, and the term amphibia exceptionable, (it having previously and more appropriately been applied by those writers, as well as by most other zoologists, to a quite different race of animals, viz., the frogs and their allies), are, nevertheless, far superior, in this respect, to the subordinate distributions, either of Linnæus, Cuvier, or Latreille. Linnæus placed the trichecus and manatus in his second order, Bruta, along with the elephant and sloth; and the seals in his third order, Feræ, along with the lion, the wolf, and other most agile beasts of prey; while the dolphins and the whales composed the seventh and last order of his scheme. Cuvier, and the followers of his celebrated school, not only keep the seals among the Feræ, one of the higher subdivisions of the scale, but add the walrus to the group now named mammiferous amphibia; while the dugong, which by some has been associated with these, by others, and by Cuvier himself, has been placed among his cetacea, which are the lowest in the mammalious department of his plan; having the ruminantia, pachydermata, edentata, and rodentia, between them and the carnassiers, in the midst of which order, between the insectivora and marsupialia, the carnivora are found, in the last subdivision of which sub-order the seals and morses are arranged; whose clubbed feet and retracted paddle-like hind limbs, scarcely agree with the definition of the order, one of the characters of which, as given by Stark, assumes, that they have four extremities *proper for walking*.

Do not these schisms among zoologists, as to the location of these animals, shadow forth an index to their more natural and correct arrangement? for the dugongs, which by some have been allied to the seals and morses, and by others to the dolphins and the whales, may not improperly become the link which will closely connect those two series of animals, in general so widely separated, and constitute, with them, a distinct and natural order.

Hence, as the three great types already illustrated, (vide Journal of Science, Nos. VI., VIII., X., and XII., N.S.) under names of Manupeds, Alipeds, and Quadrupeds, associate into a well-defined and natural group or order, the Geotheræ,—distinguished by their common possession of mammæ and *four freely*-exserted limbs,—so these lower animals will form three equally well-assorted groups, or types, distinguished by the relative development of their extremities into four or two limbs or fins; and as they are associated by the comparative abortion and retraction of their digits, so are they, by these characters, very strongly contrasted with the other types, in all of which the limbs, at least, are distinct and well developed.

As in this prodromus the chief design is to indicate in outline, not to descant at large on the natural associations which may thus be formed, let it suffice to state, that the trichecus, otaria and phoca, with all other mammalious beasts possessing *four partially*-developed limbs, the digits especially of the hind extremities being more or less abortive, and the whole bound together, or retracted greatly, are associated in one group, and form the first type of the Cetetheræ, for which their structure will suggest the name Loripeda, or Loripeds, *i. e.*, bound or club-footed beasts. The next series, of which the dugong may be considered the normal species, will include all those mammalious beasts endowed with *only two* partially exserted limbs, and these the fore-extremities; the hinder ones being wholly retracted, and joined to form a flat horizontal tail. The diminution of their limbs to half the ordinary number; and the retraction of the two remaining to half the ordinary development of the previous group, will suggest semipeda, or semipeds, *i. e.*, half-footed beasts, as an appropriate denomination for this second type. Whilst the dolphins, the porpoise, the grampus, the narwhal, the cachalot, and the common whale, in all of which the hinder extremities are merged in a horizontal tail; and even the fore-extremities are retracted to the form of mere pectoral fins, will hence most properly assume their name, pinnipeda, or pinnipeds, *i. e.*, fin-footed beasts *. Of these three types, the Loripeds answer to the mammiferous amphibia of Cuvier and Latreille, to half the amphibia of Lamarck, the Feræ and part of the Bruta, excluding the Glires, of Blumenbach's palmata, or to the misnamed pinnipeds of

* That Cuvier was right in removing the Dugongs from the genus Trichecus, cannot be doubted when we consider that the Walrus has four extremities, although but partially developed, when the Semipeda have never more than two. That Cuvier is correct in placing them among the Cetacea, may still be questioned, when we observe that the dugongs have nails on their anterior extremities, which are not merely fins, but with which they are able to hold their young to their pectoral breasts, while the fore-limbs of the whales are merely pectoral fins, that their mammæ are anal, and that they all possess the characteristic spiracles, or blowing holes, of which the dugongs are devoid.

Illiger. The semipeds agree with the other half of the mammalious amphibia of Lamarck, the herbivorous cetacea of Cuvier and Latreille, or the sirenia, which form the first section of the natant beasts of Illiger: and the pinnipeds, or warm-blooded fishes of the ancients, answer to the cete of Linnæus and Blumenbach, the cétacés of Lamarck, the ordinary cetacea of Cuvier, the hydraulic cetacea of Latreille, and the latter section of Illiger's natantia.

The very curious manner in which these beasts have been, at different times, and by different systematists arranged, and the mutual alliances which, on all hands, have been maintained, although some have associated the intermediate type with one, and some with the other extreme, would seem to justify an inquiry into their true connexions, and as the result of this, it may be enunciated that the Belluæ marinæ, or sea beasts, should form a common group, and be associated in an especial order, of which the fin-footed, half-footed, and club-footed series, will form the respective types: their ancient name somewhat restricted (so as to exclude the shark and other veritable fish) may also be resumed, and hence the order which includes the pinnipeds, semipeds, and loripeds, will resume the general appellation, cete, or cetacea; or perhaps, to avoid confusion, as these words have been so variously extended and restricted, cetetheræ, or whale-beasts, may be advantageously introduced for κήτη, κητειος, whence cete, cetaceous; and *wal*, or *whal*, whence *whale*, were formerly all used as common terms to indicate several distinct and separate, perhaps any uncouth sea-monsters, and this their derivatives, especially those of the latter, *wal*-rus, nar-*whal*, *whale*, will clearly shew.

Notwithstanding the zoological discoveries of modern times have greatly swelled the catalogues of the other types, the number of known cetetheræ is still but small; the obscurity and extent of the boundless depths in which they dwell, have hitherto forbidden a sufficiently satisfactory research; but, doubtless, hereafter Cetology will add many, now unthought of animals, to those which, at present, constitute its members and fill its ranks.

Order, Cetetheræ, *Cetethers* [*Cete* or *Cetacea*, and *Mammif. Amphibia*]. Whales and their allies. Limbs degenerate, retracted greatly within the integuments; feet unfit for walking on lands, especially the hinder ones, which are often united to form a flattened horizontal tail; mammæ pectoral, abdominal, or anal; viviparous; teeth various.

Type, Loripeda, Loripeds, [amphibia]. Seal, Walrus, &c. Fore extremities partially developed, the digits webbed; hind limbs bent backwards, and only used for swimming; three sorts of teeth; abdominal mammæ; body more or less hairy.

Kinds, Phocidæ, *Seal kind.* Three sorts of teeth in both jaws.

Trichecidæ, *Morse* or *Walrus kind*, no incisors or canines in lower jaw.

Type, Semipeda, *Semipeds.* [*Sirenia*, *Syrens* or *Mermaids*,] *Dugong*, *Manatus*, *&c.* Fore limbs very short; digits obsolete; nails on the edge of the abortive fin-like hands, hind extremities united to form a tail, two pectoral mammæ, no spiracles, no hair on body, some little about the muzzle.

Kind, Manatidæ, *Dugong kind*, teeth variable, according to age.

Type, Pinnipeda, *Pinnipeds.* [*Cete*, *Cétacés Ordinaires*,] *Dolphin*, *Narwhal*, *Whale*, *&c.* Body completely pisciform, destitute of hair, save a few short rudimentary bristles near the mouth; anterior extremities degenerated to pectoral fins, the hinder to a flattened horizontal tail; anal mammæ, spiracula.

Kinds, Delphinidæ, *Dolphin-kind*, small head, teeth in both jaws numerous, all of a molar type.

Narvallidæ, *Narwhal-kind*, small head, two teeth in upper jaw, one lengthened and tush-like.

Physeteridæ, *Chacalot-kind*, large head, many teeth—54.

Balænidæ, *Whale-kind*, large head, no teeth.

Order.	*Type.*	*Kind.*	*Genera.*	*Species.*	
CETE, CETETHERÆ; Or, WHALES AND THEIR ALLIES.	LORIPEDA, LORIPEDS, or CLUB-FOOTED BEASTS.	PHOCIDÆ. SEAL-KIND.	Phoca	P oboscidea,	Sea Elephant.
				Cristata,	Crested Lion.
				Groenlandica,	Gr. Seal.
				Vitulina,	Common Seal.
				Barbata,	Bearded Seal.
			Otaria	Jubata,	Sea Lion.
				Ursina,	Sea Bear.
				Albicollis,	Spotted.
				Coronata,	Crowned.
		TRICHECIDÆ, WALRUS-KIND.	Trichecus	Rosmarus,	Walrus.
	SEMIPEDA, SEMIPEDS, or HALF-FOOTED BEASTS.	MANATIDÆ, DUGONG-KIND, or MERMAID-KIND.	Manatus	Americanus,	Am. Manatus.
				Senegalensis,	Sen. Manatus.
			Halicore	Dugungus,	Dugong.
			Rytina	Stelleri.	
	PINNIPEDA, PINNIPEDS, or FIN-FOOTED BEASTS.	DELPHINIDÆ, DOLPHIN-KIND.	Delphinus	Delphis,	Dolphin.
				Tursio,	Blower.
			Delphino-rhyncus.	Coronatus,	Small-beaked.
				Gangeticus,	Indian.
			Phocæna	Communis,	Porpoise.
				Grampus,	Grampus.
			Delphinapterus	Leucas,	Beluga.
			Heterodon	Hyperoodon,	Two-toothed D.
		NARVALLIDÆ, NARWHAL-KIND.	Narvallus	Monoceros,	Narwhal.
		PHYSETERIDÆ, CACHALOT-KIND.	Catodon	Macrocephalus,	Cachalot.
			Physeter	Microps,	Sperm. W.
			——	Sulcatus,	Japan.
		BALÆNIDÆ, WHALE-KIND.	Balæna	Mysticetus,	Common W.
				Glacialis,	Nordcaper.
				Nodosa,	Hunch-backed.
			Physalus	Gibbar,	Jubarte.
				Boops,	Pike-headed.

Illustrations of the Herpornitheræ ; or the Arrangement of the Ornithorhynchus and Echidna, indicated in outline.

That paradox of animals which has so long defied our systems, having successively and simultaneously been esteemed a reptile, a bird, and a beast, by Geoffrey, Lesson, Meckel, Cuvier, and others, and which, although breasts have never been proved to exist therein, has still, by most naturalists, been arranged with the true mammifera, alone remains to complete this view of the first class, of the first region, of the animal reign. And by discarding the indefensible Linnæan term mammalia, or breast-bearing animals, and resuming the originally significant and very intelligible term *beast* with the definition that we mean thereby, a double-hearted, warm-blooded, vertebrated animal, breathing by lungs, externally entire, not pierced for communication with the other cavities of the body, and destitute of true wings and feathers, the ornithorhynchus, and echidna, immediately will find their appropriate location: and their curious alliances, both with birds and reptiles, instead of being a difficulty, will become an important clue in the development of the natural system.

Sir Everard Home, followed by Leech, Shaw, Latreille, Brookes, and others, have "given it decidedly as their opinion, that these animals should constitute a distinct class, whose situation should be between the mammalia and birds." They are quadrupeds, writes Sir Everard (in the Philosophical Transactions for 1802), but not mammalia; but then, as all mammalia are not quadrupeds, *e. g.* the pinnipeds and semipeds, so there is no necessity that all warm-blooded quadrupeds should be mammals. And had it not been that the Linnæan term mammalia, substituted so generally for beast, and invented purely for the sake of including men and whales with the common quadrupeds, led naturalists astray, there could have been no more occasion for considering the monotremata as a distinct *class* placed between mammalia and birds, than there was for Geoffroy considering these animals as reptiles, or Lesson esteeming them as birds. They certainly are not reptiles, for their double heart and warm blood imperatively exclude them from that class; and they as surely are not birds, for their four legs, their destitution of wings and true feathers, and their entire unpierced lungs, strongly mark the difference, and forbid the unnatural association. They are warm-blooded, lung-breathing, verteberated animals, with limbs developed in the form of legs and feet, which characters sufficiently indicate their connexion with ordinary beasts; while the want of lips, the horny bill, the cloaca, and the probable absence of mammæ, with the belief that they are oviparous, or ovi-viviparous, will justify their segregation into an order by themselves; for which, as they are extraordinary, and but lately discovered animals, indigenous to a land far distant and but little known, it is not to be expected that our language will afford any familiar name as an appropriate appellation. Monotremata has been proposed; but as it is not much more euphonious, and far less significant than Herp-ornitheræ,—*i. e.* rept-avi-peds, or reptile-bird-beasts, which term intimates their connexion both with reptiles and with birds,—this latter compound may hence be probably preferred.

The much-disputed question whether the monotremata do or do not possess mammæ, and if they do, whether or not they suckle their young, although interesting problems in zoology, are of much less importance in a systematic point of view, than they have been generally esteemed; for if breasts be shewn to exist, the other anatomical peculiarities are sufficient to distinguish them as an *order;* and even if the negative were proved, they could not fairly be severed from the other beasts so as to constitute an intermediate *class.*

Geoffroy first described them to possess supernumerary marsupial bones, which still further connects them with the beasts, and this probably led Blainville to associate them as anomalous didelphian mammifera, with the opossum and kangaroo, natives of the same country, and in some respects connecting links between these animals and ordinary beasts; for their embryo parturitions are little more than ova, and in the opossum the membranous partition is so slight as almost to degenerate to a cloaca. Whether the organs figured and described by Meckel as mammary glands, be truly such or not, much doubt may still be reasonably entertained; these same parts were noticed by Mr. Clift many years ago, and his discovery of them was known to many naturalists, both here and on the continent, but he did not then, and does not now consider them to be breasts. M. Geoffroy St. Hilaire "mentions, that, notwithstanding M. Meckel's ability as an anatomist, he has been deceived on this point, and that what he had mistaken for mammæ are something else." Geoffroy "considers them as of the same nature as the odorous glands of squirrels."

Comparatively large as these subcutaneous abdominal masses are, a question might still be raised, as to whether they may not be the repetition of a type of structure such as nature delights to preserve, even when the use no longer can exist, such as the male nipples and the vertebra prominens in man, the human plantaris, and the muscles of the external ear, the course of the recurrent nerve to a single upper larynx (the physiological reason of which Mr. Herbert Mayo has lately pointed out,) &c. &c. And these rudiments or remnants of the mammal type might be reasonably expected to be much larger and more notorious in the female than in the male, for the horny bill of the ornithorhynchus is certainly unfitted for suckling, even if there were breasts in the parent beast. Here, however, on the contrary, it must be borne in mind, that the beak may be an after-growth; and it is not impossible that the young have lips, which subsequently may become abortive, and the bill then grow, as in infants the gums at first are destitute of teeth: but this possibility is not probable. Furthermore, the double penis, which does not give passage to the urine, the opening of the uterine horns separately into the common cloaca, without forming a proper uterus, afford several parallels with oviparous animals, as birds and reptiles, and some fish; and consequently favour the accounts received on many hands, of their eggs being found, which, although not sufficiently authenticated to be acknowledged, are yet too circumstantial to be positively denied. Indeed, could system ever be allowed to suggest a discovery, it might almost be predicated that even should the ornythorhynchus be shewn to be mammalious, some other beasts will be discovered that are destitute of breasts; the general scheme of nature seems to require such a connecting grade.

The two genera, ornithorhyncus or platypus and echidna, each containing but two known species, are all that have been as yet discovered belonging to this order; Illiger indeed suggests a query as to whether the Pamphractus, commonly considered as a testudo, may not properly be associated here; doubtless, future investigations will add many to the present list: for those already known appear to indicate two separate types; to which system would assign appropriate names, say Fissipeda or Fissipeds, and Remipeda or Remipeds, the echidna having the digits cleft and free, while in the ornithorhynchus they are connected by a membrane giving them an oar-like faculty which greatly facilitates their motion in the water. These consequently have been adopted as denominations for the types; but were it not for what Latreille has called the supremacy of system, perhaps as so few are known, it might have been as well to include the whole in one division, the *Rept-avi-peds.*

Order, Herpornitheræ, *Herpornitheres* [*Monotremata, Reptantia, Asitthæ, Atthæ, Atitthæ, Reptavipeda, Reptavipeds, Reptile-bird-beasts, &c. &c.*] Four freely exserted limbs; digits armed with nails or claws; no true teeth; horny bill; no fleshy lips; cloaca; mammæ absent or unknown?

Type, Fissipeda (Fissipeds, or Cleft-footed Beasts); digits separate; body aculeous.

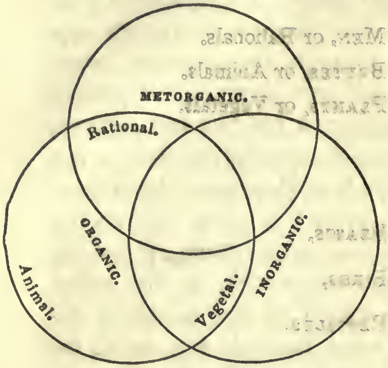
Kind, Echidnidæ, (Echidna-kind.) Tongue extensile.

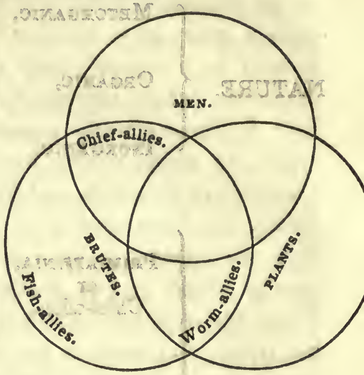
Type, Remipeda (Remipeds, Oar, or Paddle-footed Beasts.) Digits connected by a web; body hairy.

Kind, Ornithorhynchidæ (Ornithorhynchus kind.) Tail horizontally flattened.

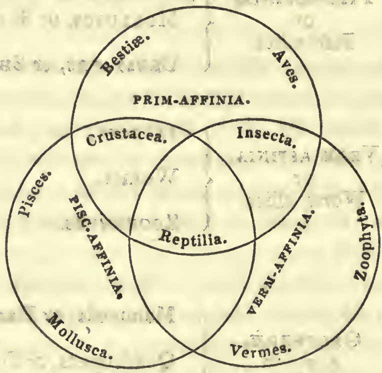
Order.	*Types.*	*Kinds.*	*Genera.*	*Species.*
Herpornitheræ.	Fissipeda.	Echidnidæ.	Echidna.	Hystrix.
				Setosa.
	Remipeda.	Ornithorhynchidæ.	Ornithorhynchus.	Fuscus.
				Rufus.

REALMS, REIGNS, AND REGIONS.

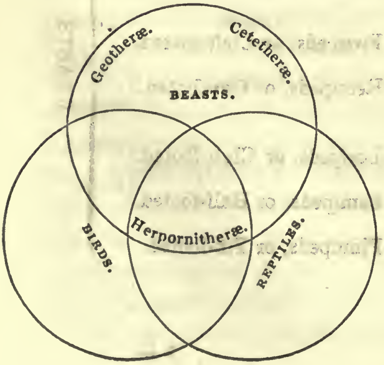


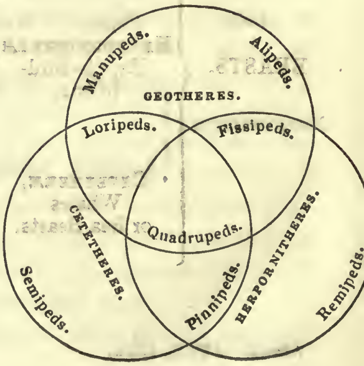


REGIONS AND CLASSES.



CLASSES, ORDERS, AND TYPES.





NATURE.	METORGANIC,	
	ORGANIC,	MEN, or Rationals.
		BRUTES, or Animals.
		PLANTS, or Vegetals.
	INORGANIC,	

BRUTES, or ANIMALS.	PRIM-AFFINIA, or Chief-allies.	BEASTS,
		BIRDS,
		REPTILES.
	PISC-AFFINIA, or Fish-allies.	FISH.
		MOLLUSCS, or SOFT FISH.
		CRUSTATES, or SHELL-FISH.
	VERM-AFFINIA, or Worm-allies.	INSECTS.
		WORMS.
		ZOOPHYTES.

BEASTS.	GEOTHERÆ, common Land Beasts.	Manupeds, or Hand-footed.	BEASTS.
		Quadrupeds, or Four-footed.	
		Alipeds, or Wing-footed.	
	HERPORNITHERÆ, Reptile-Bird-beasts.	Fissipeds, or Cleft-footed.	
		Remipeds, or Oar-footed.	
	CETETHERÆ, Whales or Sea Beasts.	Lonipeds, or Club-footed.	
		Semipeds, or Half-footed.	
		Pinnipeds, or Fin-footed.	

[To the Editor of the Quarterly Journal of Science.]

My dear Sir,

I cannot conclude these outline illustrations of the highest class of Brute animals, viz. the "Beasts," which were selected for this first series of sketches on the Philosophy of System, merely as affording some of the most notorious and familiar examples of those curious inter-alliances to be found in every grade of physical existence, and which so much distract all arbitrary methods of arrangement, in which the object seems to have too often been to *divide*, where it should only seek to *distribute*, and to *separate* where it should only labour to *connect*; without endeavouring to direct your attention to some of the advantages which result from pursuing an opposite course, and to correct some misconceptions with regard to the diagrams (published in No. VI.), which were merely introduced as convenient symbols illustrative of the connexions of the several grades, not as absolute drafts of the plan on which the associations in nature do exist; *i. e.*, the hypothesis was never entertained, that in nature the various groups and provinces are found arranged in geometric circles; but that the properties and powers, characteristic of the several existences, are more or less predominant in the contiguous grades, so that no one is essential and peculiar to one individual only, and the triply intersecting circles seemed the simplest figures by which to illustrate these mutual alliances, for by their means we segregate and distribute at the same time that we connect. A two-fold object is hence attained; for it was designed to shew, 1st, why the various grades of nature, and of knowledge, cannot be absolutely divided; and, 2dly, how they may be definitely arranged without an absolute division.

How constantly do we hear systematic writers complain, and not without some apparent show of reason, that the varieties of forms in nature so frequently return into themselves, and the multiplicity of particulars are blended in such varied connexion with each other, as to render futile their most brilliant schemes of absolute and definite arrangement! But are not such schemes too often the unmeasured manufactures of the closet? and, like ready-made garments from the commissariat stores, poor nature is obliged to put them on, and blamed when they do not fit. Yet, notwithstanding this, many philosophers, nay most, have laboured, and still do labour, to shew the boundaries of science, and to raise barriers between their respective studies, hence widely to demarcate the provinces of nature; but such efforts, as they always have been vain, so they ever will prove abortive; for the provinces, as they exist in nature, approach each other at so many points, and are so mutually and reciprocally dependent, that where the one ends and the next begins, it is often impossible to determine. Some persons have invented lines, and others circles; some have adopted numbers to be regularly repeated into themselves, and others have varied these numbers in each successive stage; some have planned a tree, some traced a group, and some have forged a chain for nature; but she laughs at such vagaries, nor will she, like the unwary, weary traveller, be maimed to suit Procrustes' iron bed.

Physically existing beings—although agreeing in the general terms of their existence, viz., that they are—present many subordinate distinctions among themselves. One realm or kingdom is found to be destitute of life and organization, whilst in another, life and organization universally prevail; and in a third, existence is believed to be essentially incorporeal and immortal. Hence we talk of spiritual beings, of animated and of inanimated nature, or of the metorganic, organic, and inorganic realms, vide p. 366, fig. 1. The organic realm, though universally endowed with life, has this vitality in its several grades, enjoyed in very different degrees. In one extreme, life in its lowest state is evident; these beings merely *live*; they *vegetate*, hence their name, vegetables; or, rather, according to Burton, Field, and Butler, vegetals. Next sense, in its various degrees, becomes progressively developed, giving a higher grade of life or animation; these beings live and feel; they are animated, hence their appellation, animals. When to the vegetation of plants, and the sense of animals, reason is superadded, the highest stage of mortal organization is attained in man, who ranks but "little lower than the angels," and hence the rational or human grade (vide p. 366, fig. 1) enters the spiritual or metorganic realm, of which a further development is not, for the present considerations, now required.

Hence animated nature is composed of three kinds of beings, or, rather, the organic realm affords three several reigns; the merely vital or vegetating, *i. e.*, Plants or Vegetables: the sensual or vital, *i. e.*, Brutes or Animals: and the rational or human, *i. e.*, Man.

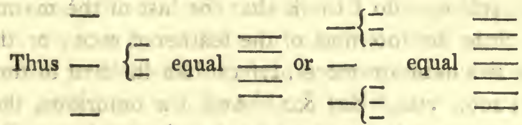
Now, as the subtile and immaterial parts of nature, Heat, Light, Electricity, Magnetism, Attraction, &c. approach and enter the metorganic circle, although still contained in the inorganic or lifeless realm, to indicate by their position their immateriality; and as the material enters the organic to furnish matter for life to organize; so does man, or the rational grade of organic beings, enter the metorganic circle, to indicate his spiritual existence, which raises and distinguishes him from the mere brute animals, which are entirely without it; and the vegetating grade, or Plants, in like manner, proclaim an analogous position, their slighter remove from the crystalline productions of the inorganic realm.

In each of these reigns, or sub-kingdoms, whether of the organic or inorganic realms, many subordinate distinctions may be traced, the analysis of which, in one direction, has been the object of that series of papers now just brought to a conclusion: and the development of the others must form the subject of future investigations.

The tables on page 367, and the diagrams on page 366, will suffice, as a conspective view of the connexions and inter-alliances which these have been intended to point out.

Cuvier has well observed, "I do not believe that the mammalia and birds, placed last, are the most imperfect of their class; still less do I think that the last of the mammiferæ are superior to the foremost of the feathered race; or that the last of the mollusca are more perfect than the first of the annelides or the zoophytes," &c. &c. And it is notorious, that the tortoises, the lizards, the frogs, and, indeed, almost all the tetrapod reptiles, are as nearly allied (if not more so) to the quadrupedous beasts, *e. g.*, the ornithorhynchus, the armadillo, &c. &c. as are the birds, which, in all ordinary arrangements, separate them so widely; and again, the pinnipeds, especially the whales, are, in many respects, more analogous to fish than to birds: and yet how distant are they placed in the linear scales!

It would be tedious to multiply such instances which at every step are continually occurring; they have not been overlooked by naturalists, and many schemes have been devised for their improvement; hence, had we Bonnet's chain of nature,—hence the dichotomous ascent proposed by several authors,—hence the occasional continuous lines terminating in themselves, and forming potential circles, as by Blainville, Green, and others; and hence has resulted the quinary circles of Mac Leay, with the osculant groups between them. Watts in his Logic has observed, that dichotomies and trichotomies in system are absurd; and when nature is forced into any prefixed number, it certainly is so; but it would be very convenient as an assistant to the memory, if such a distribution could be naturally found out. Flemming and many other systematists have adopted the No. 2, always pursuing a binary analysis. Mac Leay prefers the No. 5, and Kirby 7. Mr. Field, myself, and many others, have found the No. 3 more generally to prevail. On this subject Kirby writes, that "the No. 5 assumed by Mr. Mac Leay for one basis of his system as consecrated by nature, seems to him to yield to the No. 7, the abstract idea of which he states to be *completion, fulness, perfection.*" But without entering into any abstract speculations, it may easily be shewn how "the quinaries are resoluble into septenaries," and how both are but imperfect analyses of the ternary scheme—for the fives result merely from the analysis of one of the threes, the other two remaining unanalyzed, and the sevens from the single one being left undivided, and often being indivisible. (See Tables.)



Whereas the analysis should be, as in the former figures, arrived at in two separate stages.

To test this point, although the number 3 is not, and never has been *assumed* as an essential part of the connecting scheme, and only pursued so far as nature has presented trine distributions, and the convenience might be adopted without violating natural alliances, let us briefly examine the quinary system, which commences with 2 natural beings or existences, *viz.* continuous and incontinuous, the first of which divides into 3, *viz.* one intelligent and 2 unintelligent, time and space. The diagrams also commence with *two* circles, the vegetable and animal, in the first of which only *two* subdivisions are noted, and asterisks left to supply the other three (which has since been done) from the subdivision of the acotyledonous, or rather the cellulous plants. In the animal circle, 5 groups are made out by separating some of the zoophyta, under the name acrita, from the radiata, the vermes, and the insecta; which latter are again united with the crustacea; a union for which Linnæus has been severely blamed. Then in the quinary division of these five circles, the "consecrated number" is completed by dividing, in the acrita, the polypes into 3, *viz.* P. rudes, P. vaginati, and P. natantes, which 3, with the agastria and intestina, complete the 5; the distributions should rather have been thus—

$$\text{Acrita}\left\{\begin{array}{l}\text{Intestina,}\\ \text{Polypi,}\\ \text{Agastria,}\end{array}\right. \left\{\begin{array}{l}\text{Rudes,}\\ \text{Vaginati,}\\ \text{Natantes,}\end{array}\right\} \text{instead of} \quad \text{Acrita}\left\{\begin{array}{l}\text{Intestina,}\\ \text{Polypi Rudes.}\\ \text{Polypi Vaginati.}\\ \text{Polypi Natantes.}\\ \text{Agastria.}\end{array}\right.$$

which will shew the plan on which fives may be derived from threes. In the same manner, among the annulosa, the mandibulate and haustellate insects are divided; while the trachean and pulmonary arachnida are united. In the vertebrata, the batrachian are separated from the other reptiles, while the ornithorhynchus remains classed among the mammalia; and Mr. Brookes, who has made the monotremes into a separate *class,* has omitted to insert them as such in his publication on the quinary system, for this would have made 6 classes in one circle, instead of 5, although he has added two classes of mollusca to the three, which were all that were originally inserted in the Horæ Entomologicæ; and Kirby has well remarked, that the ametabola, mandibulata, and haustellata, approach much nearer to each other than they do to the other two classes of his circle, annulosa, or than even these two last to each other, so that, under this view, it should primarily consist of 3 greater groups, resoluble, *it may be,* into 5 smaller ones.' Thus,

Annulosa	Arachnida,		instead of Annulosa	Ametabola,
	(Insects)	Ametabola, Mandibulata, Haustellata,		Crustacea,
	Crustacea,			Arachnida,
				Haustellata, Mandibulata.

The other diagrams, page 366, I think may be allowed in great measure, to explain themselves; in fig. 2, the proximity between the infusoria of the verm-affines, and the protophytes of the vegetable reign, marks the analogy, or rather the affinity between them and the oscillatoria of the cellulous plants; and the apes and monkeys of the prim-affines claim by their position and their anthropomorphous structure, a like approach to man. In fig. 3, the beasts and reptiles, instead of being separated by the birds, are connected with each other and with them, of which alliance the ornithorhynchus becomes the link; the pinnipeds of the beasts also approach the fish, as in like manner do the chelonian reptiles, the crustacea and coleopterous insects, which two latter, although distinguished, are shewn to be allied; and the lepidoptera may be supposed to shew their analogy with birds. The mollusca, also, are not placed above the articulata, but while vindicating their rank by the high development of gills, heart, &c., in the sepia, at the same time approach the worms of the other circle: and the pisces are connected both with cetaceous beasts, and by the eels, &c., with the ophidian reptiles. But enough—this subject might be pursued to almost any extent.

Yours ever most truly,

Gilbert T. Burnett.

22, *Great Marylebone Street,*
June 14th, 1830.

Supplementary Observations on Opium and its Tests, by Andrew Ure, M.D., F.R.S., &c.

In my paper on this subject, published in the last Number of the Quarterly Journal, p. 56, I adverted to a precipitate thrown down from genuine London porter, or brown stout, by water of ammonia. When washed with water and dried, this precipitate appears as a greyish-brown powder, of considerable bulk in reference to that of the liquid. It is insoluble in water, and alcohol, either cold or hot, but it dissolves most readily in any dilute acid; and it would seem to be chiefly some form of *hordein.*

Should porter be drugged with opium, the morphia will fall down along with this matter, and it may be separated by the action of boiling alcohol. This alcoholic solution, being cautiously evaporated to dryness, will leave a minute pellicle of morphia on the capsule, which, when touched with a drop of nitric acid, about specific gravity 1.3, will take the characteristic blood-red tint of morphia, so treated. In this way, I have detected opium in the porter as exported from an eminent London brewery; but in the porter of some other great London establishments, I have not found any trace of morphia; whence I infer, that this mode of adulteration is by no means general.

Ammonia, added to even a strong tincture of hops, causes a very trifling precipitation; for not more than three or four grains of matter are obtained from the infusion of an ounce of hop in strong spirits.

In my former paper, I shewed how easily sulpho-cyanic acid might be obtained from ordinary human saliva, by simple distillation in a retort. Saliva provoked to a rapid flow by tobacco-smoke, or other stimulants, seems to contain as much of this curious acid, as saliva more slowly secreted. I have since examined saliva formed during mercurial ptyalism, and find that it contains no sulpho-cyanic acid, for it is not affected by red muriate of iron. Should this result be found universally, or very generally, to follow the active administration of mercury, it may afford a valuable indication to medical practitioners in doubtful cases, where the ordinary fætor of the breath prevents them from recognising whether or not the mercury has entered the system. During mercurial ptyalism, the saliva shews generally, with litmus paper, an alkaline re-action, though I have sometimes found it neutral, and, occasionally, acid.

Proceedings at the Friday Evening Meetings of the Members of the Royal Institution.

March 12th.

In the Lecture-room Mr. Brande gave an account of the chemical composition of urinary calculi, principally with a view of illustrating the medico-chemical treatment of the disease, and of pointing out the great importance of attending to it in its early stages, at which period it is too frequently neglected. The appearances of the urine connected with the formation of white and of red sand were particularly described, and the acid and alkaline plans of treatment explained, in reference to the morbid secretion of the phosphates, and of uric acid, and to the prevention of the concretion or agglutination into calculi.

March 19th.

Mr. Ritchie, *On the Methods which have been proposed for measuring the relative Intensities of Natural and Artificial Lights.*—In a few introductory remarks, Mr. Ritchie exhibited the rise and progress of this interesting department of optical science. He observed, that men had been accustomed to look at the sun, moon, planets, candles, lamps, &c., for an unlimited period; yet it was not till the year 1730 they had formed any definite idea with regard to the relative quantities of light afforded by these various sources. In the year 1730, the celebrated Bouguer published his ingenious little work, entitled, "Essai d'Optique," and in 1760 he completed what was imperfect in his first essay, in his elegant work, entitled "Traité d'Optique sur la Gradation de la Lumière." About the same time, Lambert, whose name is well known to the scientific world, published his profound physico-mathematical work on the Mensuration of Light, to which he gave the name of "Photometria," or the art of measuring light. Nothing more had been done towards the advancement of this interesting and useful subject till Count Rumford re-invented the method of shadows, described by Bouguer, in the introduction to his "Traité d'Optique." So little, indeed, was the ingenious work of the French philosopher known in this country, that the method of shadows, as it is commonly called, was for a long time considered as the invention of our indefatigable countryman. That Count Rumford invented the method of shadows, without knowing what Bouguer had done, there can be no doubt, as he declares he had not even heard of Bouguer's work till many years after his discovery of the method of shadows, when Laplace put it into his hands, and shewed him the method distinctly developed in the writings of Bouguer.

Mr. Ritchie then examined the principle proposed by Bishop Watson, and afterwards more fully developed by Professor Leslie; namely, the mode of ascertaining the relative intensities of different lights, by its absorption and conversion into heat. The lecturer clearly demonstrated that this method could never be employed with even an approach to accuracy, when the colours of the lights were different, as in that case the heat absorbed is by no means proportionate to the *illuminating powers* of the two lights. Mr. Ritchie then described his exceeding delicate photometer of equality, founded on the same principles with the differential photometer of Professor Leslie, and shewed that when the colours of the lights were nearly the same, the instrument gave a very good approximation to the truth; but that when the colours of the lights were different, the same objections applied to it which he had made against the use of Leslie's photometer. He then described the construction of his other photometer, founded on the principle discovered by Bouguer, viz., that the eye can detect a very small difference between two discs of white paper, placed in juxtaposition, when they are nearly equally illuminated. This instrument consists of a square box, open at both ends, and containing two mirrors, placed at right angles to each other. The lid of the box has a rectangular opening, covered with a slip of fine white paper. The instrument is placed in a line between the lights to be compared, and then moved towards the one or other, till the discs on each side of the line which separates the two mirrors be equally illuminated, and the squares of the distances of the lights from the centre of the instrument will give their relative intensities. In the course of the lecture he shewed the application of the instrument to determine the relative intensities of the brilliant light afforded by a small ball of lime ignited by the flame of oxygen and hydrogen gases, so importantly applied by Mr. Drummond, and that of phosphorus burning in oxygen gas. He stated that, from accurate experiments which he had made with Mr. Drummond at Trinity-house, the mean intensity of the light afforded by a ball of lime about the size of a large pea was equal to about eighty wax candles. The audience had a very striking proof of the accuracy of this statement, by observing that, though the lecture-room was brilliantly illuminated with gas, the shadows of the large chandelier and other objects were thrown on the walls, apparently as dark as if the ignited ball of lime had been the only light present. The lecturer concluded by examining the methods proposed for ascertaining the force of the sun's radiation, and shewed that none of those methods are sufficient for determining with any degree of accuracy the direct force of the sun's radiation at different heights, and in different latitudes.

March 26th.

Mr. Brooke, *On the Methods of constructing Tables of Lives, from which the Value of Annuities and Reversions might be computed.*—Mr. Brooke began by explaining the method of forming such tables from parochial registers; from which it appeared, that the lives embraced by tables so formed, are not selected, but taken promiscuously at their births; and that the number living at every age, according to these tables, must consist of both sick and healthy persons. The lecturer argued, that such tables were therefore unfit to be used in computing the value of annuities on healthy lives. Hence it becomes an important object to construct other and more correct tables for this purpose; and as the lives of the annuitants themselves appeared to afford the most certain data for estimating the average duration of such lives, on the supposition that the average state of health of this class of persons, at the time of their becoming annuitants, would not differ much in different individuals, it was stated, that several tables of such lives had been constructed within the last few years.

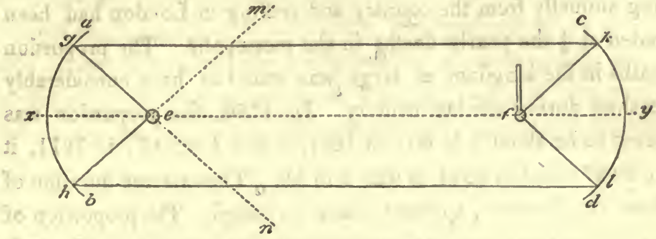
It was then shewn, that an increase had taken place in the population of the country, between 1801 and 1821, amounting to more than one-third of the number of inhabitants contained in the census of 1801; and that it was probable that the number at this time would be to that of 1801, as 3 to 2, or $\frac{1}{2}$ more; an increase which might alone (observed the lecturer) be expected to produce much of the distress now complained of. The increase in the population of London had not, it appeared, kept pace with that of the kingdom at large; in 1700, it was about $\frac{1}{8}$ of the whole population; in 1801, it was $\frac{1}{10}$; in 1811 and 1821, it was $\frac{1}{12}$; the number of persons coming annually from the country and settling in London had been estimated at $\frac{1}{4}$ the yearly deaths in the metropolis. The proportion of deaths in the kingdom at large was stated to have considerably diminished during the last century. In 1780, the proportion was supposed to be about 1 in 40; in 1801, it was 1 in 47; in 1811, it was 1 in 52; and in 1821, it was 1 in 58. The extreme duration of life does not, however, appear to have increased. The proportion of deaths in Pembrokeshire has been estimated as low as 1 in 83; and, in another county, it had been found as high as 1 in $22\frac{1}{2}$; in Russia, it was stated as 1 in 41, but attended with many remarkable instances of great longevity, there having died in 1828, 895 persons between the ages of 100 and 120; and 53 between 120 and 160. The relative mortality in summer and winter was stated to be about as 57 to 72; and the difference of deaths, in the large towns and the country, nearly as 2 to 1. The well-ascertained difference in the duration of life among males and females was also pointed out, that of the females being the greatest at all ages; and it was shewn, that, as a compensation for the greater mortality of males, the number born, to the number of females, was very nearly as 20 to 19.

Presents of geological specimens, from Ceylon, presented by Dr. Davy, and numerous other things, were upon the Library table, on this as upon all other occasions of these meetings.

April 2d.

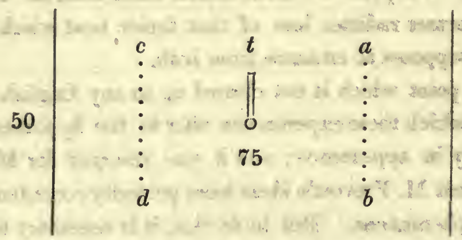
Mr. Ainger, *On the Theory of the Radiation of Heat.*—The purport of Mr. Ainger's observation was to explain one or two facts connected with the radiation of heat, and the apparent radiation of cold, which, though familiar to those who have studied the subject, are by no means generally understood, and are unexplained in all English works upon this branch of science.

The experiment made by M. Pictet, of placing a piece of ice in the focus of one mirror, and a thermometer in that of another, and the explanation of the phenomena furnished by the hypothesis of M. Prevost, are too well known to require more than a brief notice. The experiment is illustrated by the following diagram, where *a b* and



c d are sections of two parabolic mirrors, whose foci respectively are *e* and *f*, and whose common axis is *x y*. It had been observed, that if a hot body were placed in *e*, it immediately raised the temperature of a thermometer at *f*, and it was proved that this effect was not due to any direct radiation from *e* to *f*, but to the reflection of the cone of heated rays *e g h* from the mirror *a b*, and to their concentration by a second reflection, forming the cone *f c d*, at whose apex is the bulb of the thermometer. When this experiment was repeated, with the substitution of ice at *e*, and when it was found that the thermometer at *f* was depressed, much more than was due to the direct effect of the ice upon it along the line *e f*, it was inferred, that since cold is reflected and concentrated by mirrors exactly like heat, cold must be an independent principle or substance, and not merely a privation of heat. The hypothesis of M. Prevost removed all difficulty from this experiment. He supposed that all bodies in nature radiate heat in proportion to their temperatures, just as all luminous bodies radiate light. To understand, therefore, the effect produced upon the thermometer, it was necessary only to observe, that when the ice was placed at *e* it intercepted all those rays of heat from surrounding bodies comprised within the cone *e m n*, which have previously passed through the focus, and had been concentrated on the thermometer. The effect of the ice is therefore perfectly intelligible, without supposing any radiation of positive cold, but merely by considering that it intercepts rays of a higher, and substitutes its own of a lower temperature.

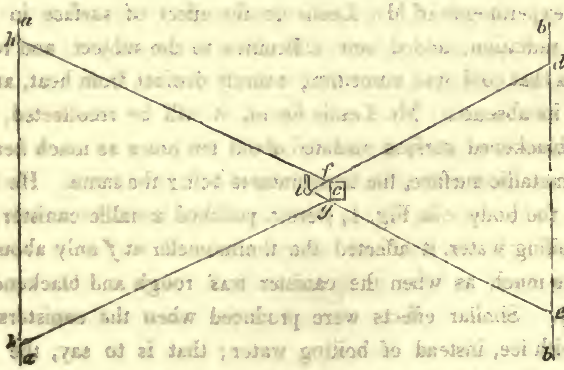
Thus far is the theory of radiant heat generally explained, though frequently with this defect, that the temperature of the body at *e* is compared with the temperature of the thermometer, and the effect ascribed to their difference. This is not the correct way of expressing the fact, and it diverts the mind from the circumstance, that the effect is due solely to the difference between the temperature of the body *e*, and that of the various bodies comprised within the cone *e m n*. The inaccuracy of this mode of describing the matter will be rendered evident by imagining a thermometer as *t* in Fig. 2, suspended be-



tween two walls whose temperatures were 50° and 100°; the thermometer would, in this case, exhibit a temperature of about 75°. If now a screen, with a temperature of 80° or 90°, were placed at *a b*, it would depress the thermometer, though of a temperature higher than that of the thermometer. In like manner, a screen heated to 60° or 70°, and placed at *c d*, would elevate the thermometer, though inferior in temperature.

The experiments of Mr. Leslie on the effect of surface in influencing radiation, added new difficulties to the subject, and revived the idea that cold was something entirely distinct from heat, and not merely its absence. Mr. Leslie found, it will be recollected, that a rough blackened surface radiated about ten times as much heat as a bright metallic surface, the temperatures being the same. He found, that if the body *e* in Fig. 1, were a polished metallic canister, filled with boiling water, it affected the thermometer at *f* only about one-tenth as much as when the canister was rough and blackened externally. Similar effects were produced when the canisters were filled with ice, instead of boiling water; that is to say, the rough black canister depressed the thermometer nearly ten times as much as the bright canister, thus appearing to indicate, that cold was something distinct from heat, and that its radiation was affected by surface precisely like heat. For it is evident, that the hypothesis of M. Prevost will not of itself explain these facts. If all bodies, whether hot or cold, radiate heat, and if their different effects be ascribable only to the different intensities of their radiation, it is obvious, that, by weakening the radiating power of a comparatively cold body, we should increase its cooling influence; and that the bright ice-cold canister ought to depress the thermometer more than the black one, because the former radiates less of that feeble heat which M. Prevost's theory supposes to emanate from both.

This is the point which is not cleared up in any English work;—the objection which these experiments offer to the hypothesis of M. Prevost is only in appearance; and it was reserved for M. Fourier to point out that M. Prevost's ideas were perfectly consistent with all the observed phenomena. But to do this, it is necessary to remember, that surface influences the *reflection* of heat in a manner directly the reverse of its effect upon radiation; that is to say, a bright metallic surface *reflects* about ten times as much heat as one that is rough and blackened. When this circumstance is combined with the other considerations, it will be evident, that Mr. Leslie's experiments suggest no difficulty, but that they are in fact perfectly reconcileable with each other, and with the theory of M. Prevost, as the annexed diagram will explain.



Here the mirrors may be omitted, as in fact they merely complicate the question, without at all assisting in its solution. Let *a a* and *b b*, Fig. 3, represent parts of two opposite walls of a chamber; suppose *c* to be one of Mr. Leslie's canisters filled with ice, and *t* to be a thermometer. The temperature of the canister is 32°; that of the walls we will suppose to be 64°: they will therefore radiate heat, as regards its temperature, with double the intensity of the canister. Imagine the thermometer to have acquired the temperature of the walls before the canister is introduced into the apartment. On placing the latter in the position shewn in the diagram, it will intercept, from the thermometer, a pyramid of rays emanating from the wall *b b*, whose section is *t d e*, and it will substitute a smaller pyramid, whose section is *t f g*, which would be equal in effect to the larger pyramid, if the temperatures were equal, because the smaller makes up by its nearness to the thermometer what it wants in extent of base. The temperatures are, however, different; the rays of the larger pyramid are from two sources, one being the direct radiation of the wall *b b*, and the other reflection from that wall of rays arriving from the opposite wall *a a*; but both these sources are at 64°; those of the smaller pyramid are also from two sources; one, which is the radiation of the canister at 32°, and the other, the reflection from the canister of the opposite pyramid of rays from the wall *a a*, having the base *h k*, and arising from a source at 64°. It is evident, therefore, that the heat flowing to the thermometer is diminished by the interposition of the cold canister, because the sum of the radiating and reflecting powers of any surface or surfaces is nearly a constant quantity; therefore, if one surface, as the wall *b b*, both radiates and reflects heat at 64°, it must give more than the canister, which reflects at 64°, but radiates only at 32°. This difference will be greater or smaller, according to the surface given to the canister being greatest with a black surface, which radiates nine parts, and reflects but one out of ten; and least with a bright surface, which radiates but one part and reflects nine. The whole may be stated and compared in figures; in doing which, it is not necessary to take into account the heat intercepted by the canister, because that is the same whatever be its surface.

In the first place, when the surface *f g* is bright, the canister radiates one-tenth of its own heat, and reflects nine-tenths of that which strikes it from the wall *a a*.

One-tenth of the heat of the canister at 32° may be called .	3.2
Nine-tenths of the heat of the wall reflected from the canister, may be called 64 × 0.9, or	57.6
Total quantity of heat supplied by the canister in return for what it intercepts	60.8

Suppose next, that the black side of the canister is presented to the thermometer,

The canister now radiates nine-tenths of its own heat at 32°; then 32 × 0.9 is	28.8
It reflects one-tenth of the heat at 64°	6.4
Total quantity of heat returned by the canister in exchange for what it intercepts	35.2

It will now be clearly seen, that the bright side of the canister gives, by the double means of radiation and reflection, nearly twice as much heat as does the black side, in return for what is equally intercepted in either case. The bright side should therefore depress the thermometer least, as it is found to do; and thus are all the difficulties of radiant heat and cold fully explained on the ingenious and fortunate hypothesis of M. Prevost, requiring no frigorific principle, nor any particular assumption concerning the nature or cause of heat. It accounts for all the phenomena hitherto observed, and relieves the mind from the unphilosophical idea of a separate principle of cold; an idea which is as painful and repulsive to the imagination as the idea of a principle of darkness independent of light; yet the latter would almost be involved in the former, when it is considered in how many and how various situations heat and light are united, obeying the same laws, and exhibiting the same relations.

April 9th and 16th.

No meetings took place on these evenings, in consequence of their occurring in Passion and Easter weeks.

April 23d.

Mr. Faraday, *On the flowing of Sand under Pressure.*—The effects illustrated this evening, by a variety of experiments, are those which were obtained by M. Huber Burnand, when he was engaged in constructing an anemometer. Our readers will find them already described in the *Journal* at p. 396, vol. v. of this series, to which, and to the original paper in the Bib. Univ., xl. 22, we will refer them.

April 30th.

Dr. Clarke, *On the Ascent and Descent of Mont Blanc.*—On a former evening in the last season, Dr. Clarke gave an account of his own ascent up Mont Blanc, and illustrated it by numerous paintings and engravings, and by an abundant selection of specimens from the animal, vegetable, and mineral kingdoms. On that occasion, he had not time to enter so largely into the natural history of the mountain and its neighbourhood as he desired, as will be seen by the report given of it at p. 385, vol. v. of this Journal. On the present occasion, he rapidly passed over the ground he formerly described, and expanded upon those parts which were then merely noticed. As before remarked, we believe that an account of the excursion will shortly be made public in a separate form.

May 7th.

On the Measurement of a Base in Ireland, for the geodetical survey now in progress under the direction of Lieut.-Col. Colby.

The subject this evening was delivered to the members by Mr. Faraday, who was allowed the use of the apparatus constructed by Messrs. Troughton and Simms, for the East India Company, and was instructed in the various parts of the subject, by Lieuts. Drummond and Portlock, who have been extensively engaged in the measurement of the base, and in the general triangulation.

The objects contemplated in the survey of a kingdom may be considered as twofold, and classed under the heads topographical and geodetical. To the former belong the construction of maps and plans, and the delineation of ground, conveying all the information that may be required for those local improvements, of which a knowledge of the ground is an essential preliminary; and serving at the same time as a basis for the valuation of the land, either confined to subdivisions of considerable extent, yet sufficient for the purposes of taxation, or descending, like the cadaître of France, to the more minute limits of private property.

To the latter belongs the task of fixing the relative positions of places; of assigning their latitudes and longitudes; of connecting the different observatories—an important object in astronomical science,

—and finally of contributing to the determination of the size and figure of the earth. Though their ultimate objects are, however, very different, the connection between these two branches is intimate; the basis of both being a triangulation, connecting the principal points of the kingdom, and proceeding from one measured base; the only difference being, that a degree of accuracy is necessary when the objects of science are contemplated, which, in the other case, would be a useless waste of labour and ingenuity.

The survey of Ireland, begun in 1825, in consequence of an application from Parliament to the government, embraced both these objects; the principal one undoubtedly, a new valuation of the land for the purpose of equalizing the distribution of the taxes levied by the Grand Juries; but at the same time the interests of science were not overlooked, and it was recommended that this survey should be executed in a manner that might conduce to the advancement of science, and be creditable to the reputation of the country. The Duke of Wellington, at that time Master General of the Ordnance, selected Colonel Colby of the royal engineers, to carry the wishes of Parliament into effect, and placed at his disposal liberal means for executing this important work.

The period at which attention began more particularly to be turned towards the figure of the earth, may be dated about the end of the 17th century. Richer, a French academician, who had been sent to Cayenne in 1762, to make some astronomical observations, was struck with the singular fact, that his clock, which had been regulated to mean time at Paris, had altered its rate, and lost 2½′ per day. He determined the length of the seconds pendulum at Cayenne, and found the opposite effect on returning to Paris; hence he inferred, that the force of gravity was different at different places on the earth's surface. Much controversy was excited by this discovery; some doubted the fact, others denied the explanation, while many attributed it to errors of observation. But Newton shewed that this effect was a necessary consequence of the figure of the earth, produced by rotation on its axis, combined with the principle of gravitation. And though the problem of the figure of the earth, deduced from the laws of equilibrium, transcended at that time the powers of analysis, his unrivalled sagacity enabled him in this, as in many similar cases, to obtain a remarkable approximation to its solution. He estimated the compression at $\frac{1}{230}$, or that the polar axis was shorter than the equatorial diameter, by that amount. Cassini, from the measurement of an arc of the meridian extending across France, had come to the opposite conclusion, that the earth was elongated towards the poles, and hence a warm controversy arose among several of the distinguished philosophers of that age.

To decide this question by experiment became an object of the deepest interest to the scientific world; for not only was it important and curious of itself, but Newton had further shewn, that to this aberration from the spherical figure, were due the phenomena of the precession of equinoxes, and the nutation of the earth's axis. It was a task, however, beyond individual means, and which governments alone could execute. By her efforts in this respect, France stands pre-eminently distinguished; she sent her ablest academicians to Lapland and Peru; provided them with abundant means, and furnished them with the best instruments that could be procured. Even Italy contributed her share. But England, whom the fame of Newton and the interests of navigation, ought to have excited to honourable rivalry, remained insensible and inactive.

Though the results of these measurements verified the general fact of compression, yet many singular discrepancies appeared, and the problem was found to be by no means of such easy solution as was at first supposed. These discrepancies are now known to proceed from partial irregularities in the surface of the earth, and also from errors committed in the measurements themselves; but they demonstrate the necessity of multiplying those measurements under different meridians, and of collecting more unexceptionable data before the problem can be satisfactorily solved.

Deduced as the figure of the earth is at present from the combination of measurements of very unequal value,—the more recent executed with every refinement of art and science,—the earlier performed with very inferior means,—with ruder instruments, and consequently with more vague notions of accuracy on the part of the observers, than would now be admitted, it cannot be wondered at that anomalies still exist, which with more unexceptionable data would cease to appear.

The mean of the best measurements may be taken at about $\frac{1}{300}$. The amount deduced by Laplace in a manner totally different, by separating and estimating those inequalities in the moon's motion due to the excentricity of the earth, is $\frac{1}{305}$.

The first and perhaps the most difficult of these operations consists in the measurement of a base line, on the accuracy of which depends that of every other distance, and finally, of the whole meridional arc. The improvement in the apparatus used for this purpose has been great and progressive, from a simple wooden rod laid on the ground without supports, without correction for moisture, temperature, or curvature, to the beautiful and delicate apparatus invented by Lieut.-Colonel Colby, and executed with great ability by Messrs. Troughton and Simms. This apparatus consists of six bars each, 10 feet in length, and constructed on a principle of compensation, so as to maintain one invariable length during every variation of temperature. This is effected with the aid of two different metals, viz. brass and iron, and according to a simple, though very efficient arrangement.

The bars, when placed in line, are not in contact, but an interval of six inches intervenes between each, which intervening space is measured by two microscopes, arranged in a similar manner to that of the bars, the foci being, in this case, the two points whose distance from each other remains invariably the same. The six bars with their microscopes, forming a length of 63 feet, were placed for the most part on one level, and the next series rose or fell according to the nature of the ground. This rise or descent took place in vertical lines, and was effected by an optical contrivance, by means of which it could be accomplished with nearly equal facility for any distance, between 4 inches and 8 feet.

The means adopted for rendering the principle of compensation perfect, by equalising the rate at which the metals change their temperature; the various mechanical adjustments and contrivances required to render the principle just described of easy and certain application, would require, in order to be properly understood, a more detailed description than the limits of the present notice will permit. But we may add, that this method was in principle the same as would be adopted for determining the length of a standard scale, and hence some idea may be formed of the labour incurred in this work; while the tests to which it was subjected afford some interesting proofs of its accuracy.

The total length of the base was 41640.879 feet, or 7.886 miles. Along the line at certain intervals, fixed points were left during the measurement, and secured with great care, and then by means of a triangulation, in which particular contrivances were employed to give more than usual precision, these distances were compared against each other. Thus, for instance, the distance between the second and third points being by measurement 1050 feet, was deduced from the distance between the first and second points, and found to be 1049.993 feet. Again, the distance between the third and fourth points, being by measurement 2100 feet, was deduced from that between the first and third, and found to be 2099.995, and the comparisons of the other portions gave results equally satisfactory.

The place selected for the measurement of this base was an extensive plain on the east shore of Lough Foyle, in the north of Ireland, and, generally speaking, was exceedingly favourable for such an operation. One obstacle, and that of rather a formidable nature, was presented by the river Roe, which, crossing the plain, intersected the line of the base. At the place of intersection, the breadth is 460 feet, and the greatest depth about four. It was deemed advisable to measure this twice, as the only doubtful part of the whole work, and the result of the two measurements was a difference of $\frac{1}{38}$th of an inch. On the whole base line, the probable error cannot exceed $1\frac{1}{2}$ inch. The extremities have been secured in such a way as to last for ages; and the government have, by the advice of the Royal Society, purchased the ground on which they rest, with a view to ensure their preservation.

While this operation was going on, the triangulation was at the same time rapidly proceeding, and has now extended towards the south as far as the Keeper Mountain, in the county of Tipperary, while Nephin may be considered its boundary in the west, and Kippure in the east. The whole forms a beautiful series of triangles resting on the principal mountains of Ireland, connected with those of Cumberland, the Isle of Man, North and South Wales, and covering the Irish Channel. These triangles are for the most part of great extent; the area of one formed by Culsheugh, Kippure, and Keeper Mountains, is about 3800 square miles, or nearly $\frac{1}{8}$ of the whole surface of Ireland; its spherical excess being nearly 50″. Many of the sides are not less than from 80 to 90 miles, while some of them extend upwards of 100 miles. These immense distances have generally been observed by the aid of plane mirrors reflecting the solar rays, a method which has proved of eminent service in the triangulation of Ireland. It remains to add the necessary astronomical observations for determining the latitudes of some of the principal stations, as well as the direction of the meridian, and it would contribute very especially to enhance the value and extend the utility of the work, if the difference of longitude between the extremities of the kingdom, now connected by triangulation, were also determined by signals. This may be considered with regard to an arc of longitude, as analogous to determining the amplitude of a meridional arc, and not only assigns the difference between the places where the signals are observed, but furnishes the means of determining the longitude of every other point connected by the triangulation, and contributes to the determination of the figure of the earth, more especially that portion embraced by the triangulation in question.

We have great pleasure in adding that the geodetical operations of India have of late received a fresh impulse; the most perfect and splendid instruments ever produced in this or any other country have of late been sent out by the East India Company for this purpose; and further they have been consigned to hands, by whom, we believe and hope, they will be used with an energy and skill, worthy the liberality and munificence with which they have been provided.

Such is an imperfect sketch of what is now doing by this country to advance this department of science; and if we add to it the late establishment of observatories at the Cape and New South Wales, it may be said with confidence, that the reproach too often justly made, can no longer be urged against the government, of indifference to the objects of science, unless their immediate tendency be visible, to increase the political power, or advance the commercial interests of the country.

May 14th.

Observations on Lithotrity, illustrated by Experiments, referring more especially to the Improvements introduced by Gruithuisen, Civiale, Le Roy, and Huerteloup.

Mr. Gilbert Burnett, who introduced this subject to the attention of the members, stated that he should purposely avoid (as far as such neutrality was practicable) entering at all into the controversy which has been, and still is carrying on, between several claimants, with respect to the original discovery of the process, and the invention of its several parts, and that his observations would be chiefly confined to points of more essential importance, viz. the operation itself, and the instruments with which it can be most effectually and most easily performed. Even in France, the country where most has been done, and the chief improvements made, much uncertainty prevails as to the just appropriation of the several stages of discovery which have led to the successive amelioration, and almost the perfection of the lithotritic process. Gruithuisen, Elderton, and Lukin, would each appear to have done something, though much less than Civiale, Le Roy, and Huerteloup; but (added he) it is a far more grateful task to shew the results of their united labours as colleagued in the advancement of lithotrity, than to view them as rivals, contending for the possession of the separate parts of that which, as a whole, sheds glory on modern surgery, but which, if divided, would be so imperfect as to add little to the reputation of either.

It is a matter of notoriety, that the female urethra may be so far dilated as to allow the extraction of stones of considerable size from the bladder; and, in the male, small calculi are sometimes voided through the natural passage, or even have been occasionally extracted by the means of lengthened forceps. What could formerly be done only in a few cases, and those where the stones were small, can now be effected in the majority of instances, and even when the stones are large; for lithotrity is an operation by which calculi of almost any size, at least to that of 24 lines in diameter, may be reduced to powder, or to fragments so small that they can be washed out of the bladder by injections, or carried away in the ordinary fluxus urinæ.

The chief principles of lithotrity, as is often the case with modern discoveries, may be found scattered through centuries, and some of them belong to very early time; for so long as men have been intolerant of pain, so long must they have attempted to mitigate the tortures of that most dolorous malady, stone in the urinary bladder; and long before even a thought of cutting into the bladder would have been suggested, repeated efforts must naturally have been made to void the calculus through the urethra; but to effect this, when the stone is large, it must be broken, and to break it, *straight* instruments must be passed through a *curved* passage; two preliminary circumstances which have baffled the ingenuity of surgeons until the present day. The ignorance of the ancients of human anatomy would seem to have led them nearer to one of these points than the comparative proficiency of the moderns; for they, unconscious of the curves of the urethra, used *straight* sounds, a practice which has been generally condemned, and even the possibility of which has, with some few exceptions, been, till very lately, most strenuously denied. The greatly curved sounds and catheters have, however, for many years, been lessening their sweeps, and a straight rod, with a quadrant curve at its extreme, is certainly the most efficient for examining the contents of the bladder. As to the destruction of the stone *in* the bladder, by means of instruments passed, *not* through the urethra, but through the perineum, so that it might be voided piecemeal, the plan is so ancient, that the word *lithotomy*, which, in truth, signifies *cutting the stone*, was thence derived, although it has since been applied to the operation of cutting the bladder for the purpose of extracting the stone entire, which otherwise might rather have been termed cystotomy; and as to the injection of the bladder, and the dilatation of the urethra, so that small stones might be extracted, or washed through its enlarged canal, the practice has never been lost sight of, and not unfrequently employed. But ancient as these principles of lithotrity undoubtedly are, they were practically of as little use for the removal of urinary calculi as if they had never been known; nor was it till the present day that surgery could boast their efficient union.

Ages have succeeded ages, during which the attention of surgeons has chiefly been directed to the improvement of the operation of cutting for the stone, which has indeed been so far perfected, that its performance is frequently completed in less than one minute of time, so that the sufferings of the patient, even if severe, are extremely short in their duration. But all would not suffice to disarm the knife, the bistoury, or the gorget of their terror; and the operation which Hippocrates regarded with so much dread as to compel his pupils to swear never to perform it, has ever continued so formidable, that every department of science has been applied to, in the hope which till now was vain, to escape its dangers. But the saxifrages of botany, and the lithontriptics of chemistry, were equally ineffectual, although the necessity of getting some fragments of the stone, in order to ascertain what menstruum would dissolve it, may seem to have led the way to the invention of those instruments by which it may now be totally destroyed: the apparatus best fitted for the destruction of various calculous concretions may thus be succinctly described. But first let it be premised that the elements of this operation are—

1. The reduction of the curves of the urethra to a right line, so that straight instruments may be passed through it, and its capacity dilated.

2. The introduction of an apparatus, the different parts of which are fitted to sound, to seize, and also to destroy the stone.

3. The injection of fluid into the bladder, by which, during the operation, its parietes are kept from coming in contact with the instruments, and by which the detritus may be afterwards washed out.

These elements of lithotrity may claim, in part, a very early date; but although long known, it was not till lately that they became of much practical importance; and to the distinguished foreigners already mentioned, is the credit due of combining them in one efficient system, as well as of constructing a series of very ingenious instruments, by which the operation not only can be, but has been, frequently performed with success.

The schemes of Elderton and Lukin are not dwelt on here, for the instruments of the former were never made, nor were his proposals reduced to practice; and the apparatus of the latter, although ingenious, and considering the date of the manufacture, highly creditable, is certainly not fitted to fulfil the purposes for which it was designed.

The simplest form of apparatus, and that which is found most useful for the destruction of small calculi, consists of a straight canula, containing within it a three-branched forceps, the elasticity of which causes its tentacles to expand when pushed beyond the end of the canula by which they are concealed; or rather, when the canula is withdrawn, so as to permit their development. The ends of the forceps are curved, and all of different lengths, so that they cannot include any fold of the bladder, even were it to contract or fall upon them: they are all united at their manual extreme, forming, by their conjunction, a tube, through which a drill [the length of which is regulated by the length of the forceps, so that it cannot injure the bladder, and the crown of which may be either excentric or not, according to the size of the calculus] acts, and grinds away the stone: if the stone be small, it may at once be crushed by the pressure of the drill when held firmly by the forceps; or, when bored through once, it need not be let entirely to slip from the forceps, in order to make another perforation; for if the manual end of the instrument be tapped while the branches are slightly relaxed, a fresh surface will be presented, which may be ascertained by the resistance afforded to the drill. At the side of the canula is a foramen, with a screw or plug, by loosening which the fluid may be allowed to escape.

For the destruction of stones exceeding eight lines in diameter, this instrument is tedious; and, when repeated sittings are required, it might so happen that one branch of the forceps might enter a perforation in the stone, and occasion much difficulty in the operation, if not danger to the patient. For the destruction of stones from eight to fourteen lines in diameter, the crown of the drill has been enlarged, so that they may be reduced at once to fragments; and this has been done in various modes, the most efficient of which would seem to be the projection of a small branch, somewhat resembling a comma, from the side of the crown of the drill, after a perforation has here begun, so that it is rendered still more excentric in its action than the mere excentric drill: this projecting branch or comma can be protruded or withdrawn at pleasure, by a rod which runs through the centre of the drill. The forceps and drill, thus modified, have been named, "Instrument à trois branches et à virgule." The invention of the previous instruments we believe to rest with Gruithuisen, Civiale, and Le Roy; but they have each been improved by Baron Huerteloup in design and construction. The following, which are well fitted to seize and destroy stones exceeding fourteen lines in diameter, and those which are flat, we are informed belong solely to the latter.

It is a fact well known, that when the stone is large, the bladder in general is small; not only relatively, but absolutely smaller than when the stone is of moderate dimensions; and, furthermore, that the bladder contracts frequently upon the stone, and assumes a preternatural muscularity, the inner surface having not only a rugose, but a columnar character. This, of course, will prevent that viscus from being freely distended by injection, will keep the stone nearer the neck of the bladder, and might interfere with the fully extended tentacula of the three-branched forceps. The larger the calculus is the more numerous of course must be the perforations made in it before its substance will break down, and even with the excentric or virgule drills, the process would still be tedious, although much more speedy than if simple perforations alone were made. The irregularity of stones likewise gives rise to difficulty in their apprehension and firm retention, as it seldom happens that all the claws, moving simultaneously, close upon the calculus, especially if it be a large one; so that when pressed upon by the drill, it is apt to slip or to elude the grasp.

Furthermore, that form of the three-branched forceps previously described, the whole of the tentacula of which are in union, and of necessity move together, can never, at its utmost stretch, project a span of more than one-third of its circumference; and even this diminishes rapidly towards the canula, there being no means of keeping the springs at any greater width from each other.

To overcome these difficulties all the instruments have been modified, and others added, by Baron Heurteloup, who designates this improved apparatus, which is fitted to destroy stones twenty-four lines in diameter, "Evideur à forceps," *(excavator with forceps,)* and it consists of several parts, which may be briefly thus described: The straight canula, instead of being a simple cylinder, as in the other apparatus, contains fixed within it another tube nearly equal to itself in length, which is four-sided, so that the chief channel is subdivided into five compartments, a square central one, and four circumferential passages, through each of which latter passes one branch of the forceps, which here consists of four instead of three tentacula. Three of the branches are terminated at their vesical extremes by simple curves, the fourth and longest by a grooved knob, which closes in the whole, and facilitates the introduction. The four branches of the forceps being separate from each other, and sliding in separate grooves, may hence, by knobs fixed to their manual ends, be each moved independently of the other, or by a case which incloses the knobs of all, be made to act simultaneously; thus combining, with all the advantages of the common forceps, the very important privilege of separate, and more or less conjoint action. The four-branched instrument, by the withdrawal of one of its tentacles, may also be converted into a superior three-branched forceps, as the span of its branches will be the diagonal of its square, instead of the mere base of an equilateral triangle, as in the former case; so that stones of much greater magnitude can be seized, and with much less extension of the instrument: furthermore, the knobbed tentacle, when withdrawn, will still more widen the grasp of the other three, and keep them fully and firmly apart. The separate motions of the tentacles will also allow each to be separately pulled on, so that every one can be firmly acting on the stone, a very important circumstance. The knobbed tentacle may, subsequently to the prehension of the stone, be detruded, and it will also close upon it; and when all four branches act, the retention is the most firm that can be conceived. Through the central cavity the subsidiary instruments are introduced; these consist of a simple steel rod, called the "Indicator," and a very delicate three-branched "assistant forceps," (*pince servante*,) designed to bring the calculus within the grasp of the larger and stronger instrument, should it not, in the first instance, be easily apprehended. The instruments destined to destroy the stone consist of a pointed steel rod, the "perforator," which, rotated by a common drill bow, makes a hole in the stone; and the "excavator," (évideur) which is a somewhat similar rod, the end of which is a very powerful rasp or file, which can be inclined, by means of a screw, to any angle the size of the calculus may require, and which, when rotated by means of the bow as before, excavates the stone, leaving nothing but a thin shell formed of its outer layers, which the withdrawal of the branches of the forceps will usually be sufficient to crush: should it, however, resist their force, the indicator may be used as a "percusseur," and one or two strokes of the simple steel rod upon the shell will inevitably reduce it to fragments.

Such is the economy of the excavating apparatus, which, for the destruction of large round stones, is incomparably the best instrument yet invented: but the seizure and destruction of flat stones, or the flattened shell-like fragments of larger calculi, have presented many difficulties to the operating lithotritist.

The "shell-breaker," or *brise-coque*, would seem to obviate these. It has been said to be merely an improvement on the *brise-pierre* of Amusat: in truth, the chief difference between them is, that, although both are ingenious instruments, the one is highly useful, the other inapplicable in practice.

The shell-breaker both seizes and crushes the calculi or fragments without the assistance of drill or perforation. Hence, it is necessarily made very powerful in its branches, and to allow such strength in its construction, without increasing too much the size of the tube, it consists but of two branches, which entirely fill up the canula, and are terminated by strong roughened jaws, which open by a spring placed between them, and receive the calculus, when detruded from the canula, and, closing in their retreat, crush the hardest stone or fragment between their hawk-bill ends. Motion is communicated to these branches by a double rack and two wheels contained within the handle, the inward and outward progress being commanded by a spring catch which regulates the rack.

It has been usual to operate on an ordinary bed or couch, and to fix the lithotritic instruments in a spring vice held by an assistant, while the surgeon completed the perforation of the calculus by the drill and bow; but when a much more powerful means, viz. the excavator, is employed, it becomes necessary that the instruments should be held more steadily; and for this purpose Baron Heurteloup has constructed a very ingenious bed, the "lit rectangle," to the front of which there is a moveable vice attached, and which can also be lowered as a whole, by moveable hind legs, so as to form an inclined plane, and elevate the pelvis, &c. without disturbing the patient. This, however, as well as the catheter with foramina several inches down the side, and the syringe with bows, although useful instruments, cannot be deemed essential to the operation; and they will, of course, be adopted or rejected, according to circumstances, of which the attending surgeon can alone be the proper judge.

In the library, amongst other objects of attention, were some calculating machines by M. Palarino of Genoa. By means of them, M. Palarino, who was present, worked various arithmetical questions put to him by those around, in doing which, the operations of addition, subtraction, multiplication, division, extraction of roots, &c. were performed with great rapidity. Besides these operations, there was a machine intended especially for ascertaining the interest of any sum at any rate, for any time, which appeared to answer its purpose exceedingly well.

May 21st.

On the Application of a New Principle in the Construction of Musical Instruments.—This was one of the series of the evenings devoted to the consideration and developement of various parts of the science of sound. The illustrations were given by Mr. Faraday, but, with the matter, were supplied by Mr. Wheatstone. The principle is the one now so well known for its popularity in the æolina, where a spring of metal being fixed by one end, in an aperture which it nearly fills, is thrown into vibration by the breath or any other soft current of air passing by it, and produces musical sound.

The general laws of the vibrations of rods and springs were first given, and partly illustrated by an instrument called a tonometer invented by Mr. Wheatstone, in which the sound produced by any length of an uniform spring could be ascertained. Then the application of these springs, in a great variety of instruments, was shewn, first in the mund-harmonica, or æolina, down to those of most recent construction. The arrangement of the single, double, and triple chords in the æolina, with the power thus given to play airs in one key, were illustrated by diagrams and practice.

The limited capabilities of this instrument then led to the consideration of those in which the power of fingering was introduced, and first of such as received wind from the mouth. Mr. Wheatstone's first keyed instrument made in 1818 was shewn, the flute harmonique, a German instrument of the same kind, and other arangements were shewn, in which springs were thrown into action, according as holes or keys were fingered, and thus performances executed. After these was introduced Mr. Wheatstone's Symphonium, a beautiful instrument, small in size, but of great power in expression, melody, and harmony: its capabilities were fully shewn by the performance of Mr. Godbé. The manner in which difficulties had been overcome, and in which also the principles of resonance had been taken advantage of, were explained and illustrated. The arrangement of the keys, so as to give extreme facility of fingering, was also explained.

Then came instruments in which more or less of the organ mechanism has been introduced. The Tshing, or Chinese organ, was first quoted as the origin of the European instruments, and the combination in it of the powers of the vibratory plate with a reciprocating column of air, was explained. Professor Kratzenstein, who had seen one of these instruments at Copenhagen, introduced the principle in a speaking machine which he was engaged in constructing. In 1780, Mr. Kernech of Petersburgh applied the freely vibrating tongues as a new stop in his organ. The Abbé Vogler also applied it to his orchestrian and various organs; Grenie applied it to organ-tubes, and probably numerous applications of this kind took place. In all these the tongues were connected with tubes, but it occurred that these might be left out, and Mr. Shultz of Vienna constructed his æol-harmonica on that principle. Then many other instruments were constructed in a similar way; and there were in the room Dr. Dowler's glossophane, Dretz's ærophone, an æolian, an instrument by Mr. Stumpf the harp-maker, and the most recently constructed instrument—an æolian organ, by Mr. Day of Cheapside. Mr. Stumpf played on his instrument to illustrate its character, and was accompanied on the harp by M. Stockhausen. Mr. Day's instrument was played upon by Mr. Wesley. The lower notes of the latter were exceedingly full and powerful. The scientific peculiarities of each instrument were noticed in succession.

May 28th.

Capt. Manby, *On the Means of preserving Lives in cases of Shipwreck, and on a New practical Mode of Hauling Life-boats,* &c.

through the Surf.—Capt. Manby this evening gave an account of his methods of casting a line from shore to a ship in distress, either by day or night, illustrating it by his own model of the apparatus used upon the coast. These having been published in various ways, we do not think it necessary to detail here. He then described the method which he has recently suggested, of hauling boats off from shore through the surf, that they may proceed to ships in distress. In cases of storm and shipwreck, the surf is generally so strong as to throw every thing back that tries to make way through it, oars being then found of no avail. Captain Manby proposes that, to meet such cases, an anchor with an attached buoy be carried out beyond the line of surf at low-water, a block made fast to the buoy, so as not to turn, and a rope rove through it, so long as to reach double from the buoy to the shore. Then, in time of need, a boat being fastened to one end of the rope, if the other end be laid hold of, and the rope hauled on, it necessarily carries the boat out to sea. But as, if left to itself at those times when not wanted, but waiting to be used, the rope would probably bury itself in the sand, provision is made against this, by attaching a buoy with a hanging noose to one of the halves of the rope, about half way between the anchor and the shore, and letting the other half or end pass through the noose; in this way the rope is suspended in the water, and retained out of the sand.

June 4th.

Mr. Brockedon offered some remarks upon the *Perception and Application of Colour.* His chief object was to state some curious facts upon ocular spectra, and to excite physiologists to such an examination of the structure of the eye, as might lead to a knowledge of the cause of those remarkable and beautiful effects which, in good vision, are always perceived after excitement by an object of any colour, when the complementary tint appears, or that colour which, combined with the impinging colour of the external object, produces white light. The most familiar mode of exhibiting these complementary spectra is, by placing wafers of different colours upon white paper, and, after gazing intently upon any one, suddenly removing it, when a contrary colour, but of the same form, is instantly perceived, and this often even when the eyes are closed. The demonstration of these complementary colours is generally known. Mr. Brockedon's mode of readily distinguishing them, we believe, is new. In the usual way, an equilateral triangle is made, each line is tinged or painted one of the three primitive colours, red, blue, yellow. Where these lines intersect, the red and yellow, overlying each other, produce orange; the red and blue, purple; and the blue and yellow, green. Draw this diagram as you will, the compound of any two primitives is always opposed to the third primitive. On the outside of this triangle a broad circle is drawn, and on the lines in this circle, which radiates from the centre of the six simple and compounded colours, these colours are painted, and each then blended into the other, so that a deviation of red towards yellow is more and more a yellowish red, until it become orange in the middle, between the red and yellow; the yellow, still increasing, becomes more and more yellow, until any red ceases, and on the yellow radius it is pure yellow. This then passes into green deeper and deeper, by accessions of blue; the blue into purple, by accessions of red, until the circle is completed in pure red. Upon this broad ring of blended colour place a card, revolving upon a common centre with the coloured ring, and having two holes cut diametrically opposite to each other, and opening upon the coloured ring below. Wherever this is placed, those colours which are seen through the holes would, if combined in coloured light, (not in the foul pigments of painters,) produce white, or colourless light. Thus, if the red and green are seen, on turning the upper card disk as the red became purpler, the green became in the opposite hole yellower, until perfect purple opposes perfect yellow, and so on. After gazing for a short time upon any colour presented through one hole, if a piece of paper be suddenly placed over it, the form of the hole will still be presented to the sensorium; but of the colour which may be seen in the opposite hole in the diagram. There are many applications of a knowledge of this fact; some, which are familiar, though before unnoticed, were exhibited by Mr. Brockedon. Coloured posting-bills printed in black ink, always excite, in a good light, the idea of the ink being not black, but the converse colour of the paper upon which they are printed; or, if the black letters be cut out, and a piece of white paper pasted behind, so as to appear like white letters, these present also the same contrast of colour: thus on a yellow paper, the black letters appear a deep purple, and the white ones, lilac, or purple combined with white, the intensity of the spectrum of colour being exactly in proportion to the actual light which is reflected from the black or white parts, and which dilutes the colour; on green paper the black letters are brownish-red, the white letters rose-colour, &c.

A curious instance of deception was mentioned as arising out of these spectra. Mr. Brockedon stated, that one day, when walking in the street, he slipt, and, like most people, turned suddenly round to see the cause, when, looking on the ground, there was the appearance of blood upon the spot where he looked, but in a moment after he perceived that wherever he directed his eyes the blood still appeared; but the impression soon weakened. Aware then that this was a deception, he looked for the cause, and found that he had been unconsciously following a man with a slop-pail painted of a bright green, of which the complementary spectrum presented was the red, which surprised him. A knowledge of these contrasts of colour is, in many ways, of great utility in decoration, in manufactures of pattern and shot silks, and other tissues; in dress, in the arrangement of an artist's colours in his picture. It does not in the least degree dispense with genius, but it is a means which talent may employ to facilitate its productions, to give vividness without intensity, and in compounding colours on the pallet, to obtain the requisite tints without *muddling*. It is important to the painter, too, to know that long gazing on one colour, renders his perception of another impure for a time. It explains, or at least offers a conjecture upon the cause of our perceptions of harmony in general arrangement. When two of the primitive colours, as red and blue, alone or chiefly, impinge upon the eye, the organ becomes distressed. If united, and forming purple, the eye is excited to compensate its impression by a yellow spectrum; these preserve the tone of the organ, or, if yellow and purple separately appear, they excite spectra which are complementary. But if, instead of blue and red united, and forming purple, they in separate colours affect the eye, then, each requiring its complement, yellow would be found in both, the red exciting green, a compound of blue and *yellow*, and blue exciting orange, a compound of red and *yellow;* the excess of yellow thus excited produces a painful emotion. Harmony may be restored by placing on the picture a small quantity of yellow, which relieves the eye from the duty of compensating the impression. The same applies to any two separate primitives.

Dr. Darwin was of opinion that mere fatigue of the organ, by any one colour, excited re-action, and produced the spectrum of another. This explains nothing; and though it may be difficult to find a structure for this curious function in the eye, there must be a physical and positive power possessed by some organ in the eye, which, stimulated by the coloured ray that falls upon it, is so exquisitely contrived, that, though whilst excited by a coloured ray, it imparts to the sensorium the idea of that colour, red for instance, yet it excites in the organ itself that idea of colour which subdues the excitement, when the impinging colour is removed, and restores to the organ of vision that perfect condition, which is requisite for the reception of new impressions.

Of some of the structures in the eye the use and principles are perfectly understood; and the laws which govern the refraction of lenses, in instruments made to assist the eye, govern also the crystalline lens of the eye itself. The profound investigations of those laws of optics by mathematics have been among its most beautiful applications; but the researches of physiologists have not been at all commensurate. They have been content with form, and regardless of function; and when a new structure has been first observed, the discoverer has been content with the honour of giving his name to a membrane or a duct, and left others to find out their use; the extreme delicacy of the various parts of the organs of vision renders it a subject of great difficulty; but Mr. Brockedon thought that it had not been so fairly grappled with as other subjects of science, which did not present half the beauty or interest in the investigation. He ventured to offer, only as conjecture, to be examined by those who had more knowledge, skill and opportunity than himself, that, in some parts of the eye might be found an organ with a structure analogous to the striæ, which, when parallel and equidistant, and amounting to 2000 or 3000 in an inch, were known, in reflecting, to disperse the light into colours of qualities, peculiar to the approximation of the striæ. Such a structure is certainly found in the delicate membrane discovered some years ago by Dr. Jacob of Dublin. When powerfully magnified, as drawn by Mr. Bauer of Kew, in the *Phil. Trans.* of

1822, it presented the appearance of a pinniform fibrous tissue, so arranged, that small changes in the tension of the larger fibres from which the parallel ones diverged would produce an immediate and general change in the proximity, but not in the parallelism of the smaller fibres; if the production of compensatory spectra lie in this membrane, confusion in the direction of the fibres will account for the imperfect vision of those who are insensible to perceptions of colour, and distinguish objects by the quantity, instead of the quality of the light which they reflect; or, when less imperfect, are sensible of certain colours only. This may account for the pictures of Paul Brill being always green, and those of Both *foxy*. The striated structure would serve also to explain why, whilst the eye still views a red wafer, a green fringe surrounds the disk, though the eye be fixed on a spot in the centre of the wafer;—'tis the intermediate fibres, which, partially excited after long gazing, extend beyond the impinging image to those fibres which are in repose. It is perhaps taking too great a license, to conjecture thus from analogy, upon a subject capable certainly to some extent of investigation, but which it does not closely follow in every point. The physical differences of coloured rays are known; their illuminating, heating, magnetising, and chemical powers differ. These, or some of these, appear to possess different stimulating qualities; and, acting upon so sensible an organ as that of vision, may account for effects whilst vitality exists, which cannot be readily perceived in the delicate organization of many of the parts after death. Besides the complementary spectra of colour, white and black mutually affect the organ with contrary spectra—as if pure light of a certain intensity only could keep the eye in repose. Mr. Brockedon thought a different compensatory power might exist for this in the collection or expulsion of the *pigmentum nigrum* which lies behind, and in immediate contact with the membrane of Dr. Jacob, where the fibres are excited to a change in their proximity;—it is evident, that a compensatory power for quantity of light exists independent of the power which governs images of coloured, or qualities of light, as those, who are insensible to colour, are sensible of form, and of the compensatory spectra of black and white. Mr. Brockedon expressed his regret, that the remarks which he had to offer were so crude, but he offered them as stimulants to the investigations of others upon a subject of which so little is known, but which is so beautiful and interesting, that he thought some benefit to science must spring from the researches to which he thus directed the attention of others; for wherever the power lies in our organs of vision, the curious effects of contrary spectra are interesting to investigate, and important to know.

Mr. Brockedon mentioned the researches of Mr. Field of Isleworth into this subject, and exhibited some instruments which Mr. Field had lent to him, by which he measured the combinations and intensities of colour. Three hollow wedges, glazed at the sides, and made to contain coloured liquids, were placed in a frame, so that a ray of light might be made to pass through all or any of them at the same time: the red wedge was filled with liquid madder; the blue wedge, with sulphate of copper; and the yellow, with chromate of lime, or tincture of saffron. These liquids were of such intensity of colour, that a ray, passed through the centre of each, would fall on paper, which received also a similar coloured ray from a prism: thus charged and placed in the frame, they were adjusted until the ray of light passing through all three was white: when this was the case, withdrawing the red wedge left a pure green; taking out the yellow, left purple; on removing the blue, orange; and, taking the centre of each wedge for zero, by graduating the top of the wedge, the proportionate quantities for the production of white light were unequal, but agreed singularly with those stated by Sir Isaac Newton. These three wedges were, from intensity and variety, capable of producing every possible tint and colour. To this instrument Mr. Field had given the name of chromometer. Another instrument invented by Mr. Field was also shewn, which, having a plano-conical lens—or, as Mr. Field called it, a lenticular prism, at one end, objects seen through it were beautifully fringed with colour, and presenting an involution of forms singularly curious. With this instrument he could so direct the dispersed light, that a yellow ray falling upon one eye, and a blue ray upon the other, these would, with both eyes open, excite the idea from both of green. Mr. Brockedon, in trying some experiments with the flat square bottles which contained the coloured fluids for the wedges, produced the same effect, laying on the eyes the flat sides of the bottles, one containing yellow, and the other blue; the impression of both at the same time was green; even with one eye, looking long at either colour, its complementary spectra would neutralize it to a certain degree, and suddenly withdrawing it, such spectra would be vividly seen. But one of the most remarkable appearances arose from looking edgeways through the bottles—when the compound colour, the pure colour, and the complementary colours were all seen at the same time: thus, if the red and blue bottles were brought near enough to touch on each side of the nose, the red on the right side, and the blue on the left, yet in looking against a window, so that each eye could perceive the window on the outsides of the bottles, the centre where the colours mixed appeared purple; and at first the right eye perceived red, and the left blue: but the complementary colour was soon so powerfully excited, that the sky from the window on the right appeared a very blue green, and on the left a red orange; and this so powerfully, that the experimenter would doubt in which hand he held the red, and which the blue. Intensity of colour thus presented to the eye is soon lessened, and even destroyed, by the complementary spectrum: having, with the flat side of the bottles covered the eye, the blue on the right excites an orange, and the yellow on the left a purple; remove the bottles, and then with both eyes open, there appears for a moment only a genial reddish tinge upon all objects: but a rapid alternation of opening each eye will present, for some time, all objects tinged with orange by the right eye, and purple with the left; and the vivid changes which arise from the contrast of colours is strikingly beautiful.

June 11*th.*

On the Laws of co-existing Vibrations in Strings and Rods.—As on former occasions on which parts of the science of sound made the subject of the evening's investigations, the philosophy on this occasion belonged to Mr. Wheatstone, the delivery to Mr. Faraday. The general nature of the vibrations of strings and rods producing sounds was first referred to, and the laws which governed them, when the vibrations were of the lowest mode, *i. e.* when the rods or strings were vibrating as a whole. Then the subdivisions of a string and rod were referred to, in which case nodes were produced. Taking the string first as the simplest case, it was shewn by experiment, that it might virtually be subdivided into two or more aliquot parts, each part vibrating with a velocity, increasing as the part was smaller, and the string in all these cases, producing a corresponding sound; the times of vibrations with the corresponding notes, were illustrated by experiments and diagrams. The subdivisions of rods were then referred to; and the forms also of the curves both of strings and rods touched upon, but it was stated that this part of the subject was still very obscure.

Having thus considered the different modes of vibrations, the co-existence of these modes in the same string or rod was taken up. Dr. Young's experiment upon the visible curve described by a pianoforte string, was here referred to; and his harmonic sliders were also made use of, to illustrate the effect of two or more series of undulations, and the form of the resulting curve, or wave. But the principal illustrations of this part of the subject were derived from the use of the kaleidophone, an instrument invented by Mr. Wheatstone, which at the same time that it allows the ordinary motion of a rod to be combined with one or more of the modes of vibrations producing sound, also renders the path of the end of the wire visible, and so gives, as it were, a picture of the vibrations, rendering each set visible and distinct from the others. As this instrument, with its principles and effects, is fully described in this Journal, vol. i., p. 344, we refer our readers to it on the present occasion.

A mode of counting vibrations and other rapid motions by the eye was referred to at the end of the evening, as promising some new and interesting results. This subject is at present under investigation.

In the Library, amongst other interesting objects, was a portable equatorial instrument, invented by Captain Kater, and laid upon the table by Captain Grover, to whom it belonged. It is a small telescope, only $7\frac{1}{2}$ inches long, with an aperture of $1\frac{1}{10}$ inch, which with a power of 80, shews very distinctly Saturn's ring, the eclipses of Jupiter's satellites, &c. The object glass is by Dollond. This is mounted in a very ingenious manner, so that right ascensions and declinations may be readily determined. There is also attached one of Captain Kater's micrometers, for measuring very small angles. The instrument was made by Mr. Robertson of Devonshire Street.

[Conclusion of the Weekly Evening Meetings for this season.]

MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *Dr. Mitchell's Method of working Caoutchouc.*—Soak the gum elastic in sulphuric ether, until soft and nearly inelastic, which in good ether will take from ten to twenty-four hours. Then if it is a plate, cut it with a wet knife or parallel knives, into such sections, or sheets, or shapes, as may be desired, and suffer them to dry; or if a bag, apply a pipe or stop-cock, and inflate with the mouth; if the bag should expand equally, inflate rapidly, but if unequally, proceed more slowly and with occasional pauses. By such means a bag may be made so thin as to become transparent and light enough to ascend when filled with hydrogen. By graduating the extent of inflation, a sheet of caoutchouc of any given thickness is produced. If for blow-pipes, or other purposes for which it is desirable that the bags should possess contractility, let them be inflated to the desired size, and after an hour let out the air. Ever after they will suffer as great a degree of extension, and again contract. If permanent sheets are wanted, the inflated bags are to be hung up until dry, after which no sensible contraction will ensue.

Bags softened by ether may be stretched readily by the hand, over lasts, hat-blocks, or other moulds, so as to assume the shape desired, and may be so applied to a variety of useful purposes. In the form of straps and twisted strings, its elasticity offers many useful applications. It is easily formed into tubes to connect apparatus, &c.

Some of the bags have been extended to six feet in diameter; one of these being filled with hydrogen, escaped, and was found 130 miles from the place. A bag, originally the size of an English walnut, was extended until 15 inches in diameter.

Dr. Mitchell states, that oil of sassafras softens caoutchouc so that it can be applied with a brush, and that upon drying by exposure to air, it becomes again simple elastic caoutchouc. Many applications of it as a varnish, in this state, are suggested.—*Franklin Journal*, v. 122.

2. *Force of Draught of Carriages.*—The following results upon the draught of carriages are by Mr. Bevan, and appear in the Philosophical Magazine.—"The actual *force of draught* of carriages upon common roads has become a subject of interest and importance, as it relates to the principles of conveyance by rail-roads, and more so as it demonstrates the importance of attention to the surface of the road. I, therefore, take the liberty of sending you the results of some experiments of mine on that subject, conducted with considerable care, in August, 1824.

"These experiments were all made or reduced to roads perfectly level or horizontal, to separate the mechanical force due to the inclination of the hill or plane, from the force necessary to overcome the friction of the carriage in its ordinary state as affected by the condition of the road, and by way of rendering them comparable with other experiments, which have been or may yet be made on this subject." I have considered the gross load of the waggon and burden to be divided into 1000 parts.

Description of Road.	Force of Draught.
Loose sandy road	222, 240, 165, 163, 166, 190, 240, 215, 240
Mean	204 or 1-5th.
Turnpike road new gravelled.	121, 130, 180
Mean	143 or 1-7th.
Ordinary bye road	91, 121
Mean	106 nearly 1-9½

Description of Roads.	Force of Draught.
Hard compact loam	61, 36, 61
Mean	53 nearly 1-19th.
Dry hard turf	40, 40
Mean	40 or 1-25th.
Turnpike road with little dirt	30
More dirt	39
Mean	34½ or 1-29th.
Turnpike road free from dirt	30, 30, 31
Mean	30⅓ or 1-33d.

From which it appears that five horses will draw with equal ease the same load upon a good hard turnpike road, as *thirty-three* horses can do upon loose sand! Or, if we assume the value of draught upon a well-formed road in good condition, at sixpence per ton per mile, the equivalent price of draught will be—

	s.	*d.*
Upon hard turf	0	7½
hard loam	0	9½
ordinary bye road	1	7
newly gravelled road	2	2
loose sandy road	3	1

—Vol. vii. p. 286.

3. *Strength of Wine and other Bottles.*—M. Collardeau has constructed a machine for the purpose of trying the strength of wine bottles. It has been presented to the Académie des Sciences, and reported upon by MM. Hachette and D'Arcet. The bottle to be tried is held at the neck by means of a lever, having three branches, which grasp it below the ring; being then filled with water, it is connected by means of pipes, with a forcing-pump, the pipe having a cap furnished with leather, which is firmly held down by the apparatus upon the mouth of the bottle; the pressure upon the parts here increases with the pressure of the water within the bottle. Besides the pump, levers, and connecting pipe, there is also a mano-

meter connected with the interior of the bottle to shew the pressure exerted. When a bottle is burst in this way by the hydraulic press, no violent dispersion of its parts takes place, unless, indeed, in place of being *filled* with water, a portion of air is left in; then when it breaks it flies to pieces, and would cause danger if exposed.

Bottles intended for the manufacture of brisk Champagne or Burgundy, being tried, were found to break with a force between 12 and 15 atmospheres, exerted from within outwards: a few rose to 18 atmospheres. Bottles which had contained Champagne of the finest quality, broke at the same pressures. Bottles which resisted the pressure of 12 atmospheres, usually broke with one or two atmospheres more, but the number of these was small. The fracture of bottles in the manufacture of brisk Champagne is from 10 to 20 per cent.; and in certain cases, which, however, are rare, almost the whole have been broken. It appears quite certain, that during the fermentation of the wine, the pressure rises above 12 atmospheres, but the full extent can only be ascertained by careful experiments made by the wine proprietors.

The commissioners then remark, that the best bottles intended for brisk wines are too weak; the general fault is want of strength and uniformity in the belly of the bottle, especially at the junctions with the neck and with the bottom.

As the greater number of bottles for brisk wines are of the same quality, it becomes a question why some should break and others not. This difference is supposed to depend upon the form of the neck and quality of the cork, allowing a little gas to escape in some cases and not in others. If the bottles and corks were all alike, all those which contained the same liquor at the same temperature would probably break at the same pressure. The only means of avoiding fractures is either to make the bottles sufficiently strong, or to allow a little escape of gas by the cork. The least thickness of glass in the belly of the bottle should be 2 millimetres (.079 of an inch), but generally it is only 1 millimetre at the part next to the bottom.—*Bull. Univ.* E. xiv. 80.

4. *On the Optical Influence of two coloured Objects on each other.*—The production of complementary colours in the eye, when coloured objects are observed either alone or in association, has frequently been an object of investigation with philosophers. Lately M. Chevreuil has had his attention drawn to it by his situation, and conceives that he has been able to generalize beyond what others had done before him. He has ascertained this general fact, that when two differently coloured bodies are in juxtaposition, their colours are modified by the effect of vicinity. If one is lighter than the other, it becomes more light whilst the other becomes darkened. By experiment, the influence of the seven primitive colours, with black and white, upon each other, has been ascertained; the law has been sought for which governs these modifications, and is given as follows:—When two colours A and B, are viewed simul-

taneously, to the colour of A is added the complementary colour of B, and to the colour of B is added the complementary colour of A. —*Bull. Univ.* A. xiii. 113.

5. *Alloy for the Construction of Pumps and Cocks.*—This alloy consists of 4 parts of tin, 4 of zinc, and 1 of antimony; these metals, when fused and well mixed together, have been found well suited to make good pumps. Cock metal is usually an alloy of lead, zinc, and antimony, to which more or less tin is added. The alloy described as good for pumps, is fit for cocks, but one to be mentioned is still better; of the two parts of a cock, namely, the box and the plug, the latter should be rather harder than the former, and therefore contain more antimony. An alloy of 80 parts of tin, with 20 of antimony, is well suited for the plug, and one consisting of 86 parts of tin and 14 of antimony for the boxes of cocks.—*Industriel de Bruxelles.*—*Bull. Univ.* F. xiv. 36.

§ II. Chemical Science.

1. *New Method of preparing Iodic Acid.* Serullas.—Berzelius shewed that the silicated hydrofluoric acid might be easily volatilized without leaving any residue. Serullas, therefore, hoped by its means to separate the potash from the iodate of potassa, and obtain iodic acid pure; but, in fact, the acid has power to remove only one part of the alkali, and leaves a tri-iodate as hereafter described*. As, however, soda does not appear to form an acid iodate, hopes were entertained that the alkali there might be separated by the silicated hydrofluoric acid, and the following process was devised. A solution of iodate of soda was heated with excess of the silicated hydrofluoric acid. When sufficiently evaporated and cooled, the insoluble fluate of silica and soda was separated, and the ebullition slightly continued, a little water being added from time to time, until the excess of silicated hydrofluoric acid was volatilized: this requires some time, but the close of the operation is ascertained by the disappearance of the odour of the acid; during the ebullition no fear of any reaction on the iodic acid need be entertained. When the liquid is of a syrupy consistence and cold, it is to be put on a filter; the acid will pass through and leave a small quantity of double fluate on the paper. By further concentration at moderate temperature, it may then be obtained solid.

This easy process yields much iodic acid; 100 parts of the iodate of soda contains 75 parts of the acid; it is not quite pure, but sufficiently so for most of the experiments in which it is required to be employed: 100 parts of it by heat left little more than 1 part of impurity.—*Annales de Chimie*, xliii. 127.

By subjecting this acid to the following operations, M. Serullas detained it in the crystalline form, which it had never before received. It was dissolved in water, then mixed with hydrofluoric acid, filtered to separate a white precipitate, and left to evaporate

* Page 415.

in a warm air-stove. Crystals were formed, being hexagonal plates; when separated from the fluid they retained a little hydrofluoric acid, but merely warming them on paper caused the dissipation of the latter. In these experiments the funnels and capsules were of course covered with wax to prevent the action of the hydrofluoric acid.

A solution of iodic acid mixed with sulphuric acid, and evaporated spontaneously in a stove, gave the former acid in crystals. Thick syrupy iodic acid, in a dry place, also gradually crystallized.

A solution of iodate of soda, mixed with at least twice the quantity of sulphuric acid necessary to saturate the ioda present, being boiled for 12 or 15 minutes, is to be filtered and concentrated; the liquid, left in a stove at 68° or 77° F., in a short time yields a crystalline mass, from which the crystals are to be separated, and washed with a very little water; being then placed on filtering paper, they are to be allowed to drain and then dried in a warm air-chamber. When pressed they divide into small brilliant crystals. The mother-liquor contains sulphuric acid, sulphate of soda, and a little iodic acid. This crystallized acid is usually quite pure, and when heated leaves no residue. If a little sulphate of ioda be present, the acid should be recrystallized from sulphuric acid.

This experiment is important, as it shews that iodate of soda may be made to yield iodic acid, by the simple action of sulphuric acid; and it also led to the observation, that the compounds of iodic, with other acids mentioned by Davy, had no real existence. The precipitates he obtained were in fact iodic acid crystallized, and moistened with the acid used in each experiment.

Crystallized iodic acid is not sensibly altered by exposure to air, and does not appear to be deliquescent. It has a particular odour, partly that of iodine. It is very soluble in water, and but very slightly in alcohol; the latter substance precipitates the aqueous solution. It does not, as some have said, act upon gold.—*Annales de Chimie*, xliii. 216.

2. *On Chloride of Iodine.* Serullas.—A solution of chloride of iodine in water, even when dilute, may be precipitated by sulphuric acid, if it be added in successive portions, until in sufficient quantity, the vessels being at the same time cooled. The chloride appears as a caseous white matter, which, as it acquires consistence, assumes the orange yellow colour of the perchloride. If the precipitate be heated in the liquid, it is dissolved, but appears again upon cooling. When the mixture is distilled, the perchloride volatilizes and condenses in the neck of the retort.

It is still a question whether chloride of iodine when put into water, remains unaltered, or becomes muriatic and iodic acids. As these acids mutually decompose each other, the former appears to be the most probable opinion. When the muriatic and iodic acid are mixed, and then sulphuric acid added to the solution, chloride of iodine is separated. The colour also produced upon mixing the two acids is another fact in favour of the view above. Also, when iodic acid in powder is dropped into a vessel filled with dry muriatic acid, a quick ebullition is observed at the iodic acid, heat is evolved, chloride of iodine is formed, which, at first liquid, as the temperature falls, becomes solid in long crystals, part on the sides of the vessel and part at the bottom. It is solid from 59° to 68°, and fuses between 68° and 77° F.—*Annales de Chimie*, xliii. 208.

3. *On fuming Nitric Acid—Hyponitrous Acid, &c.*, by M. Mitscherlich.—Whilst the temperature of the laboratory was at 14° F., from 10 to 20 lbs. of fuming nitric acid were slowly heated in a retort, placed in a sand bath, to the neck of which a very long tube was attached, cooled by a mixture of choride of calcium and snow, and connected with a recipient and a pneumatic tube. No gas passed from this tube; when the acid was heated, a liquid condensed in the receiver, consisting of two layers, the separation o which took place after every mixture by agitation. The lighter liquid boiled at 82°.4 F., retaining that temperature until the last portion had evaporated; its specific gravity was 1.455; it decomposed in contact with water into nitric acid, and oxide of azote: in one word, it presented all the properties of the combination of nitrous with nitric acid, discovered by M. Dulong.

The heavy liquid being heated, its point of ebullition rose continually from 82°.4, to more than 259° F., in proportion as the distillation proceeded. This liquid is of a deep red colour, like ordinary fuming nitric acid; it becomes colourless by the time that one half of it has been distilled. The product is formed of half light and half heavy liquid. The heavy liquid has a specific gravity of 1.539. Fuming nitric acid behaves in the same manner.

From these experiments it appears that fuming nitric acid is a solution of hyponitric acid in nitric acid; the latter, however, can only dissolve a certain quantity, or about half its weight; so that when ordinary fuming nitric acid is distilled, a heavy liquid (being a saturated solution of nitrous acid in nitric acid) and a lighter liquid or hyponitric acid are obtained.—*Annales de Chimie*, xliii. 220.

4. *Decomposition of Water by Heat and Metals.* Despretz.—It has long been known that iron decomposes water at a red heat evolving hydrogen, and that hydrogen, at a red heat, can entirely reduce the oxide formed. Gay Lussac has shewn, that the decomposition and recomposition take place at the same temperature. M. Despretz has ascertained that zinc, nickel, cobalt, and tin, act, in this respect, like iron.

The oxide of manganese is not completely reduced by hydrogen. Pure peroxide of this metal, exposed to a current of the dry gas, at the highest heat of a good forge, left fused protoxide, having a very fine green colour.

5. *On Mellitic Acid,—Carbon, and Oxygen, by* MM. Wohler and Liebeg.—These philosophers have undertaken an analysis of mellitic acid, which they had been led to suspect, from its habitudes when heated, to consist of carbon and oxygen only. The mellitate of silver, for instance, when dry, gave no trace of water, or a hydrogenous product, by distillation, nor even when burnt by means of oxide of copper. On further examination, it was found also to be destitute of azote, for the gaseous products of its decomposition, by oxide of copper, were totally absorbed by caustic potassa, 0.236 grammes of mellitate of silver = 0.07058 of mellitic acid gave 66 cubical centimetres of carbonic acid gas, from which the composition may be calculated at 50.21 carbon, and 49.79 oxygen per cent. Then calculating from the known equivalent number of mellitic acid (62.3), the substance would appear to be composed of

4 atoms carbon . . .	3.05748
3 —— oxygen . . .	3.00000
	6.05748

To test this result, the neutral mellitate of ammonia was decomposed by oxide of copper, the last portions of gas from it were a mixture of 4 volumes of carbonic acid, and 1 volume of nitrogen, which confirms the conclusion, that 4 atoms of carbon enter into the composition of 1 atom of the acid.

On comparing the composition of mellitic acid with that of succinic acid, it appears that the only difference is, in the latter containing hydrogen; and that if the latter be theoretically abstracted, there remain the elements of mellitic acid in true proportions. It is probable, therefore, that mellitic acid may be derived from the decomposition of succinic acid, which, as we know, is often found in fossil wood, although amber is wanting where the mellite is found.

Endeavours have been made to obtain mellitic acid by fusing and subliming succinic acid in dry and moist chlorine, without, however, obtaining any favourable result; the acid is not decomposed by chlorine; succinic acid, heated with caustic potash, disengaged hydrogen, but produced only oxalic acid.

Succinic acid was re-analyzed, but the results obtained agreed with those of Berzelius. The succinic acid was first cleansed by passing chlorine through its aqueous solution, until all its peculiar smell was destroyed, and then purified by repeated crystallizations. It was obtained in perfectly white crystals, which were then sublimed in a mattrass by the heat of boiling sulphuric acid, and afterwards decomposed. The composition obtained was, per cent.,

Carbon	44.38
Hydrogen	5.00
Oxygen	50.62
	100.00

1.060 of the succinate of lead gave 0.995 sulphate of lead. It is supposed that a half proportion of water is contained in the above succinic acid, and abstraction being made of its elements, the acid then agrees in composition with that which Berzelius analyzed in combination with oxide of lead.

When the mellitate of ammonia was heated in a glass tube, it was decomposed, giving peculiar results, there being first water, then hydrocyanic acid, and, finally, a sublimate of brilliant green crystals, which dissolved with difficulty in water, and produced a bitter solution.—*Annales de Chimie,* xliii. p. 200.

6. *Decomposition of Carbonic Acid by Metals.* Despretz.—Carbonic acid presents the same phenomena with heated metals as water. Iron, zinc, and tin, reduce it to carbonic oxide, and carbonic oxide reduces the oxides of these three metals. The oxide of carbon had been prepared from a heated mixture of oxalate of potash and sulphuric acid, and deprived of any acid it might carry over, by washing in an alkaline solution.

M. Dulong has also observed the reduction of carbonic acid to carbonic oxide by zinc, and the reduction of the oxide of zinc to the metal, by carbonic oxide.—*Annales de Chimie,* xliii. 222.

7. *Decrepitating Common Salt—Condensation of Gas in it.* Dumas.—M. Dumas has examined and described a very curious effect which occurred when some rock-salt, obtained from the mine of Wieliczka, in Poland, and given to him by M. Boué, was put into water. It decrepitated as it dissolved in the water, and gradually evolved a sensible portion of gas. The bubbles of gas were sensibly larger when the decrepitations were stronger, and the latter frequently made the glass tremble. This salt owes its property of decrepitating to a gas, which it contains in a strongly-compressed state, although no cavities are sensible to the eye. When the experiment was made in perfect darkness no light was disengaged. The gas disengaged is hydrogen slightly carbonated; when mixed with air it burns by the approach of a light.

This disengagement of gas will assist in explaining the numerous accidents which have happened from fire-damp in salt-mines. Several portions of the salt were nebulous, others were transparent. The nebulosities indicated the existence of numerous minute cavities, probably filled with condensed gas, and, in fact, a nebulous fragment, dissolved in water, gave more gas than an equal-sized fragment of the transparent salt.

This new fact, described by M. Dumas, shews how frequent, in the course of geological accidents, are the phenomena to which are due the accumulation of gas in the cavities of mineral substances, and how varied are the substances upon which these phenomena have been exerted. M. Dumas has endeavoured to reproduce salt having the power of decrepitating in water like that described.—*Revue Ency.* xlvi. 245.

8. *Iodates of Potassa. Chloriodate of Potassa, &c.* Serullas.—*Biniodate of Potassa.* When an alcoholic solution of chloride of iodine is mixed with an alcoholic solution of potassa, an acid iodate of potassa is precipitated. This salt may be obtained more abundantly, by adding to an aqueous solution of chloride of iodine a pure solution of carbonated or caustic potassa, not quite sufficient to saturate it; heat is evolved, but, on cooling, a crystalline precipitate is formed, which consists of definite proportions of chloride of potassium and acid iodate of potassa; chloro-iodate of potassa. This deposit being separated, is to be dissolved, filtered, and placed in a warm air-chamber at 77° F. In twenty-four hours, if the solution is sufficiently dilute, very regular and pure crystals of the acid iodate of potassa will be formed; they are right rhombic prisms, terminated by two diedral summits: 1 part dissolves in 75 of water, at 59° F. The liquid is then to be quite neutralized, for the purpose of procuring the neutral iodate in the ordinary way.

Being carefully heated until all the water was dissipated, which happens just as violet vapours appear, further heat then evolves oxygen and iodine, and leaves iodideof potassium; 5 parts gave 2.1 parts of iodide of potassium, and then, by the usual means, gave 3 of iodide of silver. By continuing the analysis, it was ultimately proved that this substance was a biniodate of potassa, consisting of 1 atom potassa, or 22.246, and 2 atoms of iodic acid, 155.508. The truth of Gay Lussac's analysis of the neutral iodate was also proved, namely, 22.246 base, and 77.754 acid.

Tri-iodate of Potassa.—When neutral iodate of potassa is acted upon by sulphuric, nitric, phosphoric, muriatic, or silicated fluoric acid, a part of the base is removed, and an acid iodate results, containing thrice the quantity of acid as the neutral salt. It is crystalline, and generally rhomboidal in form. The best process is to heat a solution of the neutral iodate of potassa with great excess of sulphuric acid; to filter the solution, which should not be concentrated, and leave it to evaporate spontaneously, or in a warm air-chamber at 77° F. Regular rhomboidal crystals are readily formed, which are transparent and very pure tri-iodate of potassa. This salt is distinguished from the bin-iodate by its property of gradually becoming red; 1 part dissolves in 25 of water, at 59° F. This salt is readily formed by adding potash to great excess of iodic acid, and crystallizing the result. By analysis the composition was determined to be

to be	Potassa . .	8.76 . . .	1 atom.
	Iodic acid . .	91,29 .	3 atoms.

This salt, dissolved and crystallized, passes into bin-iodate, but if the solution be concentrated after the first crop of bin-iodate, then crystals of the tri-iodate appear.

M. Serullas then endeavoured to form those salts which have been said to exist, containing a double acid, the iodic acid being one; for this purpose, the neutral or acid iodate of potash was dissolved and heated with that salt of potassa to be associated with it, the liquid being strongly acidified with the same acid as that contained in the latter salt. No combinations were in this way produced.

But a sulphate and iodate of potassa may be obtained by concentrating the mother-liquors of the process for preparing tri-iodate of potassa to a certain point; very regular transparent crystals are formed, which are a singular combination of bi-sulphate with bin-iodate of potassa. When analyzed, it gave the elements of such a combination. When dissolved in water and crystallized, it gave pure bin-iodate, and afterwards an acid sulphate of potassa.

Chloriodate of Potassa.—The double compound formed by the action of muriatic acid on the iodate of potassa is easily formed and well characterised. The solution of chloride of iodine is to be imperfectly saturated with caustic or carbonated potassa, the solution filtered and evaporated spontaneously; if not too dilute, crystals soon form, which are brilliant and transparent, and either long prisms or hexagonal plates. When exposed to air they lose their transparency. When dissolved, the solution is acid. 1 of the compound dissolves in 18 or 20 of water, at the temperature of 59° F. Muriate and iodate of potassa put together do not produce the compound, unless there be excess of muriatic acid. When analyzed, the salt was found constituted as follows:

Chloride of potassium . .	0.7806
Bin-iodate of potassa . . .	4.0887
	4.8693

Iodate of Soda.—Solution of chloride of iodine was imperfectly saturated with soda instead of potassa; no precipitate or crystallization took place: concentrated alcohol was poured in, an abundant precipitate fell, which was separated on a cloth, washed with alcohol until neutral, and pressed. The solid substance being dissolved in water, filtered, and placed in a stove at 77° F. the liquid became acid, and in 24 hours deposited large crystals of pure neutral iodate of soda. They were right octoedral prisms, terminated by hexaedral pyramids; they effloresced in the air. Further evaporation gave other crops of crystals of this salt, and only a little chloride of sodium was found in the mother-liquor.—*Annales de Chimie*, xliii. 113.

9. *Detection of Baryta or Strontia when present with Lime.*—The methods which have been proposed for this object are well known to be imperfect. Mr. Andrews recommends the following as the best, it being an extension of that proposed by Bucholz. Dissolve the carbonates of lime and baryta or strontia in nitric acid, evaporate the solution to dryness, and decompose the nitrates by heat. To the dry mass, add boiling water (pure in the case of baryta, but saturated in the cold with sulphate of strontia in the case of that earth,) and boil it for a few minutes, keeping the cru-

cible at the same time covered with its lid. Throw the whole on a covered filter, and to the liquid which passes through add sulphuric acid, or a soluble sulphate; a white powder will precipitate if either baryta or strontia be present; but if not, the liquid will retain its transparency. Mixtures of 99.75 parts nitrate of lime, with 0.25 parts of either nitrate of baryta or strontia, being thus examined, gave the precipitates so that a 400th part of these two earths with lime can in this way be detected.—*Phil. Mag., N. S.*, vii. 404.

10. *Magnesium. Metal of Magnesia.*—M. Bussy has stated at the Academy of Sciences that he has been able to eliminate the base of magnesia by a process similar to that practised by M. Wohler, and he submitted a specimen for examination.

Magnesium has a brilliant silvery white appearance; is perfectly malleable and ductile; is fusible at a moderate temperature; like zinc, is volatile at a temperature somewhat higher, and may be condensed again into small globules. It does not decompose water at common temperatures, but is oxidized in the air at high temperatures when in small masses, and gradually forms magnesia. Its filings burn with brilliancy, emitting sparks like iron in oxygen. It is imagined that this metal may be useful, and M. Bussy is engaged in searching for a cheap and easy mode of reducing it.

11. *New Metal Thorium, and new Earth Thorina.*—Berzelius has published an account of the discovery of a new metal, which, when oxidized, forms a new earth, and has given the names above to these substances.

Professor Esmark discovered a mineral which, from its great weight, he thought might be tantalite. It was black, had the external appearance of gadolinite from Ytterby; was frangible and not hard. Its specific gravity was 4.63. When heated, it lost its black colour, and evolved water and a little hydrofluoric acid. The mineral appears to exist in very small quantities. None has been found since the first discovery.

The new metallic oxide present in it agrees so nearly with the description given of the subphosphate of Ittria, when mistaken by Berzelius for a new earth, and called *Thorina*, that he has given this name to it, and called the metal obtainable from it *Thorium;* the mineral has been called *Thorite.* On analyzing the mineral, numerous substances were found in it; the following are their names, with the quantities of some of them for 100 parts of the thorite: thorina, 57.91; silica, 18.98; oxide of iron, 3.4; lime, 2.58; oxide of manganese, 2.39; oxide of uranium, 1.61; oxide of lead, 0.8; magnesia, 0.36; oxide of tin, potassa, soda, alumina, water, 9.5; undissolved powder, 1.7: loss, 0.49.

Thorium.—The metal cannot be obtained by the action of potassium on the oxide, but may by the action of potassium upon the double fluoride of thorium and potassium, or the dry chloride of thorium. The latter process is most easily practised, and the thorium most pure. The chloride is prepared by passing chlorine over a heated mixture of thorina and charcoal, and being mixed with the potassium and heated, so little additional elevation of temperature occurs, that the experiment may be made in glass vessels. When the result is put into water there is obtained a heavy grey metallic powder, which, when dry, allows of compression, and, when burnished by a polished agate, acquires a metallic lustre and the grey colour of iron. Its metallic characters about equal those of aluminum. It does not oxidate either in hot or cold water, but if moderately heated in the air it enflames, and burns with most intense light and heat into the earth *thorina*, which remains as white as snow, and without the slightest appearance of fusion or coherence. This oxide appears to be the only one which can be formed. It is colourless, heavy, and dissolves in no other acid than the sulphuric, which must be boiling hot, and performs its part only slowly.

Thorina.—The preparation of the earth from the *thorite* is as follows:—The pulverized mineral is to be dissolved in hot muriatic acid evaporated to dryness, the substance redissolved, and the silica separated, sulphuretted hydrogen is to be passed through the solution, and then the earth is to be precipitated by ammonia. The precipitate is to be well washed, dissolved in dilute sulphuric acid, and the solution evaporated at a high temperature. A bulky sulphate is deposited; when but little liquid remains it is to be decanted, the salt washed with boiling water, pressed and calcined, when the pure earth will be left.

The hydrate of thorina may be obtained by dissolving the sulphate of thorina in cold water, after it has been washed in hot water. The solution goes on but slowly, but is at last complete; then caustic potassa throws down a precipitate, which, when washed and separated on a filter, is gelatinous, like the hydrate of alumina, but may readily be pressed together. When dried in the air, it forms hard vitreous pieces, but *in vacuo* with sulphuric acid gives a white powder. Heat drives off the water. The moist hydrate dissolves easily in acid, the dry hydrate with more difficulty, and the heated earth not at all in muriatic and nitric acids.

The hydrate is insoluble in caustic alkalies, but readily soluble in strong solutions of the carbonates, even of ammonia. When carbonate of ammonia is added to a solution of thorina in a flask, and the vessel closed and heated, the liquid becomes turbid, and much thorina is deposited; but, after cooling, this is gradually redissolved, and the whole becomes clear again.

The earth does not fuse with an alkali, nor is it, after the operation, soluble in muriatic or nitric acids. By heat, the earth becomes hard. Its specific gravity is then as high as 9.402.

By experiments on the sulphate of thorina, for the purpose of deducing the composition of the earth from them, it would appear that the acid, in 1.732 of sulphate of baryta, will neutralize and combine with 1 of thorina, from which, with some other experiments, it is presumed that the earth consists of

Thorium	88.16	or	59.367
Oxygen	11.88	or	8.000

According to Berzelius's numbers, the atom (as it is called) of thorium = 744.9.

Thorina differs principally from the other earths in the properties of its sulphate, the solution of which, by ebullition, deposits a salt, which, after cooling, is again gradually dissolved; but this property is not evident when bases are present, with which double salts may be formed. The earth is distinguished from alumina and glucina by insolubility in caustic potassa; from yttria, by forming a double salt with sulphate of potash, which is insoluble in saturated solution of sulphate of potash; from zirconia, by the latter remaining insoluble in water and acids, when precipitated from a hot solution by sulphate of potassa; from oxide of cerium, by remaining colourless when ignited, and by other properties; and from all other bodies by certain differences of properties.—*Ann. de Chimie*, xliii. 5.

12. *Sulphuret of Zinc.* Despretz.—Chemists well know the difficulty of preparing sulphuret of zinc directly, or by a mixture of oxide and sulphur. I have obtained, by the second process, a product so identical with blende, that experienced mineralogists could not distinguish the one from the other. The result is quoted as a new illustration of the possibility of producing artificial substances which shall be identical with natural bodies.—*Ann. de Chimie*, xliii. 223.

13. *Pure Oxide of Cobalt.* Liebeg.—The ore of cobalt is to be reduced to very fine powder, and then roasted with much care. One part, by weight, is then to be introduced in successive small portions, into an iron vessel, in which three parts of acid sulphate of potassa has been previously fused at a moderate temperature. The mixture, at first fluid, soon becomes thick and firm, when the fire is to be increased, until the mass is in perfect fusion, and all white vapours have ceased. It is then to be taken out of the crucible with an iron ladle, the crucible recharged with acid sulphate of potash, and the operation continued as before, until the vessel is useless. The fused mass contains sulphate of cobalt, neutral sulphate of potassa, and arseniate of iron, with a little cobalt. It is to be pulverized, and boiled in an iron vessel with water as long as the powder continues rough to the touch. The white, or yellowish-white residue may be allowed to separate from the solution, either by deposition or filtration. Carbonate of potassa, free from silica, is then to be added to the solution, and the carbonate of cobalt thrown down is to be separated and well washed, if possible, with warm water; the same water may be used to wash other portions

of the fused mass. The filtered liquid which first passes is a saturated solution of sulphate of potassa; being evaporated to dryness in an iron vessel, it may be reconverted into acid sulphate by fusing it with one-half its weight of sulphuric acid: this salt is then useful as before.

The oxide of cobalt thus obtained contains no nickel; so little oxide of iron is present, that infusion of galls does not shew its presence; it may contain a little copper, if that metal be present in the ore, but it is easily separated by the known methods. Sometimes sulphuretted hydrogen will produce a yellow-brown precipitate in the solution of the fused mass; this, however, contains no arsenic, but is either sulphuret of antimony or bismuth, or a mixture of both.

It has been found advantageous to add to the fused mass sulphate of iron calcined to redness, and one-tenth of nitre; then the residue is arseniate of iron, and contains no arseniate of cobalt. There is then no occasion to act upon the residue a second time for the cobalt in it.

This process is founded on the circumstances that the sulphate of cobalt is not decomposed by a red heat, and that the arseniates of iron and cobalt are insoluble in all neutral liquids. It is quite evident that to obtain a perfect result, the excess of acid in the bisulphate of potassa must be completely driven off by the red heat applied.—*Ann. de Chimie*, xliii. 204.

14. *Preparation of pure Oxide of Nickel.* Liebeg.—The nickel ore, or kupfer nickel, is to be carefully roasted and mixed with half its weight of fluor spar. This is to be put into a leaden vessel, with three or three-and-a-half parts of oil of vitriol, and moderately heated. So soon as the heat rises above 212°, the mass thickens, and would become attached to the bottom of the vessel; but this must, by careful stirring, be avoided. A large quantity of fluoride of arsenic rises in vapour, to avoid the bad effects of which the operation should be carried on under a chimney with strong draught.

So soon as the mass dries, it is to be taken out, broken into large pieces, and lightly calcined with care in a reverberatory furnace, to expel the excess of sulphuric acid; it is then to be dissolved in hot water, and after the sulphate of lime has been separated, the oxide of iron is to be precipitated in the well known way. The roasted ore may also be dissolved in sulphuric acid, to which one-third of nitre has been added, and afterwards mixed with the fluor spar, following the same process as before. The advantage so obtained is, that after calcination the iron is as a peroxide, which facilitates its separation.

If the nickel is intended for the preparation of an alloy with copper, it is sufficient to evaporate the solution of sulphate of nickel to dryness, to decompose it by heat, and reduce the oxide of metal, containing a little iron, by the usual means. The small quantity of sulphate of lime which is present is not injurious. This method is established on the circumstances that sulphuric acid converts arsenic only into arsenious acid, and that the latter, when present with fluoric acid, is decomposed, and produces water, and a very volatile fluoride of arsenic, which passes away.—*Annales de Chimie*, xliii. 207.

15. *Preparation of Sugar from Starch.*—M. Heinrich says, that from one to two parts of sulphuric acid for each 100 parts of potato starch is sufficient, if the heat applied be a few degrees above 212° F.; and also that then two or three hours are sufficient to give crystallizable sugar. He applies the heat in wooden vessels by means of steam.

16. *Estimation of the Vegeto-Alkali in Peruvian Bark.*—It is often important in pharmacy to be able to tell the value of a sample of bark, by ascertaining the quantity of quinia or cinchonia which it contains. MM. Henry and Plisson, and also M. Tilley, have published processes for this purpose. Professor Gobel applies the following method to obtain the same end:—Two ounces of powdered bark are acted upon, at successive times, by sixteen ounces of water and 180 grains of muriatic acid, specific gravity 1.13, ebullition being occasioned; all the liquids are to be put together, and caustic potassa added, which produces a brown precipitate: this is to be redissolved in dilute muriatic acid, again precipitated, and so on, until the precipitate is quite white; it is then to be dried, and treated with cold strong alcohol, to separate the quinia and cinchonia from each other.

M. Veltman has devised the following process, which may be applied to small quantities, is easy of execution and exact:—55 grains of the bark in fine powder is to be mixed with an equal quantity of washed siliceous sand, the grains of which are about half the size of poppy seed; this is to be well mixed with five drops of muriatic acid, and 20 drops of alcohol, and pressed lightly into a glass tube $4\frac{3}{4}$ inches long, and 0.6 of an inch in diameter, one end of which has been covered with a little piece of muslin, and then inserted into a close vessel. The other end of this tube is to be connected by a bent tube with a small flask filled with a mixture of an ounce and a half of alcohol, and 20 drops of muriatic acid; the bent tube should be 0.2 of an inch in diameter; one end should go to the bottom of the flask, the other should reach the surface of the mixed bark and sand. The alcohol in the flask is then to be boiled by a small spirit lamp. It will pass through the tube and extract all that is soluble. If the ebullition is performed slowly, the last drops of alcohol pass nearly colourless. The reddish brown alcoholic tincture is to be precipitated by hydrated lime; after twelve hours it is to be separated by a filter, the liquor is to be rendered slightly acid, evaporated until in a soft state; then dissolved in 120 grains of water, and precipitated by a few drops of caustic ammonia. The precipitate being dried, indicates the quantity of alkali in the bark. In this way, M. Veltman found that from 3.3 to 6.0 parts of vegeto-alkali were combined in 100 parts of different varieties of bark.—*Bull. Univ.* C. xx. 297.

17. *Taste of Sulphate of Quinia.*—The bitter taste of sulphate of quinia is so strong, that the mixture of one part with 160 of sugar still has it sensibly. It is, however, remarkable, that if one part of the same salt be mixed with ten or fifteen parts of the powder of valerian, fennel, anis, orange peel, &c. a mixture is obtained which has scarcely any bitterness. Sugar, therefore, is a bad thing to remove the bitterness of sulphate of quinia: the end may be better obtained by the use of some aromatic powder.—*Mag. für Pharmacie.*

18. *Phosphate of Quinia.*—The phosphate of quinia, rendered slightly acid, is, according to M. Harless, a much milder medicine than the sulphate or the free alkali. It is better retained on the stomach where irritation exists, or by nervous patients, or by those who are subject either to congestions of blood or inflammation; its use does not occasion that unpleasant feeling which is sometimes produced after taking the sulphate: it does not so readily accelerate the motions of the heart, nor does it irritate the bronchiæ or lungs. In consequence of its insolubility and pulverulent state, it is administered in pills, from one to four grains being a dose.—*Bull. Univ.* C. xx. 240.

19. *Sertuerner's supposed new Alkali, Chinioïdia.*—MM. Henry and Delondre have sought for this vegeto-alkali, but could not find it, and they believe its non-existence fully proved. The substance mistaken by M. Sertuerner appears to have been a compound of quinia or cinchonia, with peculiar yellow matter, rendering them uncrystallizable; but this substance being removed, the true vegeto-alkalies appear with their distinctive characters. These experiments were made with the mother-waters resulting from the treatment of 200,000 lbs. of yellow bark.

20. *Mutual action of Iodic Acid and Morphia.* Serullas.—If a single grain of morphia, or acetate of morphia, be put into a solution of iodic acid, the liquid becomes of a reddish-brown colour, and exhales the odour of iodine. The effect is sensible with one hundredth of a grain of acetate of morphia. The action is quick if the solutions are strong; more tardy if weak: but is sensible in a few seconds when 7000 parts of water are present.

Quinia, cinchonia, veratria, picrotoxia, strychnia, brucia, and narcotine, have no action on iodic acid of this kind. Hence, iodic acid becomes an important test of the presence of morphia, or its acetate, sulphate, nitrate, or muriate, even when mixed with other substances or vegeto-alkalies, provided they have no action of this kind on the iodic acid. To render the iodine more apparent, it is better to triturate a little gelatinous starch with the liquid containing the morphia or its salts, and then add a few drops of the solution of iodic acid; the blue colour of the iodide of starch is immediately developed.

The same test may be applied to opium: a few drops of laudanum, or an aqueous solution of opium mixed with starch, and added to iodic acid, give the characteristic blue colour. The acid iodates of potassa, as well as the chloro-iodate and sulpho-iodate, act as iodic acid. The neutral iodate does not do so, unless a drop or two of sulphuric acid has been added.

During this action iodine is evidently set at liberty: the mixture of iodic acid and morphia, when diluted, yields a solution and deposit both of a reddish-brown colour. By exposure to air the iodine flies off, and both become clear yellow. If the liquid be evaporated, it leaves more of the solid yellow matter, but with a crystalline aspect.

This substance is but slightly soluble in water; it fuses below a red heat, and, when heated in a tube, is decomposed with a dull explosive action, certain gases escape, iodine is set free, and charcoal is left in the tube. Further experiments appeared to prove that it contained both iodine and iodic acid combined as with a base; and it appears probable, that by the action of the iodic acid in excess upon morphia, the latter is transformed (probably by the loss of hydrogen) into a new substance, which combines with the iodine and iodic acid, thus forming the new yellow compound, which, being comparatively insoluble, is separated from the solution.—*Ann. de Chimie*, xliii. 211.

21. *On crystallized Acetic Acid.* Despretz.—The process by which crystallized or glacial acetic acid is prepared is held secret. After many trials, M. Despretz succeeded by heating an atomic mixture of fused and dried acetate of lead and boiled sulphuric acid (203.4 parts of the first and 61.4 parts of the second). The result was very excellent. The anhydrous acetates ought to give the same result as acetate of lead.—*Annales de Chimie*, xliii. 223.

22. *Pollen of Cedar.*—MM. Fourcroy and Vauquelin examined the pollen of the date-tree, and found in it malic acid, phosphates of magnesia and lime, a soluble animal substance, and also an insoluble animal matter, intermediate between gluten and albumen; they concluded that probably the pollen of all plants was identical and contained azote. M. Macaire Prinseps, having obtained some pollen of the cedars of Beaulieu, examined it chemically. It was yellow, pulverulent, without odour or taste, and burnt in the flame of a lamp, but with less facility than lycopodium. It contained acid malate of potash, sulphate of potash, phosphate of lime, silica, sugar, gum, yellow resin, and a substance which, by its characters, approximated to starch. Being analyzed as a whole, the pollen

gave per cent., 40 carbon, 48.3 oxygen, and 11.7 hydrogen, but no azote. Lycopodium gave per cent. 50.2 carbon, 39.2 oxygen, and 8.6 hydrogen. Again no azote.—*Bibliothèque Universelle*, 1830. 45.

23. *Production of Formic Acid.*—Dobereiner first obtained this acid artificially, by the distillation of tartaric and sulphuric acids with oxide of manganese. Tunnerman observed that if a mixture of starch, peroxide of manganese, and weak sulphuric acid were heated in a large retort, rapid effervescence, with the evolution of much carbonic acid, occurred at a certain moment, and the mixture acquired the odour of formic acid. By distillation, formic acid came over, accompanied by an odorous substance, acting upon the eyes, but separable by combination of the acid with bases. Tunnerman mistook the nature of this acid. Wohler established its true character. Liebeg, about the same time, obtained it, not only from starch, but by the similar use of many other substances. Gmelin obtained it in the same way from sugar, sugar of milk, starch, lignine, the root of the marsh-mallow, mucous acid, &c.; but it was always soiled, particularly when obtained from starch, by a peculiar principle separable by the process of forming salts, and decomposing them by sulphuric acid.

A very pure formic acid may be obtained by distilling alcohol with sulphuric acid and black oxide of manganese; but to prevent the formation of sulphuric ether, it is convenient to employ dilute alcohol or ordinary spirit of wine: for if the alcohol be strong, there will be produced sulphuric and formic ether, which will not only diminish the quantity of formic acid, but when the latter is combined with oxide of lead, will occasion the formation of a coloured salt, crystallizable with difficulty only. Acetic acid acted upon in the same way gives no formic acid. The fibrine of blood produces a little, but it is very inferior.—*Annales de Chimie*, xliii. 199.

24. *On a new Acid contained in the Urine of Herbivorous Animals, by* M. Liebeg.—The following abstract is made from M. Liebeg's paper, published at length in the Annales de Chimie, xliii. 188. Fourcroy and Vauquelin discovered a particular acid in the urine of certain animals, which they took for benzoic acid, and which appears to be the same as the one to be described. When the urine of the horse is mixed with excess of muriatic acid, a yellow-brown crystalline precipitate is gradually formed, having an unpleasant odour, which cannot be removed by simple washing with water. It is to be boiled with quick lime and water, the liquid filtered, a solution of chloride of lime added, until all urinous odour ceases, and then animal charcoal added, until the liquor which passes the filter is colourless. This liquid, whilst hot, is to be mixed with great excess of muriatic acid, and left to cool. There separate prismatic crystals 2 or 3 inches long, of a shining white colour, translucent, and which differ from benzoic acid, not only in the form of its crystals, but by its smaller degree of insolubility in water, by the differ-

ence of its salts from the benzoates, and by its composition including azote. It fuses by heat, then is decomposed and becomes black, a crystalline sublimate rises, an odour of bitter almonds is perceived, and much charcoal is left. Being mixed with four times its weight of hydrate of lime and heated, much ammonia is disengaged.

Being obtained from the urine of the horse principally, it has been distinguished by the term *hippuric acid.* Sulphuric acid heated to 248° F. dissolves it without change, water precipitates it; a higher heat enables the acid to decompose it, and ultimately white crystals sublime. It dissolves also in nitric acid, and by heat is then decomposed. It is soluble in hot muriatic acid, but crystallizes out upon cooling. Aqueous solution of chlorine does not act upon it, but boiling with much chloride of lime decomposes it entirely.

When burnt very carefully by means of oxide of copper, it gave a mixture of carbonic acid and azote in the proportions of 100 to 5. This result appearing important, was confirmed by another mode of investigation, namely, by burning, not the acid, but its ammoniacal salt, by means of peroxide of copper, and observing the relation of the azote to the carbonic acid. The oxalate of ammonia burnt in this way yields 1 volume of azote and 2 of carbonic acid, the latter indicating so many volumes, *i. e.*, 2 of carbon. The acid hippurate of ammonia so burnt gave 2 of azote and 27 of carbonic acid, confirming the result above. By other analytical processes the quantity of water given by 0.300 parts of the acid when burnt, was found to be 0.180, and from these and other data the following composition was made out:

Azote . .	.046	or, theoretically,	14	1 atom.
Carbon . .	.393	. .	120	20 atoms.
Hydrogen	.031	. .	10	10
Oxygen . .	.155	. .	48	6
	.625		192	

The mean result of experiments on its atomic weight was 197. In the hippurate of lead, 100 parts of the acid combine with 55.31 oxide of lead, and the crystallized salt contains besides 25.64 of water. The hippuric acid, when crystallized, is anhydrous and requires 600 parts of water at 32° for its solution.

Hippuric acid easily dissolves most of the metallic oxides. The soluble compounds precipitate solutions of iron of a rusty colour, and the nitrates of silver and protoxide of mercury, in a white flocculent state. The neutral ammoniacal salt crystallizes with difficulty, the acid salt easily. These, when heated, evolve ammonia, and leave principally hippuric acid. The hippurates of potassa, soda, and magnesia, are very soluble and difficultly crystallizable. The hippurates of baryta and strontia are soluble and crystallizable. The sub-salt of baryta has peculiar properties. The salt of lime crystallizes in rhomboidal prisms, &c., which are anhydrous: they dissolve in 18 parts cold water, and 6 parts of boiling water; it consists of 87.28 acid, and 12.72 lime. A neutral, and a sub-salt of lead may be formed; the former, by mixing a hot solution of a salt of lead with a hippurate, will be obtained, as the mixture cools, in nacerous plates : these are anhydrous, and consist of 64.38 acid, 35.62 oxide. Other salts have been formed with other bases.

When the dry hippuric acid is decomposed by heat, there is found, as already stated, a crystalline sublimate, which condenses in the neck of the retort, and has a yellow or rosy colour; if much hippuric acid has been used, this substance ultimately obstructs the neck of the retort. This substance dissolves in hot water easily, and contains ammonia; when combined with lime, filtered, and separated again by muriatic acid, it has all the properties of benzoic acid; it forms salts like the benzoates, and, in fact, it is benzoic acid. Hence Fourcroy and Vauquelin were right when they said they had obtained benzoic acid from the urine of horses; but it had not existed there ready formed.

If the hippuric acid be mixed with four times its weight of quick lime and distilled, it is entirely transformed into a yellow oily liquid, with an agreeable odour, containing ammonia, and resembling the fixed oils. If the hippuric acid be mixed with sulphuric acid, and heated only until sulphurous vapours begin to appear, if then the black mass be mixed with water, boiled with lime and then muriatic acid used, benzoic acid may be separated, being formed in this way as well as by heat alone. When the hippuric acid is boiled with nitric acid, a little nitrous acid is evolved, and then water precipitates pure benzoic acid.

M. Liebeg remarks, that he has not been able to extract the smallest trace of benzoic acid from the food of horses of which he has examined the urine; the crystalline form makes him doubt whether the substance which M. Vogel found in the anthoxanthum odoratum, and holcus odoratus, is really benzoic acid, as announced.

25. *On the Decomposition of Urea and Uric Acid, at high Temperatures, by* M. Wohler.—Most chemists have remarked the singular property of urea as an organic body, of leaving no charcoal when decomposed by heat; but with the exception of observing the formation of a large quantity of carbonate of ammonia, the products have not been closely examined. Fourcroy and Vauquelin have remarked, that during the ebullition which it undergoes, occasioned by the evolution of much carbonate of ammonia, a dry infusible substance is formed, which, at a higher temperature, sublimes and forms a white crystalline crust which they conclude to be uric acid.

The urea used by M. Wohler was obtained from urine, and was in fine large regular crystals. If only in small plates, or not regularly crystallized, it cannot be trusted, usually then containing a little alkali. There are two precautions in its preparation which must not be neglected: first, when precipitating it from evaporated urine, the nitric acid used must be free from nitrous acid, for the latter destroys urea; and, secondly, the nitrate of urea must be washed several times in water at 32° F.; it is then to be strongly pressed in bibulous paper. Without these precautions, the nitrate of urea always retains much colouring matter, which can only be imperfectly removed by animal charcoal. If the urea be ultimately coloured, it is better to form it into a nitrate and proceed as above, than try to change it by animal charcoal.

To separate the nitric acid, carbonate of baryta is better than carbonate of potassa, for the nitrate of baryta is much less soluble in alcohol than nitre; the urea assists, however, the solution of the nitrates, and when the former is to be dissolved out from the nitrates, the alcohol should be used at common temperatures, and not boiling; although a large quantity must be used, the process is safer.

It is said in most chemical works, that a boiling aqueous solution of urea is converted quickly into carbonate of ammoniate; but this is a mistake, occasioned by considering boiling fused urea as a solution. A solution of urea very concentrated has been retained in a boiling state for a long time, without evolving any carbonate of ammonia; but the moment all the water was evaporated, the temperature rose, and the urea itself boiled and underwent decomposition. Urea fuses at 248° F., and is decomposed at a temperature a little higher than this. During decomposition it boils rapidly; from the carbonate of ammonia evolved, the latter forms a crystalline crust, and contains no hydrocyanic acid. After a little while, a solid granular substance separates from the urea, which increases in quantity, rendering the urea turbid, and the whole ultimately becomes a dry grey or whitish powder; after this, the application of heat occasions no further production of carbonate of ammonia.

This particular residual substance appears to be insoluble in cold water, but dissolves in boiling water, leaving only a little impurity. The filtered solution deposits small white brilliant crystals as it cools, and these are identical with the *cyanic acid* of M. Serullas, and have precisely the same properties as the latter philosopher has described. It is only, therefore, relative to the quantity of water, and the phenomena occurring during sublimation, that M. Wohler makes any remarks.

M. Serullas does not seem to have observed that the crystals contain much water. If exposed to the air, they lose their transparency, become milky, but retain their form. Being slowly heated, they lose 23.4 per cent. of water, the oxygen in which is equal to $\frac{1}{2}$ of that which remains in the acid. The experiments were made with cyanic acid prepared as above, and that prepared according to M. Serullas's process from the chloride of cyanogen. The hydrated acid crystallized in an oblique rhombic prism, and the anhydrous acid in flattened octoedra with square bases. The latter are obtained by dissolving the substance in hot and concentrated muriatic or sulphuric acid.

When the anhydrous cyanic acid is heated in a little retort, a white partially crystalline powder soon sublimes; this is undecomposed cyanic acid; but it dissolves with difficulty in boiling water, being, in this respect, like calcined alum. A smaller part of the acid is decomposed during the sublimation, but without leaving any charcoal, but disengaging a mixture of gases which, apparently, are azote and carbonic acid, possessing, in a very great degree, the penetrating odour of cyanous acid. If, during the operation, the neck of the retort and the receiver are cooled considerably, etherial striæ are deposited within, which are liquid cyanous acid (formerly called cyanic acid), and which, to this time, had never been obtained in a separate state.

The liquid is limpid, colourless, very volatile, having a very penetrating odour, and strongly affecting the eyes. In contact with water, it is immediately decomposed with the evolution of heat, producing carbonate of ammonia. If the products of the distilled cyanic acid be sent into a moistened recipient, the water and the cyanous acid immediately become carbonate of ammonia; but if the vapours be sent into caustic ammonia, the evaporated liquid gives colourless crystals of urea. The same substance is formed in a bulky vegetation at the mouth of the retort if the recipient be merely moistened with liquid ammonia. Urea appears to be formed even with pure water, for if the recipient contain only a few drops of water, soon after the vapours are sent into it, the former evolves gas, which is no doubt carbonic acid, from the carbonate of ammonia formed; it is finally decomposed, and becomes urea by the action of a further portion of the cyanous acid. M. Wohler remarks, that when cyanites are decomposed by aqueous acids, the pungent odour analogous to nitric acid, which the carbonic acid evolved possesses, is due to a small quantity of cyanous acid which is volatilized with the carbonic acid without being decomposed.

The accordance of the cyanic acid in properties with that usually called the pyro-uric, as well as the physiological analogy between urea and uric acid, induced M. Wohler to examine the pyro-uric acid and other products of the distillation of the uric acid. He used the excrements of snakes, which, for their purification, were digested for 24 hours in muriatic acid, to separate phosphate of lime and ammonia, and being then well washed and dried, were distilled at a red heat. The pyro-uric acid then proved to be cyanic acid. These facts immediately suggested that urea might be produced by the distillation of uric acid, *i. e.*, from the ammonia which is evolved in these circumstances, and from the decomposition of the cyanic acid formed. Nothing was easier than to verify this conjecture, and to prove that the essential part of the product, arising from the decomposition of uric acid by heat, was half cyanic acid and half urea.

On distilling well-dried uric acid no liquid product was obtained, but a very large quantity of hydrocyanic acid. The sublimate is at first soft, but soon hardens in the air; it is of yellow or clear brown colour, and smells strongly of hydrocyanate of ammonia: here and there are appearances of thin colourless crystalline plates: it is impossible to separate the cyanic acid in its pure state, without destroying the urea. The operation is easily practised by continuing to heat it until the disengagement of carbonate of ammonia ceases, the residue then dissolved in boiling water, filtered, and cooled, yields crystallized cyanic acid. The same end may be obtained by dissolving the impure sublimate in hot nitric acid, which destroys the urea, and on cooling the crystallized cyanic acid is obtained.

The urea may be obtained from the rough sublimate by means of cold water; the solution is to be evaporated, and the urea separated by alcohol; but the urea solutions always give crystalline grains of cyanic acid, which substance is either very soluble in urea, or forms some kind of combination with it. Repeated treatment with alcohol is therefore required. Fourcroy and Vauquelin appear to have observed this production of urea by the distillation of uric acid.

Perhaps henceforward these transformations of the two essential parts of urine, of urea into cyanic acid and carbonate of ammonia, and of uric acid into urea and cyanic acid, may become remarkable in a physiological point of view, and throw light upon certain diseases, and on the irregular deposits of the urine; in fact, it does not appear very unlikely that when uric calculi are examined anew, with great attention, concretions of cyanic acid may be formed; for this acid, because of its insolubility and other resemblances with uric acid, may very well form solid masses analogous to those of uric acid.

As a continuation of these researches, I have made some experiments on the decomposition of cyanogen in water, especially to observe whether, in this case, any cyanic acid or urea would be formed. As water dissolves but little cyanogen, the same portion was saturated with cyanogen, and decomposed twice in succession. The liquor separated from a brown deposited substance, was yellow, and being evaporated to the consistence of syrup, upon cooling became a soft brown mass, of which one part dissolved in water, whilst a yellow-brown residue remained. The solution being again evaporated, there remained a crystalline mass, from which, either by the action of alcohol or nitric acid, well-characterized urea could be separated.

In this case the urea is formed from substances perfectly inorganic, water and cyanogen; probably cyanous acid and ammonia had been produced in the course of the changes. The other substances found at the same time were not examined, except to ascertain they were not, and contained no cyanic acid. Two colourless crystallizable substances appear to be formed at the same time, of which one is a salt of ammonia.—*Annalen der Physik.* xv. 619.—*Annales de Chimie,* xliii. 64.

26. *Bromide of Carbon.* Loewig.—This compound may be prepared in two ways. Bromine is to be mixed with alcohol of specific gravity 0.837. As the former is added, the mixture heats, and at last boils suddenly, evolving hydrobromic acid and free

bromine. When cold, an alcoholic solution of caustic potassa is to be added, until the colour is gone; a little water is then to be put in, and the alcohol evaporated at a moderate temperature. As it cools, a small quantity of heavy yellow oil separates, and immediately after a crystalline concrete substance; or if the alcoholic solution be diluted with much water, the separation is equally effected. The other process is to put bromine into ether, leave them together some time, and then distil. At first hydrobromic acid passes over, and then a heavy clear oil. When the distillation has continued some time it is to be discontinued, solution of pure potassa added to the residue, and the whole diluted; a white voluminous substance separates, which being washed with water on a filter, and fused at a low temperature, becomes solid on cooling: this is bromide of carbon.

Bromide of carbon forms white opaque scales, soft and greasy to the touch, like camphor; friable, having an aromatic odour like nitric ether, and a sharp taste resembling that of peppermint. When liquid it is transparent and colourless; it burns in contact with flame, disengaging hydrobromic acid. It is heavier than water, easily fuses, boils at 212°, and sublimes, condensing into nacreous needles. It dissolves slightly in water, communicating its particular taste and smell. It fuses under water at 122° Fah.; at a higher degree it evaporates with the water. It dissolves readily in alcohol and ether, and is not then affected by nitrate of silver: alkalies have no action upon it; sulphuric, nitric, and muriatic acids have no action upon it. When fused in chlorine gas, chloride of bromine is formed; when heated with metallic oxides or metals, it is decomposed, forming bromides and either carbonic acid or carbon, according to circumstances. Being analyzed, it was found to be composed of 9.01 carbon and 91.99 bromine.—*Bull. Univ.* A. xiii. 53.

27. *Anhydrous Subcarbonate of Ammonia.* Van Mons.—A mixture of calcined carbonate of potassa and sal alembroth (ammonio-muriate of mercury) is to be sublimed in a small dry retort. The anhydrous salt obtained has only a momentary existence, and the contact of the smallest quantity of water is sufficient to take from it a part of the carbonic acid.—*Bull. Univ.* A. xiii. 132.

28. *Manufacture of Bicarbonate of Soda.*—M. Creuzberg has found a ready mode for the manufacture of this salt, in the circumstance that the dry alkalies absorb carbonic acid much more quickly than those in solution. Carbonate of soda is therefore deprived of much of its water by efflorescence, and is then subjected to a current of carbonic acid gas until the bicarbonate is formed; the time when this takes place is rendered evident by the evolution of heat, and the exhalation of water, which is deposited in drops upon the interior of the vessel.—*Bull. Univ.* A. xiii. 134.

29. *Separation of Strontia from Baryta.*—The formation of these earths into muriates, and the separation of them by the action of alcohol, in consequence of the solubility of the strontia muriate, and the insolubility of the baryta salt, is well known. M. Kastner remarks, that to be a good process the salts must be anhydrous, and then treated with alcohol at 84° (183° Fah.) Care must also be taken that the alcohol is guarded so that it cannot attract water from the atmosphere.—*Bull. Univ.* A. xiii. 134.

30. *Sulphate of Potash and Copper.*—When equal quantities of sulphate of potash and sulphate of copper are mixed, a particularly clear green precipitate is gradually formed, which Vogel considered as a subsalt. Having been analyzed by Brunner, it appears to consist of

Oxide of copper . . .	39.23
Potassa	12.12
Sulphuric acid	39.70
Water	8.94
	100.00

31. *Preparation of Cinnabar in the humid way.*—Kirchoff first shewed that by commingling and triturating mercury, sulphur, and potash together, and applying heat, cinnabar might be obtained. But the process was uncertain, and gave variable quantities of vermilion. The following is a process recommended by M. Brunner: 300 parts of mercury, 114 of sulphur, 75 of caustic potash, and from 400 to 450 of water are used. The mercury and sulphur are first triturated together for from three hours to a whole day, according to the quantities used. When the mixture is homogeneous the solution of potash is added, the trituration continued, and the mixture heated in a vessel of earth, porcelain, or on a large scale, of iron; at first the stirring must be constant, afterwards from time to time. The heat should be sustained at 113°; it should never pass 122°. The liquid should not be allowed to diminish by evaporation, but be made up. After some hours, the mixture will acquire a reddish-brown colour, and then great care is required; the mixture must not pass 113°. If it becomes gelatinous, a little water should be added; the mixture of sulphur and mercury should always be in a pulverulent form in the liquid. The colour becomes more and more brilliant, and at times increases with astonishing rapidity: when it has attained its highest intensity the vessel is to be taken off the fire, but still to be retained warm for several hours. The time necessary for the application of heat appears to be directly as the quantity operated upon. If the proportions above be in grammes (about 15½ grains each), the red colour will appear in about eight hours, and the operation be finished in ten or twelve hours.

The cinnabar is then to be washed, and the small quantity of metallic mercury that may be present separated; from 328 to 330 parts of vermilion will be obtained of a colour eqalling that of native cinnabar, and far surpassing that of cinnabar obtained by sublimation. The mercury and the potash should be quite pure.—*Annalen der Physik*, 1829.

32. *Action of Platina on Silver.*—According to Lampadius, silver, when alloyed with platina, burns and volatilizes much more readily than when fused alone. The vapour which rises is oxide of silver.

§ III. Natural History.

1. *Proportion between the Nervous System and other parts.*—M. Tiedmann has reported several cases of defective development of the spinal marrow as correspondent to a congenital absence of the limbs; and on the other hand, cases of excessive development of the brain and nerves, having relation to the existence of supernumerary organs. M. Tiedmann regards these phenomena as constantly observable in such cases, and concludes that the nervous system, as being the first existing part of the animal, regulates the formation and ulterior development of the embryo, and determines the particular form and disposition of the other organ.—*Med. Journal*, lxiii. p. 358.

2. *Sense of Touch.*—The presence in insects of the ganglion which represents the brain, is not absolutely necessary for the existence of the sense of touch. After decapitation they feel on the surface, and in their limbs, by means of their other ganglions, such impressions as may be made on them. The spinal marrow of reptiles, young birds, and young mammiferous animals, seems also capable, after the destruction of the brain, of being modified by irritations, of feeling them, and of occasioning, in consequence thereof, durable and calculated movements, which are not to be confounded with those convulsive and fugacious motions that are attributable solely to irritability. M. Calmeil thinks that this faculty of the spinal marrow is probably diffused throughout its whole extent. Further, it is probable that in the natural state of our functions the brain is the sole centre of irritability, and that the spinal marrow only becomes sensible when the brain ceases to exist. The co-ordination of our voluntary motions is doubtless attributable only to the brain.—*Med. Journ.* lxvi. 358.

3. *Paralysis of one half the Body without loss of Motion.*—Few cases like the following, where sensibility and voluntary motion are distinguished, are upon record. M. Loriot, 73 years of age, had always enjoyed good health; for a few days he had felt occasional giddiness, when on the 10th March he suddenly experienced a

numbness in the whole of the left inferior extremity. It appeared to him as if his foot was sinking into the earth, and he held his thigh firmly with his hands to prevent himself from pressing so heavily upon the ground. Almost at the same instant, the numbness extended itself over the whole of the left side of the body; he hastened home in much alarm, and walked without difficulty. The next day he was in the following state:—intellectual faculties unimpaired; pulse unaffected; he could walk and move his arms as usual, except that the elevation of the arm by means of the deltoid was somewhat limited. He was not conscious of the movements he executed, nor of the impression of the bodies he touched. The skin of the whole of the left side of the body was perfectly insensible, it was pinched and pricked violently, without producing pain or indeed any sensation at all. On the anterior part of the body, the median line was not the precise limit of the sensible part. On the left side the skin was still easily sensitive to the extent of about an inch beyond the central division, but beyond this distance it was perfectly insensible. On the affected side neither the sense of sight nor hearing was altered, but those of smell and taste were abolished. If the left side of the tongue was moistened with strong vinegar, the patient was unconscious of the slightest impression, whilst the flavour of the liquid was keenly perceptible by the other half of the organ. Strong odours placed under the nostril of the affected side were not perceived, excepting when a deep inspiration was made, but then the odoriferous molecules arrived at the right nostril by the posterior opening of the nasal fossa. When a hand was placed across the patient's head, that part only of it was felt, which pressed upon the right half of the head. Every treatment that was suggested was ineffectual.

A few months after the attack, M. Loriot had resumed his ordinary occupations. For some time he felt occasionally dull transient pains on the hemiplegic side. He could move the limbs on that side without difficulty, but still he felt their motive powers were inferior in degree to those of the other side. In other respects the constitution of the patient had not apparently suffered.—*Bull. des Sci. Medicales.*

4. *Spontaneous Combustion of both Hands.*—The following case of spontaneous human combustion, from the Archives Gén. de Médecine, for March 1829, is given as superior in point of authenticity, to any former one on record. A gentleman of a robust healthy constitution, and temperate habits, 24 years of age, extinguished with his hands the burning clothes of his brother, who had accidentally set fire to them with sulphur, and was immediately afterwards attacked with such acute pain in both hands, as to compel him to cry out for assistance. A woman who came to his succour, observed that both hands were surrounded by a blue flame. This at first was supposed to be occasioned by the sulphur adhering to them, and an attempt was made to extinguish the flame with cold water,

but without effect. The gentleman then ran down stairs to a cutler's shop, and plunged his hands into a quantity of mud; from this he derived very little relief. After suffering in this manner much torture for half an hour, he ran to the house of Dr. Richard de Brus, by whom the case is related. On the way, both he himself, and the woman who accompanied him, observed distinctly the blue flame surrounding the hands. The physician met him at the door, and observed the hands to be red, swelled, and exhaling a kind of smoke or vapour. He immediately directed his patient to plunge his hands into a well which was opposite, and to keep them there, until he experienced relief; on his doing so, the pain abated considerably, and the flame ceased, but he had not gone more than 150 paces homewards, when it reappeared. On reaching his dwelling, he emersed each of his hands in a bucket of water, which as it got *rapidly heated*, he had repeatedly renewed. As often as he took them out of the water, he remarked a sort of unctuous matter flow along his fingers, and the blue flame reappeared, the latter was not however visible, except in a situation where the light of the candle was shaded, as under the table. A young gentleman who remained in the room with him, saw the blue flames several times in the course of the night; towards day-break only *sparks* were visible. During the succeeding day the pain was very severe, and large vesications filled with a reddish serum had formed on the fingers in some places, indeed the cuticle was entirely removed, and the cutis greyish and corroded. The vesications being opened, cerate was applied to the denuded surfaces, and the whole covered with poultices. The inflammation which followed was moderate, the suppuration healthy, and in six weeks the ulcers caused by the burning were entirely healed, but the cicatrices were very distinct, and several of the nails had dropped off.—*Medical Journal,* lxiii., 360.

5. *Use of Belladona in cases of Frontal Neuralgia.*—In 1826, Mr. Henry, an English military surgeon, pointed out the efficacy of the extract of belladona in cases of frontal neuralgia. Since that period, M. Claret has frequently employed it with the greatest success in frontal tic douloureux, and considers it a specific in such cases. In the course of two years he has treated five cases with this remedy, and completely cured them. Encouraged by this success, he tried the belladona in other neuralgic affections, in sciatica, for example, but without any benefit. He attributes the failure of the remedy in such cases, to the difficulty of acting upon the deep seated nerve, which is affected. In gastralgia and odontalgia, he has also procured temporary relief from frictions with the belladona.

One lady had periodical attacks of an intense pain over the left eye, affecting the eye also; the pain began about seven in the morning, and continued until four or five o'clock in the afternoon: 10 grains of the extract mixed with water, to the consistency of pomatum, were rubbed in upon the part; being imperfectly applied, this application failed. On repeating it, the pain immediately diminished, and in three hours was gone. The application was repeated twice more, after which the pain did not return—health was restored, and a weakness in the sight caused at first, soon disappeared.

In several other cases from two to three frictions effected a perfect removal of the pain.—*Med. Journal*, lxiii. p. 317.

6. *Communication of Hydrophobia.*—The following are the results of experiments by Dr. Hertwich, on rabid dogs, described in Groufe and Walther's Journal.

i. Of 59 dogs which were inoculated, 14 became affected with real rabies.

ii. Where the inoculation failed, no assignable cause of failure could be discovered. There exists, therefore, a peculiar disposition for the virus of rabies, as for that of other contagious diseases. A mastiff four years old went through a regular series of experiments without any effect being produced, while seven other dogs which were inoculated with him, and in the same manner, became rabid. Some dogs were several times inoculated before any contagion took place, in others this effect was observed after the first experiment.

iii. It appears, therefore, that in cases of doubtful rabies, one or two accidental or artificial inoculations are not sufficient to serve as negative proofs of the existence of rabies.

iv. No communication of the disease ever took place by the perspiration: the contagious matter of rabies connot therefore be of a volatile nature.

v. Its vehicle is not only saliva and the mucus of the mouth, but also the blood and the substance of the salivary glands. It does not appear to exist in the nervous pulp.

vi. The power of infecting exists at every period of the confirmed disease, and even for twenty-four hours after the death of the animal.

vii. The virus of rabies appears to be inactive if administered internally: of twenty-two dogs who were made to swallow it none took the disease.

viii. The application of saliva to fresh wounds appears to be as often followed by rabies, as the bites of rabid animals.

ix. It is consequently beyond all doubt, that the disease is neither produced by the lesion, according to Gerard's opinion, nor by the fear of the patient, as has been repeatedly asserted.

x. The opinion of Baden and Capello, that in dogs which had become rabid from the bite of an animal primarily affected with the disease, the saliva did not contain the contagion, and that it consisted only in primary rabies, has been proved by several experiments to be erroneous. This particularly agrees with Magendie's experiments, who having inoculated a dog with saliva of a patient affected with hydrophobia, the animal became rabid after a month, and bit two others, which were also infected; from these last no further contagion was observed.

xi. During the period of the inactivity of the virus, there are no morbid alterations observable, either locally or in the general health of the dog thus infected, nor does the lower surface of the tongue ever exhibit vesicles. There exist accordingly no precursory symptoms as in other contagious diseases.

xii. The disease generally breaks out within 50 days after the inoculation, or the infliction of the wound. It was never observed at a later period.

xiii. Inoculation or infection from animals affected with fierce rabies, very often produces the other modifications of the disease, and *vice versâ;* they are consequently only different forms of one and the same disease.

xiv. It is an erroneous opinion that healthy dogs are able to distinguish those affected with rabies by the smell; this is not the case, nor do they abhor food mixed with the secreta or excreta of rabid dogs.—*Med. Jour.* lxiii. 460.

7. *On the Development and Growth of Cantharides.* Zier.—The flies always deposited their eggs on the smooth sides of the vessel in which they were inclosed; but it was found requisite that these sides should not be transparent; so that when glass capsules were used they were covered with black paper, and there the eggs were deposited. Each female produced from 100 to 200 in a small heap. Nothing is more difficult than to observe the transformation of these eggs into larvæ, in consequense of the momentary nature of the change. M. Zier knowing about what time to expect the change with certain eggs, waited for and watched them under the microscope, and was fortunate in catching the moment. He first remarked certain slight motions followed by others much stronger and quicker, at one end of the egg, and instantly it was converted into a living being, a small larva. It was impossible to discover any envelope which might be supposed to be left by the insect; the whole egg appeared to be vivified.

The larva is at first colourless and formed of 13 rings, of which the first is the head, the three next have each a pair of feet, by which the insect moves with considerable rapidity, the nine other rings form the body. Two black points on the first rings are the eyes, above is a sort of black antennæ, the last ring has two hairs. Almost immediately after the change, the posterior part of the larva acquires a dark tint, which advances gradually to the fifth ring, the fourth and third remain pale, but the second and first become black.

These small animals move very quickly, and soon leave the place where they were deposited as eggs. When they feel any movement in the neighbourhood, they roll themselves up so as to look like black points. The metamorphosis of all the eggs into larvæ, and the disappearance of the insects, does not require more than a quarter of an hour. The young larvæ reach the earth and then penetrate downwards.—*Bull. Univ.* B. xx. 181.

8. *Effect of Light on Plants.* M. Leuchs.—It is well known that solar light, by enabling plants to decompose and assimilate carbonic acid, gives them the power of forming volatile and aromatic principles, and of acquiring a green colour. Its presence is so necessary to flowering and fructification, that ripe seeds have never been obtained in darkness; on the contrary, if an etiolated plant be exposed for three, four, or five hours to the sun, it immediately becomes of an equally intense green colour, with those which have continually grown in light. Plants raised in the open air, when put into darkness, become pale and fade in two or three days; those which, after being raised in darkness, have been exposed for a time to sunlight, cannot again support the privation of light, but die; and water charged with camphor, or essential oil, which has great power of invigorating plants, cannot prevent their destruction. The perfect absence of light is therefore very injurious to plants, and M. Leuchs concludes, that, without the light of the moon and stars, nights would destroy vegetables.

The light of a lamp can, although imperfectly, replace that of the sun, the plant becomes green and tends to the light. When seeds were germinated in three vessels, the first uncovered, the second covered with single, and the third with double paper, those of the first vessels exhibited less external development, but when dried, they gave more solid matter; those in the second were more developed, but were more aqueous and loose; the difference was still greater in the third vessel.

The texture of various plants appears to be more or less aqueous (if the word may be used), when deprived of light according to the nature of the plants. When plants were placed in a damp cellar or cave, enlightened by a flame, those nearest the flame contained most solid matter; the results were so regular, as to present something like a law, relative to the action of various quantities of light on vegetables.

Light reflected by mirrors appeared to have a very beneficial influence upon plants, and M. Leuchs' thinks that many hill sides are rendered fertile by the similar reverberation of light from the neighbouring rocks.—*Archiv von Kastner*, xv.

9. *On the Size of the Pear and other Fruits.* M. Jaume St. Hilaire.—M. Saint Hilaire observed in certain wall pear trees that the fruit which was supported on the branches or otherwise, grew to a larger size than that which hung freely in the air, and thought that the difference probably arose from the weight of the hanging fruit stretching the vessels in the stalk, and therefore, by contracting their diameters, rendering them less able to convey that abundance of nourishment which they might otherwise receive. In consequence of

this opinion, certain experiments were made. On the 15th September last, two pears of a young tree were selected; one near the middle of the tree was nine inches four lines in circumference, and was left hanging from its branch; the other was eight inches ten lines in circumference, and was supported on a little shelf on the top of a stake fixed in the ground. On the 30th of the same month the pears were gathered; the first had increased only two lines in circumference, the second nine lines, which is a great deal for a fruit already so large, and in the space of only fifteen days.

To remove objections arising from difference of position in the tree, other experiments were made. Two pears, coming from the same part of the same branch of the same tree were chosen on the 15th September; the one left to hang naturally was eight inches four lines in circumference; the other, which was supported, was eight inches in circumference. On the 7th of October these were gathered; the first had increased two lines in circumference, the second eight lines: here the larger remained suspended, the smaller was sustained; the reverse experiment was therefore made, and on the 15th September, two pears were selected springing from the same place, and the larger was supported; it had three lines more of circumference than the smaller. On the 15th of October following the fruits were gathered, the larger had then nine lines more of circumference than the smaller, or had gained six lines, which, as the fruits had all nearly arrived at maturity, must be considered as a great deal.

There is, therefore, reason to believe, that if such experiments as these were commenced in July and August they would give still more marked and satisfactory results, and that the theory and practice may be applicable to many other fruits, as quinces, apples, oranges, &c. A fact which is in favour of the theory is, that large pears have generally short stalks, whilst the stalks of small pears are unusually long. It is intended that many experiments on various kinds of fruit shall be made this season.—*Annales de l'Agriculture*, 1829.

10. *On the Structure of the Cellular Tissue of the Pith and Bark of the Cereus Peruvianus, and the Existence therein of prismatic Crystals of Oxalate of Lime.* By M. Turpin.—This memoir is reported by MM. Chevreuil and Cassini. M. Turpin first refers to analogous observations made previous to his own, and especially by Jurine in the *Journal de Physique* for 1802. His memoir on the organization of leaves gives, for the first time, correct details relative to this point. M. Jurine thought the needles he saw were organs; since then, others have proved they are crystals, and M. Decandolle, without referring to their nature, has called them *Raphides*. M. Turpin believes that these crystals had never before been found within the vesicles, but only in their interstices, and remarks that they had never been seen except separate one from another, or easily separable, very narrow and acicular. Those, on

the contrary which he has observed are formed within the vesicles; they cohere firmly in round conglomerates, and they are not of a long fine filiform appearance, but true crystals, broad and short, presenting in the microscope distinct faces and angles. These curious observations have been made by M. Turpin on a *Cereus Peruvianus* which died lately in the Jardin du Roi, where it had vegetated 130 years. When looking with the naked eye at the cellular tissue of the pith of this old tree, he remarked that it was crammed as it were with a fine brilliant white sand. A lens sufficed to shew that each of these grains was a mass of crystals, and then, on closely examining the whole by the microscope, the following facts were determined. The crystals in question were white, or rather transparent, prismatic, tetragonal, rectangular, terminated by a tetrahedral pyramid, which most usually was destroyed, because of the extreme fragility of the crystals. They were rarely isolated, but usually in rounded radiating groups, of which the diameter was about the sixth of a millimetre (0.00656 of an inch). M. Turpin then wished to observe these same crystals in the living tissue, so as to ascertain their situation and origin: the cellular tissure of the pith appeared to him to be composed of unequal vesicles, membranous, diaphanous, round, placed irregularly one on another, filled with water and air, and containing also grains of green fecula; and he assured himself that the crystals were formed in the interior of the vesicles, and that the masses of green fecula served as bases or nuclei for the first crystallizations. The abundance of these crystals in the dry tissue of the present instance allowed of a chemical analysis being made, and they were found to consist of oxalate of lime. The reporters verified all these facts, and express their conviction that M. Turpin's memoir adds very important and remarkable facts to the few previously known, relative to the crystallizations which occasionally form in the interior of plants.—*Revue Ency.* xlv. 765.

11. *Russian Diamond Mines.*—When in the year 1826, Professor Engelhardt undertook a scientific journey into the Uralian Mountains, he remarked that the sands in the neighbourhood of Koushra, and those of the platina mines at Nigny-Toura, strikingly resembled the Brazilian sands in which diamonds are found. Baron Humboldt, during his late residence in the same country, confirmed this resemblance; and examinations having been made according to his advice, a young countryman who was employed in washing the auriferous sand, on the grounds of the Countess Polier, discovered a diamond on the 20th of June last, which was in nothing inferior to those of Brazil; soon after, many others were found superior in weight to the first. Thus Russia has added this source of riches to those which of late years it has obtained in the form of gold and platina mines from the Ural chain of mountains.—*Revue Encyc.* xlv. 460.

12. *On the Nature of Earths which, without Cultivation or Manure, are more or less favourable to the Nourishment and Growth of Plants.*—This subject has been investigated by M. Jaume St. Hilaire, whose memoir is reported upon by MM. Thenard and Sylvestre, nearly in the following words—" The author remarks that most philosophers who have analyzed arable earths, devoted themselves exclusively to such as had been cultivated, and in which the primitive constitution was of course more or less altered. He believes that the various kinds of earth in their first state have particular powers of nourishing such and such plants, and thinks that the exact knowledge of these peculiarities would enable cultivators to put those seeds in the ground which are most suited to it. To this end the author has analyzed, under the inspection of Vauquelin, two kinds of earth, which he had collected; one from the wood of Meudon, in the plain of La Patte d'Oie, where the spontaneous vegetation is vigorous and varied, and the other from the plain des Sablons, where no cultivation had been carried on, and where the plants were so few as to be scarcely visible. From these analyses, and from others, which the death of Vauquelin prevented him from concluding, he draws the inference,—i. that all earths are composed of silica, alumina, lime, magnesia, &c., in different proportions, plus a vegeto-animal matter, which is more abundant as the earth is more fitted for the nourishment of plants; ii. that plants placed in earths, of which the constituent parts have an analogy with the particular nature of the plants, do not exhaust the soil; iii. that a series of observations on the different species, genera, and families, which grow naturally and in great numbers, perpetuating themselves on certain soils, with the analysis of these same soils, would be of great utility in agriculture. The reporters think that agriculture would draw, from such labours, general inductions, rather than positive directions, but still think they would possess great interest.—*Revue Ency.* xlv. p. 762.

METEOROLOGICAL DIARY for the Months of March, April, and May, 1830, 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 MARCH, 1830.

		Thermometer. Lowest.	Thermometer. Highest	Barometer. Morn.	Barometer. Eve.	Wind. Morn.	Wind. Eve.
Monday....	1	37	55	30.10	30.14	W	WbS
Tuesday ...	2	44	51	30.19	30.20	SW	SW
Wednesday.	3	42	49	30.20	30.20	SSW	SE
Thursday ..	4	26	46	30.09	29.90	SE	ESE
Friday	5	25	50	29.82	29.82	ESE	SE
Saturday...	6	28	48	29.82	29.82	ESE	EbN
Sunday	7	25	44	29.84	29.83	EbN	EbS
Monday....	8	30	48	29.83	29.78	EbS	SE
Tuesday ...	9	32	48	29.53	29.41	SE	SE
Wednesday.	10	38	50	29.41	29.50	W	WbN
Thursday ..	11	39	56	29.46	29.69	W	W
Friday.....	12	42	56	29.88	29.74	SW	SW
Saturday...	13	34	51	29.92	30.10	W	W
Sunday	14	36	52	29.93	29.72	SW	SW
Monday....	15	37	52	29.32	29.19	SW	SW
Tuesday ...	16	30	50	29.24	29.50	SW	WbS
Wednesday.	17	33	59	29.50	29.72	SW	WbS
Thursday ..	18	44	58	29.87	29.98	WbS	SW
Friday	19	45	55	30.00	29.97	SW	WbS
Saturday...	20	36	53	29.95	29.90	SbW	SW
Sunday	21	33½	56	30.14	30.14	W	WbS
Monday ...	22	37	51	30.00	29.93	S	W
Tuesday ...	23	33	53	29.78	29.83	WbS	W
Wednesday.	24	35	56	29.83	30.00	W	W
Thursday ..	25	36	60	30.10	30.18	WbS	W
Friday.....	26	39	64	30.32	30.39	W	W
Saturday...	27	30	64	30.39	30.39	W	NW
Sunday	28	31	64½	30.30	30.20	NW	NE
Monday ...	29	30	63	30.08	29.98	NE	SW
Tuesday ...	30	32	59	29.90	29.85	SW	NE
Wednesday.	31	29½	58	29.72	29.61	E	NW

For APRIL, 1830.

		Thermometer. Lowest.	Thermometer. Highest	Barometer. Morn.	Barometer. Eve.	Wind. Morn.	Wind. Eve.
Thursday...	1	33	44	29.61	29.62	NE	ENE
Friday	2	31	39	29.54	29.37	NE	E
Saturday...	3	30	40	29.20	29.50	NE	W
Sunday	4	23	39	29.98	30.00	NW	NW
Monday....	5	20	44	30.06	29.88	W	WbS
Tuesday ...	6	25	44	29.66	29.66	W	W
Wednesday.	7	42	56	29.69	29.69	W	SW
Thursday ..	8	41	64	29.51	29.41	SE	SE
Friday.....	9	38	62	29.30	29.30	SE	SW
Saturday...	10	38	57	29.28	29.42	W	SSW
Sunday	11	39	56	29.42	29.47	WbS	SW
Monday....	12	41	58	29.35	29.35	W	WSW
Tuesday ...	13	41½	56	29.50	29.73	W	NW
Wednesday.	14	33	60	29.81	29.78	W	SW
Thursday ..	15	46	61	29.63	29.50	SW	SW
Friday.....	16	48	64	29.48	29.51	WSW	SW
Saturday...	17	49	62	29.50	29.48	SW	W
Sunday	18	38	60	29.63	29.64	W	SW
Monday....	19	36	57	29.54	29.54	W	W
Tuesday ...	20	38	58	29.60	29.76	S	WbN
Wednesday.	21	35	58	29.73	29.40	SW	SW
Thursday ..	22	47	59	29.40	29.27	SW	SW
Friday	23	47	60	29.20	29.10	SW	SW
Saturday...	24	45	56	28.98	29.42	SW	W
Sunday	25	35½	59	29.81	29.96	W	SW
Monday....	26	45	63½	29.92	30.00	SW	SSW
Tuesday ...	27	34	63	30.03	30.03	SbE	SE
Wednesday.	28	38	68	30.00	29.90	ESE	ESE
Thursday ..	29	41	71	29.88	29.80	EbS	EbS
Friday.....	30	40	73	29.77	29.71	EbS	SW

For MAY, 1830.

		Thermometer. Lowest.	Thermometer. Highest	Barometer. Morn.	Barometer. Eve.	Wind. Morn.	Wind. Eve.
Saturday...	1	46	63	29.77	29.80	SW	WbS
Sunday	2	40	62	29.90	29.90	SW	W
Monday....	3	29	61	30.00	30.02	W	WbN
Tuesday ...	4	31	63	30.02	29.98	EbN	SE
Wednesday.	5	35	69	29.95	29.91	SE	SE
Thursday ..	6	43	74	29.80	29.64	E	EbS
Friday	7	51	74	29.53	29.41	SW	ESE
Saturday...	8	40	68	29.47	29.37	SW	SW
Sunday	9	48	58	29.23	29.17	E	W
Monday ...	10	43	52	29.20	29.45	NE	NE
Tuesday ...	11	36	52	29.53	29.58	N	N
Wednesday.	12	39	55	29.60	29.64	NW	NW
Thursday ..	13	41	53	29.76	29.90	NbN	NE
Friday.....	14	34	60	29.94	29.94	E	SW
Saturday...	15	44	64	29.94	30.00	SW	W
Sunday	16	37	66	30.05	30.05	W	W
Monday	17	50	71	30.01	29.98	W	W
Tuesday ...	18	46	70	29.89	29.77	W	SW
Wednesday.	19	44	66	29.70	29.72	SW	SW
Thursday ..	20	50	63	29.78	29.78	NE	NE
Friday	21	39	60	29.72	29.68	NE	NE
Saturday...	22	41	58	29.61	29.70	NNE	WbN
Sunday	23	42	64	29.77	29.69	E	E
Monday	24	48	64	29.57	29.50	NE	SW
Tuesday ...	25	45	65½	29.50	29.38	SW	WSW
Wednesday.	26	43	62	29.27	29.27	W	W
Thursday ..	27	45	57	29.27	29.40	WbS	WbN
Friday	28	45	58	29.60	29.80	NW	NE
Saturday...	29	36	62	29.84	29.82	W	SW
Sunday	30	47	61	29.53	29.58	SW	WbS
Monday....	31	43	63	29.58	29.70	SW	SW

INDEX.

O.

P.

Q.

R.

S.

16/M.P.

THE END.