GLEANINGS

IN

SCIENCE.

JANUARY TO DECEMBER.

VOL. I.

The Gleaners spread around, and here and there,
SPIKE AFTER SPIKE, their *scanty* harvest pick.

THOMSON.

In the knowledge of bodies we must be content to *glean* what we can from particular experiments; since we cannot, from a discovery of their real essences, grasp at a time whole sheaves, and in bundles comprehend the nature and properties of whole species together.

LOCKE.

CALCUTTA:
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1829.

TO

THE HON. SIR E. RYAN, KNT. PRESIDENT,

J. CALDER, ESQ. }
A. STIRLING, ESQ. } VICE-PRESIDENTS,

AND TO

THE OTHER GENTLEMEN

MEMBERS OF THE

CLASS OF NATURAL HISTORY AND PHYSICS

OF

THE ASIATIC SOCIETY,

THIS FIRST VOLUME OF AN ATTEMPT TO CO-OPERATE,

HOWEVER HUMBLY,

IN THE LAUDABLE EFFORTS MADE BY THEM FOR THE CULTIVATION OF SCIENCE IN INDIA,

IS, WITH PERMISSION,

RESPECTFULLY INSCRIBED

BY

THEIR VERY OBEDIENT SERVANT,

THE EDITOR.

CALCUTTA, Dec. 1829.

PREFACE.

The first year of our Editorial labours has closed, and we may be allowed to congratulate those who have taken an interest in the success of our experiment, on the completion of one volume, at least, of our Scientific Gleanings in India. We have every reason to be satisfied with the encouragement we have received; as our list of subscribers published herewith will fully prove. The work may now be considered to be fairly established, as it will involve no charge, or but a very small one, to the conductor, for the ensuing year. What has occurred in the past has been chiefly the effect of an over-estimate as to the number of copies likely to be required. This will of course be corrected.

That there should be (at least for some time to come) a demand for such a work, sufficient to remunerate a competent Editor for his trouble, or enable him to purchase the assistance of able contributors, as was proposed at the outset of the present work, is perhaps scarcely to be expected. One of our brethren of the weekly press has observed, that till our system of education be improved, Science must continue at a low ebb; and we may add, not only in India, but in England. At present the object of education, as he has justly said, is to teach the knowledge of words, not things. The almost total ignorance in which men of excellent talents, and otherwise of the best education, are immersed, on subjects of the greatest curiosity and interest, is truly surprising; nor can it be accounted for in any way, but by considering the strong bent given to the mind when young, by the undue importance attached to the study of a dead language. The folly of supposing all knowledge worth acquiring, to be confined to the classics, is fortunately, however, losing ground every day; and indeed, if nothing else should produce such a reform, the great progress of real knowledge amongst the lower orders would compel it; otherwise, "the peasant will be treading on the courtier's kibe."

Comparing our encouragement with that given in England to similar labours, we have no reason to complain; nor does India suffer in the comparison. The modern Babylon, with her countless multitudes, cannot support two monthly*, and one Quarterly Journal on general science; while India, or rather Bengal, with a reading public of little more than 2000, has one. We do not here speak of course of the value of the respective publications, but of the encouragement; which indeed appears the more laudable, as it is given rather with the view of fostering a puny effort, than with that of receiving any new stores of information, or of adding any very valuable accession to our knowledge.

But while we disclaim the arrogance of affecting an equality with the English journals, we may yet be permitted to claim for some of the papers in the Gleanings some little consideration. To particularize would be invidious; we shall, therefore, merely say, that we think some of the articles in this first volume are of real merit, and would be very welcome contributions even at home; while a majority of them do not yield in interest to the average quality of those we find in the journals there. We may fairly claim the merit of having rescued from oblivion or destruction many, if not all, of these; nor do we doubt but we shall, in the succeeding volume, add to their number.

Although the knowledge we had of the existence of many such short papers in the portfolios of the curious in India, (often on subjects inaccessible to research in Europe,) and the hope of embalming them in print, was not without its share in our inducement for projecting the work; yet it was by no means the only motive, nor even the principal one by which we were influenced. Our views were, in fact, less directed to add any very interesting contributions to the general stock of science, than by a periodical discussion of various subjects of more or less interest, to foster, if possible, and spread a taste for those pursuits; and to furnish, occasionally, assistance to the student or beginner in a country where, as in India, he must have so many difficulties to contend with. Scattered over so extensive a surface, our numbers so scant, and our intercourse so limited, with books so expensive, both in cost and carriage; it requires some effort, and even some sacrifice of prudence, for any, but those most favorably situated, to keep up or improve any little acquaintance they may originally have had with such subjects. It was thought that by establishing the practice of communicating to one another the various difficulties occurring in our pursuit of any enquiry, the new views which

* The Philosophical Magazine and Annals of Philosophy, which have merged into one, and Brande's Quarterly Journal of Science. There are doubtless many Journals published in London, but they are either confined to particular subjects, or are addressed to particular classes. The above are the only journals devoted to general science, and addressed to the educated classes. The volumes of Transactions of Learned Societies are numerous, but of these we have our share in Calcutta. Dublin, the second city in the empire, could not support a journal of science, though published only once in six months.

might strike us, or the criticisms and detections of error which might be forced on us in our references, a feeling might be created, which would tend, in some measure, to assist this effort; while to the student, the practice might afford an opportunity of obtaining information which he would otherwise seek in vain. It was thought, in fact, that by showing to the scientific community of India, small as it is, their own strength, and by suggesting and supporting a combination of effort, the apathy and indolence which are the bane of our Indian clime, might be in some measure counteracted.

It has been said, and by an authority which many will deem it presumption in us to question, that "A mere man of letters, retired from the world, and allotting his whole time to philosophical or literary pursuits, is a character unknown among Europeans resident in India; where every individual is a man of business in the civil or military state, and constantly occupied, either in the affairs of government, in the administration of justice, in some department of revenue or commerce, or in one of the liberal professions. Very few hours, therefore, in the day or night can be reserved for any study that has no immediate connection with business, even by those who are most habituated to mental application."

We cannot agree to this as a true explanation of any supposed disregard the sciences may have suffered under in India. It proves indeed too much; for it proves that nothing but our official duty can engage our attention. In England, men of science have equally their business to attend to, their profession to follow; yet they find leisure for cultivating science*. And why so? Because their labours are appreciated. There is an enlightened public opinion to reward their improvements and discoveries with praise, should the more solid return of profit, from any cause, be withheld. In England every thing has its value, from a new theory down to a patent corkscrew. And when it is so in India; when we find an enlightened public opinion taking an interest in the subject; we shall be sure to find leisure for those researches, which, as in India (at least for a time) they can scarcely look for any stimulus beyond that of opinion, must languish or revive, as that opinion is withheld or administered.

With views like these, the opportunity for establishing the work could not have been better selected, or in every respect more favorable. The Asiatic Society, after so many failures to keep up the meeting of their Physical Committee or Class, had roused themselves to another well-meant effort; and with a spirit, and under circumstances, which promised more

* It is altogether an erroneous idea that science owes much to men of leisure. The less a man has to do, the less he will do. For a proof of what we assert, look at the works of Bacon, of Newton, of the celebrated author of the above passage, and of many others; above all, look at Brougham.

success to this than to any of the former attempts. The establishment of some kind of journal that should appear at less distant intervals than the volume of their Transactions, to support and be supported by this movement in favor of science, was so obvious an idea, that it occurred to many at the time. The Editor, who had many years before projected such a work, finding opinion generally favorable to the project, at once set it on foot. That the experiment has owed great part of its success to the suitableness of the juncture at which it was tried, we are quite ready to acknowledge; whether it has repaid this debt in any way, by fostering the spirit of scientific research, must be left to others to determine. But even if this point were satisfactorily established, we should still have to acknowledge our obligations to some of the members of the Committee individually, not only in placing at our disposal materials for publication, but in making generally known and recommending the work. We wish we could permit ourselves to dilate on this subject,—so far at least as to express the grateful sense we entertain of the valuable aid we have derived from each of these gentlemen; but we are sufficiently aware, that to particularise any further would take from the acceptableness of our acknowledgments. Many gentlemen too entirely unconnected with the Asiatic Society, have been kind enough to favor us with their contributions; and for these also we beg to return our best thanks. We owe much, also, to our friends in the country, for their valuable and timous assistance. Indeed, it must not be concealed, that the majority of the papers are of provincial origin. Amongst our contributors from this quarter is one gentleman, whose productions have not been thought unworthy the pages of the first scientific work in England, if not in Europe. The warm interest he has taken in the success of our small work, and the very material aid which either by himself or friends he has afforded us, command our best thanks. This gentleman has, we think our readers will agree with us, been ably supported by his moofussil brethren. Physics, Geology, and the Natural History of the Invertebrata appear as departments in science to be ably represented by them; and without intending any thing offensive to our town contributors, we might remark, that the strength of the house is in the country gentlemen. In conclusion, we must not forget to say how much we are indebted to the daily and weekly press of Calcutta. The liberal and judicious notice which the conductors of that press have taken of the work, has no doubt introduced it to the acquaintance of many who would otherwise never have heard of it. To them and to all our supporters we once more beg to offer our best acknowledgments, happy if, in the succeeding part of our voyage, our course may be as smooth, and our vessel as buoyant as in this our first essay.

During the course of this first volume our work has so far changed in character, that from being, as was originally intended, chiefly a vehicle for reprinting interesting articles from the English Journals, it has become almost, if not entirely, original. We confess we have been not only flattered but surprised at the number of communications we have received; nor can any thing better demonstrate the groundlessness of the objection of want of materials, (which we so often heard urged when first suggesting our project,) than the fact that we have never, after the second or third number, had occasion to begin printing before we had already collected materials, not only for that number, but for the succeeding one. Can it be doubted, but that if circumstances allowed justice to be done the work, this encouragement would materially increase? At all events, it is clearly proved, that there are materials for such a work in India, even at its outset. Owing to the narrowness of our limits, and the above circumstance, we fear we may have sometimes appeared to delay our correspondents' favours; but in future we shall be less restricted as to room, owing to the new postage regulations. We shall, therefore, in the ensuing volume not hesitate to increase the number of our pages when any subject of pressing interest, for which there might not otherwise be room, may appear to demand such a measure.

Of the subjects of practical utility, which we noticed as fairly coming within our beat, yet without interfering with those, to the investigation of which the public societies of India have devoted themselves, we have more or less illustrated those of Indigo, Colouring matter, Sugar, Bridges, Cooling of liquors, Raising water, Strength of timber, &c. There are still many which remain untouched; Internal communication by roads and canals, Boat building, Steam navigation of rivers, Architecture, Draining of towns, Ventilation of houses, &c.&c. To these we invite the attention of our readers, who we trust will not refuse to record in our pages any hint which may occur to them calculated to throw light on any of these truly important subjects. The advantage of a periodical is, that the most busy may find time to contribute to it, satisfied that the seed is not cast upon the waters, but upon a soil where, in due time, it may vegetate and produce fifty fold. Nor have those averse to their names being handled by the public, any thing to fear on this score, as their incognito may, if they wish it, be perfectly preserved; and thus the value of any suggestions they have to offer be fairly appreciated, without being subjected to that prejudice which the knowledge of the author is often found to create.

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CORRIGENDA.

Page 20	Line 4	*from b. for* ‘Lemineeab by Col. Boyle,’ *read* ‘Semaria by Col. Voyle.
52	6	*after* ‘(f'),’ *insert* ‘D.’
—	8	,, ‘(f),’ ,, ‘D.’
—	10	Ditto, ditto.
—	11	Ditto, ditto.
—	5	*for* ‘*f*or,’ *read* ‘for.’
56	9	*from b. for* ‘economica,’ *read* ‘economical.’
59	11	*from b. for* ‘Dr. Butler,’ *read* ‘Dr. Butter.’
76	36	dele and.
81		*In Table—temp.* 80° *and tension* 10°. *for* ‘25°,1,’ *read* ‘24°,1.’
82		*Supply the signature* P.
85	13	*from b. for* ‘ofl ogarithms,’ *read* ‘of logarithms.’
88	10	*for* ‘pistion,’ *read* ‘piston.’
89		*Supply the signature* ‘D.’
—	8, 9, & 10	*from b. for* ‘Ar Co—sine,’ *read* ‘Ar Co. Sine.’
92	15	*for* ‘union,’ *read* ‘unringed.’
—	38	*for* ‘quart of a water,’ *read* ‘a quart of water.’
114	13	*from b. after* Cumulostrati *supply comma and dele comma after* nimbi.
132		*in column S. article* SOONDREE *for* ‘3574,’ *read* ‘2574.’
148	6	*from b. for* ‘develop,’ *read* ‘develope.’
150		*1st Note at foot of page insert* a comma *before the decimal in* 2517,6.
154	11	*for* ‘28 s,’ *read* ‘,28 s.’
155	2	*for* ‘secunng,’ *read* ‘securing.’
—	28	*for* ‘tension, 75 inch,’ *read* ‘tension ,75 Inch.’
190	32	*from b. for* ‘695,’ *read* ‘69,5.’
—	33	*for* ‘inch ,59°,’ *read* ‘inch, 59°.’
193	8	*from b. for* ‘$\frac{D'n}{D}$,’ *read* ‘$\frac{D'n}{Dn}$.’
210		*Note at foot of page, for* ‘tranferred,’ *read* ‘transferred.’
211	32, & 33	*for* ‘regularly meetings monthly,’ *read* ‘regular monthly meetings.
263	17	*from b. for* ‘Subanrie,’ *read* ‘Súbanríca.’
267	5	*from b. for* ‘inclinaton,’ *read* ‘inclination.’
270		*Note last l. for* ‘supposiny,’ *read* ‘supposing.’
		for ‘40,’ —— ‘4.’
283	24	*from b. for* ‘Chóci,’ *read* ‘Chaúci.’
—	16	*from b. for* ‘molabariça,’ *read* ‘malabarica.’
301	17	*from b. for* ‘malaribarica,’ *read* ‘malabarica.’
304	23	*for* ‘lading,’ *read* ‘ladling.’
310	8	*for* ‘nostri,’ *read* ‘nostrum.’
315	9	*supply* ‘6,’ *before* ‘tubes.’
—	22	*from b. for* ‘case lenther,’ *read* ‘leathern case.’
322	27	*from b. for* ‘to the,’ *read* ‘in the.’
325	27	*from b. for* ‘tone,’ *read* ‘true.’
337	3	*from b. for* ‘premised,’ *read* ‘promised.’
339	24	*from b. for* ‘all,’ *read* ‘also.’
363	39	*for* ‘breath,’ *read* ‘breathe.’
—	40	*for* ‘Paludinæ,’ *read* ‘Paludina.’
—	—	*for* ‘belong,’ *read* ‘belonging.’
364	5	*for* ‘Paludinæ,’ *read* ‘Paludina.’
—	15	*for* ‘be,’ *read* ‘lie.’
—	16	*dele* ‘or.’
—	18	*for* ‘ablutions,’ *read* ‘collections.’
—	20	*for* ‘lnwa,’ *read* ‘larva.’
—	25	*for* ‘cave,’ *read* ‘Cén.’
—	32	ditto. ditto.
—	36	*for* ‘Paludinæ,’ *read* ‘Paludina.’
—	51	*for* ‘there,’ *read* ‘these.’
365	18	*from b. for* ‘should,’ *read* ‘shall.’
367	3	*for* ‘trapperian,’ *read* ‘trappean.’
373	1	*dele* ‘on.’
380	32	*for* ‘$V'=V\frac{440\times T'}{448+T}$,’ *read* ‘$V'=V\frac{448+T'}{448+T}$.’

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DIRECTIONS TO BINDER.

The numbers 33. & 44. have been omitted, by mistake, in the paging. The plates to be placed together at the end of the volume. The covers of the work containing the metereological registers to be bound up with it. They may be put together at the commencement of the volume before the fly leaf.

GLEANINGS IN SCIENCE.

VOL. I.] JANUARY 1, 1829. [NO. I.

I.—*ON THE CONSOLIDATION OF THE STRATA OF THE* EARTH.

By Sir James Hall, Bart. F. R. S. Lond. & Edin.

[*From Transactions R. S. Ed. Vol. X.*]

The public attention, animated by scientific controversy, has of late years been much directed to Geological subjects; and the certainty of many important facts, has in consequence been ascertained beyond dispute, which were formerly unknown, or at least involved in such obscurity, that no person could have ventured to assert them, without being charged with extravagance. But though, no doubt, many branches of this science still remain to be investigated, such inquiries may now be said to have acquired a considerable degree of consistency and interest, from the substantial basis upon which they have been found to rest.

Thus, in the present day, it is universally admitted, that a great part, I believe, in point of bulk, by far the greatest part, of the solid rock which constitutes the external mass of our globe, is stratified: that these strata, or at least a considerable portion of them, have at one period consisted of a loose assemblage of sand and gravel, broken from rocks of still higher antiquity: that these fragments are infinitely various in quality, in bulk, and in form; some retaining their original sharpness, others rounded and polished by agitation in the water: that these beds alternate with others of limestone, composed, in a great measure, of the shells of sea-fish, which shells are also occasionally scattered through the other strata. So that on the whole, it seems to be ascertained to the satisfaction of all parties in geology, that the strata,—those, at least, of later formation,—have once constituted collections of incoherent parts. And it is further admitted, that these beds have undergone various remarkable changes, some chemical, some mechanical.

The chemical changes consist in the consolidation of these loose assemblages into their present state of rock, passing, in that transition, through boundless varieties, in point of flexibility and toughness, and occasional brittleness. The mechanical revolutions are no less remarkable, principally in the change of the strata to their present contorted shape, and elevated position, often many thousand feet above the surface of the sea; though there is full reason to believe that they all once lay in a horizontal position at its bottom.

I have said that the greatest part of the crust of our habitable globe seems unquestionably to be stratified, and produced from detritus or fragmented materials. The other portion, though probably the least in bulk, is, generally, the most conspicuous, owing to its durability, elevation, and picturesque beauty. This kind of rock is contrasted with the former class, particularly in its negative qualities; in being, according to some geologists, altogether devoid of stratification in the general mass, and entirely free from component fragments; the whole being made up of crystalline forms, moulded upon each other, in obedience to certain chemical laws.

This crystalline rock, as the Society are well aware, abounds in the neighbourhood of Edinburgh, in Arthur's Seat, Salisbury Craigs, and in Corstorphine Hill. It is decidedly posterior to the stratified class, of which it penetrates the crevices at all angles, in the form of dykes or veins, like stucco cast in a mould; frequently also lodging between the strata in vast shapeless masses.

As the rock in question never fails to preserve this quality of universal and perfect crystallisation, I heartily concur with Dr. Hope in bestowing upon it the general name of *Crystallite*, under which are comprehended all substances of this kind, including not only Whinstone and Basalt, but also Porphyry, Granite, and Sienite of every description.

The solid mass of our globe, then, in so far as it is naturally exposed to our view, or has been penetrated by the labours of the miner, would appear, (with the exception of some streams which have flowed from Vesuvius, Lipari, and other volcanoes in which the rock possesses a glassy structure,) to be comprehended under these two classes, Aggregates and Crystallites.

The whole of these rocks, of both classes, furnish, at every turn, proofs of their having undergone revolutions of the utmost magnitude; and much ingenuity has been exerted, in endeavouring to trace these changes to some consistent and rational system. But of all the active powers of nature, one only has occurred to me as capable of affording a solution, in any degree satisfactory of the phenomena, —I mean the power of internal heat, which, in all ages, and in various countries, has made its appearance at the surface of the earth, not unfrequently from under the ocean, and which still, in our own days, gives occasional proofs of its unabated activity.

To ascertain the reality and sufficiency of this agent, and to trace the volcanic fire to its source, with tolerable probability, is, doubtless, an object of great interest and curiosity; but it has always appeared to me, that the progress of geology was retarded by a premature anxiety to enter into such investigations.

Taking it for granted, however, as, indeed, no one can dispute, that there frequently do arise violent exertions of heat from under the bed of our ocean, Dr. HUTTON held that this might furnish a rational and sufficient theory of the earth, without entering into any inquiry as to the origin of that heat; and admitting that there are many geological facts which cannot be accounted for by such a fire as that of Vesuvius, now acting at the surface, in free communication with the air, he contended that the case may be very different, where that same cause acts at the bottom of a deep sea, and under various modifying circumstances, by which its operation could not fail to be influenced.

This, indeed, constitutes the essence of the Huttonian Theory, which I learned principally in conversation with its illustrious author; and which, since his death, I have taken every means of submitting to a variety of chemical tests; being for ever on the watch for such natural scenes as might illustrate these principles, as well as for opportunities of making experiments, to determine whether such modifications on the action of heat were, or were not, sufficient to justify these expectations of Dr. HUTTON.

It was in prosecution of these views that I formerly undertook a set of experiments, proving, I believe, to the satisfaction of the scientific world, the identity of Whinstone and Lava, of which a full detail is given in your Transactions. In farther illustration of the same topic, my experiments on Carbonate of Lime were formerly undertaken, by which it was shewn, that calcareous matters, exposed to heat under pressure, might be fused; and, on cooling, would crystallise, so as in every respect to resemble marble. To these I beg leave likewise to refer the Society.

The immediate object of the paper I have now the honour of submitting to the Society—the consolidation of the strata—has been pursued in a similar spirit, and with similar views to those formerly announced. In making efforts to trace the modifications which the action of heat would undergo, when compelled to act under the influence of compression, or of other circumstances, all of which, in company, I have always been willing to distinguish by the name of *Plutonic*, (although the term was originally suggested, ironically, by one of our keenest antagonists, the late celebrated Dr. KIRWAN,) I was led to the particular topic of this paper, by an unexpected scene which presented itself in my own neighbourhood, in the country.

It had often been urged, and apparently with good reason, against this branch of the Huttonian Theory, that no amount of heat applied to loose sand, gravel, or shingle, would occasion the parts to consolidate into a compact stone. And as all my experience led to the same conclusion, I saw that, unless, along with heat, some flux were introduced amongst the materials, no agglutination of the particles would take place. The striking circumstance above alluded to, as occurring near Dunglass, and which will be particularly described presently, having suggested to me the idea that the salt of the ocean might possibly have been the agent in causing the requisite degree of fusion, I instituted a series of experiments, the details of which I am about to bring before the Society. By these, I conceive it will be shown, that this material, under various modifications, is fully adequate to explain the consolidation of the strata, and many other effects which we see on the surface of the earth.

My success, from the first, was such as to promise the most satisfactory result, though it is only within the last year that I have been able to command the repetition of the experiments in a manner fit to be laid before this Society This must be my apology to those who hear me, and to such of my friends as take an interest in these investigations. for having so long delayed the publication of a set of facts, some of which had presented themselves to my view many years ago.

Whoever, indeed, has had any experience in the prosecution of new subjects of experimental inquiry, knows that, owing to his ignorance of the requisite adjustment of the proportions of the ingredients, and of other similar arrangements, he must depend, in a great degree, upon chance for the success of his first results, and that he must often submit to spend much time and labour upon a subject, even after it has been made out to his own satisfaction, before he has acquired sufficient command over its details to answer for the result of any particular experiment, so as to be able to produce it with confidence to the public.

It may be interesting, in the first place, to describe, in a general way, the geological structure of the country, in the neighbourhood of the singular scene which gave rise to these speculations.

On different occasions I have laid before this Society observations made on the rugged shore which occupies the southern shore of the entrance of our estuary the Firth of Forth, which, from being frequently washed by a very boisterous ocean, presents to view a distinct exhibition of its internal structure. The eastern part is occupied by the promontory of Fastcastle, composed entirely of the elder quality of strata, called by the Germans Grey Wacke. Further to the west it consists of cliffs formed of Sandstone, nearly in a horizontal position. These two meeting in the crag called the Siccar Point, afford the most distinct view we any where have of the peculiar relation and mutual history of these two rocks.

More inland, on the borders of Lammermuir, a set of horizontal beds occur, consisting of a loose assemblage of rounded stones, intermixed with sand and gravel, which bear every appearance of having been deposited by water, and which, as to their general history, seem to have undergone no change since the overwhelming, though transient agitations of water, of which I have frequently had occasion to speak in this Society.

In the summer of 1812, as I was returning from visiting the granitic range which occurs in the water of Fasnet, in the hills of Lammermuir, and riding down the little valley of Aikengaw, which deeply indents this loose collection of gravel and shingle, about two miles above the village of Oldhamstocks, and at the distance of eight or ten miles from the sea, I was struck with astonishment on seeing one of these gravel banks, formed, as above described, of perfectly loose materials, traversed vertically by a dyke, which, in its middle, consisted of whinstone, and was flanked by solid conglomerate; but this solidity abated gradually till, the conglutination of the rounded masses diminishing by degrees, the state of loose shingle and gravel was entirely restored on both sides. The agglutinated mass adjacent to the dyke bore no resemblance to the result of calcareous petrifaction; scarcely ever gave effervescence with acid; and, by its gradual termination, differed from any whinstone-dyke I have seen to penetrate the strata; for, in the ordinary case, the termination of the crystallite against the adjoining aggregate through which it passes, is almost always quite abrupt.

About a hundred yards higher up the valley of Aikengaw, there occurs an agglutination *similar* to the *last*, *though* without any whin-dyke, and sufficiently strong to resist the elements, by which the surrounding matters had been washed away, leaving the pudding-stone, or agglutinated shingle, to stand up by itself in a manner remarkable enough to have attracted the notice of the peasantry as something supernatural, since they have bestowed upon it the name of the Fairy's Castle.

Farther up the stream, other agglutinations occur frequently, as we could see in little narrow glens cutting through the mass; and higher still, they are so numerous as to meet and convert the whole into one unbroken mass of pudding-stone, occupying all that is exposed to view.

These very remarkable, and, to me at least, novel appearances, were the first which suggested the idea, that the consolidation not only of this class of conglomerates, but of sandstone in general, had been occasioned by the influence of some substance in a gaseous or aëriform state, driven by heat into the interstices between the loose particles of sand and gravel, where it had acted as a flux on the contiguous parts. On considering what this penetrating substance might be, and from whence it could have come, the following circumstance presented itself to my recollection at the moment, and promised to afford some assistance to these conjectures.

A few miles lower down the valley in which the above facts were observed, at the distance of more than a mile from the sea, and between two and three hundred feet perpendicularly above it, there occurs a crag of sandstone, in which a numerous succession of strata are distinctly visible. Several of these beds have yielded much to the action of the air, and, in dry weather, exhibit a considerable white efflorescence, which has completely the taste of common salt; and so remarkable is this circumstance, that the rock has acquired, in the country, the name of Salt-Heugh.

Here, then, it immediately occurred to me, was probably the source of an abundant supply of the elastic substance or fumigator, whose action as a flux had been pointed out by the agglutinations in Aikengaw above described.

I conceived, that, if there were at the bottom of the sea a bed of sand and gravel, drenched with brine of full saturation, and that heat were applied to it from beneath according to Dr. HUTTON's hypothesis, the first effect would be, to drive the water from the lowest portion of the sand, and to convert the salt which remained amongst it, together with the sand, into a dry cake. During this operation, or until the cake became quite dry, the absorption of latent heat would prevent the temperature from surpassing the boiling point of brine. But no sooner was this dryness accomplished, than, I imagined, the temperature of the mass would begin to rise above that pitch; the portion of it next the fire would gradually acquire a red-heat; that then the salt, being made by the heat in part to assume an elastic form, would be sent in fumes through the dry cake just described, and thus, by partially melting the contiguous particles, produce an agglutination.

Such being my theoretical views, no time was lost in submitting them to the test of experiment. Taking it for granted that a quantity of sea-salt must frequently be formed and deposited, along with sand and gravel, at the bottom of the ocean, (in the manner I shall have occasion to describe at another stage of this paper,) where the water has been collected by its superior specific gravity, in the form of brine, I proceeded to make the following experiments.

Dry salt was placed along with sand, sometimes in a separate layer, at the bottom of the crucible, and sometimes mixed throughout the experiment: the whole was then exposed to heat from below. I found that the salt was invariably sent in fumes through the loose mass, and by its action produced solid stone in a manner completely satisfactory, as illustrative of the facts in Aikengaw; and so as to give a good explanation of the production of sandstone in general.

These artificial stones are of various degrees of durability and hardness;—some of them do not stand exposure to the elements, and crumble when immersed in water;—some resist exposure for years;—others are so soft as not to preserve their form for any length of time;—while some bear to be dressed by the chisel; and, it may be remarked generally, that, as far as the results of my experiments have been compared with natural sandstone, the same boundless variety exists in both cases. A striking instance of this resemblance occurs in the case of the Salt-Heugh, the sandstone of which, when immersed in water, crumbles down, exactly in the same manner as those results of my experiments which taste much of salt.

The fumes of the salt, no doubt, act in all these cases, as a flux on the siliceous matter, and thus cement the adjacent particles together. The Society are, doubtless, well aware of the power of salt fumes in glazing pottery; and the analogy, I conceive, is complete. It is the application alone that is new.

So far the results were satisfactory. But it next occurred, that it might be plausibly objected, that the presence of the superincumbent cool ocean would interfere with the process, on the principles of latent heat. To put this to the test, I proceeded to expose a quantity of sand, covered to the depth of several inches with common salt-water, to the heat of a furnace, and, as the liquid boiled away, replenished it from time to time by additions from the sea. Of course it gradually approached to a state of brine. But this proved a very tedious operation, requiring a continued ebullition during three weeks, without ceasing, before it became sufficiently saturated with salt by the discharge of the fresh-water; and I thought it much easier, and no less satisfactory, to employ brine from the first, formed at once by loading the water with as much salt as it could dissolve, amounting to about one-third of its weight.

The vessels employed in these early experiments, were the large black-lead crucibles used by the brass-founders. I filled the vessel, which was 18 inches high and 10 broad, nearly to the brim with brine of full saturation, the lower portion being occupied, to the depth of about 15 inches, with loose sand from the sea-shore, and thoroughly drenched with the brine. In order to have a view of the progress of the experiment, I placed an earthen-ware tube, about the size and shape of a gun-barrel, closed at bottom, and open at the top, in a vertical position, having its lower extremity immersed in the sand, and reaching to within about an inch of the bottom of the pot, while the other end rose a foot above the surface of the brine, and could be looked into without inconvenience.

After a great number of experiments, furnishing an unbounded variety of results, I at length obtained a confirmation of the main object in view. I observed that the bottom of the porcelain barrel, and of course the sand in which it rested, became red-hot, whilst the brine, which, during the experiment, had been constantly repleuished from a separate vessel, continued merely in a state of ebullition: the upper portion of the sand, drenched with the liquid, remained permanently quite loose, but the lower portion of the sand had formed itself into a solid cake.

On allowing the whole to cool, after it had been exposed to a high heat for many hours, and breaking up the mass, I was delighted to find the result, occupying the lower part of the pot, possessed of all the qualities of a perfect sandstone, as may be seen in the specimens now presented to the Society. Whenever the heat was not maintained so long, the sandstone which resulted was less perfect in its structure, tasted strongly of salt, and sometimes crumbled to sand when placed in water.

Many of these early experiments were accomplished with tolerable success. But still the result was somewhat precarious, and could not be announced with the confidence that I felt in presenting my former experiments to this Society.

The cause of this uncertainty I traced to the chemical operation of the salt, acting as a flux upon the porcelain vessels employed. This very action, I was well aware, was the main agent and cause of our success, when kept within proper bounds; but, on being allowed to pass those limits, and to act on the containing vessel as well as on the experiment, it destroyed the vessel, and converted the whole into a confused mass of slag.

After numberless unsuccessful attempts, and after returning again and again to the charge, with an interval sometimes of years, I at last met with a quality in some of the materials to me altogether unlooked for, by means of which may be obtained successful results, with scarcely any risk of failure.

I found that the action of the salt upon the substances of the crucibles of clay, did not exert itself in the same manner upon iron; but that a large vessel of cast-iron, 18 inches deep by 10 wide, and a common gun-barrel welded up at the breech, and open at the top, enabled me to work with the heat of melting gold, without injuring the vessels, and at any time to produce a perfect freestone; thus satisfying our theoretical expectations.

Similar results, in all respects, were produced by exposing pure pounded quartz to the action of the salt fumes,—and also when gravel, or any other mass of loose materials, was used instead of sand.

Having now shewn, in a satisfactory manner, that salt, whether in a dry state mixed along with loose materials, or driven in fumes through them, or applied in the state of brine, and exposed to heat, is a sufficient agent to produce a consolidation, such as we see in natural sandstones and other stratified rocks, it remains to be investigated, whether an adequate supply of this flux may be reckoned upon in nature.

It is well known that great diversity exists in the degree of saturation of the sea by salt, at different places: and Buffon has been at much pains in collecting examples of this diversity in his geological volumes, introductory to his Natural History. It is known that, in many of the communications between sea and sea, a constant current sets one way, indicating that the evaporation from the sea, to which this stream flows, surpasses in quantity its supply of fresh-water from the rivers, rains, and springs. This is remarkably the case with the Mediterranean, into which a perpetual stream sets from the ocean, at the Gut of Gibraltar. We have reason, then, to conclude, both that the surface of the Mediterranean is lower than that of the ocean, and likewise that the quantity of salt in the former is perpetually on the increase; so that the specific gravity of the waters, and the intensity of their saturation, must be perpetually advancing to a state of brine. I am well aware, that an attempt has been made to render such a conclusion unnecessary, by the supposition of a counter-current flowing at the bottom, out of this great basin; but such suppositions are, in my opinion, altogether gratuitous.

What is here said of the Mediterranean, will apply no less to other seas, and even to the great oceans. And wherever a basin occurs, in which a bottom of great depth is surrounded by a ridge comparatively shallow, we may expect to find the lower portion, at least, of the water in a state approaching to brine.

Without any such theoretical explanation of the manner in which a supply of salt is supposed to be formed, it may perhaps be considered sufficient for my purpose, to recal to the recollection of the Society, that there are in almost in every part of the world vast districts of rock-salt, and in some countries extensive salt lakes and salt rivers ; and in our country we have many instances of brine springs, besides rock-salt in abundance.

Here then, it seems to me, we are plentifully furnished with the means of accounting, in the manner experimentally shewn, for the agglutinations of such gravel as that of Aikengaw, and for the strata of the Salt-Heugh, which, by an easy analogy, may be transferred to sandstone in general, and, perhaps, to stratified rocks of every description.

A member of this Society, however, well known by his scientific acuteness, alleged, first in his public lectures, and afterwards, upon my requesting an explanation of his objection, again repeated, that I was not justified in such theoretical conclusions, respecting the influence of heat at the bottom of the sea, since the neighbourhood of the cool water would necessarily counteract that influence.

In answer to this difficulty, I must beg leave to remark, that, in all my experiments above alluded to, the sand (viewed by means of the gun-barrel) was seen to become red-hot during the process of consolidation, while the superincumbent brine remained boiling above : and it was even found easy, by supplying cool brine in sufficient quantity, to maintain the temperature of the fluid permanently such, that the hand could be plunged into it at top, without injury, the sandstone below remaining all the while at a full red-heat. But whenever I repeated this experiment, with every circumstance the same, both as to duration and temperature, as in the example above detailed, but in which, instead of brine, *fresh* water was used, the result was very different. The lower part of the gun-barrel, immersed in the sand, and in which gold had melted in the brine experiment just mentioned, now remained permanently black and cold ; and the whole of the sand in the pot, when removed from the furnace, fell out loose by its own weight ; not the least trace of consolidation having taken place.

We may thus, I trust, presume to have added one more new and important modifying circumstance of heat, to those already advanced in support of the Huttonian doctrines; for, since it has been experimentally shewn, that heat, under the modifications produced by the presence of salt, as above described, is fully adequate to the consolidation of loose materials, exposed to its action, it may fairly be presumed, that salt has performed a part, and a very important part, in the consolidation of the strata of the globe.

I should be doing injustice to the subject, were I not to state, that, besides the views developed in the foregoing paper, and supported by actual experiment, many others have occurred to me, respecting the agency of salt under various modifications, and all bearing more or less directly upon the Huttonian Theory of the Earth. Some of these views have been submitted to the test of experiment, and the results, as far as they have yet been carried, give me great hopes of ultimate success. Others are still in the shape of mere conjecture ; and none of them are yet in a state to lay before the Society in detail. A simple allusion to one or two of the most important of these views may probably be received with indulgence ; and I shall be very happy if gentlemen possessed of adequate leisure, shall be induced to follow up, by actual experiment, what I have thrown out as mere matter of speculation.

I conceive that salt, in the state of fumes, and urged by a powerful heat, possibly also modified by pressure, or perhaps combined with other substances, may have penetrated a great variety of rocks, acting as a flux on some, as in basalt, granite, &c. ; agglutinating others, as in the case of sandstone, puddingstone, &c. ; softening others, as in the case of contorted strata of greywacke. In many cases, too, I conceive that these fumes may have had the power of carrying along with them various other materials, such as metals in a sublimed state, which would in this way be introduced into rents, veins, and cavities, or may even have entered into the solid mass of the rocks, which I imagine these fumes may have had power to penetrate. I have already tried some experiments in pursuit of these ideas. Salt, for instance, has been mixed with oxide of iron, reduced to fine powder, and then exposed to heat along with quartzose sand. The iron, I found, was borne up along with the salt fumes. The sandstone, formed in this way, was deeply stained with iron, and other most curious appearances presented themselves.

Every one who has seen a sandstone quarry, must have noticed evident traces of iron, the rock being stained in a great variety of ways; sometimes in parallel layers,—sometimes in concentric circles, or rather in portions of concentric spheres, like the coats of an onion,—and, generally speaking, disposed in a way not accountable by deposition from water. All these appearances I would account for, by supposing the rock, either at the moment of its agglutination into sandstone, or at some subsequent period, to have been penetrated by the fumes of salt, charged with iron, also in a state of vapour.

I may mention one very curious result of my experiments with salt and iron, acting upon sand, namely, that, upon breaking up the specimen of artificial sandstone, an appearance often presents itself of incipient crystallisation, if I may use this term; a number of large, shining, parallel faces pervade the whole mass, and, by holding the specimen at the proper angle to the light, this appearance becomes very obvious. What the nature of these crystals is, I have not investigated; but as they very much resemble what we see in different kinds of sandstone, I am of opinion that they hold out a fair expectation of our being able to produce many of the crystalline appearances with which we are familiar in nature.

Common sea-salt, such as I have used, as is well known, is not pure muriate of soda: and, in my experiments, I have mixed various other substances with it. In Nature, we must suppose that various contaminating substances would in like manner occur, to diversify the phenomena; and, accordingly, we do find a boundless variety, in the aspect not only of sandstone, but of almost every kind of rock; and I am by no means without expectation, that, in the course of time, we shall be able to imitate in our laboratory as many of these varieties as we choose to exhibit.

I have long been engaged also in a series of experiments on the formation of *Crystallites*, the name by which, as I have before stated, every crystallised rock might, perhaps, be usefully distinguished in contradistinction to *Aggregates*, or those formed of fragments. This great object in experimental geology, I hope to accomplish by means of an instrument which I have long had in use, for the regulation of high heats, a description of which may probably soon be laid before the Society, together with some further results in support of the Huttonian Theory of the Earth.

II. An Account of Professor Carlini's Pendulum Experiments on Mont Cenis.

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We believe that no account of Professor Carlini's pendulum experiments on Mont Cenis, has hitherto appeared in the periodical scientific publications of this country: the experiments are, however, well deserving of such notice, having been conducted with great care, and having had a specific object in view, which object seems to have been satisfactorily accomplished. The following brief account of them, taken from the original memoir published in the Appendix to the "Ephéméride di Milano" for 1824, may not be unacceptable to those of our readers who interest themselves in subjects of this class.

The length of the simple pendulum vibrating seconds, is a measure of the intensity of gravitation; *i. e.* of the excess of the force of gravity over the centrifugal force. In consequence of the ellipticity of the earth, and of the difference in the direction of the two forces, the intensity of gravitation varies according to the different latitudes. It also varies, in the same latitude, according to the greater or less elevation of the pendulum above the level of the sea; *i. e.* according to its greater or less distance from the centre of the attracting force.

Had the earth a perfectly level surface, such, for instance, as it would have if it were everywhere covered by a fluid, the force of gravity, in receding from the surface, would diminish in the duplicate proportion of the distance from the earth's centre. In the actual state of the globe, however, its continents and its islands are raised above the general level of the sea, by which it is only partially covered; and if a pendulum be raised, on the surface of the land, to a known elevation above the sea, the diminution of gravity will not be, as in the more simple case, proportioned to the squares of the respective distances from the earth's centre; but that proportion will require to be modified, by taking into account the attraction of the elevated materials, interposed between the general surface and the place of observation.

When pendulums are employed in different latitudes, to obtain the ratio of gravitation between the equator and the pole, for the purpose of deducing the ellipticity of the earth, all the places of observation, being on land, are more or less elevated above the sea; inland stations, in particular, are sometimes at considerable elevations: to render these results comparable one with another, It is necessary to reduce each result to what it would have been, had it been made at some level common to all the experiments; and the surface of the sea has hitherto been taken as that common level. Previous to the publication of a paper of Dr. Young's in the Philosophical Transactions for 1819, the consideration which we have mentioned, that of the attraction of the matter interposed between the place of observation and the level of the sea, was generally unheeded in estimating the allowance to be made for the reduction of different heights to the common level: in that paper, however, Dr. Young took occasion to point out the probable effect of the interposed matter in modifying considerably the usual allowance: that supposing its density to be about half the mean density of the earth, the effect of an hemispherical hill of such matter, on the summit of which the pendulum should be placed, would be to diminish the correction, deduced from the duplicate proportion from the earth's centre, about $\frac{1}{4}$th; that, in like manner, a tract of table-land, considered as an extensive flat surface, of the same relative density, would diminish the correction about $\frac{3}{8}$ths; and that, accordingly, in almost any country that could be chosen for the experiment, the proper correction for the height would vary, according to the form and density of the interposed materials, from rather more than a half to rather less than three-quarters of the usual allowance. This view has been subsequently acted upon by the English pendulum experimenters, in reducing their observations; but it has not been yet adopted by the French. The experiments of Professor Carlini were calculated to afford a practical illustration of the correctness of Dr. Young's reasoning.

Professor Carlini was engaged, in the summer of 1821, in concert with Professor Plana, in determining the amplitude of the celestial arc, between the Hospice on Mont Cenis and the Observatory at Milan, by means of fire-signals made on the Roche Melon, and observed simultaneously at Milan, and at a temporary observatory established at the Hospice. Whilst thus engaged, Professor Carlini, being stationary for several days on Mont Cenis, and obliged to have time very accurately determined, for the purpose of comparing with the observatory at Milan, availed himself of the opportunity to employ a pendulum apparatus of the same general nature as that used by M. Biot at Paris, which had been prepared at Milan some years before, under the direction of a commission of weights and measures, with the view of determining the value of the divisions of the national linear scale. As this apparatus differed in some few particulars from the original employed in France, we shall briefly notice the differences, presuming our readers to be acquainted with the apparatus of M.M. Borda and Biot.

1. In the Milan apparatus, by means of two microscopes furnished with wire micrometers, the length of the pendulum may be measured without touching it; without approaching it; without even opening the case which contains it. The measure is obtained by bringing the wires in contact with the images of the knife-edge suspension, and of the upper and lower borders alternately of the platinum disk suspended to the thread: thus preventing the risk of deranging the equilibrium, and avoiding the effect which the heat of the body might have on the very dilatable metallic thread.

2. The half sum of the distances taken between the suspension, and the upper and lower edges of the disk, gives the distance of the centre of the disk itself, without measuring its diameter with a compass, an operation exceedingly difficult to execute with the necessary precision. By this apparatus of microscopes, the length may be measured at pleasure, even during the time of oscillation; and being attached to the wall, instead of supported by the floor, the risk of derangement by the tread of the observer is avoided.

3. The pendulum, and the clock by which its oscillations are measured, were not, as usually, near together and resting on the same base, but were perfectly separated. The coincidences of the oscillations were observed, by bringing the image of the pendulum of the clock, reflected by means of an oblique mirror, in contact with the image of the simple pendulum seen direct through a telescope.

By this modification, the risk of the mutual influence of the pendulum and the clock is avoided.

4. The disk was attached to the thread by means of knots in the thread itself; avoiding the correction for the small cup usually employed for that purpose.

5. An alteration was made in the weight and shape of the knife-edge suspension; reducing its weight to about 10 grains, and giving it the shape of a rotella, instead of that of a triangular prism.

The simple pendulum and miscroscopes were attached to a strong wall, in a room on the ground floor, contiguous to the temporary observatory, and well sheltered from the sun and weather. The clock with which the pendulum was compared, was supported by a pyramid of masonry resting on the ground, and occupying the middle of the room. The experimental length between the microscopes was referred to three standard metres, in perfect agreement with each other: one received from Paris by the commission of weights and measures at Milan; a second brought more recently from Paris by Conte Moscati; and a third in the possession of the Royal Academy of Turin.

The experiments were commenced on the 3d of September, and terminated on the 27th, being interrupted by M. Carlini's absence at Chambery from the 7th to the 12th. The distance between the microscopes, and the oscillations and length of the pendulum, were measured alternately. Thirteen independent results were thus obtained, of which the greatest discordance from the mean, was not more than $\frac{13}{10000}$ths of a British inch. The mean result was 39,0992 British inches, the length of the pendulum vibrating seconds in a vacuum, at the place of observation on Mont Cenis, 1943 metres, or 6374 feet above the sea, in the latitude of 45° 14′ 10″. To compare with this determination, we may obtain a tolerably fair approximation to the pendulum at the level of the sea in the latitude of 45° 14′ 10″, such as its length might have been found, if the mountain could have been removed and the pendulum placed on its site, by deduction from the lengths actually measured with a similar apparatus, on the arc between Formentera and Dunkirk, at stations not far removed from the level of the sea, in the adjacent parallels to Mont Cenis, and in the countries adjoining. Of these there are five, not including the station at Clermont, in consequence of its great elevation: they are as follows:—

Dunkirk	51 02 10;	its pendulum at the	level of	the sea	=	39.13771
Paris	48 50 14;	”	”	”	”	39.12894
Bordeaux............	44 50 26;	”	”	”	”	39.11295
Figeac	44 36 45;	”	”	”	”	39.11212
Formentera..........	38 39 56;	”	”	”	”	39.09176

The mean length of the seconds pendulum at the level of the sea, in the latitude of 45° 14′ 10″, deduced from these determinations, is 39,1154; and it is so equally, whether an ellipticity of $\frac{1}{288}$th, or of $\frac{1}{304}$th, or any intermediate ellipticity be assumed in the reduction.

We have, then, 39.1154—31.0992=·0162 inch, as the measure of the difference in the intensity of gravitation at the place of observation, elevated 1943 metres, and at the level of the sea. The radius of the earth being 6,376,478 metres, this measure, according to the duplicate proportion of the distances from the earth's centre, should be ·0238 inch. The attraction of the mountain is, then, equal to ·0238—·0162=·0076 inch. Whence it appears that, in this particular instance, the correction for the elevation is reduced, by the attraction of the interposed matter, to $\frac{68}{100}$ths, or to about $\frac{7}{10}$ths of the amount immediately deducible from the squares of the distances.

It is obvious that, if we possessed a correct knowledge of the density and arrangement of the materials of which Mont Cenis is composed, so as to enable a computation of the sum of all the attractions which they exercise on the place of observation, this result might furnish, as well as Dr. Maskelyne's experiments on the deviation of the plumb-line produced by the attraction of Mount Schehallien, a certain determination of the mean density of the earth. Professor Carlini considers that the form of the eminence may be sufficiently represented by a segment of a sphere, a geographical mile in height, having as its base a circle of 11 miles diameter, the distance from Susa to Lanslebnrgo; the attractive force, on a point placed on the summit, would, in such case, be equal to $2\pi\delta(1-\frac{2}{3}\sqrt{\frac{1}{11}},)$ or in numbers to 5·020, δ, δ being the density of the mountain, and 2π the ratio of the circumference to radius. The attractive force of the earth, on a point at its surface is $\frac{2}{3}\pi r\Delta$, =2̇4394 Δ, r being the radius of the earth = 3437 geographical miles, and Δ its mean density. Now these two quantities, 14394 Δ and 5.020 δ, should be, to each other, in the proportion of 39.1154,—the pendulum at the level of the sea, representing gravitation at the surface of the earth,—to ·0076, the portion of gravitation at the summit of the mountain due to the attraction of the mountain. By the observations of M. de Saussure and other geologists, Mont Cenis is chiefly composed of schistus, marble, and gypsum; the specific gravities of which substances were ascertained, from numerous specimens in the possession of M. Carlini, to be respectively as follows:—

The schistus	. . .	2.81.
The marble	. . .	2.86.
The gypsum	. . .	2.32.

In the absence of a precise knowledge of the quantity and position of each of these three component parts, we may take the mean, 2.66, of their several densities as approximatively the density of the mountain, $= \delta$. We have then

$$\Delta = \frac{5.02\,\delta \times 39.1154}{14394 \times .0076} = 4.77,$$

a result little differing from that of Cavendish, as recently corrected by Dr. Hutton, and still less from that of the Schehallien experiments.

The most hypothetical element of this calculation, is the width assigned to the base of the mountain; but by the very nature of the question, it has but little influence on the final result; since, by even doubling the assigned diameter, the total attraction would not be altered a twentieth. In regard to the mean density of the mountain, if it were taken at 2.75, instead of 2.66, that of the earth would result 4.94, instead of 4.77, as given above.

III.—ON ARTIFICIAL COBWEBS FOR MICROMETERS.

By R. C. Goring, M. D.

[*From the Quarterly Journal of Science and Arts, Vol. I. N. S.*]

A common cobweb has always appeared to me to be a very strong substance in proportion to its extreme tenuity, for a bulky spider will frequently depend from a single fibre of great length;—small insects are incapable of overcoming the resistance of the meshes of a web;—which will also endure for a considerable time in wet and windy weather without requiring much repair. Nevertheless, I have always found astronomers extremely nervous about the threads of their micrometers, which, it is said, may be destroyed by the trifling current of air which passes through the tube of a telescope when its objective and eye glasses are displaced; probably because the cobwebs, being of animal origin, are subject to decomposition in course of time, for I do not think a cobweb, fresh from the body of a spider, could be so easily affected. Be this as it may. I have accidentally discovered a mode of making artificial ones, which are not subject to decomposition,—which may be stretched by mechanical violence to double their length without snapping, returning again to their original dimensions,—which may be blown upon by a powerful pair of bellows, with its nose close to them, without injury, and which can be rendered of any degree of tenuity and fineness, being a perfect substitute for the ordinary ones, as it seems to me; but as I am not a connoisseur in such matters, it must be left to Messrs. Troughton and Sims, who have certainly arrived at the extreme verge of improvement in the construction of micrometers; to determine the point at issue between me and the spider. The following is the receipt for making them:—Procure some of the purest essential oil of turpentine, have some caoutchouc extremely thin, and put it into a small wide-mouthed phial, observing not to fill it more than one quarter; pour in the turpentine to the top, and secure it carefully with a cork and bladder. Let this be worn in the pocket for a few days, and the Indian rubber will absorb the essential oil, and become greatly increased in bulk; then let a portion of it be put into another bottle with some more turpentine, and in a few days it will be completely dissolved. It is necessary that there should be more turpentine employed in the first instance than is required to reduce the India rubber to the consistence necessary to form cobwebs, in order to allow impurities to subside to the bottom of the phial, and to insure the complete solution of the caoutchouc; for if any particles of it are left untouched, smooth even cobwebs will not be obtained*. This solution must be kept about the person till a good deal of the turpentine has evaporated, to aid which process it will be advisable to have the phial merely closed by a piece of paper tied over it: the older it is the better. If any heat is employed to effect the solution, exceeding that of the human body, the consequence will be, that the caoutchouc will undergo a change, and will never dry or return to its original state. When the India rubber in the phial has assumed a viscid consistence, like that of bird-lime, it is in a fit state for making cobwebs: the precise condition in which it is most fit for that purpose may be known by experiment, and by its sticking two other pieces of India rubber together in such a manner, that they cannot be separated when the interposed cement is dry.

The making of the cobwebs is the most simple affair imaginable; all that is necessary is to take a small quantity of the solution on the point of a bit of wood, and to stick it to a frame, producing the viscid thread, which will proceed from it to the opposite side: in this way any quantity may be made. This process should be carried on in a very warm room, otherwise the liquid thread is apt to snap. The fibres thus prepared should be carefully preserved from dust for a day or two, till completely dry; they may then be examined by a microscope, and the most even and parallel selected. In this way we obtain artificial cobwebs, having the same opacity, and the same power of enduring the solar spectrum as common ones, and which appear to me to bear as much rough usage, damp, &c. as can reasonably be desired in such delicate fibres; for when the turpentine has evaporated, they are left neither more nor less *than unchanged caoutchouc.*

IV.—MISCELLANEOUS NOTICES.

[*From the Quarterly Journal of Science and Arts, Vol. I. and II. N. S.*]

1. *Peculiar Formation of Nitre.*—The leaves and stems of beet-root contain oxalate and malate of potash. Some leaves were tied together, and hung up in a warm and slightly-humid place, where there was but little light, to dry. Being examined at the end of several months, they were found penetrated with and covered by, an immense number of minute crystals of nitre. The oxalic and malic acids had been replaced by nitric acid; but whether from animalized matter naturally in the leaves of the plant, or from the action of the air, or in what manner, is not known.—M. Henri Braconnot, *Ann. de Chimie*, xxxv. 260.

2. *On the Existence of Crystals of Oxalate of Lime in Plants.*—M. Raspail has read a memoir to the Academy of Sciences, to prove the analogy which exists in arrangement between the crystals of silica, which are found in sponges, and those of oxalate of lime, occurring in the tissue of phanerogamous plants.

The latter crystals were observed, for the first time, by Rafan and Jurine, who regarded them as organs of which they knew not the use. They were then observed by M. de Candolle, who called them *raphides*, and gave a figure of them, which, however, is inaccurate. These crystals are really very regular tetraedrons. In many plants, as *orchis, pandanus, ornithogalum, jacinthus, phytolaca decandria, mesembryanthemum deltoides*, &c. they are very small, not being more than $\frac{1}{20}$ of a millimetre (.0002 of an inch) in width, and $\frac{1}{10}$ (.004 of an inch) in length. But,

* There will always be the same difficulty in obtaining truly parallel and even threads from the caoutchouc as there is from common cobwebs. I have one of Messrs. Troughton and Sims's best micrometers by me at this moment: and the cobwebs, though of course picked, and abundantly even enough for practical purposes, are certainly far from being of the same diameter throughout. I suspect, from microscopical examination, that a common cobweb is a flat thread like a piece of tape, not a round one; so that, if it is at all twisted, it must present different apparent diameters to the eye. The best way of getting parallel threads with the India rubber paste, appears to me to be, to let it stand for a considerable time without any agitation or stirring up whatever, and when the instrument is inserted to draw out a thread, to disturb the matter as little as possible, touching only its surface; the more the thread is drawn out, the more even and true it becomes. It may not be irrelevant to mention, that in order to give the fibres their utmost degree of strength, they should be put on in the lowest degree of tension possible: in drawing them out on the frame, therefore, let the instrument recoil as much as it will before the end of the thread is fastened down, that at least the caoutchouc may not be stretched till its ultimate attachment to the micrometer is completed. Rectified white naphtha may be tried instead of the turpentine.

in the tubercles of the Florence iris, they are as much as $\frac{1}{50}$ (.0008 of an inch) in width, and $\frac{1}{3}$ (.01312 of an inch) in length, so as to be easily capable of examination.—*Bull. Univ.* B. xi. 376.

3. *Method of increasing the Odour of Roses.*—For this purpose, according to the author of the method, a large onion is to be planted by the side of the rose tree, in such a manner that it shall touch the foot of the latter. The roses which will be produced will have an odour much stronger and more agreeable than such as have not been thus treated, and the water distilled from these roses is equally superior to that prepared by means of ordinary rose leaves.—*Œkonom. Neuigk ;—Bull. Univ.*

4. *Quick Method of putting Insects to Death.*—The following method is by M. Ricord, for the use of naturalists. The insect is to be fixed on a piece of cork, and put under a jar or vessel with a little ether; the latter being placed either in a capsule, or on the plate on which the jar or glass is placed: the vessel should apply closely, that the vapour of the ether may be retained, and the air within be prevented from changing its place. The insect thus immersed in the ethereal atmosphere will soon die, without having time to hurt its form or appearance by violence. —*Bull. Univ.* B. xii. 295.

5. *Chloride of Lime in cases of Burns.*—The good effect of chloride of lime in cases of burns is confirmed by the experience of M. Lisfranc. He has applied it in many cases of that kind, sometimes immediately after the accident, sometimes after the application of emollient cataplasms. Lint is moistened in a solution more or less strong of chloride of lime, and then applied to the place, being covered over with waxed cloth. The cure has been singularly hastened under its influence; and in one case, where almost the whole of the lower limbs, the arms and face, had been burnt, the use of the chloride recovered the patient from the stupor into which he had fallen at the end of four days, and a perfect recovery was effected two months after the accident.—*Bull. Univ.* C. xi. 77.

6. *Paper to resist Humidity.*—This process, which is due to M. Engle, consists in plunging unsized paper once or twice into a clear solution of mastic in oil of turpentine, and drying it by a gentle heat. The paper, without becoming transparent, has all the properties of writing paper, and may be used for the same purposes. It is especially recommended for passports, workmen's books, legal papers, &c. When preserved for years, it is free from injury, either by humidity, mice, or insects. It is further added, that a solution of caoutchouc will produce even a still better effect.—*Kunst und Gewerbe-blatte.*

7. *Cementation of Iron by Cast Iron.*—Pure iron, when surrounded by and in contact with cast-iron turnings, and heated, is carbonised very rapidly, so as to harden, to temper, and, in fact, to exhibit all the properties of steel. M. Gautier finds this is a very advantageous process in numerous cases, especially where the articles to be case-hardened, or converted into steel, are small, as iron wire, or wire gauze. The temperature required is not so high as that necessary in the ordinary process of cementation, and the pieces to be carbonised are not injured in form. The kind of cast iron used should be the gray metal, and the more minutely it is divided the more rapid and complete is the operation. By covering the mass of cast metal, in which the iron to be carbonised is enveloped, with sand, oxidation, from contact of the air, is prevented, and the cast metal may be used many times. Plumbago, experimented with in the same manner, does not produce the effect.—*Jour. de Pharmacie*, 1827, p. 18.

8. *Test of the Presence of Opium.*—Dr. Hare says he can detect opium in solution, when the quantity is not more than that given, by adding ten drops of laudanum to half a gallon of water. The following is the process:—A few drops of solution of acetate of lead is to be added to the solution containing the drug; after some time an observable quantity of meconiate of lead will fall down: from six to twelve hours may sometimes be required, and the precipitation is best effected in a conical glass vessel; for then, by gentle stirring now and then to liberate that which adheres to the side, the insoluble salt may be collected together at the bottom. About thirty drops of sulphuric acid are then to be poured on to the meconiate by means of a glass tube, after which as much of a solution of red sulphate of iron is to be added in the same manner. The sulphuric acid will liberate the meconic acid, and thus enable it to produce with the iron the appropriate colour, which demonstrates the presence of that acid, and consequently of opium.—*Silliman's Journal*, xii. 290.

Original Communications.

I.—*Experiments on Indigo.*

Chemists in Europe, who have engaged in the examination of Indigo, have generally had to deal with the prepared colouring matter as manufactured for the market; and have therefore limited themselves to the separation and measurement of the foreign ingredients with which it was contaminated,—to the properties of the pure colouring matter itself,—and to the analysis of its chemical composition. The rationale of what passes in the process of the manufacture may be, and has been, deduced with tolerable certainty from the discoveries thus made; but where we have the whole fermentation carried on among us on an immense scale,—when we have the Indigo in its nascent state, and in its colourless soluble state too, capable of being submitted to tests and processes, it becomes a matter of curious interest to follow the changes of this singular substance, and compare them with the theories formed in the laboratories at home.

It is however more than a mere matter of curiosity to set on a sound basis the causes of the different manipulations, and to examine the real effect of processes in which different manufacturers have a diversity of practice;—although it must be confessed that, setting aside the difference of quality in the plant, from season, care in its cultivation, soil, and other causes, the business of the vats is so simple as to allow of little deviation in practice or result.

The most convenient manner of bringing before the reader the various subjects of a short series of experiments which I made this season, in conjunction with an intelligent friend engaged in an Indigo establishment, will be to incorporate them with a relation of the general process of manufacture; but I think it will save a good deal of repetition and explanation, to give in the first place an epitome of what has already been written on the subject; that is, on the qualities and properties of Indigo itself, which have been elaborately examined by Bergman, Berthollet, Chevreul, Thompson, Crum, and latterly by Berzelius. Their notices extend to minute details of the action of every neutral salt, acid, and alkali of the chemical calendar; but the general results which are likely to be useful to the manufacturer may be condensed into a very small space.

Indigo is a definite vegetable product which appears to exist in greater or less quantity in a variety of plants, or rather the elements of which it is composed are found in these plants; for it is not until the juices of the vegetable begin to act upon one another in fermentation, that Indigo is developed. It might at first be supposed that the green colour of plants was connected with the presence of Indigo; but this is not the case. as after the leaves have been steeped in the vats, they retain entirely their original colour.

When first dissolved from the plant, the Indigo is in a colourless state, and is readily soluble in water; but it becomes blue on absorbing oxygen from the air, and appears then to have assumed the nature of a peroxide, for it is very unalterable, quite insoluble in water, alcohol, ether, saline infusions. alkalies, and dilute acids. Concentrated sulphuric acid alone acts as a solvent, without changing its nature. Nitric acid converts it into a yellow bitter principle. Acetic acid by degrees deoxydizes it. Chlorine also destroys its colour immediately.

In saying that concentrated sulphuric acid effects no change upon Indigo, I am not strictly correct. It does not destroy the colouring matter, or cause a decomposition, as would be the case with most other vegetable compounds; but the researches of Crum and Berzelius have rendered it probable, that three different modifications of Indigo may be brought about by the agency of this acid, differing from one another in the quality of oxygen or of water in their composition.

The three modificatons are thus distinguished:—

1. The *Pure Indigo*, which is obtained by sublimation from the crude cakes at a temperature of 550°. This is crystallized in long flat prismatic needles; has a copper colour by reflected, and a fine blue by transmitted light. It sublimes entirely at the temperature stated, without residue: its specific gravity is 1.35.

2. *Cerulin, or Saxon Blue*, is obtained by digesting the former substance in sulphuric acid: it is supposed by Crum to have lost its water of composition thereby. It is soluble in water, but is precipitated by most of the neutral salts.

3. *Phenicin*, or *Purple Indigo*. is obtained by suddenly diluting the sulphuric acid which has begun to dissolve Indigo. It separates as an insoluble powder, which when filtered and washed, is soluble in pure boiling water, and may be procured as a precipitate again by the addition of any neutral salt. Phenicin is supposed by Berzelius to be an intermediate state between soluble and insoluble Indigo; but Crum asserts that 100 parts of Indigo will yield 120 of Phenicin.

The Indigo of commerce most probably ranks under one of the two latter denominations, or perhaps both of them; for there can be little doubt that some new combination of elements takes place in the sublimation of the "pure" or "crystallized Indigo," since, with the utmost care, not more than one-fifth of the weight of crystals can be procured; and, during their formation, first, aqueous vapour, then gas is extricated; and a red coloured oil; and a large residue of charcoal is left behind; whereas the crystals, once formed, are volatilizable without any loss or carbonaceous residue. As the crystallized state, and the resistance to destructive agencies at an elevated temperature, are the sure signs of a definite, and generally of a simple atomic composition, we should expect to find tolerable accordance in the analyses of the crystals of Indigo. I have only been able to find two made by different chemists, but they are greatly at variance with one another*.

Pure Indigo, analyzed by Thompson		by Crum.
Consists of Oxygen,	46.154	12.60
Carbon,	40.384	73.22
Azote,	13.462	11.26
Hydrogen,	——	2.92
	100.	100.

The other varieties are stated by Crum to be thus composed:—

	Cerulin.	Phenicin.
Oxygen,	29.0	21.6
Carbon,	57.8	64.9
Azote,	8.4	9.5
Hydrogen,	4.8	4.0
	100.	100.

If the composition given by Thompson be correct, Indigo ought to be a remarkably easy substance to analyze; since it would merely be necessary to submit it to a destructive heat,—to determine the proportion of carbonic acid, carbonic oxyde, and azote,—and to weigh the surplus carbon. The quantity of oxygen in the analyses of Mr. Crum will be proved hereafter, I think, to be too small.

From the almost total insolubility of pure Indigo, it becomes a very simple matter to separate the foreign ingredients which are found with it, and consequently to obtain it in a state of purity. All that is necessary is to boil it for a sufficient time.

First. In pure water; which removes yellow extract, green matter, &c.

Secondly. In alcohol, which carries off red colouring matter, and resin.

Thirdly. In dilute muriatic acid, which takes up lime, oxyde of iron, and magnesia, &c.

These methods will not, however, remove sand and clay, alumine or gypsum, the presence of which can only be detected by burning a portion of the blue cake.

The quality of Indigo seems materially to have improved of late years, for Chevreul only obtained 45 per cent. of true colouring matter from Guatimala Indigo,—Bergman found 47,—Brande states it at 50 per cent.; whereas in two specimens which I analyzed in Calcutta, one contained 75, the other nearly 80 per cent. of pure blue; neither were these the finest produce of the market.

Deoxydizing substances, such as the sulphurets, protoxyde of iron, phosphorus, the sulphites, &c. have the power of depriving Indigo of a part of its oxygen, whence it becomes again soluble in water or alkaline leys, preserving the power of regaining its colour the moment it afterwards meets with oxygen. It is thus that the dyers are enabled to prepare a solution for the purposes of their profession. They are said to make use generally of sulphate of iron and slaked lime, which are mixed up intimately with the Indigo, in the proportions of two of the

* Dr. Ure gives in the appendix to the 2d edition of his Dictionary, Ox. 14.3, Carb. 71.4, Az. 10, Hydr. 4.4. But he thinks these numbers may require a little alteration.—Ed.

sulphate, 50 of lime water, and 1½ parts of Indigo, and then boiling the mixture in water. The colour of the solution is yellow.

Indigo thus deprived of its oxgyen has been called Indigogene by Liebig:—Berzelius calls it "reduced Indigo." It is said to be obtained with facility in precipitation from the dyer's solution by muriatic or acetic acid, to which a small addition of sulphite of ammonia must be made, to prevent the access of oxygen.

Liebig asserts that this Indigogene, at a moderate temperature, absorbs oxygen suddenly from the air with a species of combustion, and that in the mercurial eudiometer the absorption is found to amount to 11½ per cent. of its weight. The colour changes simultaneously from white to a rich purple.

Indigogene is soluble in the caustic alkalies and lime water, in which it may be kept without alteration for any period, provided air be entirely excluded. The solution in potash forms an excellent eudiometrical liquid, as it absorbs the oxygen of the air with great avidity, without giving out any gas to complicate the result. I had occasion to remark this circumstance in the course of the experiments hereafter described; and as the solution may be prepared direct from the manufacturer's vats in any quantity, it may prove a valuable discovery in the laboratory as a useful substitute for Sir Humphrey Davy's eudiometrical liquid, composed of green sulphate of iron saturated with nitrous gas, which is difficult to preserve, and may give out a little nitrogen in its operation. It remains, however, to ascertain how long the alkaline solution of Indigogene will keep unaltered.

Having thus briefly enumerated some of the principal properties of Indigo, as a substance sui generis, (and there appears to be no other vegetable product which resembles it in containing so much oxygen without being acid, and in the absence of hydrogen, and the presence of azote) I shall proceed to the experiments on the process of manufacture which form the immediate subject of this paper.

Manufacture.

"The plant, after being cut and carried to the factory, is thrown into the steeper or superior vat, where it is pressed with timbers adapted to the walls of the vat, to prevent its rising in the water, which is then filled in from a reservoir, so as completely to cover the plant."

During the fermentation which follows, bubbles of gas rise to the surface, to ascertain the nature of which our first attention was directed.

The bubbles collected from the vats were found to contain merely 7 or 8 percent. of carbonic acid; the remainder being common air, with from 12 to 18, instead of 21, per cent. of oxygen.—Earthen vessels were inverted, and left with their mouth immersed all night in the vats; but the air in them was found unchanged. When bottles were partially filled with the liquor of the vat, and well closed, the air, after a day, was always found in them contaminated with about 18 per cent. of carbonic acid,—the rest being common air, without diminution of oxygen, excepting that portion due to the original air now replaced by the carbonic acid gas.

By way of examining in a more unexceptionable manner, the gas given out during fermentation, the operation was conducted on a small scale, by steeping some of the leaves in a glass cylindric vessel furnished with stopcocks and tubes to convey the gas, which should be emitted, into a glass receiver.

After 24 hours, (for the process of fermentation does not proceed so rapidly as in a large vat,) the quantity of air given out by 12 sicca weight (=2160 grs.) of leaves, was 26.1 cubic inches: the disengagement still went on, but very slowly.

The gas was analyzed at two different periods: towards the middle of the disengagement it was found to be composed of

Carbonic Acid,	27.5
Oxygen,	5.8
Azote,	66.7
	100

And at the conclusion, it contained:

Carbonic Acid,	40.5
Oxygen,	4.5
Azote,	55.0
	100

Probably the atmospherical air of the apparatus, as well as that contained in the water, and that which remained entangled among the leaves, may account in some measure for the presence of the oxygen and azote in the first analysis; but the large proportion of azote, especially in the second experiment, so much surpasses what might be expected from this source alone, that it seems necessary to attribute it, in part at least, to an emission from the leaves during the fermentation; or probably the leaves, still retaining atmospheric air on their absorbent surfaces, convert the oxygen thereof into carbonic acid, and allow the azote to escape. After the disengagement has ceased, it will be seen by the next experiment that the proportion of azote decreases very much. Some of the fermented liquid of a large vat was well boiled, and the gas collected over water: on analysis, it proved to be composed of

Carbonic Acid,.......... ...	78.
Oxygen, ...	2.3
Azote, ...	19.7
	100.

And in a second experiment, conducted with greater care to exclude external air, the result was 86 per cent. of carbonic acid, and the residue contained too feeble a portion of oxygen to explode with the electric spark on the addition of a requisite proportion of hydrogen.

In no case, therefore, does the extricated gas in practice appear to be pure carbonic acid; but from the prevalence of the latter increasing with the precautions taken to exclude common air, it may in fact be the only gas strictly due to the fermentation properly so called, the remainder of common air having been suspended in contact with the leaves, and being deprived of a portion of its oxygen by the free carbon, or by the liquid in its passage to the surface. No carburetted hydrogen or other combustible gas was found among the gaseous products

The Indigo manufacturer does not wait until the extrication of gas is concluded, but withdraws the liquid from the steeping vat as soon as he considers it to be sufficiently fermented; judging either from the smell, from the greenish tint of the liquor on the surface, or from the formation of an irridescent scum on the bubbles of gas. In fact when the liquid, which is of itself of a bright yellow colour, begins to assume a greenish tint, it is evidently caused by an incipient precipitation of the blue colouring matter; and it would be attended with a loss of produce, to allow this precipitation to take place in the steeping vat among the leaves and branches of the plant.

" The length of the fermentation depends upon the temperature, the weather, the wind, the water employed, and the ripeness of the plant; it may last in common cases from seven to fifteen hours. It is generally longer when the temperature is high, the weather cloudy, but not rainy; the wind eastward and moderate, the plant ripe and fresh."

Upon these several points, constant experience leaves little for the experimentalist to advance; but it may be remarked, that almost all the criteria of a good fermentation, as far as the weather is concerned, concur in one simple principle,—the prevention of the water of the vat from being cooled; for the *west wind*, being dry, cools it by evaporation;—strong wind does the same;—clear sky cools it by radiation; and rain, by the low temparature of rain water. Thermometers placed in the vats did not present any satisfactory results; the mean temperature was about 85 degrees Farenheit.

" The fermented liquid is drained off into inferior vats, which are called the "beating vats."

At the time when the vat is opened, the liquid is found by experiment to have a specific gravity (at the surface) of 1001.5: and at the bottom of the vat, 1003.1. The leaves appear to have lost nothing, being as green and fresh as when they were first strewed in the vat. By carefully weighing a portion of leaves, however, before immersion, and washing and drying them in the air after it, they were found to have lost more than three quarters of their weight:—of this loss the greater part was water, which they apparently cease to have the power of retaining when the soluble juices have been withdrawn. The solid matter taken up by the vat amounts to between 12 and 14 per cent. fo the weight of the leaves.

When the fermented liquid runs off into the lower vat, a frothy extrication of gas covers the whole of its surface. It is a good sign if the froth, in subsiding, assumes a rosy tint, which is in fact nothing more than a very thin film of Indigo, and it proves that the deposition is ready to take place.

" In this vat the liquid undergoes a beating for about two hours—it is continually stirred about and agitated by a number of men, either with their arms or with a sort of short oar "

The object of this operation appears to be threefold. In the first place, the agitation extricates a large proportion of the carbonic acid gas which still remains combined with the liquid :—in the second, it exposes fresh surfaces continually to the contact of the air, whence the oxygen is rapidly seized by the nascent Indigogene :—and thirdly, it coagulates the feculæ of the Indigo in larger grains, so as to render it more easily precipitable.

By way of understanding more clearly what takes place in the beating vat, a number of bottles were at different times carefully filled with the yellow liquor, just as it was ready to be drawn off from the upper vat, for experiments in the laboratory.

Neither keeping, boiling, the addition of acids or alkalies, nor even putrifaction, appeared materially to affect the power of depositing Indigo,—the solution always became blue the moment it came in contact with oxygen.

It may be remarked, however, generally, that the longer the liquid had been kept, the less rapid and determinate was the deposition :—the feculæ remained in part suspended in the liquid, giving it a green hue ; but in time it invariably subsided, and the quantity appeared to be the same in all cases.

It is sometimes customary in the beating vat, when the precipitation does not proceed with vigour, to throw into the vat a little lime water, or some other precipitant, to assist the Indigo in subsiding ; the effect of such additions was tried on a small scale, taking care to exclude the air during the immediate application of the reagent.

The acids and carbonated alkalies caused an immediate extrication of carbonic acid gas from the liquid, but produced no precipitate.

The caustic alkalies and lime, on the contrary, produced a copious deposit, unattended with effervescence. The colour of the deposit was yellowish white, if the air was quite excluded ; but it became green and blue, upon the slightest contact with oxygen. Careful experiments, however, proved that the blue colour was only produced by the Indigo attaching itself to the precipitate ; for all of the Indigogene, or vegetable matter convertible into Indigo, remained suspended in the supernatant alkaline liquid. The precipitate was composed of a yellow extractive matter, to which I shall again advert presently.

The measurement of the absorption of oxygen during the transition from the colourless to the blue state, was the next object of experiment. It was easily put beyond a doubt, that such an absorption took place ; but several trials to measure it failed, on account of the extrication of carbonic acid gas, which was always much greater than the oxygen absorbed.

I thought that boiling would have driven off all the carbonic acid ; but I was astonished, on filling an eudiometer bottle with liquid which had been entirely purged of all its free gas by ebullition, to perceive that the moment the tube containing 100 measures of oxygen was connected with it, a brisk emission of carbonic acid (about 50 measures) took place, and confused the results.

At length, by adding a little caustic potash to absorb the carbonic acid which might be generated, the diminution of oxygen became apparent, and in a few minutes all the oxygen contained in the eudiometer tube disappeared, the tube being too small to supply enough for saturation.

As the employment of potash was on some accounts objectionable, I also tried, and succeeded in another method of obviating the presence of carbonic acid.

In a glass balloon, furnished with a stopcock, a vacuum was made, into which the liquid was suddenly introduced by a connecting tube. By this means a great deal of gas was separated, and, by repeating the action of the airpump, much of what remained was withdrawn.

To the vacuum above the liquid, pure oxygen was admitted from a mercurial gasometer, its quantity being measured ; and the air in the balloon was further analyzed by withdrawing a small portion. The balloon was then agitated, and water from time to time admitted to replace the oxygen which had been absorbed. The residual gas was again analysed, to find whether the carbonic acid or azote had altered in quantity during the experiment, and the weight of the oxygen absorbed was ascertained from that of the water which had entered the balloon.

Four experiments conducted in this manner, yielded the following results.

	1 Expt.	2 Expt.	3 Expt.	4 Expt.
Weight of liquid grains,	8455	5500	5166	6478
Weight of oxygen absorbed,	2.00	0.82	0.84	1.22
Weight of Indigo collected,	6.8	2.9	3.9	4 9
Proportion of oxygen in the Indigo per cent.	29.4	28.2	21.5	25.0

Making an average of 26 per cent. of oxygen absorbed ; a quantity nearly double what Crum supposes to exist in pure Indigo, and an average between that which he ascribes to Cerulin and Phenicin. It is, however, little more than the half of what Thompson states in his analysis.

There is hardly any occasion to seek a nearer concurrence between experiments made in the manner just described on the liquid of the vats ; because the Indigo forms but a very small proportion of the matter held in solution therein, and it is very possible that the other ingredients may also absorb oxygen, especially when the putrid fermentation commences.

It is evident, however, that a further analysis of pure Indigo in the dry way is a very desirable object. It seems to have escaped the attention of Gay-Lussac and Thenard, and latterly of Marcet and of Dr. Prout, in their Researches upon the Constituents of Vegetable Compounds.

It is worth while in this place to mention a fact observed in the course of these experiments, namely, that where a slight excess of potash is mixed with the vat liquid, the Indigo formed remains in solution, and passes through the filter with ease, leaving the precipitated extractive matter behind. This blue solution will keep for any time, and does not deposit its Indigo even in the open air ; but as soon as the alkali is saturated by an acid, it immediately precipitates, leaving the liquid colourless. If the dose of potash be not sufficient to throw down all the yellow matter, the solution retains a green colour, and cloth dipped into it takes a green dye. When hung up to dry, however, the mixed action becomes evident ; for the dye remains fixed in the lower part of the cloth, while the yellow, more perfectly dissolved, spreads with the liquid to the upper part which was not dyed. There is no reason to believe in this case, that the air changed the colour from green to blue, although such may be true of the dyer's vat-liquid.

The quantity of Indigo deposited *per se* from 1000 parts of yellow liquid of the specific gravity 1003.1, varied but little, and may be estimated at 0.75 parts. In practice, a vat of 637 cubic feet is considered to yield a good produce at 16 seers, which is as nearly as possible 0.75 per 1000 of liquid. The common produce of the vats in this part of the country does not exceed 0.5, or one five thousandth of their weight of Indigo.

But when potash, lime, or other precipitants are employed, the weight of the Indigo is much increased ; not, as experiment proves, from an increase of the actual produce, nor from any union of the precipitant with the Indigo, but from its causing the deposition of another matter, to which I have given the name of yellow or brown extract.

To obtain this yellow extract in an insulated state, 10,000 grains of mother liquid were evaporated to dryness : a solid residue was in one case collected, weighing 47 grains. With another liquor it was 20 grains heavier ; and in the experiment made on a small scale with the leaves, where the fermentation continued for 24 hours, the same weight of mother liquor yielded 246 grains of residue. As this element of the plant seems to be so variable, and as it must evidently produce much influence in the manufacture, it is probably one of the chief causes of the nicety usual in timing the fermentation, and of the variable tendency of the Indigo to precipitate in the beating vat. The dried extract has a dark brown colour and vitreous surface similar to that of dried gluten, or the brown extract of toast-water : it has a peculiar, not unpleasant smell, and rather a bitter taste : it deliquesces in a damp atmosphere, and dissolves in water, which it dyes of a deep brown or bistre. Although the original mother liquor is exceedingly liable to ferment and become putrid, the brown matter undergoes no change, either when kept dry, or in a moistened state.

It is precipitated from its aqueous solution by potash, soda, ammonia, lime, and their carbonates; by infusion of galls, acetate of lead, and nitrate of silver. The acids, and prussiate of potash, did not affect it; but the action of reagents was not investigated in detail.

Sufficient has been adduced to prove, that whenever lime or the alkalies are used in the vat, the Indigo must be adulterated more or less with this substance; and I suspect that the brown and green matters separated by Chevreul and others in their analyses of Indigo, are attributable to this source. In an experimental way, Indigo of a true dark green colour was easily collected, which weighed more than twice as much as the blue colour alone: it was also harder and more compact, and more liable to shrink and crack than the pure Indigo cake; for besides the impurity just described, there was always found a large proportion of earthy residue, on burning Indigo, where a precipitant was used, than where it was not; the quantity was even as much as double or triple in amount.

Carbonate of lime, alumine, and oxyde of iron, are the chief ingredients of the earthy residue, and I have seen them vary from five to nearly 50 per cent. of the Indigo: the last, as may be supposed, was merely refuse, and quite unsaleable. Carbonate of lime is seldom absent, for whatever lime may be in the plant or in the water is sure to be precipitated by the carbonic acid developed during the fermentation. Some manufacturers are in the habit of employing rain water purposely to avoid this source of adulteration, but it is doubtful whether the plant will not itself bring a portion of earths into solution.

Upon the whole, it may be safely decided, that the purer the water is, and the cleaner and the more simply all the operations of the manufacture are conducted, the more beautiful and rich in colour will be the Indigo: and it should be the first maxim of the planter, that it is purity, and not weight, which gives the value to his produce in the market.

In conclusion of the present desultory notice, which will I hope at least serve the purpose of leading others to bestow further attention upon the subject, I annex an analysis of a specimen of Indigo, denominated fine blue in the Calcutta market, made in the year 1820, to which I have referred in a former part of this paper.

Analysis of Calcutta Indigo.

100 grains heated white in a closed platina crucible left a porous grey carbonaceous mass, with metallic lustre, weighing	49.0
Burnt with access of air, the 49 grains were reduced to	7.42

1.—*Examination of earthy Residue*, 7.42.

1. Boiled dry in nitric acid, and then digested in muriatic acid, a brown residue of oxyde of iron and alumine remained	2.7	
2. From the solution, ammonia threw down alumine	0.75	
—— ———— Oxalate of ammonia—lime equal to	0.9	
3. The clear liquor evaporated left red oxyde of iron	3.05	
		7.4

2.—*In the humid way.*

1. 100 Grains of Indigo digested in boiling water, some green and dark brown matter was dissolved, which when dry weighed	1.6
2. Alcohol boiled over the remainder became of a bright claret colour, and yielded on evaporation a dark brown matter and a little yellow resin, weighing	2.0
3. Dilute muriatic acid then took up a mixture of greenish vegetable matter, and earths which were afterwards separated by burning. The green matter was thus found,	7.2
4. The Indigo now deemed pure weighed only	79.5

This sample of Indigo therefore was of the following composition.

Oxyde of iron,	5.75
Alumine,	0.75
Lime,	0.90
Green vegetable matter,	8 80
Red or brown ditto,	2 00
Pure Indigo,	79.50
Loss,	2.30
	100.00

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II.—PROCEEDINGS OF SOCIETIES.

I. Asiatic Society.

A meeting of this Society was held on Wednesday, 12th November. The Hon. Sir C. Metcalfe, Bart. in the chair.

Captain Troyer and Mr. Hamilton were elected Members, and M. DeBlossville and Mr. Græme, Honorary Members.

The following presents were made to the Museum and Library.

A Statue of Devi from Kumaoon, by Captain Vanzetti. The skin of an Emu. The tail and feathers of the Manula Superba, and a piece of net, of native fabric, from New South Wales, by Mr. R. M. Martin. For the Library: A new edition of the Chrestomathie Arabe, and three other pamphlets, by the Baron Sylvestre De Sacy. Several Statistical works, by Mons. Cesar Moreau. A memoir on the Sources of the Irrawadi and Brahmapútra rivers, by Mons. Klaproth. Exposure of a fictitious traoslation of Confucius, by Schotts, from the pen of Lauterbach. Transactions of the Royal Asiatic Society, Parts 1 and 2, and Reports, &c. by the Society. Parts 1 and 2 of the 7th volume of the Proceedings of the Horticultural Society. Journal Asiatique, for February, March, and April, by the Asiatic Society of Paris. Annual Report of the Proceedings of the Yorkshire Philosophical Society. Reports of the prevailiog Diseases of the Europeans serving under the Madras Presidency, published by order of the Medical Board. Recueil des Voyages, by the Societè de Geographie of Paris. Monumens Celtiques, by Dr. Burlini. The Meteorological Register for September was presented by Lieut.-Col. Hodgson.

The Fac-similes of two inscriptions found in Malwa, with an account of the place where they were discovered, by Captain Franklin, were laid before the Meeting, and some observations upon them, by the Secretary.

Some Remarks on the climate of various parts of the Himmalaya, by Captain Gerard, were referred to the Committee of Natural History and Science.

The Secretary laid before the Meeting, Reports on the Nilgherry Mountains, derived from the records of the Medical Board at Madras, with Observations on their Meteorological features, communicated by Government.

A Sketch of the Aborigines of New Holland, by Mr. R. M. Martin, was communicated through Dr. Adam.

Committee of Natural History and Science.

In consequence of resolutions passed at a special meeting of some of the members of the Asiatic Society, confirmed by a subsequent general meeting which took place 2d January 1828, the Committee, so often organized for the particular cultivation and encouragement of scientific enquiries, once more resumed its meetings; and it is trusted, under such favourable circumstances as leaves little chance of their being again discontinued. We regret we have not room for the resolutions in the present number, but we shall seize the first opportunity of giving them a place in our pages. Consideriog one of the principal objects of our work to be, in the full extent of our small power, to aid and assist the meritorious efforts made by the gentlemen who originated this measure, we are desirous, particularly as the period does not extend far back, to give a connected, but brief abstract of the proceedings from the date above mentioned. Snch a detail will not be without its interest to our brethren of the West: it will at least serve to show what is the course and progress of Indian science, and what is the encouragement given to so rational a project for the extension of its boundaries.

At the general meeting above mentioned, the following papers were transferred to the charge of the committee.

1. Report of the Mineralogical Survey of that part of the Himmalaya Mountains lying between the Satlaj and Kali rivers, by Captain Herbert, Superintendent.
2. Report of the Proceedings of the Zoologist and Botanist to the Survey, by Dr. G. Govan.
3. Barometrical Measurements, from Bangalore to Cape Comorin, by Captain Cullen.
4. Account of the Waterfalls of Lemineeah, by Col. Boyle.
5. Geology of Bandilkhand and Jabalpur, by Dr. Adam.
6. Remarks on the Dugong, by Dr. R. Tytler.
7. Account of the Gold Ore of the Mysore, by Col. Warren.

8. Account of the Saltpetre Works in Java, by Col. Mackenzie.
9. Three Zoological Articles, by M. Duvaucel.
10. Account of the Hot Spring at Mungher, by Captain Herbert.
11. On the Diamond Mines and Geology of Panah, by Mr. E. Stirling.
12. On the Hot Spring at Samarang, by Mr. J. Palmer.
13. Sundry Metereological Journals, by Lieut. Gerard.
14 Metereological Journal, kept at Singapúr, from 1820 to 1825, inclusive, by Captain Davis.
15. Metereological Observations made at Ságar in 1819, by Col. Thomas.

And the following Office bearers were elected.

Hon. Sir E. RYAN, President.
JAMES CALDER, Esq. Vice-President.
CAPT. JENKINS, Secretary.

D. Ross, Esq was requested to officiate as Secretary during the indisposition of Captain Jenkins.

On Monday, 11th February, 1828, the Committee held their first meeting.—Sir E. Ryan in the chair.

The resolutions, as confirmed at the general meeting of the Society, were read; and some private business being gone through, there were laid before the Committee,

A bottle of water from the Hot Springs at the foot of the Altnam Hills, presented by Mr. Avdall, in the name of Captain Bruce.

A series of Rock Specimens, illustrative of the Geology of the districts of Rajpur, Masúri in the Dehra Doon, &c. presented by Dr. J. Adam, in the name of Dr. Royle.

The following papers were also presented. A summary view of the progress and present state of Geology in India, by J. Calder, Esq. V. P. C. Geological description of a portion of Bandilkhand, Bágilkhand, and the districts of Ságar and Jabalpúr, illustrated by a map, section, and an extensive series of specimens. This paper was read by Captain J. Franklin, M. A. S.

D. Ross, Esq. was elected a joint Secretary.

Thanks were voted to the several contributors, and the meeting adjourned to Wednesday, February 27th, 1828.

Wednesday, February 27th, 1828.
Hon. Sir E. Ryan in the chair.

Amongst the visitors at this meeting were eight Hindoo youths of, or late of, the Anglo-Indian College.

A. Stirling, Esq. was elected a Vice-President, and Mr. Piddington Corresponding Member.

A circular letter, requesting contributions on geological subjects, with specimens, was ordered to be addressed to all those likely to co-operate with the Committee.

There were then laid before the Committee—

A series of Specimens from Ságar and the adjoining districts, by Captain Jenkins, in the name of Captain Coulthard, B. A.

A paper on certain peculiar formations of Stratification near Biána, illustrated by a rough section, by J. Calder, Esq in the name of Mr. J. Hardie.

This paper was read.

Read also,

Notice of the Hot Spring at the foot of the Altnam Hills, in the province of Martaban, by N. Wallich, Esq.

Notice respecting the calcareous deposit found about the hot spring in Bencoolen, with a specimen, by Mr. Wind.

Thanks were voted, and the meeting adjourned to Wednesday, March 19th, 1828.

Wednesday, 19th March, 1828.
Hon. Sir E. Ryan in the chair.

A series of Meteorological Journals, by Lieut. P. Gerard, and the continuation of Dr. Govan's report respecting the products of the Himmalaya, were received from the general Secretary, and transferred to the records of the Committee.

Some specimens of minerals from Persia, the islands and coast of the Gulf of Persia, and from the coast of Tenasserim, were presented by Captain W. Bruce.

D. Ross, Esq. was requested to undertake a descriptive catalogue of the above.

Specimens of the minerals, in and near the coal veins at Hassingabád, were presented by Captain Jenkins in the name of Lieutenant Ferris.

Mr. Calder's paper on the progress of Indian Geology was begun.

Thanks being voted, the meeting adjourned to Wednesday, 2d April.

Wednesday, April 2d, 1828.
Hon. Sir E. Ryan in the chair.

A specimen of Lithographic printing from the Lias Limestone, sent to the Society by Capt. Franklin from Bandalkhand, was presented.

A well-preserved specimen of the long eared Bat (Vespertilio Auritus) was presented by Dr. Adam, in the name of J. Grierson, Esq.

A catalogue of the Minerals presented in Captain Coulthard's name, 27th Feb. was submitted.

A paper on the Geology of the banks of the Ganges and Jumna, by Mr. Benson, presented by Dr. Adam, was read.

Also extracts from Dr. Govan's letters to Government, under date 28th July and 5th December 1827, relating to the Geology and Meteorology of the Himmalaya*.

Thanks were voted,

And the meeting adjourned to Wednesday, the 23d April.

Friday, April 25th, 1828.
J. Calder Esq. in the chair.

Dr. Butter of Ghazipur, was elected a Corresponding Member.

A series of specimens illustrative of the Geology of the vicinity of Gaya and Rotás Garh, with a short account of them, was presented by Captain Jenkins, in the name of Lieutenant J. Thomson. This paper was read.

A series of specimens. No. 1 to 26, illustrative of the secondary rocks containing organic remains, from the neighbourhood of the Giant's Causeway in Ireland, also a specimen of two very perfect joints from one of the basaltic columns of the Causeway, were presented by Mr. Calder.

Three well-preserved specimens of the Mantis insect, with a short paper, were presented by Dr. Adam.

A paper on the Trap formation of the Ságar district, and of those districts westward as far as Bhagulpúr, was received from Captain Coulthard, B. A,

The reading of Mr. Calder's paper was resumed, and concluded.

Thanks voted to the several contributors.

The meeting adjourned to Wednesday, 21st May.

Wednesday, May 21st, 1828.
Hon. Sir E. Ryan in the chair.

It was resolved at this meeting, that the papers already received by the Committee, and approved of, should be forthwith printed.

Dr. Adam's observations on three specimens of the Mantis tribe of Insects was read.

Captain Coulthard's paper, presented last meeting, was commenced on.

The Secretary, D. Ross, Esq. laid before the Committee a detailed catalogue of the minerals from Persia, &c. and the Coast of Tenasserim, presented by Captain Bruce on the 19th March last.

The meeting adjourned to Wednesday, 11th June.

Friday, June 13th, 1828.
Hon. Sir E. Ryan in the chair.

The reading of Captain Coulthard's paper on the Trap formation resumed and concluded. A singular species of Mollusca from the Coast of Ceylon, presented by Mr. Calder, with remarks by Dr. Adam, which were read.

The meeting adjourned to Friday, 27th instant.

* This term appears to us to be used with an unwarrantable latitude, as if applicable to every part of the great belt or zone of mountain land of which, in its real sense, if it mean any thing, it can mean only the most elevated or snowclad ridges.

Friday, June 27th, 1828.
Hon. Sir E. Ryan in the chair.

A letter was submitted to the Committee from G Swinton, Esq. enclosing a communication from Dr. Brewster on the subject of his Polyzonal Lens, and requesting aid from the encouragers of science in India to raise a sum of £300 for the purpose of constructing a compound Lens upon his plan. Mr. Swinton proposed subscribing 500 Rs. and Captain Ellis offered 30, but the further consideration of the subject was referred to a general meeting of the Society.

Adjourned to Wednesday, 23d July.

Friday, August 1st, 1828.
J. Calder Esq. in the chair.

A paper from Dr. Govan on the geology of the vicinity of Simla, received from Government, with specimens illustrative of the same, presented by Mr. Calder.

Remarks on the geology of the country on the route from Baroda Udipúr via Beerpúr and Sulúmpúr, with sections by Dr. Hardie, presented by Mr. Calder.

A supplementary paper on the geology of Bandilkhand was received from Captain Coulthard.

Extract of a letter from Dr. Leslie, with some fossil bones of an elephant, found in the river* near Calpí, by Dr. Duncan, were read.

The reading of a paper entitled Observations on the Geological appearances and general features of portions of the Malayan Peninsula, and of the countries lying between it and 18° north latitude, by Captain James Low, M. A. was commenced.

The meeting adjourned.

Wednesday, August 20th, 1828.
Hon. Sir E. Ryan in the chair.

A letter from Captain Franklin to the President was read, detailing the nature of his recent researches, and pointing out the objects which yet remained to be accomplished.

It being understood that Captain Franklin was desirous of supplying those blanks in the geology of the country he had been employed in, free of any charge to Government beyond his regimental allowances, it was unanimously resolved by the Committee, That the President should be requested to wait on the Governor General to represent these circumstances, as well as the interesting nature of Captain Franklin's previous researches, and the importance to geological science of the completion of the work.

Mr. Pearson, of Midnapore, was elected a Corresponding Member.

Specimens of Lithographic printing from stones sent down from Agra, by Lieutenant Boileau of Eng. and an impression from a Rotás Stone, sent by Lieutenant Thomson, were presented by Captain Jenkins.

The reading of Captain Low's paper on the geology of the Malayan Peninsula, was resumed and concluded.

A communication by Mr. Piddington, C. M. giving an account of an analytical examination of some water from the Hot Spring at the foot of the Altnam hills in the province of Tenasserim, was presented.

Adjourned.

Wednesday, October 23d, 1828.
Hon. Sir E. Ryan in the chair.

A letter from the Secretary to Government, General Department, in reply to the application of the Committee in favour of Captain Franklin, through their President, in which the proposition submitted to them is sanctioned, but the period of employment limited to the ensuing cold weather, being read,

It was resolved, That the Secretary should be directed to acknowledge the receipt of the letter, and further, to request, on the part of the Committee, the boon of an extension of the period fixed, should that extension be necessary to the completion of the researches.

The first part of a paper on the Geology of the Valley of Udipúr, by Dr. Hardie, was presented by Mr. Calder.

* Is not this a suspicious locality for *fossil* bones?

Some specimens of the prevailing rocks about Simla, in the northern mountains, from Major Beatson, were presented by Mr. Calder.

A third selection of specimens from Dr Govan, illustrative of his paper on the geology of a portion of the Himmalaya range, was presented by Mr. Calder.

A paper on the Diamond Mines of Panah, by Captain Franklin, illustrated by a series of specimens which were handed round, was read.

The meeting adjourned to the 18th Nov.

Wednesday, November 19th, 1828.
James Calder Esq. in the chair.

Read the Secretary's letter to Government, ordered at the last meeting, with the reply to it. The Government did not think proper to grant the indulgence solicited.

A paper by the late Dr. Voysey, on the Geology of the Sítabald Hilli, also an account of some Fossil Shells found in the Gáwilghar range of hills by the same, were presented by Mr Calder.

A specimen of Rock and Earth from the summit of Mount Ophir, Malacca, was presented by Mr. Calder, in the name of Mr. Lewis.

A description of a doe from Baxar, by Dr. Leslie, was presented and read.

An enlarged copy of Connybeare and Phillip's geological Map of England, was presented by Mr. Calder, in the name of Colonel Hodgson.

A paper on the fertilising principle of the Silt left by the inundations of the Húgli, by Mr. Piddington, C. M. was presented by Mr. Calder.

A notice was communicated from Lieutenant T. J. Boileau, of his having despatched a block of the yellow Limestone of Jesalmér, from a small specimen of which good lithographic impressions had been previously obtained in Calcutta.

The reading of Dr. Hardie's paper on the geology of Udipúr was commenced.

The meeting adjourned.

2. Agricultural and Horticultural Society.

A meeting of this Society was held Wednesday, the 3d of December, in the hall of the Asiatic Society, the President in the chair.

Dr. Carey was elected Secretary, in the room of Mr. Barnett, deceased.

The President proposed that the Garden Committee should be authorised to expend monthly on the garden the sum of 267 Rs. for the next quarter. This proposal was introduced by a statement, that during the past month the account sales of articles from the garden had exceeded the expenses by a trifling sum. The motion was carried, though opposed by Mr. Robison, on the ground that there were no funds in the hands of the Treasurer, as he understood the whole (amounting to Rs. 11,000) had been expended by the Committee, not only without the knowledge and consent of the Society, but contrary to a deliberate resolution passed in May 1827.

Mr. Robison then moved, that an extraordinary meeting should be called on the 2d Wednesday of January next, before which meeting the Garden Committee should be requested to submit a report of what had been effected in the garden since it has been given to the Society by Government, what sums had been expended, and under what sanction from the Society. This motion was agreed to.

Dr. Carey proposed, that an early day in January should be fixed for the show of vegetables by native Mális in the Town Hall, and the distribution of the prizes and medals of the Society. Agreed to, after some discussion as to the state of the funds.

The first volume of Transactions of the Society was reported by the President to be ready for delivery, and he proposed that five copies should be presented to Government, a copy gratis to each Member of the Society, and a few copies sent to the Court of Directors. The copies to Government and the Court of Directors were ordered to be forwarded; but it was proposed, and agreed to, that the order of copies to Members should be deferred till a future meeting of the Society took place, and it appeared *how* the expense of printing and publishing was to be defrayed.

The President proposed, that the Society should undertake the expense of translating and publishing the *Hortus Bengalensis*. The value of the work was admitted by the meeting, and also the expediency of the undertaking, if the Society's funds could afford it; but it was agreed to defer the consideration of this question till another opportunity.

The President stated, that the remittance of 1000 Rs. for the purchase of English fruit trees during the past year, had been replaced in the hands of the Treasurer by the sale of the trees, besides the affording a number of trees for the Society's garden.

The President stated, that a remittance had been made to England in December 1827, for a supply of garden seeds for distribution during this present season, but that they had not yet arrived.

The President read a short paper recommending the raising of garden and other seeds, for transplanting, in shallow earthen porous pans filled with sand, which pans are to be kept moist by being set on a stand half immersed in water. He stated, that the plan had been very successful with himself, and proposed it for the adoption of others Three such pans, with different kinds of seeds in a state of beautiful vegetation, were exhibited to the meeting.

The meeting then adjourned to the 2d Wednesday of January; when the matters contained in Mr. Robison's motion will be taken into consideration, as well as the other subjects which were deferred.

3. Medical Society.

A meeting of this Society was held in the apartments of the Asiatic Society, on the 6th December, which was very numerously attended. H. H. Wilson, Esq. in the Chair.

Dr. Haliday was elected a member of the Society, and several candidates were proposed for admission.

A letter was read from Dr Traill of Liverpool, President of the Royal Institution of that place, acknowledging the receipt of the Society's three first vols. of Transactions.

The following papers, received since the last meeting, were then laid before the Society.

1st. Observations on the quantity and quality of food necessary for man, by Mr. R. M. Martin.

2d. On the use of the Chloride of Lime in India as a disinfectant by Dr. R. Voss.

Several books were presented for the Library by individual members. Six copies of a work on the diseases of Europeans in the country, drawn up by the Medical Board of Madras, were transmitted by that Board, and a printed oration read before the Medico-Botanical Society of London, was presented by that body.

The papers of the evening were then read and discussed, viz. Dr. Voss on the Chloride of Lime, and Dr. Wise on the Pathology of the Blood-vessels. In the course of the discussion on the first of these, it was suggested by a member, that the internal exhibition of Chloride of Lime or of Soda might prove a valuable addition to the remedial means of the profession. A case was adduced of severe dysentery, in which the gentleman in question had used the Chloride of Soda with success, on the previous failure of every other remedy. The dose was 36 minims every six hours. Two doses produced a decided effect, and the patient ultimately recovered.

Previously to the breaking up of the meeting, it was intimated from the chair, that agreeably to the laws of the Society, the election of the Vice-Presidents, Secretary, Assistant Secretary, and Members of the Committee of Papers would take place at the next ensuing meeting, being the first of 1829.

III.—SCIENTIFIC INTELLIGENCE, NOTICES, MEMORANDA, DESIDERATA, &c.

1. *Sanpu and Irawadi Rivers.*

Our readers will remember the discussions which have appeared from time to time in the Government Gazette on the subject of the identity of the Sanpu river of Thibet with the Brahmaputra of Bengal. A very good abstract of the question was published in the Oriental Quarterly Magazine, which appears to us on the spot to leave little further to be wished for, except the actually tracing either of the two rivers to such point as shall set the question entirely at rest. The principal antagonist the advocates of the above opinion have had to contend with is M. Klaproth, the Editor of the Journal Asiatique, who, on the authority of some Chinese works, insists that the Sanpu of Thibet is the Irawadi. To this opinion the writer in the Government Gazette satisfactorily objected the journey of Lieutenants Wilcox and Burlton, in which they visited the Irawadi in latitude 27° 31′, and found it of so moderate a volume as not to warrant more than a comparatively short course from its origin. M. Klaproth, obliged to admit this objection, yet unwilling to abandon his Chinese authorities, turns to one of the eastern branches of the Irawadi, which he now thinks must be the Sanpu. This new view of the case is set forth in a pamphlet which has been transmitted to the Asiatic Society by the author.

We notice the subject for the purpose of making our readers acquainted with the progress of the discussion, and the fact that nothing which has been yet advanced has carried conviction to M. Klaproth's mind. His advantage in being the only one engaged in the controversy who has access to the authorities he lays so much stress on, is great ; for it may be, that the sharp-sightedness of an opponent might detect a flaw in those documents, which is overlooked by the complacency of the party to whose views they are subservient.

There are some particulars yet to be brought forward, which will strengthen considerably the opinion of our Calcutta Geographers ; and we expect, even occasion some degree of scepticism to M. Klaproth as to the value of his Chinese authorities.

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2. *Circular Instrument for observing double Altitudes.*

Those of our readers who receive the Transactions of the Astronomical Society will remember a plan of a new circle proposed by Dolland in the vol. 2d The object of it is to combine the direct and reflected observation of any heavenly body by means of two parallel circles, each furnished with a telescope, and thus prevent the possibility of any derangement of the instrument while being moved from the direct to the reflected object. It is sufficiently well known that the method of observing by reflection which renders us independent of plumb line or level is, particularly when thus facilitated, greatly superior to the old one. Were any authority wanting to recommend the instrument, the approbation of the Astronomer Royal ought to be sufficient.

The use of the instrument is obvious. While one telescope and circle is addressed to the direct object, the other is employed with the reflected : thus are obtained 2 readings, multiplied into 8 by 4 verniers. The telescopes being then respectively changed, 8 new readings are obtained, making in all 16. By turning it half round in azimuth, this number is doubled, so that the errors of division must be greatly diminished in a mean of 32 readings*. Such an instrument is particularly adapted to Delambre's method of C. M. altitudes.

The expectations excited by Mr. Dolland's description have been fully gratified by the sight of one of these instruments, which we had an opportunity of viewing about two months ago, the property of Captain Fisher, Surveyor in Sylhet. In symmetry of appearance, a feature, as observed by Troughton, by no means unconnected with the good performance of an instrument, it is hardly inferior to the works of that great artist. The two vertical circles are divided on their edges very beautifully on silver, and instead of the four verniers proposed in his paper, he has given two micrometers†, which subdivide to 2″ a severe test, as the maker observes, of an engine divided instrument‡. The horizontal circle is furnished with five verniers,—a preferable arrangement to six, as in reversing the telescope the verniers do not come on the same set of divisions. In verniers which lie on the extremities of a diameter, as in the case of 2, 4, 6, 8, &c. reversing the telescope, of course, merely occasions them to interchange places, and the measure has therefore no tendency to reduce the errors of division.

* It appears to us that the verniers in the case would only change places, and no new readings be obtained.

† We doubt if the maker has improved the instrument by this change. In his description he mentions four fixed verniers which apply to both circles, so that in using the instrument for meridian altitudes in the method practised at the Greenwich observatory, there could be no uncertainty in what Mr. Pond calls the index error. But in the instrument as constructed, the two micrometers are in some measure independent ; and though there be no great probability of such a thing happening, yet it is possible that unequal changes may occur in them, the tendency of which would be to alter the index error.

‡ Though this remark be perfectly just, we would have preferred three verniers.

This instrument is by far the most generally useful we have seen of its size, and we have no hesitation in offering our recommendation to any one who may contemplate the possession of an instrumont of this price to order one from Mr. Dolland. The circles are 12 inches diameter. It is contained in two boxes, of moderate size, and quite portable.

Every part of it except the divisions is covered with paint, which we think an improvement. The price we understand was £113, and in this country, including charges, Rs. 1230.

3. *Lunar Distances.*

It is well known that the great desiderata in physical geography are good differences of longitude, those of latitude being easily obtainable, and with no great apparatus, nor involving much trouble. Of the several methods of obtaining the former generally recommended, there is but one that does not require expensive instruments, and involve the loss of much time, if not troublesome calculation:—that is, the method by transference of time. Even this method requires at least two chronometers and an instrument for taking the time, while if the difference of longitude exceed a certain quantity, they require several repetitions of the operation, and consequently time. The method I am about to recommend may be used by a solitary observer, whatever the difference of longitude. It requires but a Troughton's circle and sextant, with a tolerable pocket watch. He may choose his own time for the operations, nor is the calculation (in the present day) such as to form any valid objection.

The method I would propose is that of Lunar Distances, which I am not aware of having ever seen recommended for this purpose*. When absolute longitudes are determined in this way, the result is vitiated by the error of the tables, or calculated place of the moon. But as it is probable that this error, whatever it may be, has a uniform effect within a certain interval of time, it is likely that though two longitudes determined under this condition would have nearly the same error, yet that the difference of longitude would not be very far removed from the truth. Thus, to give an example.

The longitude of Parsauntpúr was found by the lunar method (Troughton's circle) to be

	H.	M.	S.	Miles.
	5	38	27.9	
Of Loten village,	5	40	40.3	
Difference,		2	12.4	34.14
By protraction,				30.01
Error,				4.13

Now as however great the difference of longitude, the probability is that the error would be much the same, we see here a method by which an unassisted individual might greatly improve the geography of a country. And if it should so happen that a second and a third individual were similarly employed about the same time, it is obvious that their differences of longitude would be still more correct. To a traveller, who is generally restricted as to time, I consider this method far preferable either to Lunar Transits or Satellites; for besides that no opportunity may offer with them, there is the great incumbrance of the instruments,—a serious objection, whichever way it be considered. Nor is it possible to set up a transit satisfactorily on any such temporary stage as a traveller would be likely to have at his command. In the method I propose, a Troughton's circle, artificial horizon, and good pocket watch are all that is required:—articles sufficiently portable, not involving much expense of outlay, or liable to serious injury in moving. The reductions have been so simplified lattorly, as to form no objection to any one conversant with astronomical calculation. To sum up, I am of opinion it is the most extensively available to the purposes of general geography of any method yet proposed, while the instruments required are such as no geographical traveller will be unprovided with—such in fact as, and no more than, are requisite for the determination of the latitude. This in itself is a strong recommendation of the method. Its value would be greatly enhanced, were regular, daily distances observed by some fixed observer in any part of India. D.

* I mean, for *differences* of longitude.

Meteorological Summary. Benares, 1827.

Month.	Hour.	Barometer.	Therm. indoors.	Therm. outside.	Regist. extremes.	Wet bulb Therm. outside.	Depression.	Aqueous Tension outside.	Hair Hygr. inside	Aqueous Tension in doors.	Rain inches.
February,	10 a. m.	29.918	69.7	71.4	63.3	59.7	11.7	.39	63	.35	—
	6 p. m.	.816	72 2	77.0	81.4	61.2	15.8	.28	66	.38	—
March,	10 a m.	29.85[illegible]	75.1	77.7	67.8	63.6	14.1	.36	64	.36	0.20
	6 p. m.	.73[illegible]	77.0	84.3	88.4	64.9	19.4	.27	59	.30	—
April,	9½ a. m.	29.72[illegible]	83.1	87.8	74.5	70.5	17.3	.34	63	.35	0.32
	6½ p. m.	.52[illegible]	85.5	91.9	100.3	71.6	23.3	.25	58	.29	—
May,	9½ a. m.	29.632	87.1	91.7	80.0	70.8	21.9	.25	67	.40	1.60
	6½ p. m.	.530	90.0	96.7	101.0	73.6	23.1	·27	61	.33	—
June,	9½ a. m.	29.45[illegible]	89.8	91.9	83.1	81.6	10.3	.58	81	61	3.22
	6½ p. m	.37	90.4	94.	100.6	79.6	14 4	.46	78	.56	—
July,	9½ a. m	29.402	86.3	87.3	81.8	80.8	6 5	.69	89	.76	8.86
	6½ p. m.	.332	86.9	87.8	92.4	81 6	6 2	.71	90	.78	—
August,	10 a. m.	29.50[illegible]	84.9	85.6	77.7	81.2	4 4	.79	93	.84	7.88
	6½ p. m.	.417	86.0	85.7	89.2	81.8	3 9	.80	94	.87	—
September,	10 a m.	29.636	84.5	84.9	79.5	82.5	2 4	.86	89	.76	0 88
		.569	86.3	84.5	91.1						—
October,	10 a. m.	29.827	8[illegible].1	83.1	73.5	70.2	12.9	.44	67	.38	0.09
		699	83.7	——	92.7						—
November,	10 a. m.	29.927	70.7	69.9	61.6	69.6	9.3	.47	64	.36	1.55
					81.7						—
December.	10a. m.	29.937	64.8	63.0	56.2	69.6	2.4	.78	85	.68	1.50
					71.2						

The Barometer stood 25 feet above the ground—the other instruments being on the top of the house, in a current of air through a balcony to the north, shaded from the sun's rays. The instruments marked in-doors in the observatory.

TO SUBSCRIBERS AND CORRESPONDENTS.

In consequence of the length to which the proceedings of the Committee of the Asiatic Society extend, we have been obliged to increase the number of our pages to 28 a circumstance for which we were unprepared, and therefore unable to substitute the lighter paper mentioned in our Prospectus. We will not allow this circumstance, however, to affect the dák charge to our Moofussil subscribers. As soon as the number of our subscribers shall warrant the permanent increase of our limits, we purpose extending them to 32, which, by using the lighter paper, will still be within the Post-office weight.

In our Prospectus we gave an enumeration of the subjects, which it was proposed our work should embrace. Some of our correspondents, however, appearing not to understand clearly the limits within which we have confined ourselves, it may be proper to state, that every communication relating to Science, exclusively so called, or the knowledge of external nature, will be within our plan.

The article on Hygrometry shall appear in our next.

Lithographicus on the rise and progress of the art in India also in our next.

The several notices under the signature P. have been received and shall appear.

H. "On the Irrigation of Land in India" has been necessarily postponed. Our Correspondent shall hear from us privately on the subject.

The account of the Visit to the Snowy pass Lépu Dhúra has also been postponed for want of room.

The Monthly Meteorological Register has been omitted partly for want of room; partly from the consideration, that it would be preferable to begin with the first month of the year.

Several notices and shorter papers are under consideration.

We invite our subscribers to notice any particular paper or information of any kind in the Scientific Journals, or Tables, or Extracts from larger works which they wish to be printed; and we shall be happy, as far as the arrangements of the work will permit, to meet their wishes.

ERRATUM. P. 20. l. 4. from bottom, *for* "Lemineeab, by Col. Boyle." *read* "Semaría, by Col. Voyle."

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GLEANINGS

IN

SCIENCE.

No. 2.—February, 1829.

I.—*On the Measure of Temperature, and the Laws which regulate the Communication of Heat**. By MM. Dulong and Petit.

[From the " Journal de L'Ecole Royale Polytechnique," T. XI.]

Introductory Observations.

From the earliest cultivation of experimental physics, it has been generally admitted, that amongst all the effects of heat, we ought to adopt change of volume as preferable to every other class of phenomena due to that cause, in our attempts to measure its modifications, whether natural or artificial. Still there was a wide interval between this general conviction, and that knowledge of the subject required in constructing thermometers on just principles; such in fact as should render them comparable one with the other. But the frequent employment of these instruments, and the value of the data which they furnish, having continued to attract attention to the subject, and to those circumstances which might in any way affect their indications, all the particulars of their construction have been studied with so much care, and in such detail, as to leave little to be wished for on this head.

It was, in fact, indispensable to use the greatest accuracy in thermometrical observation; but it did not follow, that such accuracy would lead us to a full knowledge of the theory of heat. For we may refer the varying phenomena to an arbitrary scale of temperature, and then investigate empirical formulæ, which will exactly represent observation; but we could not hope to discover general properties, or what may be called the most simple laws of heat, without comparing thermometers constructed with substances in each of the three different states, of which matter is susceptible, and determining the relations which exist between the indications of these instruments, and the quantities of heat added or subtracted, to produce determinate changes of temperature.

Although this subject of research must have suggested itself to the mind of every inquirer, it must be allowed, that it has not yet been followed out with all the care, and in that fulness which its importance demands. The labours of

* This truly valuable paper was crowned by the Academy of Sciences in its sitting of the 16th March 1818, and the prize in physics decreed to the very ingenious and profound authors.

It has been noticed with high commendation in all our recent works in which the subject is treated of; but in so imperfect a manner, that the true value of the results cannot be collected. It appeared, therefore, desirable to give a more complete idea of its contents than has yet been done; and it was at first supposed, that a tolerably clear abstract might be made of it. On making the attempt, it was found, that the authors had been any thing but prolix, and that without sacrificing clearness it could not be shortened. As however, the paper is too long for our limits, we are compelled to divide it into different portions; and this is easily done, as the Essay itself, embracing different topics, allows a subdivision without any break of connection. It is divided into two parts; the 1st treats of the measure of temperature; the 2d of the laws of cooling. The first part again is subdivided into the following sections:

1.—On the expansion of the gases.
2.—On the expansion of mercury.
3.—On the expansion of solids.
4.—On the specific heat of bodies.

The present Number contains § 1.—Ed.

De Luc and Crawford include too limited a portion of the thermometric scale, to allow of our deducing from them any general results. And this is a defect common to all the inquiries into the theory of heat yet made. We may, in fact, easily conceive, that phenomena subject to very different laws, may yet, within a certain interval of temperature, appear to have the same progression. For, if we content ourselves with observations, between those limits where the divergence of the laws is scarcely sensible, we shall be naturally led to attribute such small differences to error of observation; and we are thus left without data for investigating their real cause. We shall have frequent occasion, in the course of this memoir, to acknowledge the truth of this conclusion.

Mr. Dalton has considered this question from a more elevated point of view, and has laboured to establish general laws, applicable to the measurement of every temperature. These laws, we acknowledge, form an imposing whole by their regularity and their simplicity. Unfortunately, this able philosopher has been too hasty in generalizing—conceptions highly ingenious, it is true, but which rested upon very uncertain data. Accordingly, there is scarcely one of his positions, that is not contradicted by the results of the experiments of which we are about to give an account.

These experiments have had for object, to discover the laws of cooling, to which bodies are subject when immersed in an elastic fluid* of a certain nature, density, and temperature. Before commencing this inquiry, however, it was necessary to supply the perfect blank which exists, and thus render more precise, our notions on the measure of the higher temperatures. We therefore commenced our labours with an examination of this question, secondary, it is true, but still itself possessing great interest; and we propose to enter in the first instance on the details connected with this subject.

This paper then, will be divided into two parts perfectly distinct: in the one, we propose to investigate the measure of temperature; in the other, the laws of cooling.

PART I.

On the Measure of Temperature.

If we could find a substance, the expansion of which were regulated by a law so regular and simple, that the addition of successive quantities of heat should be accompanied by equal increments of volume; such a substance would possess all the qualities which philosophers have been accustomed to look for in a perfect thermometer.

Such an instrument might, however, be found not to answer all the expectations at first excited. If it should so happen, for instance, that the specific heat of every other body when referred to its scale, proved variable, and unequally variable in each; it is evident that no conclusion could be drawn *a priori*, from the indications of this instrument, as to the quantities of heat gained or lost in a given change of temperature.

We see then, that our first step in this inquiry, is to ascertain whether the capacities of a number of bodies referred to the same scale, vary in the same manner, and whether the expansions of substances of the most opposite natures, are regulated by one and the same law. This latter comparison, with which we shall begin our details, being susceptible of greater precision than the first, we have entered into it much more fully; and we do not believe, we have neglected any of the precautions, necessary to ensure correctness in the results.

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§ I.—*On the Expansion of the Gases.*

When our object is merely to establish a general comparison amongst the rates of expansion of different bodies, the choice of the thermometrical substance, to which the different valuations shall be referred, is evidently to be determined on considerations purely arbitrary. Superior facility of construction and greater convenience in observing, were our inducements for employing the mercurial thermometer in almost all our experiments.

* It appears to us, that the term fluid might be conveniently confined to liquids or non-elastic fluids, and that of gas to what are here called elastic fluids. Precision of nomenclature in science, is a point of no little importance.—Ed.

The comparison of the mercurial thermometer with that of air, was long ago made by M. Guy Lussac, within the limits of temperature, answering to the freezing and boiling points of water. The result of his experiments is, that within that interval of temperature, the scales scarcely differ in their indications.

Mr. Dalton again thinks, that the mercurial thermometer marks about 1° in excess of the air thermometer towards the middle of the scale, where, in fact, it might be concluded, the difference of the two would be greatest, since by hypothesis they coincide at the two extreme points of 0° and 100 degrees.

We see then, that if there be any difference in the rates of expansion of air and mercury, it must be very small between the limits of the freezing and boiling points of water.

We extended this comparison to low temperatures. Our first experiment made at 20°, gave a perfect agreement between the indications of the two instruments; and in a great number of observations made at 30° to 36°, we have found small differences, sometimes on one side, sometimes on the other, showing, in fact, the mean of each series to be the same*. So that within an extent of 130°, the difference of the two scales is so small, as not to be appreciable amongst the errors of observation.

No experiments are more easily prosecuted than these. while we keep within the boiling point of water, but in attempting to pursue the inquiry amongst the higher temperatures, so many difficulties are met with, as oblige us to have recourse to methods of a more tedious as well as complicated nature. Those which we followed, and which we are now to describe, appeared to us to promise as much precision as researches of this nature will admit.

The apparatus consists of a rectangular vessel of copper seven decimetres in length (in. 27,5), one decimetre in breadth (in. 3,9), and one in depth (in. 3,9). This vessel has on one of its sides two apertures; in one of these a mercurial thermometer is inserted in a horizontal position, in the other a tube is similarly placed, well dried, and filled with perfectly dry air.

The vessel is fixed in a furnace in such a manner that every part of it can be equally heated. It is filled with a fixed oil which, it is known, is capable of being heated to upwards of 300° before it reaches the boiling point.

The tube which contains the air, is terminated on the side of the aperture by a short tube of a very small diameter, which projects a little beyond the vessel. The quantity of air contained in the external part of this tube, and which is not heated equally with the rest, may be entirely neglected. We know that it never exceeded $\frac{1}{500}$th of the total mass; but besides this, we took the precaution to heat it during each experiment, in order, as much as possible, to diminish even this small error.

The vessel has a cover with several openings in it. In some are fixed vertically, thermometers which serve to indicate, whether the different parts of the liquid mass are at the same temperature; others carry spindles furnished with flyers or vanes, the rotation of which agitates the liquid, the object of which is to establish a uniformity of temperature.

The following is the order which was followed in each experiment. The vessel was first heated, to a temperature very near that which it was wished finally to establish, and all the openings of the furnace shut. The heat having a tendency to be equally distributed throughout the apparatus, the temperature of the oil still rose several degrees, when it became stationary and consequently easy to measure with accuracy. This was done by the horizontal thermometer, which

* In order to give an idea of the closeness of these comparative valuations, we shall give the following particulars of some made between 36° and 30°.

Mercurial thermometer.	Air thermometer corrected for the expansion of glass.
—36,29	—36,18
—34,72	—34,84
—33,31	—33,40
—32,27	—32,13
—31,63	—31,54
—31,26	—31,04
—30,46	—30,59
—29,68	—29,64
Mean—32,452	—32,420

was immersed in the oil with every care, so that the whole column of mercury should be covered by it: at this instant the fine point of the air tube was sealed by a blow pipe, and the height of the barometer noted. The tube was now withdrawn, and removed to a different room, the temperature of which was almost stationary. It was there fixed in a vertical position, in such a way that the extremity dipped into a bath of mercury perfectly dry. The point being then broken off, the mercury rose till the equilibrium with the outward pressure was restored, the tube being left in this situation till it acquired the temperature of the room, indicated by a very sensible thermometer hung at a short distance. When this effect took place, the height of the column in the tube was measured by means of a vertical scale furnished with a vernier. The height of the barometer was noted, and the difference of these gave the elasticity of the cooled air. The tube was then withdrawn, every precaution being taken to retain the whole of the mercury that had risen into the tube. The tube and mercury were weighed; afterwards the empty tube; and lastly the tube filled with mercury. Deducting now from this last result, the former two, we have the weights of two volumes of mercury, the one equal to that of the heated air, the other to that of the cooled. From these weights, we conclude what are the volumes, which are afterwards reduced to what they would have been under the same pressure; as the elasticity of the cooled air was known, having been measured as above indicated, and that of the heated air was equal to the pressnre of the atmosphere at the moment the tube was sealed*.

In order to give a clear idea of the degree of confidence to be placed in the results which we have established, it may not be without its use to enter into a little detail, as to the precautions adopted to ensure accuracy in each experiment.

One of the principal difficulties we have to contend with in such experiments, is that of establishing a perfect uniformity of temperature in a considerable mass of liquid, heated to two or three hundred degrees above the temperature of the surrounding air This condition however may be perfectly attained, when the temperature of experiment is that of ebullition of the liquid employed, as in that case it is necessarily fixed. But in every other case, the process of heating or cooling affecting different parts of the liquid unequally, is opposed to the establishment of that uniformity. We are, however, of opinion, that the arrangement of our apparatus does in a great measure, obviate this particular objection, and for these reasons: 1st, the vessel of copper being fixed within the furnace, the whole forms a considerable mass which must cool very gradually, particularly when close upon its *maximum* of temperature; and 2dly, the liquid being kept in a constant state of agitation, the heat must distribute itself pretty equally throughout it. But to remove every doubt that could possibly arise on this head, we have inserted two thermometers in a horizontal position in the vessel, both being at the same height, and then conducting the experiment in the usual manner, we never had occasion to observe more than two or three tenths of a degree between the two thermometers.

Even if we suppose that all the points of the liquid stratum which surrounds the air tube were not exactly of the same temperature, the error would be less than might on a first view be supposed; for in the arrangement of the apparatus it appears, that the bulb of the thermometer corresponds nearly to the middle point of the length of the tube, and consequently this instrument ought, in every case, to indicate a temperature. differing but little from the mean of the temperatures of the different parts of the tube. It was, in fact, this consideration that induced us to adopt a cylindrical tube, in preference to a bulb of any other form. We may mention here also as connected with the present subject, the necessity of having the

* All our experiments made in the manner just described, have been calculated after the following formula.

Calling P the weight of a volume of mercury equal to that of the heated air, T the temperature of that air reckoned on the mercurial thermometer, H its elasticity, P′T′H′ the same quantities for the cooled air; also taking D to be the value of air at 0° which has been heated to T, but under uniform pressure, and d the mean expansion of glass between the temperatures T and T′, we have

$$V = \frac{P\,H\,(1 + d\,(T - T')\,)\,(1 + 0.00375\,T')}{P'\,H'}$$

from which expression we may see, that an air thermometer corrected for the expansion of glass, would show for any temperature T of the mercurial thermometer a number of degrees which may be thus expressed:

$$\frac{P\,H}{P'\,H'}(1 + d\,T - T')\,)\,(266.67 + T') - 266.67.$$

whole of the thermometrical column immersed within the liquid, if the true indications are required. This precaution, though it may appear unnecessary *(minutieuse)* in lower temperatures, cannot be neglected in high ones; for in this case the column of mercury contained in the stem undergoes a considerable increase. Thus, for example, we remarked that at the temperature of 300° there was frequently a difference of 12° between the indications of the same thermometer, according as the bulb only or stem also was immersed in the liquid. We might indeed estimate the error arising from any part of the stem being excluded, when we know the expansion of mercury, but the impossibility of saying what is the exact temperature of the stem, entailing on us errors the more to be deprecated as they would increase with the magnitude of the correction, it has always appeared preferable to us to place the thermometers horizontally.

Although the experiments performed in the manner we have just described, have always shown a remarkable agreement in the results, we endeavoured to verify these results in another manner.

In these other experiments, we made use of an air tube of much greater capacity than in the first set, placed in the same manner, with the exception, that the small tube attached to it was bent on leaving the copper vessel, and continued downwards to a length of about five decimetres. The heating process was conducted with the same care already described, and when a steady temperature was attained, and the height of the barometer noted, the lower end of the tube was made to dip into a capsule of dry mercury. The whole was then allowed to cool, till the oil was nearly of the temperature of the air; during which process the mercury continued to rise in the vertical tube, until the internal air was completely cooled. The elasticity of the air became then, an equivalent to the external pressure of the atmosphere, diminished by the height of the column of mercury that had been raised; that of the heated air again, was equal to the barometric pressure observed at the moment when the temperature was stationary: we could thus calculate by the aid of Mariotte's law, what had been the expansion of the air, supposing it to have preserved the same elasticity.

To render this method perfectly exact, it became necessary to allow for the capillary depression of mercury, in the narrow tube into which it enters. We had determined beforehand the value of this depression, and took care to use a tube of uniform caliber, in order that this value should have little or no variation.

A second point to be considered is, that the volume of air was not precisely the same. The portion contained in the small tube was forced of course into the large one as the mercury rose, and this portion suffered no change of temperature. We have calculated the amount of each of these errors, and have applied the necessary corrections. This correction, which depends on the ratio of the capacities of the tubes, is deduced by a calculation too simple to require any explanation here*.

Not only have the experiments made in this way confirmed all the results with which the first set had furnished us, but they have also taught us that Mariotte's

* It will be sufficient merely to indicate the formula we have used in the reduction of these new experiments. H represents the weight of the barometer, which is the measure of the elastic force of the heated air, T the temperature of this air, indicated by the mercurial thermometer, T′ that of the cold air, H′ the height of the column supported after cooling, this height being corrected for the capillary depression, A′ the total height of the vertical tube, r the ratio between the capacity of this tube and of the large horizontal one, d the mean dilatation of glass between T and T′. V means a volume of air such as it would be when expanded to the temperature T, without change of pressure, and supposing it 1 at 0°. Then

$$V = \frac{H\,(1+d(T-T'))\,(1+0{\cdot}00375\,T')}{(H-H')\left(1+r\,\frac{A\,H'}{A}\right)-r\,H}.$$

we may also conclude that at any temperature T of the mercurial thermometer, the air thermometer corrected for the expansion of glass would indicate a number of degrees

$$= \frac{H\,(1+d\,(T-T'))\,(266.67+T')}{(H-H')\left(1+r\,\frac{A-H'}{A}\right)-r\,H}\;266.67$$

law is true at every temperature. So that the changes of elasticity, which heat occasions in a gas the volume of which is constant, are subject to the same law as the changes of volume would be, were the pressure to continue the same.

We shall now give the mean results, deduced from a great number of experiments, made by both of the preceding methods. These results will be found in the following table, which includes the full scale of mercury, from its freezing to its boiling, that is to say an interval of about 400 degrees.

Temperatures as shown by the mercurial thermometer.	Corresponding volumes of a mass of air.	Temperatures which would be shown by an air thermometer corrected for the expansion of glass.
36	0.8650	36.
0	1.0000	0.
100	1.3750	100
150	1 5576	148 70
200	1.7389	197.05
250	1 9189	245.05
300	2.0976	292.70
Boiling point of mercury, 360	2 3125	350.00

The numbers in the third column have been corrected for the expansion of glass, which we shall presently establish*.

There is a considerable difference amongst the numbers given by different experimenters, as the boiling point of mercury on its own scale. This in part arises from the different degrees of care, with which different observers construct their instruments, but principally on account of the want of precision, in the correction made for that part of the stem not inserted in the liquid. The method we have followed, and which furnished the result given in the preceding table, enables us to dispense altogether with this correction. Instead of measuring directly the augmentation of volume of a constant mass of matter, as is done in the ordinary thermometers, we determined the loss of weight which a mass of mercury, capable of filling a certain vessel at 0°, sustains when this vessel is completely immersed in boiling mercury. Knowing at the same time the apparent expansion of mercury in glass for the first 100°, we may by a very simple calculation find the corresponding temperature on the mercurial thermometer, the stem of which is of the same temperature with the bulb†. In order to prevent the contents of the vessel from entering also into ebullition, we had taken the precaution to make it terminate by an upright narrow tube about six centimetres (in. 2,36) in length. The column of mercury it contained did not constitute the $\frac{1}{5000}$th part of the total mass, yet by the pressure it exerted within the vessel, it completely prevented the formation of vapour. It is scarcely necessary to say, that every precaution had been taken to expel completely every trace of air or of humidity.

The corresponding temperature of the air thermometer, has been calculated by means analogous to those we have constantly employed, in our experiments on the expansion of gas. The number inserted in the table is the mean of four results, the extremes of which do not differ one degree.

From the beautiful observation of M. Guy Lussac, that all elastic fluids expand precisely in the same manner between 0° and 100, it was at least probable that this uniformity would also be found to hold in more elevated temperatures, and consequently that the preceding table derived from experiments made with common air, would also hold true of every kind of gas. But to leave nothing doubtful in a subject of so much importance, we made an experiment also with hydrogen, which, as is known, differs the most in some of its properties from atmospheric air.

* As we are averse to extend this memoir to an undue length, we are prevented from entering into the details of each particular experiment. We must therefore content ourselves, in what follows, to give the results, suppressing the particular determinations and intermediate calculations that have led to them.

† Let P represent the weight of mercury which fills the vessel at 0°, p the quantity that is expelled by heating it to t, we have:

$$\frac{t + 6480.\ p}{P - p}$$

The results were within the limits of those obtained from air*. We may then establish it as a general law, that all gases expand in the same manner, and in like quantities, for equal increments of temperature.

The determinations which we have just given would suffice, if it were only required to know the volume of a gas at any temperature, as measured by the mercurial thermometer or the reverse; but the object we had proposed, was to compare the progressive expansions of mercury and air, and this is not yet fully attained. In fact, as a thermometer only indicates the *difference* of expansion of the fluid and its containing vessel, it is evident, these differences cannot be proportional to the absolute expansions of this liquid, excepting in the single case in which the expansions of the two bodies happen to follow the same law. If, for example, the vessel were to expand in a less ratio than the liquid which it contains, it is clear the differences of the thermometer would appear to have an increasing ratio, even though the rate of expansion of the liquid were uniform In the reverse case, a partial and unequal compensation would take place, which would also disturb the regularity of the results. It became necessary then to inquire into the changes which, in elevated temperatures, the absolute expansions of one of the two bodies suffer which are used in the construction of a thermometer.

When we consider all the difficulties inherent in the question of the expansion of solids, even within the boiling point of water, how much more must we think of those of a still different kind, introduced into the question in extending the inquiry to the higher temperatures. After a full examination of every experimental resource which we could look to, the uncertainty of attaining to success such as we wished for, and the enormous complication of the apparatus which it would be necessary to employ; we determined finally for the direct measurement of the absolute expansion of mercury.—This shall be the subject of the next section.

II. *Haidingerite, an Ore of Antimony, of a new species, found in Auvergne.* By M. P. Berthier.

[Repertory of Patent Inventions, (Annales de Chimie, xxxv, 351, August, 1827.)]

Sulphuret of antimony exists in a great number of places, in the gneis formation which occupies Auvergne. It is found there sometimes in regular veins *(filons)*, sometimes in masses; but if the beds are very numerous, unfortunately they are, at the same time, almost always but little productive. On this account, the people who work them are obliged to attach a great many at once, to be able to keep the supply uniform.

The Auvergne sulphuret of antimony is in general sufficiently pure, and regulus of a good quality is obtained from it; but a few years since, some was discovered near the village of Chazelles, in a vein which was soon obliged to be abandoned, because the ore which it yielded gave the manufacturers of regulus a dull antimony, which the consumers were unwilling to use; the person working it, having sent me some specimens of this mineral, I discovered that it constitutes a new and distinct species, and I have named it *Haidingerite*, in honour of Mr. Haidinger, a learned mineralogist, residing in Edinburgh, who every day enriches science by his researches, and whose friend I have the advantage of being.

Haidingerite has not yet been found in regular forms; but it exhibits in some cavities, rudiments of prismatic crystals, which, though not rigorously determinable, are yet sufficient to assure us, that its principal form is not the same as that of sulphuret of antimony. The new mineral is commonly in masses confusedly lamellar, mixed with hyaline quartz, light chestnut coloured ferriferous carbonate of lime, and pyrites, in cubical grains; its colour is iron gray; its surface is frequently covered with rainbow tints; it has not by a great deal, so much brilliancy as sulphuret of antimony, and its shade does not at all incline to blue; it does not impart the smallest motion to the magnetic needle. I have not been able to procure pieces sufficiently pure, to enable me to take its specific gravity.

I purified a certain quantity of it for analysis, by pounding, sifting, and washing it, by which means I separated the greatest part of the quartz and pyrites, and the whole of the carbonate of lime.

Before the blowpipe, the powder readily fuses; but it exhibits no peculiar character; muriatic acid attacks it easily, the action commencing even in the cold;

* The volume of hydrogen being 1 at 0°, we found it 2.1003 at the temperature of 300° of the mercurial thermometer. The extremes of the determinations with common air, under the same circumstances, were 2.0948 and 2.1027.

pure sulphuretted hydrogen is disengaged, and the whole dissolves; excepting a small quantity of quartz and of iron pyrites, but without any deposit of sulphur. The liquid holds in solution only antimony, iron, and a very small quantity of zinc. These properties show sufficiently, that Haidingerite is composed of sulphuret of antimony, and sulphuret of iron, and that the two metals are found at the *minimum* of sulphuration.

The analysis was conducted as follows. To determine the quantity of sulphur, I heated four grammes of the powder well ground, with twenty grammes of dry carbonate of soda, and ten grammes of nitre in a silver crucible; a slight action appeared to begin at a dull red heat, but without deflagration, and without swelling up; at a red heat, the matter entered into a very liquid and complete fusion. After being diluted and well washed with water, there remained on the filter antimoniate of iron, of a pale, ochreous, red colour, and which, judging from its weight, must retain potash in combination. The solution made to boil, after having been saturated with pure nitric acid to drive off the carbonic acid, yielded with muriate of barytes, a precipitate of sulphate of barytes, from the weight of which it was easy to determine the proportion of sulphur.

To ascertain the quantity of quartz and of pyrites, a certain weight of the powdered mineral was boiled in concentrated muriatic acid; the residue was then dried and weighed, and afterward treated with aqua regia, calcined, and weighed anew. The insoluble matter was pure quartz. The difference gave the proportion of pyrites.

I sought the proportion of the antimony and iron in two ways:—

1. I dissolved by means of muriatic acid, a certain quantity of Haidingerite which I had placed in a retort, the beak of which dipped into a vessel containing water; I remarked that towards the end of the operation, a brown flaky deposit was formed in that vessel, of sulphuret of antimony. The quantity of it was very small, but was ascertained. The production of this sulphuret proceeds from the muriatic vapour disengaged, when the solution has acquired a certain degree of concentration, and carrying along with it a small quantity of chloride of antimony, which, condensing in the water already charged with sulphuretted hydrogen, must be decomposed immediately. I added a great deal of water to the strongly concentrated solution, to precipitate the major part of the antimony in the state of oxy-chloride, the composition of which is known. This oxy-chloride was mixed with the quartz and pyrites; but the proportion of these substances was determined previously.

The rest of the antimony was precipitated from the solution, by means of a current of sulphuretted hydrogen gas; after which, the liquor was concentrated and boiled with nitric acid, to convert the iron to the state of peroxide, which was afterwards precipitated by an excess of ammonia. The ammoniacal solution gave a slight white precipitate of sulphuret of zinc, on adding a few drops of an alkaline hydrosulphate.

2. After having dissolved the Haidingerite as abovementioned, I added tartaric acid to the solution, as pointed out by Mr. H. Rose, of Berlin, and diluted it with water; it did not become at all turbid, and I was afterwards able to precipitate the whole of the antimony from it in the state of sulphuret, by means of sulphuretted hydrogen. I weighed this sulphuret while warm, and convinced myself that it contained no excess of sulphur, by dissolving it in muriatic acid. The iron was afterwards determined by ammonia.

The mean result of several analyses:—

Quartz	0.032
Iron pyrites	0.032
Sulphur	0.283
Antimony	0.483
Iron	0.149
Zinc	0.003
	0.982

or, independently of the *gangue* or rock,

Sulphur	0.303	which gives	Sulphuret of antimony	0.715
Antimony	0.520		Proto-sulphuret of iron	0.255
Iron	0.160		Sulphuret of zinc	0.005
Zinc	0.003			
	0.986			0.975

From this result it is evident, that Haidingerite is composed of 4 atoms of sulphuret of antimony, and 3 atoms of proto-sulphuret of iron; for, in that supposition, calculation gives:

Sulphur ..	0·2985-18 at.	or sulphuret of antimony	0. 732-4 at.
Antimony...	0·5330- 4 at.	proto-sulphuret of iron	0. 268-3 at.
Iron	0·1685- 3 at.		1·000
	1·0000		

Its formula then, is $3\,f\,s^2 + 4\,sb\,s^3$. This formula, complicated in appearance, expresses, however, a very simple relation; for it shows that Haidingerite is so constituted, that in the negative sulphuret, (sulphuret of antimony,) there is twice as much sulphur as in the positive sulphuret, (sulphuret of iron:) the same ratio has been already found in jamesonite, analysed by Mr. H. Rose. and which is composed of 4 atoms of sulphuret of antimony, combined with 3 atoms of sulphuret of lead.

The existence of Haidingerite as a peculiar species, seems incontestible; for, in the first place, the sulphurets of which it is composed, are found in atomic proportions and in a simple ratio; in the second place, one of these, the sulphuret of antimony, is found indeed in the isolated state in nature, but the other sulphuret, the proto-sulphuret of iron, does not exist in that state, since magnetic pyrites, which is the least sulphurized of all the native sulphurets of iron, is a combination of proto-sulphuret and of per-sulphuret. In fine, as the proto-sulphuret of iron is very strongly magnetic, if it was only mixed with the sulphuret of antimony in the Chazelle mineral, it would certainly communicate to that mineral the property of moving the magnetic needle: now, I have already remarked, that Haidingerite does not possess that property: therefore, there is a combination between the two sulphurets.

Haidingerite is very easily imitated by synthesis. The two sulphurets of antimony and iron will combine in all proportions, at the temperature of their fusion; and it is probable that they will be discovered also in nature, united in other proportions than in the mineral of Chazelle.

In the manufactories of France, for preparing regulus of antimony, the practice is to roast the sulphuret in a reverberatory furnace, and to melt the roasted matter afterwards in crucibles, with carbonate of potash and charcoal. It is evident that by applying this process, as it has been done, to Haidingerite, pure antimony cannot be extracted from it; for the roasted material is a mixture of oxyde of antimony and oxyde of iron. Now, these two oxydes being reducible almost equally easy, and the two metals having a great tendency to combine together, there must be produced with the reducing fluxes, antimoniuret of iron. This is actually the case, and I have ascertained, by experiments on a small scale, that the smallest trace of iron does not remain in the scoriæ, when the ore has been completely desulphurized by roasting.

It will be easy, however, to extract good regulus from the Chazelle ore, and being abundant, it is desirable not longer to delay working it. I published in the *Ann. de Chim. et de Physique*, xxv. 379, several methods that might be employed. The easiest to practise, would be to melt the ore in crucibles, or reverberatory furnaces, with the addition of iron and a little sulphate of soda, mixed with charcoal. By adding only the proportion of iron strictly necessary, the antimony alone will be desulphurized, and the sulphuret of iron found in the ore, as well as that which would result from the reaction of the metallic iron on the sulphuret of antimony, would remain entirely in the scoriæ, rendered very fluid by the presence of sulphuret of sodium.

The exact proportion of iron to be employed, would be 6 atoms, for 1 atom of the pure ore, or 337 per cent; but it would be advisable to keep always below that proportion, at the risk of losing a little sulphuret of antimony in the scoriæ, because that, if there was an excess of iron, a certain proportion of it would combine with the antimony, which would greatly alter its quality.

III.—*Account of a new Hydraulic Machine, invented by* Mr. Joseph Eve.

[Journal of Science and the Arts, No. XLIV.]

SIR,

I have not perceived in any of the scientific publications of the metropolis, amongst which yours holds such a distinguished rank, a notice of an hydraulic machine, invented by Mr. Joseph Eve, (for which a patent was obtained,) which is now exhibiting in the manufactory of Taylor and Jones, 11, Jewin Crescent, Cripplegate.

The machine I allude to, is a pump, with no valves to open and close, the moving parts being rotatory; their speed may, therefore, be increased at pleasure, and to an almost unlimited degree. The water pumped up is in proportion to that speed of the revolving parts, and to the force applied.

It is the most ingenious, and probably one of the most valuable inventions in which the late years have been so prolific. The few scientific, unprejudiced, and practical persons who have seen the machine, have given the most favourable opinion as to its utility. However, I will proceed to describe this pump, and its performance. In Plate I, A shows a front view; and B an interior view, after the end of the case with the cog-wheels is removed.

The principle is this. Two cylinders of equal diameter (3½ inches,) and equal length (6 inches,) move in close contact on axes or pivots, and revolve in opposite directions, in an outer case or box. These cylinders have each two wings, of three-fourths of an inch area, and two grooves; and as they revolve, the wing of one cylinder falls regularly into the groove of the other, alternately, and so on in rotation; and in order that the groove may present itself regularly to the wings of the opposite cylinder, and let them pass, cog-wheels, placed outside the case, are fixed to the axes or pivots, which project: these cog-wheels ensure not only an even revolution of the two moving parts, but they communicate the power, which is applied by means of a handle, to the axis of a large toothed wheel gearing into one of the two cog-wheels.

The pump case is placed upon a common pipe, descending down to the well 21 feet below. Two men, turning the handle, raise half a ton of water in three minutes with this small pump; which is allowed to be a most satisfactory result, considering that, as the first pump constructed on this principle, it has of course many imperfections.

By substituting an air-vessel with a hose and pipe, the machine becomes the most simple, strong, and effectual fire-engine. It may be converted into a water-wheel, where a small stream with a high fall of water exists, or be acted upon by steam as a rotatory steam engine.

The advantages which it possesses over common pumps are too manifold and self-evident. The most conspicuous are,—a saving of power, on account of the friction being much less than in ordinary pumps. It requires no leathering, being made entirely of metal; it does not wear, as no parts touch or rub except the axes or pivots on their bearings. Its simplicity, strength, and elegance, and the ease with which it is turned into a fire-engine, the saving of room, and weight in pump-work, if applied to deep wells or shafts in mines, the advantages as a ship's pump, are peculiar, and too numerous here to enumerate; but so self-evident, that I shall not dwell longer on the subject.

I would invite you, and your scientific readers, to inspect the pump at Messrs. Taylor and Jones's, who, I am informed, are authorized to execute orders for pumps and engines on this principle.

I am, respectfully, Sir,
Your obedient servant,
A SUBSCRIBER and ENGINEER.

Goswell Road, Nov. 22, 1826.

IV.—*On the probable Decomposition of certain Gaseous Compounds of Carbon and Hydrogen, during sudden Expansion.*

[Journal of Science and the Arts, No. I.]

Some very singular appearances have been observed by Mr. Gordon, of the Portable Gas Works, which have led him to believe that chemical changes are occasioned by the sudden expansion of oil gas, which do not happen when the expansion is gradual; a striking result of the change being the separation of carbon from the gas. The effect referred to is exhibited when oil-gas, compressed into vessels by a power equal to that of 30 atmospheres, is suddenly allowed to escape through a small aperture into the air. It was first observed accidentally, in consequence of the derangement of the valve of a large apparatus, into which the gas had been compressed to 27 atmospheres. The gas escaped with immense velocity, and when an examination took place of what had happened, it was found that all the metallic part of the valve upon which the gas had rushed was covered with a black, moist, carbonaceous substance, and the contiguous brick wall with dry, black carbon, the moisture in this case having been absorbed by the brick. Since that time, Mr. Gordon has repeatedly shown the effect, by allowing the gas to rush out with very great violence from a portable lamp against a piece of white paper, which becomes immediately covered with black carbonaceous deposit.

The general conclusion is, that as the gas thus rapidly expands, a partial decomposition takes place, and carbon is separated. If this explanation should ultimately prove by further experiments, to be true, it will be highly important, as affording an instance of the exertion of mechanical and chemical powers in those circumstances where they most closely verge upon each other. At present, we have but little knowledge of such phenomena, though the announcement in France of the production of several new compound bodies, possessed of peculiar properties solely by the exertion of physical powers, may lead us to hope for an accession of information on the subject; that which we thought we had, was in part rendered uncertain by the contrary conclusions arrived at by Mr. Perkins and Dr Brewster, the one believing that in a case of crystallization the effect was produced entirely in consequence of pressure*; the other, that pressure had been the only cause why bodies, otherwise ready to crystallize, had retained the fluid state.

A natural suspicion, upon first hearing of and seeing the results obtained by Mr. Gordon, was, that the rapidity of the current of gas had carried away a minute portion of the metal from the surface of the valve past which it rushed, or of the interior of the air-way against which it was thrown, and that that metal had caused the stain upon the paper; but upon examination, this proved not to be the case; for the black deposit upon a card, when subjected to acids, remained insoluble, and when burnt and tested chemically, gave no traces of copper.

Further examination of the substance showed that it was not a pure carbon, but one of those compounds, containing a very large proportion of carbon, combined with a small quantity of hydrogen analogous to tar, pitch, or asphaltum, for it dissolved readily in the fluid hydro carbons obtained by the compression of oil-gas. As these black carbonaceous compounds are formed in the process of making oil-gas, a suspicion cannot but arise, that the effect observed *may* have been produced by the current of gas having swept off small portions of such substances previously deposited, or slowly formed in the interior of the vessels at former periods; and have left them upon the wall in the accidental result, or upon the paper placed in the current of the gas, when the effect has been purposely shown.

It may, however, be remarked that, in experiments made in the laboratory of the Royal Institution, upon the fluid product obtained by condensing oil-gas at high pressures, it was observed that, after rectifying the products and separating the more fixed from the more volatile, that although they were perfectly clear and transparent at first, yet by spontaneous evaporation through the corks which closed the vessels, and after a lapse of time, chemical changes were produced; for, ultimately, there remained nothing in several of the receivers but a brown substance, heavy, adhesive, like honey or treacle, and in certain cases even almost solid. From the circumstances of the experiments, no hesitation could arise in concluding that a spontaneous chemical change had taken place; and it does not seem at all unlikely that a similar change, or one to a much greater extent, may have occurred *suddenly* during the rapid alteration in the mechanical condition of the gas in Mr. Gordon's experiment; the most condensible of the substances in the mixture of elastic matters which constitute oil-gas, being perhaps those which are most altered, and in that case Mr. Gordon's account of the phenomena would be correct.

V.—*Miscellaneous Notices.*

1. *On the Influence of the Atmosphere on the Circulation of the Blood.*—August 29, 1825. MM. Cuvier and Dumeril made a report upon the Memoir by Dr. Barry, *concerning the Influence of the Atmosphere on the Circulation of the Blood.*

* Philosophical Transactions.

—This memoir has for its principal object the determination, by positive experiments, of the power by which the blood is forced and directed from the smallest ramifications into which it has been carried, back again to the heart.

Whilst studying the phenomena of venous circulation, Dr Barry was led to observe, that by the act of inspiration, a void was made in the cavity of the chest tending to dilate it, and that all liquids in communication with the interior of the thorax should be drawn towards it, as forced by the atmospheric pressure. It must be acknowledged, that all the known facts are explained by this physical effect. Of this kind are the swelling of the jugular vein during expiration, and the collapsion during the opposite movement; the cessation of certain hæmorrhages by forced inspiration; the absorption of air by the veins, and the accidents which have followed from the opening of any of the great canals near the heart.

The author does not content himself with quoting facts in support of his opinions, but has endeavoured to corroborate it by direct experiments, of which the following are the principal.

Having fixed the end of a glass-tube, furnished with a stop-cock, upon one of the large veins, as for example, the jugular of a living animal, and having placed the open end in a coloured liquor, he observed, after opening the stop-cock, that at each strong inpiration made by the animal, the liquid was rapidly absorbed; that on expiration it remained stationary, or occasionally slightly receded. The same effects followed whenever the experimenter introduced the tube, and this was done very skilfully, into one of the cavities of the thorax, and even of the pericardium.

In order to render the motion of the liquid absorbed more evident, Dr. Barry made use of spiral tubes, in which the space over which the fluid moved being larger, the ascent was more distinct, and to make this still more evident, he introduced into the coloured liquids some drops of oil, or bubbles of air, which facilitated the observation of their motion.

These experiments were executed with the greatest skill, and with every satisfactory precaution requisite to meet the objections which might be made. In all of them the author of this memoir, of which it is our object to relate the results, is satisfied that the motion towards the heart in the large vein is coincident with the instant at which the animal tends to form a vacuum in the breast; that the dark blood traverses the veins only during the act of inspiration; and that the venous movement is always under the influence of atmospheric pressure.

Dr. Barry is so convinced of this atmospheric action upon venous absorption, that he thinks the absorption of poisonous matter may be prevented by the application of a cupping-glass, or exhausted vessel, upon the recently infected part, or into the interior of which any deleterious substance has been introduced.—*Ann. de Chim.* xxx. 192.

The conclusions at which Dr. Barry has arrived, with respect to the blood, are adopted by him with respect to all other fluids similarly circumstanced, and he has embodied some of his opinions upon this subject in a memoir, read before the Academy of Medicine at Paris, on the effects produced by the application of cupping-glasses to poisoned wounds.

Of this memoir no particular details have been given to the public, but the following abstract of the *Report* presented to the Academy by the committee, to which it had been referred for consideration, will give an idea of the estimation in which it is held.

The Report observes, that the principal statements contained in the memoir of Dr. Barry may be reduced to the three following: viz. 1st. That the immediate application of a cupping glass to a poisoned wound will *prevent* the absorption of the poison, and avert all untoward accidents. 2d. That the application of a cupping-glass to a poisoned wound, even after a part of the poison has been absorbed and has began to produce its proper effects upon the system, will *arrest* the progress of these events, and prevent their recurrence so long as it is permitted to remain on the part. 3d. That after the cupping-glass has been applied to a poisoned wound for a certain time, the poison may be removed from the surface, and all unpleasant consequences averted, by simply washing the part with a little water.

The accuracy of these statements, the Report continues, was fully established before the Committee, by experiments, performed with various poisons on rabbits and dogs. The influence, therefore, of atmospheric pressure on the process of absorption, may now, it is added, be incontestably proved; and the establishment of this fact, for which we are indebted to Dr. Barry, may justly be regarded as a true discovery, notwithstanding some vague ideas previously put forth by others on the subject; and the empirical practice of sucking poisoned wounds, which have been so long known to the profession.

The poisons employed were arsenic, prussic acid, strychnia, the upas tienté, and finally, that of the viper, the living animal being made use of. Wounds were made upon the back and thighs of full-grown rabbits, and when the blood had ceased to flow, two or three grains of strychnia, or two or three drops of prussic acid, were introduced into the wounds, and after intervals of three, five, and ten minutes, a cupping-glass was applied, which was renewed as often as it fell off. No symptoms of poisoning occurred in these cases, but if the precaution was neglected death ensued.

A cupping-glass applied to a wound into which some strychnia had been put, prevented the effects of this substance from manifesting themselves, and also suspended them when beginning to be apparent. Eight grains of white arsenic were introduced into a wound in the thigh of a dog; three quarters of an hour after, a cupping-glass was applied to the wound, and kept on for four hours, and the animal suffered no inconvenience. Another dog similarly poisoned, and left unassisted, died at the end of fifteen hours.

Six drops of prussic acid were poured into a little wound made in the thigh of a rabbit; the cupping-glass was applied for twelve minutes, and the animal showed no signs of having been poisoned; but when it was taken away, convulsions came on so suddenly that it was thought to be dead, but a fresh application of the cupping-glass restored it to its former state of tranquillity; the same effects ensued upon removing it again, and it was only half an hour after the introduction of the poison that it could be removed with impunity. Another rabbit, treated with the same quantity of acid, where no cupping-glass was used, died in two minutes.—*Med. Rep.* ii. 176. *Med. Jour.* lv. 67.

2.—*Employment of the Acetate of Ammonia in Drunkenness.* By M. Masuyer.—This remedy dissipates all the symptoms of intoxication, in the gentlest and most fortunate manner, without occasioning the inconveniences of pure ammonia, which has been hitherto employed. The manner of using acetate of ammonia in drunkenness is extremely simple: it is sufficient to put 25 or 30 drops of it into a glass of sugared water, and to give the draught to the person intoxicated. When there is indigestion with vomiting, and the draught is thrown up, a second draught should be given. When it is not thrown off the stomach, if it should not produce a good effect in five or six minutes, the person should take half a dose more. In megrims it may be given the same cold, in the dose of 30 or 40 drops; or warm, if not successful cold, in a glass of infusion of lime-tree flowers in the dose of 20 drops, with a second glass ten or fifteen minutes afterwards. In general, the disorder of the head rarely resists the second or third glass of the remedy.—*Rep. Pat. Inv.* vi. 167. *Archives des Decouvertes*, 1826, *p.* 207.

3.—*Purple Precipitate of Cassius.*—Dr. Clarke appears to have determined that in the purple precipitate of Cassius, obtained from the muriate of gold by means of the muriate of tin, the two metals are thrown down as oxydes, which however do not chemically combine in a constant relative proportion to each other. The proportion of tin always exceeds that of the gold, and the difference observable in the hues of the precipitate, made at different times, is to be ascribed to the different proportions in which the oxydes of the two metals have combined together, and perhaps also, to their different degrees of oxydation.—*Cam. Ph. Tr.* i.

4. *Composition of Apatite.*—According to M. Rose, the apatite from the following localities gave the annexed proportions of chloride and fluoride of calcium, the rest being phosphate of lime with occasional traces of iron and magnesia:—

	S. G.	Chlo. Calc.	Fluor. Calc.
Apatite from Snarum in Norway	3 174	4.280	4 590
Cabo de Gota in Spain	3.235	0.885	7 049
Arendal	3 194	0.801	7.010
Greiner in the Tyrol	3 175	0.150	7.690
Faldigl, ditto	3.166	0.100	7.620
St. Gothard	3.197	trace	7.690
Ehrenfriedersdorf	3.211	trace	7 690

Annales de Chimie.

5.—*On the Determination of the Mean Temperature of the Air.*—This subject has been investigated by M. G. G. Hallstrœm, who gives the following algebraic formula, which correctly represents the mean temperature for all Europe.

$v = \frac{1}{2}(x f + x e) - 0.33 + 0.41 \sin [(n - 1) 30° + 124° 8')]$

v = mean temperature.

n = the ordinal number of the month for which the temperature is to be calculated (thus, for March, $n = 3$).

$\frac{1}{2}(x f + x e)$ = the mean temperature taken as the mean of observations taken at ten o'clock in the morning and evening.

In winter $\frac{1}{2}(x f + x e) = v$ very nearly; whilst, in summer, this quantity is $\frac{3}{4}$ of a degree greater than v at Paris, Halle, and Abo.—*Annal. der. Phys. und Chem.* 1825, *p.* 373.

6.—*On the Barometer.*—The following are conclusions at which M. Bohnenberger has arrived relative to the barometer. 1st, The surface of mercury in a tube 14.5 lines in diameter, is slightly rounded at the edge; but, at the distance of two lines from the glass, capillary depression disappears, and the surface is level. 2d, The mercury in a tube 5.8 lines in diameter, is convex over the whole surface, the depression being .035 of a line. 3d, The depression is generally less in a vacuum than in the air, so that a syphon barometer gives results too high, and the more so as the tube is smaller. 4th, Barometers constructed with tubes five lines in diameter, do not require tapping to cause them to assume their proper height; and comparatively slight blows easily make the mercury rise too high in tubes of a smaller diameter.—*Annal. der Phys. und Chem.*

7.—*Heat evolved from Air by Compression.*—By a mathematical investigation of the heat extricated from air, when it undergoes a given condensation, and by a reference to the experiments of Clement, Gay Lussac, and others, Mr. Ivory finds, that "*the heat extricated from air, when it undergoes a given condensation, is equal to three-eighths of the diminution of temperature required to produce the same condensation, the pressure being constant.*" Air, under a constant pressure, diminishes 1.480th of its volume*, for every degree of depression on Fahrenheit's scale; and therefore one degree of heat will be extricated from air, when it undergoes a condensation equal to $\frac{1}{480} \times \frac{3}{8} = \frac{1}{1280}$. If a mass of air were suddenly reduced to half its bulk, the heat evolved would be $\frac{1}{2} \div \frac{1}{180} = 90°$.—*Phil. Mag. N. S.* i. 89.

8.—*Specific Gravities, &c. of Vapour.*—The following table is from Mr. Tredgold's work on the steam engine.

SUBSTANCE.	Specific gravity in liquid state, water being unity.	Specific gravity in vapour, air being unity.	Volume of vapour for one of liquid at 60°, and 30 in. mercury.	Constant No. for formula.	Volume at the boiling point of the liquid.	Boiling point. (Fah.)
Water	1·000	0·625	1324	76.5	1711	212°
Alcohol	·825	1·6133	422	24.5	476	173°
Sulphuric ether	·632	2·586	203	11·7.	220	104°
Sulphuret of carbon	1·272	3·6447	393	23	440	116°
Naphtha	·758	2·833	224	13	280	186°
Oil of turpentine	·792	5·013	130	7·5	193	316°
Oil-gas liquid..............	·850	2·700	260	15	837	186°

Rep. Pat. Inv. vi. 109.

* At 32° F.

Original Communications.

I.—*On Hygrometry.*

To the Editor of Gleanings in Science.

SIR,

The subject of hygrometry is one of such curiosity and interest, that I hope you may be able to give an early insertion to the accompanying paper. Its principal object is to elicit discussion, by attracting the attention of observers to a department of meteorology in which, notwithstanding the progress latterly made, much yet remains to be done, before the scientific edifice can be considered complete. And if the remarks I have ventured to make on the errors which disfigure a work by many considered to be of authority, should serve but to teach distrust to those who take every thing for truth that has the IMPRIMATUR attached, they will have done some service. Many valuable facts might be collected in this country, by those who perhaps have neither the time nor the inclination to enter into all the niceties of the theoretical question, or wade through the useless heap of algebraic notation, with which it is the fashion of many writers to perplex plain men, and obscure a simple subje t. Such observers are content to take the result, without any examination of the steps by which it was obtained; and to them it must be of consequence that such result be correct.

I am, Sir,

Yours obediently,

D.

Hygrometry has been attracting in Europe more attention latterly, than it had heretofore obtained, chiefly, it appears to me, from the happy experiments and deductions of Mr. Dalton, who I think must be considered the father of scientific hygrometry. Before his time, to the generality of philosophers this subject proved an impenetrable mystery. For although the exp riment of the Florentine Academicians was made some centuries ago, and though Le Roy had pointed out a simple method of observing what is now called the temperature of deposition, yet no use seems to have been made of these valuable hints, till Mr. Dalton, by his ingenious discoveries, stamped a certainty and a value on these researches which they before wanted That I do not overrate what we owe to this celebrated philosopher, will be evident to any one who considers the numberless unsuccessful attempts made to construct an accurate and universally comparable hygrometer, while as yet no standard of comparison or means of verification existed. We see, even in following the course of De Saussure's labours in his attempts to investigate the theory and value of his own invention, (the only instrument that has stood the test of subsequent examination,) what difficulties he laboured under, from ignorance of those valuable facts which the ingenuity and perseverance of Dalton first brought to light. And in particular, we cannot but admire the fact of its most beautiful property having been detected very recently, by one who had the advantages which De Saussure wanted*.

Of the many hygrometers that have been proposed, depending on the expansion and contraction of organic substances, it must be confessed, that this of De Saussure's alone deserves the attention of the scientific observer. For, besides the beautiful property above alluded to, it has the great merit of always speaking the same language; that is to say, that under the same circumstances its indications will be the same. It has still another merit, that of being extremely sensible, so that its indications may be used with the greatest confidence to determine the actual quantity of moisture in the air at any particular moment, at least within two or three per cent. of the truth. But notwithstanding these advantages, there are still, I conceive, objections which apply to this instrument, that will militate against its general adoption. Those objections have a reference to the delicacy of the instrument, and its consequent unfitness for transport or hard usage, to say nothing of those which arise on the score of economy. For the ordinary observer, there can be no doubt that the method of dew points, as founded on Dalton's general law, is preferable. Even to the possessor of this instrument, some such means

* The relation which its indications bear to the hyperbola, as discovered by Gay-Lussac. From the experiments of Mr. J. Prinsep however, which form the subject of a paper in the 43d No. of the Journal of Science, it appears that the figure is a parabola.

of verification must be resorted to, for as Mr. Prinsep observes, the graduation of the instrument makers cannot be depended on.

Mr. Dalton's law is this, that at a given temperature, whatever be the density of the air, or even if there be no air, there exists the same actual weight of moisture within a given space, supposing that space to be saturated with invisible vapour. All that we have to do then, is to determine the temperature of saturation, that is the temperature below which if the vapour be cooled, a portion will be deposited in the form of dew. That temperature, with the tables of Mr. Dalton, will immediately give us the tension or elastic force of the vapour; and from that element, knowing the specific gravity at 212°, and correcting for the temperature of deposition, the actual weight of moisture is easily deduced.

Mr Dalton's method of determining this temperature, is the same as the experiment of LeRoy, before alluded to. Water contained in a common drinking glass was cooled down, either by dissolving saline mixtures in it, or ice till moisture is observed to be deposited on the sides of the glass. The temperature of the water at this moment, as given by a thermometer constantly immersed, is taken as that of saturation, and the tension as well as weight of aqueous vapour due to it, as that of the vapour actually existing in the air. It is evident, that the solution of the problem may be thus rendered quite perfect, guarding against such sources of fallacy as are sufficiently obvious: the only desideratum seems to be some more ready method of arriving at the conclusion. The process described, though abundantly simple, requires still a little time and attention. It also requires, that the observer should be provided, at all times, with some method of cooling the vessel or substance, on which the moisture was to be deposited. This want of some more ready as well as portable apparatus, must have led the way to Daniel's hygrometer, an instrument founded on the above principle, and which appears to have completely answered the inventor's expectations, in the temperate climates of Europe.

Even in Europe, however, it would seem to be attended with much the same inconvenience as the older and cheaper apparatus, imposing as it does, on the observer, the necessity of carrying with him a supply of ether, and requiring the same time and attention in performing the experiment. The first objection is not perhaps important in the more civilized countries of Europe, but in India it is otherwise. Here we must always prefer the apparatus and ingredients, which are most easily replaced or replenished; and consequently, saltpetre and sal ammoniac, which may be had in every bazar, are preferable to the ether required for Mr. Daniel's instrument. On the score of economy too, there can be no comparison between the two methods. But there is a still more serious objection to the use of Mr. Daniel's hygrometer in India. It is the almost total want of action under which it labours, owing to the impossibility, I suppose, of keeping our ether in any thing like a state of purity, in temperatures which for some part of the year exceed that of its boiling point, and for the greater part are not removed perhaps 10° from it. It is evident, that no stopper will confine the vapour of ether in a temperature of 96°. Even at 86°, it exerts a force equal to nearly 25 inches of mercury. Accordingly, I have never yet been able to obtain a correct result with it. So far from the depression reaching the dew point, which is I believe occasionally even 60° below the temperature of the air, I have never been able, however liberal of my ether, to obtain a greater depression than 6° *.

Finding then, that this hygrometer, whatever might be its superior facility and neatness of action in Europe, over the more simple, cheap, and available apparatus used by Mr. Dalton, was altogether useless in this country, I gladly turned to the consideration of another process, by which, it was averred that the temperature of saturation (or the dew point) might be readily determined, though indirectly. This process was, I believe, first suggested or tried by Dr. Hutton of Edinburgh, and has been since adopted by Professor Leslie, or re invented, for I do not pretend to decide so delicate a question. The process consists in determining the difference of temperature between the air, and a moist evaporating surface. It was assumed, that such evaporation would be some function of the hygrometrical state of the air and temperature jointly, and that consequently, the depression of temperature of the evaporating surface would also be some function of the same. The experiment is readily and most conveniently performed. A thermometer, the bulb of which is covered with cambric muslin, being wetted, is hung up, and the depression below one

* This was at a time, however, when the air was was not so dry, the dew point being by Mr. Dalton's method about 14° below the temperature of the air.

similarly coated, but dry, observed. Here the apparatus is portable enough, eminently cheap, and requiring no auxiliary substances except a little water. So far it has every claim to our notice as superior to every method yet proposed. But unfortunately, when we come to determine from this result, so easily and cheaply obtained, the actual moisture in the air, we find ourselves beset with difficulties, "puzzled with mazes and perplexed with errors."

The process, as I said before, has been adopted by Professor Leslie. By applying it to his differential thermometer, he has, to those not conversant with the subject, in some measure complicated and obscured it. In particular, he loses the great advantage of expressing the depression of the thermometer, in the ordinary scale of that instrument. There are some other objections to the use of his hygrometer, which it is the less necessary to dilate on, as it is not probable that any one will ever prefer it to the more convenient, available, and economical arrangement of the two thermometers.

Professor Leslie has given a formula for converting the depression of the moist thermometer, into an expression of the moisture in the air. His solution of the problem is, however, disfigured by the peculiarity of his views regarding the solubility of water in air: nor indeed, does it appear very intelligible even with this allowance. It is further grounded on an incorrect assumption of the value which the capacity of air for heat bears, when compared with that of water. Taking, however, his meaning to be such as I understand it, and correcting the constants, I have found it agree tolerably well with a great majority of the experiments, with which I have had an opportunity of comparing it. In this respect it is indisputably superior to the only other solution of the problem, with which I am acquainted,—that given in the article Hygrometry in Brewster's Encyclopædia.

This remark is the more called for, as the author of the article in question pronounces with the greatest confidence against the Professor's formula in favour of his own. At p. 587, comparing the result of his calculation with that given by the Professor, he says, "This result would correspond by the table to 66° nearly, being 7° higher than the point of deposition found by Mr. Leslie." Now, it is not a little singular, that in this particular example the dew point must have been extremely near 59°, the Professor's determination. In fact, in a great majority of instances, the formula of the latter, though not universally true, will give very tolerable approximations, while that of the former leads in almost every case to erroneous results, in many to impossible. The comparative excellence of the two formulæ may be easily appreciated by any one, who will take the trouble to calculate, according to each, the results of a valuable set of experiments on this subject made by M. Gay-Lussac, and inserted in the 15th vol. of the Journal of the Institution.

The errors in the article we have just noticed, are truly surprising to those who consider the general character of this work, and the attainments of the editor who conducts it. It was not, indeed, for a long time that I could satisfy myself, that the fault was in the author rather than in his reader; not in fact until repeated consideration had familiarized me with the subject, and enabled me to detect the mistakes and paralogisms with which it abounds. It is a wish to save others from that loss of time which I have suffered, that induces me now to notice it; and it is the more necessary, because the author has followed up the subject in the Edinburgh Journal, vols. xii and xiii, where two papers are given, in which every error of the above article is repeated. Nor have I yet seen any attempt to detect these errors. Mr. Daniel in his work simply notices the article, and supposes the method itself uncertain, because the solutions of the theoretical question are no two alike. It will not then, I trust, be without its use to enter a little into particulars, and show the grounds on which I object to the solution given in that article.

In § 54, p. 583, the author thus announces the principle upon which his investigation is founded. "Since the quantity of water evaporated in (under) the same circumstances is proportional to $F-f$ it follows, that the cold produced by evaporation must be a function of the same quantity. Therefore if D represent the reduction of temperature by evaporation, from the moistened bulb of a thermometer covered with some bibulous substance, F the entire force of vapour for the temperature of the air t, and f the force of the vapour actually existing in the atmosphere; we may evidently have an equation of the form $D=m(F-f)$ in which m is a constant to be determined by experiment *and afterwards modified, if necessary, by a correction depending on the diminished temperature of the evaporating surface*." It would be a fortunate thing for hygrometry, if the relation between the quantities could be expressed by so simple an equation as this; but that it is a much more complex relation*, will be evi'ent to every one who has considered the subject. It may be here sufficient to ob·erve, that the assumption on which the whole investigation is based, is notoriously contrary to experiment Add to this, that the varying specific heat of the water for different temperatures must be considered The passage in italics is not very intelligible. Why shou d it be necessary to modify the co-efficient which, it is said, is to be determined by experiment, when that very dimiuished temperature is obviously one of the conditions of the experiment?

In § 56, he goes on to say: "It must be evident, however, from the view we have taken of the cooling process, that a thermometer with a moistened bulb ought to be reduced through the same number of degrees in equal times" Now the very reverse of this is evident, if we are o adopt the account of the process given in § 55, in which it is stated, that the evaporation being always equal in equal times, depending as he says on the constant quantity $(F-f)$ is modified by the influx of caloric, which being ne.rly proportional to the depression, increases as that increases, so that the apparent velocity of the cooling process is continu.lly diminishing, till at length the ingress of heat balances the egress, and the thermometer is then stationary. Nothing can be more glaring than this misstatement of his own views, or more calculated to puzzle the student.

But these inconsistencies are trifling, compared with the algebraical process employed in the treatment of the equation $D=m(F-f.)$ In Art. 57, it is said: "Since the extent of the evaporation, together with the reduction of temperature which it occasions, is diminished by the cooli g of the evaporating surface, the expression $D=m(F-f)$ will require some correction, and as this correction must have a direct relation to D, the simplest way of applying it is to give the equation this form $D-\frac{D}{n}=m(F-f.)$" Now it is quite evident, that these two equations are perfectly similar and of the same import, the new form merely leading to a change in the value of the constant m, the one being to the other in the ratio of $D : D-\frac{D}{n}$. Indeed, the author himself gives the following transformation of it $D=\frac{m\,n}{n-1}(F-f)$ and immediately after, substituting p for $\frac{m\,n}{n-1}$ it becomes $D=p\,(F-f)$. This formula differs in no respect from the original one; so that the reader is left to wonder what correction has been introduced, or how the suostitution of p as a constant for m can have improved the formula.

The next step is, however, more important. "To render the expression consistent with the properties of the curve, whose ordinates represent the progressive reductions of temperature by evaporation from the moistened bulbs, *it seems necessary* to give it the form $D=(p-\frac{D}{r})(F-f)$" &c. Now, it is worthy of remark, that this curve, the properties of which are made to effect this very important change, is not a curve drawn on a scale to represent the actual depressions in equal times, nor yet one the nature of which is defined by any equation derived from the conditions of the experiment, but a mere hypothetical diagram drawn for illustration, and as such, being entirely arbitrary, may be conceived to have the properties of any curve whatever.

The expression having now taken this form by a purely gratuitous assumption, we are naturally led to ask, Would it not have been more intelligible and mo.e direct to have begun with it as an avowed assumption? In fact, as the formula was meant to be deduced empirically, the wonder is why the author should waste his time and the patience of his readers, in an attempt to obtain it by reasoning *a priori*. A more important fault, however, is that the formula, though so arbitrarily moulded to the form desired, will not represent the facts.

"A series of the most laborious experiments," it is stated, "was undertaken to determine the value of the factors p and r." Two only, however, are de-

* M. Gay-Lussac, whose experiments on the subject I have already referred to, appears to have considered the question to be of so complicated a nature, as not to be worth the trouble and time which its investigation would demand. He preferred abiding by the experiment of Le Roy. In this opinion he is countenanced by the editor of the Journal of Science.

tailed, and in these even the numerical operations are full of errors ; errors too which it is evident, are not attributable to the press. But in fact, the labour of these experiments might have been spared, and the inapplicability of the formula clearly established, by considering the results to which, in particular cases, it will lead. Let us suppose, for instance, two cases, in one of whic , the temperature of the air is 100° and that of the dew point 91°5′ : in the other 64° and 30°. In each case $F - f = .4$in. of mercury nearly, and therefore, according to the principle on which this formula is founded, D should be equal to D′ ; whereas their ratio is that of 1 to 4 nearly, as any one may satisfy himself by experiment ; nor will t e arbitrary change which the original expression is made to undergo, help the argument. In this case $(F - f)$ being constant $D : D' :: p - \frac{D}{r} : p - \frac{D'}{r}$ Now p and r being constants, if we suppose D′ to be greater than D, then will $p - \frac{D'}{r}$ be less than $p - \frac{D}{r}$ whereas by the hypothesis it is greater, which is absurd. It is truly surprising, how such an obvious conclusion could have escaped any one who had paid common attention to the subject.

The hypothetical case I have adduced, though it places the error of the formula in so clear a light to those who are at all familiar with the subject, may yet for want of the experiments alluded to, fail of carrying conviction to those who have not the means of performing them. It may not, therefore, be without its use to adduce a *practical instance* of the curious results an observer may be led to, by the employment of this formula.

The full expression in which a correction for the barometer is included, is

$$f' = F - \frac{\frac{1}{8}\, B\, D}{180 - \frac{1}{2}\, D}$$

To apply this:—in April I observed the temperature of the air to be 93° and that of a moist bulb thermometer 62° ; the barometer being in. 27·7. Substituting these numbers in the above formula, we get 63°.4 as the point of deposition. This result is 1°.4 above the temperature of the moist bulb thermometer, and is too absurd to require a single remark.

But I have not yet done with the mistakes of this article. In the preceding formula, the depression is assumed to be inversely as the barometer, other things being alike. This conclusion, it is said, is derived from an experiment performed with an air pump, the particulars of which are fortunately given. Dry air being enclosed in the receiver, and kept in that condition by means of concentrated sulphuric acid, a Leslie's hygrometer was exposed to its action. The gauge, which at the commencement of the experiment indicated the ordinary density of 29. 6, was successively depressed to 23. 6, 17. 6, 11. 6, and 5. 6 inches, and the corresponding indications of the hygrometer were found to be 27, 34, 44, 62, 91. These numbers are pronounced to be inversely as the first set. The first three certainly are, nor is the fourth very far removed ; but the fifth obviously bids defiance to any such law. Supposing the indications of the hygrometer to bear this relation to the densities, they would be 27, 34, 45, 69, and 143 ; the last number differing by 52 degrees from the observed result.

It is, however, not improbable that in the progress of the experiment, the moistened cover of the bulb had become nearly dry, and this supposition, as the great discrepancy occurs only in the last experiment, might furnish a sufficient answer to the objection. It might even be said, (and with jus ice,) that the hypothesis which will bring out true results for an atmosphere so rarefied as only to support a column of 11. 6 inches of mercury, possesses all the accuracy which is sought for, or required in a practical formula. Unfortunately, however, the law is not accordant with facts, even in this limited application, excepting in a case scarcely ever likely to happen in nature, that in which the experiment was performed,—the case of perfectly dry air. In extending it to air of every possible variety of hygrometrical condition the author was obviously blind to the very erroneous conclusions to which his too hasty generalisation will in most cases lead. In fact, as the degree to which air can be rarefied may be considered to have no limit, except that of a vacuum, so the depression might become, were this the true law, INFINITE ; a result which though, perhaps, involving no absurdity in the case of dry air, can never be applicable to an atmosphere holding any vapour in suspension. In air of this condition there must be a limit to the depression; otherwise, the temperature of the moistened thermometer might be reduced below that of deposition, which is obviously impossible. Thus let us suppose, as in one of the experiments, air having a temperature of 67.2, the moistened thermometer to be 52°, and the point of deposition 35°.7, the barometer being 29.75, air of this condition being enclosed in a receiver and rarefied to a barometer of 14.88, the point of deposition being kept stationary, the moistened thermometer ought to be depressed to 37°. A very little more rarefaction would be sufficient to reduce it below 35° 7, and were the gauge lowered to 7·44 inches, the thermometer ought to indicate 7°2.

The truth is, that the only general result deducible from this experiment is, that the EVAPORATION *is inversely proportional to the density;* a result in itself sufficiently probable*. That this is a very different conclusion from the one adopted, will be made clearer by considering, that the depression of temperature is not measured by the absolute evaporation, but by the proportion which the quantity evaporated bears to the mass to be cooled. Thus, suppose a mass of water to be cooled weighing 1000 grains, and a second, the weight of which is only 500. Let the rate of evaporation in the first case, be double what it is in the second; the depressions will still be equal. For if we assume the latent heat of water to be 1000°, the evaporation of one grain will in the first case produce a fall of 1°. But during the time in which one grain is converted into vapour, in the first supposed state of things, half a grain is lost in the second. Now this is precisely the quantity required to be evaporated, to produce a depression of temperature of 1°. It is to an ignorance of, or inadvertence to, this very obvious principle, that most of the errors in this and the other papers of the author are attributable.

But the rate of evaporation even, as before observed, is not correctly estimated. Dalton had shown by his experiments, that it was very nearly as the difference of tensions due to the temperature of the air, and that of the dew point, unless, he adds, *when the temperature of the evaporating surface is different from that of the air.* In that case, the tension due to the former must be taken instead of that due to the latter. The author of this article however takes $F - f'$ as the function which influences it, although the difference of temperature may amount in some cases to 30°, or even more. It is, I think, a question whether even Dalton's theorem would lead to correct results, when the difference of temperature of the air and evaporating surface is great, even though the diminished tension due to the latter should be the same. But however this be†, any possible error which might spring from our neglecting this modification of the expression, is a mere nothing compared with that resulting from the above evident mis-statement; which would, indeed, in some cases, represent the ratio of evaporation as three or four times too great.

The author of this article is one of the latest writers on the subject of hygrometry. His paper in the Edinburgh Journal, on the application of the preceding formula to the correction of heights measured by the barometer, is I believe the very last publication connected with this subject. Mr. Daniel's book is chiefly calculated for the possessor of his instrument, and gives no information on the subject of the present paper. In Rees's bulky Cyclopædia, there is nothing applicable to this problem; but in the Supplement to the Encyclopædia Britannica, under the head Meteorology, there is an account of Professor Leslie's solution. It is certainly ingenious, though as I before remarked, disfigured by being conveyed in the language of a hypothesis in the support of which Mr. Leslie stands, I believe, alone in the scientific world. Another fault it has in common with all of the Professor's writings which I have perused, that of being drawn up in a style so involved and laboured, as to be very often obscure, if not unintelligible.

His solution of the problem, as far as I understand it, is this. Air will take up a quantity of moisture which is proportional to what it requires for full saturation. A thermometer with moistened bulb must then, lose moisture in this proportion, and consequently will have its temperature depressed in an equal ratio. This solution is any thing but empirical, and yet it represents tolerably well the facts in a great majority of the experiments with which I have compared it. But that it cannot give correct results in every case is obvious. It involves the same paralogism which is found in the other solution,—the proposition, that the depression

* De Saussure has stated that the densities being 1 : .66, the evaporation will be as 1 : 2.

† Perhaps in the case under consideration, this modification of the process may be neglected; because, the temperature being stationary, it is evident that the depression which has been attained, is always the same function of the diminished tension whatever its value.

is proportional to the evaporation,—a proposition which I have shown is untenable. It contains another error*, which is common to both investigations, and which I shall now proceed to explain.

In considering the process which takes place, when a thermometer with moistened bulb is exposed to the power of evaporation, we at once perceive that it is not until the depression has attained its maximum, and is consequently stationary, that we can deduce any useful result. It is then that two forces or powers, the value of one of which we are seeking, are in *equilibrio:* this value we obtain in consequence of its equality with that which is known. For at this period of the experiment, it is evident, that the indefinitely small decrements of caloric, the effect of evaporation, are balanced or neutralized by the indefinitely small increments arising from conduction and radiation in the equally small moments of time. By determining the latter, we obtain an expression for the heat lost by evaporation ; from which may be deduced the rate of evaporation ; thence the tension of the moisture in the air ; and lastly, its weight.

Thus we see, that every thing depends on our correctly estimating the increments of heat, furnished by conduction and radiation to the moist bulb thermometer, depressed a certain number of degrees below the temperature of the air. Newton supposed that these increments were as the differences of temperature of the body to be cooled (or heated), and that of the medium. This hypothesis was shown by experiment to be incorrect, but it is only lately that MM. Dulong and Petit, by separating the effects of these two causes, have established the true law. With regard to radiation, the effect of it is so small in comparison with that of conduction, in the case we are considering, and being indeed easily got rid of altogether, we may leave it out of the question. The rate then, at which a body, cooled below the temperature of the atmosphere, receives caloric, is proportional, not to the simple difference of temperature, but to that difference raised to the 1.233 power. Thus, when the depressions are as 1 : 2, the increments and consequently the decrements will be as 1 : 2.35. This very important law is entirely unnoticed in either of the solutions of which I have given an account.

Notwithstanding the failure of both these attempts, the theoretical solution of the question appears to me sufficiently obvious. In applying it to practice is the sole difficulty, owing to our not having any correct determinations of the constants, or any series of carefully conducted experiments of the corresponding dew points to various depressions of the moistened thermometer. In the hope of exciting the attention of inquirers, and of eliciting some valuable contributions to the subject, I shall terminate this paper, by giving what appears to me the most direct solution of the problem. The application of the thermometer to determine the moisture in the air, is every way so desirable, that no means should be neglected of fixing the subject on a secure basis.

Let e = evaporation in grains from a surface of one square inch for one inch hygrometric tension, and barometer 30 inches in one unit of time.

For any other tension $(F'-f)$,† it will be $e\,(F'-f)$, in which F' is the tension due to the indication of the moist thermometer ; and f, to that of the dew point.

For any other surface s, it will be $s\,e\,(F'-f)$, and for any other barometer B

$$\frac{30.\ s\,e\,(F'-f)}{B}$$

Let w represent the weight *in grains of water* of the mass to be cooled, and L the proportional part to be evaporated of any mass of water at the temperature t, to produce a fail of 1°, then will the evaporation of $\frac{w}{L}$ evidently produce a fall of 1° in the temperature of w.

Now let i = increment of caloric in the unit of time for depression D and barometer $B = c + r$, where c is the effect of conduction, and r is that of radiation.

Then will $\frac{w\,i}{L}$ express the amount of evaporation in grains, in the unit of time.

But $\frac{e\,(F'-f)\,30}{B}$ was also found to be an expression for the same, therefore

* It has, however, greatly the advantage of the other solution, by adopting the tension due to the temperature of the evaporating surface, instead of that due to the temperature of the air.

† F' represents the tension of vapour due to the temperature of the moist thermometer ; f as before, of that actually existing in the air.

the two expressions are equivalent, and by transformation will become $i = \frac{L\, s\, e\, (F' - f)\, 30}{w\, B}$ or substituting $c + r$ for i, $c + r = \frac{L\, s\, e\, (F' - f)\, 30}{w\, B}$

Let $r = (f')\, n'$. n' being a co-efficient determined from experiments with a thermometer, the surface of which $= s'$ and mass (expressed in grains of water)$= w'$, and (f') representing the particular function which r is of n'. Then for any other thermometer with surface s and mass w, the value of r will be $= (f')\, n' \frac{s\, w'}{s'\, w}$

In like manner, let $c = (f)\, n$ for surface s' and mass w'. For s and w, it will be $(f)\, n\, \frac{s\, w'}{s'\, w}$

Now substituting these values of c and r we have

$$(f)\, n\, \frac{s\, w'}{s'\, w} + (f')\, n' \frac{s\, w'}{s'\, w} = \frac{L\, s\, e\, (F' - f)\, 30}{w\, B}$$

Dividing by $\frac{s}{w}$. $(f)\, n \frac{w'}{s'} + (f')\, n' \frac{w'}{s'} = \frac{L\, e\, (F' - f)\, 30}{B}$

From this change of the equation, we see the reason why, whatever the size and shape of the thermometer, the depression is still the same, no function of the size or shape being found in the equation. $n \frac{w'}{s'}$ is a constant quantity from whatever thermometer it be determined, because it is certain that n varies inversely as $\frac{w'}{s'}$ the product must therefore continue the same.

With regard to the nature of the functions (f) $(f',)$ MM. Dulong and Petit have determined them apparently with every accuracy. $n \frac{w'}{s'}$ must be found from observation. A table of the values of L may be found in a late No. of the Quarterly Oriental Magazine, and of F and f in the article Hygrometry before referred to: e has been determined by Mr. Dalton; but from some trials made by myself and others, his determination does not appear quite satisfactory. I shall resume the subject in another paper, in order to discuss and fix these several particulars, and by comparing the results of the formula with the best observations, show what are still the desiderata in this question.

II.—*Description of the Animal of Ampullaria, a Genus of fresh-water Testacea, with a Notice of two Species inhabiting the fresh-waters of the Gangetic Provinces.*

It being desirable, that the animals which inhabit shells, should be studied and described by those who have opportunities of taking them alive, with a view to their more natural arrangement, I have little hesitation in offering the following observations on the animal inhabiting the shell of the genus Ampullaria of Lamarck and Cuvier, inasmuch as no notice on the subject has, to my knowledge, yet appeared.

The genus Ampullaria is placed by Lamarck in his fourth family of Trachelipodes, "les peristomiens," which includes Paludina and Valvata. The animal is stated in Dubois' Epitome to be the same as that of his third family, "les Melaniens," in which the animal has two tentaculæ, with the ocular points (at least in Melania) on their upper side, at a short distance from their base.

By Cuvier, Ampullaria is placed as a subgenus, with Melania and Phasianella and Janthina, under Conchylium, the third division of his Pectini branches. Cuvier says: "The animal has not yet been described, but it is probable that it more or less resembles that of Paludina."

In Ampullaria the ocular points are placed on the summits of pedicles situated at the external base of the tentaculæ, as in the genus Nerita; while in Paludina the eyes are placed on the tentaculæ at about a third of their length. The great thickness of the tentaculæ below the eye favours the supposition, that in the last-mentioned genus the pedicle, which in Ampullaria is free, is included within the epidermis of the tentacula, from the side of which its summit only is exposed.

The animal of Ampullaria differs also from Paludina, Nerita, or any other neighbouring genus, by the elongation of the lobes of the head on each side of the mouth, the genæ being produced to about a third of the length of the tentaculæ than which they are stouter, and possess, but in a less degree, the power of extension and retraction; like the rest of the head, they are of a black colour, while the tentaculæ are dirty yellow*. The mouth is vertical, and occupies the sinus between them. At first sight the animal appears to have four tentaculæ, and it is only on more particular examination that the illusion is dispelled. These retract, ile lobes probably serve as palpi to explore substances, and to enable the ampullaria to select its food.

In other respects it resembles Paludina. As in that genus, the ala is formed into a tube for conveying water to the branchiæ. This tube it can extend to some length; a bubble of air frequently escapes from it. The ampullariæ hereafter mentioned have calcareous opercula; of Paludina I have shells with calcareous as well as horny opercula.

The Paludina has setaceous tentaculæ, which it can roll up spirally on a plane, while in the genus under consideration the tentaculæ are somewhat obtuse at the summit, and do not appear to have so much action. Like Planorbis, Lymnea, and as I have sometimes observed, Paludina; it possesses the power of floating, as might be supposed from the lightness of the shell in comparison with its capacity. On going to examine, at night, some specimens which were placed in a tub of water, I observed one, which had crawled up the side to the surface of the water, detach itself from the tub and float away. I have not observed them do this in a smaller vessel, out of which, if left in the dark, they are apt to crawl and precipitate themselves to the ground. This faculty of floating must be of much use to them in ponds and jheels, enabling them to pass from one plant to another without descending to the bottom. To these plants they are generally found adhering, just under the surface of the water.

The following are descriptions of the only species I have met with in the country.

1. Ampullaria — Shell thin, ovate-globose, with transverse fuscous bands; epidermis olivaceous, polished; spire obtuse, pale-orange, bordering on purple in some specimens; aperture chesnut, with fuscous bands: outer lip reflected in old specimens: inner lip yellow, sometimes tinged with purple.

2. Ampullaria. — Shell ovate-globose, brownish yellow; with longitudinal stripes of a darker colour in young specimens; spire obtuse, generally eroded and greenish in old specimens, in which also the outer lip is reflected; aperture, white; lips, yellow or orange.

The shell of the last described species is rather thicker than the first, and the epidermis is often worn away on the body-whorl, near the inner lip, by the operculum, which is turned back on that part while the animal is crawling. It attains a much greater size than the first species, and is generally found in jheels, while the other appears to affect ponds and tanks, though it sometimes accompanies the second species in jheels. They are found in most parts of the Gangetic provinces, but the second species is the most generally distributed. It is probable, that one or other of them is to be met with in the fresh-waters near Calcutta, as I have observed the shells dug up, with their opercula, in the neighbourhood of Calcutta, and included in the clay banks of the *Bhagirathi* at different places

I am almost doubtful whether these two shells should be considered as separate species, or merely classed as varieties of the same, on account of the similarity of form; but as they are found sometimes together, and at other places one species is to be found to the total exclusion of the other, I am inclined to consider the differences observable sufficient to constitute them separate species. Not having the second part of the 6th vol. of Lamarck's Animaux sans Vertebres, I am unable to state whether these shells are described in that work, and have therefore declined naming them from an unwillingness to run the risk of increasing the number of synonyms. The first species appears to be nearly related to, if not identical with A. reflexa of Swainson; but as he does not give the colouring of the body of the shell, having apparently taken the characters from an uncoated specimen, it is impossible to determine.

W. H. B.

* This description was taken from the second species. If my memory serves me, the colour of the first species is darker.

Should the present communication prove acceptable, I have it in contemplation to forward, when leisure permits, other short notices on subjects connected with natural history, which may prove interesting from their novelty.

Great allowances should be made for correspondents in these provinces, situated at a distance from any public library or museum, and indebted for all the knowledge they may possess on the subject of their remarks to a limited collection of books and their own meagre collections of natural objects. I shall only add, that truth being the grand desideratum in natural science, those who cultivate it must always receive with thanks any correction which may be necessary when they have erred either from ignorance or inadvertence.

III.—*On the Rise and Progress of the Lithographic Art in India, with a brief Notice of the native Lithographic Stones of that Country.*

It is now thirty years since Senefelder first discovered and nursed through its struggling infancy, an art, which in its giant growth has already added another and powerful lever to that mighty engine the PRESS. At the time when it burst upon the astonished sight of the literary world, any possible addition to the established methods of printing even by mechanical means, would have been scouted as a chimera, but to attain the unthought of object by a chemical process, peculiarly simple as to its theory, but requiring the nicest exercise of the judgment in its practice, would have been pitied as a day-dream, and the unfortunate inventor considered little better than a madman.

Whoever has perused that unassuming production, in which Senefelder sets forth the whole history of his attempts to establish the principles of his invention in the face of pecuniary difficulties, active rivalry, and apathetic incredulity, will be struck with the idea, that the great inventions which have distinguished past ages, and which stand conspicuous as landmarks in the waste of time, although the offspring of chance, were committed to congenial spirits, whose enthusiasm and inbred energy were not to be daunted or turned aside by obstacles which, to ordinary minds, would have appeared insurmountable.

It was thus at least with Lithography: originating in one of those h ppy accidents caused by the mother of all inventions*, it has, in the short period of half a century, been spread from "Indus to the Pole," already supplying a great desideratum in the economy of the press, almost superseding the costly and tedious manipulations of copperplate, for all purposes of utility, and even treading on the heels of type, as an earnest of what it will become, when in the course of a few years, the concentrated genius of the amateur, the artist, and the practical workman shall have been brought to bear upon, and elicit, its yet untried powers.

It is no small honour to the man whose active forethought first introduced this art into the emporium of the East, and though a tinge of national regret may stain the record, still it will be found merged in the feeling, that an individual domiciliated amongst us, and as eminent in his profession as respected in private life, was the first to open this new field of enterprise to the energies of exiled Eng ishmen.

As far as we can trace, the earliest recorded fact of the employment of Lithography in this country is that of the present superintendent of the Government Lithographic Press, which dates in the early part of 1822; but we know that, considerably more than a year previous to that time, Mr. Savignac had built (after a drawing and description in Senefelder s history) a large, but perfectly serviceable wooden press on the exploded star construction, with which he executed very tolerable impressions. To this individual, therefore, undoubtedly belongs the credit of having introduced the Lithographic art into India. Trivial as this notice may seem, its present registry may obviate much future learned research when, in the lapse of ages, it may become a subject of contention.

There are now three or four Lithographic presses in the place, besides several private and amateur, in the possession of individuals in various parts of the country. To judge

* The narrative mentions that Senefelder, being "au desespoir" for a piece of clean paper to register his washing list, wrote it on a piece of Solenhofen stone with some chemical ink, with which he was making experiments on metal plates, and in attempting to rub off the characters, the idea of Lithography shot through his mind.

by their productions, they are, even with all the disadvantages they must naturally experience in being almost wholly dependant on their own resources, fully equal to those of London in all that relates to writings or line works; but in that most fascinating branch, chalk drawing, they are far behind; in fact in this way scarcely any thing tolerable has been produced, the absence of the depth of colour which is the beauty of those works, being a leading defect; the want of keeping in the half tints is also generally observable: how far further experience may remedy these is a matter of question. Two publications consisting of chalk and line drawings are now in course of publication: the one entitled the Amateur's Repository, issuing from the Asiatic press, which contains some elegant drawings; but the printing does not approach to the beauty of the London specimens. In the absence of other productions of a similar nature it may, however, be pronounced good, and a creditable attempt to supply the deficiency in this branch.

The Commercial press has also issued its proposals for publishing an interesting work under the title of the Architectural Illustrations of Hindostan, in which the chalk and line manners will be introduced. We understand it has met with good encouragement, and that the Governor General has patronized the work.

From the leisure enjoyed by individuals in this country, it is very probable, some amateur will carry the chalk drawing branch to a great pitch of perfection.

Having thus given a brief view of the rise and progress of an interesting branch of the arts in this country, we shall proceed to a not less important subject, the

Native Lithographic Stones of India.

Several amateurs in the interior having experienced the difficulty and expense of procuring the true German lithographic stone in Calcutta, were of necessity led to make experiments on such as the country afforded, and to this end, small pieces of stone of various descriptions were, at different times, collected and sent down to Calcutta for experiment.

The first was the Sung Bádul or calcareous clay slate from Agra, of a pretty firm structure, and close grain: effervescing readily with acids, taking a high polish and an uniform grain when dry ground with sand for the chalk manner. This was tried both with chalk drawing and writings, for both of which it appears adapted: barring the defect of its colour, which is marbled in a variety of forms, perhaps it may be pronounced more suited for writings than drawings, as the dark veins present a serious objection to putting in the delicate tints of chalk drawings.

A yellow coarse grained stone also from Agra was next tried, and afforded the unusual exhibition of a stone not possessing the characteristics of a true lithographic stone, yet producing good impressions.

This stone is hardly at all acted on by dilute nitric acid, yet a drawing made upon it held firmly and printed with great depth of colour, although from its naturally coarse grain nothing delicate can be expected from it.

While its colour of a dull orange yellow is a great advantage over the generally dark coloured specimens hitherto experimented on, its want of compactness is a drawback which will require plates of proportionately greater thickness to be used. For writings it is quite unsuited, no polishing carrying it to a greater pitch of smoothness than that waxy feel, found on the surface of an ordinary sandstone which has undergone the process of polishing.

The next stone coming nearest in its properties to true lithographic, and the one which we are inclined to think will answer every useful purpose, is a slate from the bed of the Són river at Rotas Gerh.! The specimen was rather dark coloured, but afforded the best results of any. Acids readily acted upon it, while at the same time it was exceedingly compact, took a moderate polish, and was excellent when grained. Particles of mica interspersed are an objection against this stone, which however is found, as it is stated, in great variety and of large dimensions, amongst which, undoubtedly, some of superior purity of colour, and containing less admixture of the rock above noticed, may be obtained.

It was, we believe, on specimens of stone similar to this, that the amateur press of Behar made some experiments, and produced very neat impressions; but in this, as in the others, the extreme tenuity and small dimensions of the specimens were a great drawback to any thing like a fair trial; the only wonder is, how impressions even tolerable could have been elicited under such unfavourable circumstances.

A yellow veined stone from the Lias formation in Rewa and Bundélkhand promises fair, being clearer coloured, and the veins not so shaded as to obscure the surface. It is, however, considered too soft for general purposes, though from the extent of the formation in which the stone is found, it is probable more compact specimens might be procured. Most of the stones which have undergone experiment, it must be premised, are pieces picked up in the haste of travelling, and without any opportunity of ascertaining by further search whether they were fair specimens of the several kinds.

The last received, was a small piece of a limestone slate (if it may be so termed) too small to determine its properties by actual experiment, but from the purity of its texture, and general appearance, there is no hesitation in pronouncing it well suited for lithographic purposes, provided it can be procured of a sufficiently clear colour, for when ground with sand and left unpolished the surface is of a tolerable white: it would be desirable to procure a slab of this stone sufficiently large to make fair trial of its capabilities.

While on the subject it may be permitted to be stated, that the experiments were made upon very small pieces of the respective kinds, and that the favourable results from such imperfect materials warrant us in expecting better success when specimens shall have been obtained.

A China stone of a light green colour, ponderous, compact, close grained, and effervescing strongly with acids, was also found to be well suited for chalk drawings; in fact, preferable to any of the foregoing, and little inferior to the best lithographic stone in this respect. For writing it is unsuited, taking but an indifferent polish.

If these experiments are repeated by those who have leisure to devote to the purpose, and who reside in the very habitat of the rocks themselves, we expect the inconvenience and heavy expense of importing stones from England or France will be avoided, and the amateur thus be enabled to draw his resources from his own immediate neighbourhood.

Since writing the above, letters have been received from which the following are extracts; they contain some further particulars which will be interesting to the lithographic reader.

"I have packed you up a block of yellow limestone,* very highly polished on one side, one foot long, eight inches wide, and two and a quarter inches thick. I am convinced from the polish it takes, that it may be made very useful in printing transfers or line drawings of any kind. I have also put up a brick made of Korundum and Lac, and I think its application in grinding the surfaces of the stones would be very useful; for when two stones are not ready, any one may be made ready for printing with this brick, or rather with a series; and as they may be made to cut to any degree of coarseness, so they would be particularly useful in preparing the different parts of the same stone for chalk drawings. It is made ranker (as a carpenter would call it) by putting more korundum into it, and the last rubber used before the polishing material (calcined tin, wet and applied through a fine linen rag) is used should be pure shell lac. A good quantity of water makes the rubber work better."

"I have just received your note of the 12th, and have every reason to believe, that the stone in question may be procured in slabs large enough for ordinary purposes. Without giving any directions as to size, I have had it brought to me in slabs a foot square; and I dare say, that with care in taking it up, much larger might be obtained. The best comes, I believe, from a nullah in the Pergunnah of Jettasanker, near a place called Deori Chakaridha, about 2½ Coss N. W. from Hatta."

IV.—*Some Account of a new invented Beam, proposed as an economical means of working Pumps or communicating similar alternate Motion.*

In Robison's Mechanical Philosophy, vol. II. there is a description given of a very simple pump, in which there are none but foot valves and scarcely any friction. The author states, that in a machine working two of these pumps by means of a beam with alternating motion, an old and feeble man weighing 110 pounds raised 7 cubic feet of water 11½ feet per minute, working 8 to 10 hours per day. A stout young man loaded with 30 lbs. managed 9¼, and this without fatigue. The manner of

* From Jysulmer, the Sung Khattoo of the natives.

working it, that is, by walking along the beam carrying the pump rods, is said to be the most easy and the most effectual method of applying a man's strength; and in proof it is averred that the above performance exceeds Desagulier's estimate of the maximum performance of a man in the proportion of 9 to 7.

In considering these statements, it was thought that such a pump would be applicable to many situations and circumstances in India. The idea suggested itself, that if for the man, there cou'd be substituted any mass of inert matter, which should yet have the property of changing its place, from one end of the beam to the other by the assistance of a small application of power, another step would be gained.

One idea led to another till, not to fatigue our readers, a plan of a hollow beam to be half filled with water or other fluid was fixed on. Such a beam, it was conceived, would have a greater tendency to oscillate than one of solid matter of equal weight, and that the application of a very small power would be sufficient to keep it in motion. The water rushing violently towards the descending arm of the beam would have a tendency to return upon itself; and thus a very slight counterbalance, it was supposed, would be sufficient to produce another stroke.

The investigation of the subject has now been steadily pursued by the inventor for some time, and many other improvements and modifications have suggested themselves. But the great object was, if possible, to execute the experiment on such a scale as should leave no doubt of the practical utility of such an arrangement. The experiment has been, after many difficulties and much loss of time as well as money, at last made, and we shall allow the inventor to speak for himself in the following extract of his letter to us on the subject. We need not point out the very great utility of so simple a machine in this country, if it be found to answer all the inventor's expectations. To the indigo planter, opium and cane cultivator, and indeed to the general farmer, the benefits are obvious and great. But it is not merely to this application that the invention is confined. Our readers will see it has a much more extensive range; but in the present state of the invention it would be premature enlarging on this subject.

"I send you a sketch of an experiment which was made yesterday, the result of which turns out, as I calculate it, in favour of the power employed. A weight was used to measure the effect, and its descent of 18 feet, in about 5 seconds of time, caused an action of all the communicators of motion to be made, (with some intermittent irregularity for want of a fly,) and with a very perceptible degree of acceleration towards the last part of the descent.

"The inertia of the whole system was about 445lbs. and the actuating weight was made at first 28lbs. or $\frac{1}{16}$ of this, but the descent being too quick it was reduced to 18lbs. and this acted very well in about 6 seconds, the whole fall. The work proposed to be done was that of raising 112lbs. alternately by each stroke of the beam, which was put in motion by means of crank rods attached eccentrically to two toothed wheels, moving in opposite directions and set in motion by a pinion wheel fixed on the axis of the cylinder. Round this cylinder was wound the rope to which the moving weight was attached.

"The accompanying sketch * will make the arrangement more intelligible.

"The contents of the beam consisted of 35lbs. fine loam and 85lbs. of water, in all 120lbs.

The beam itself weighed, empty 125lbs. ———— Total	245
Wheel train, crank rod, and line roller, ————	38
Single weights uniting the stroke, ————	112
Lever, roller, &c. lbs, each 25 ————	50
Total,	445
Actuating weight (with line) first trial 28lbs. ————	$\frac{1}{16}$
———— second do. 18, 67lbs. ————	$\frac{1}{24}$

"In the first trial, the attendant was obliged to check the velocity of descent by putting a slight pressure on the winch handle; in the second trial the winch was removed.

"The velocity of the line roller was equal to about 3 feet per second, (being 12 inches in diameter as measured from the centre of the line;) that of the working point was measured by the roller end of the weighted lever being depressed from 6 to 8 inches, and the weight on the opposite and equal arm being raised as much each time. One revolution of the crank wheel answered to two of the roller wheel, and each crank wheel revolution produced two upward and two downward crank movements in 1 second, that is, 2 strokes of the beam or one at each end.

* Fig. 3, Pl. 1.

"Now here we have 18 67lbs. falling through $37\frac{1}{2}$ inches to raise 112lbs say 7 inches — 18 67 × $37\frac{1}{2}$: 112 7 : : 1 : 1.12 nearly, so that none of the power was lost. It would even appear that there must have been in theory a gain, as the friction of the train must have been considerable; perhaps however, it might be better explained by some inaccuracy in the measurement of the small quantity 7 inches. Allowing every possible abatement, you will still see enough, I hope, to warrant further experiment and investigation.

"You are aware that few machines of any size realize half the power expended on them. Montgolfier's ram, in which the momentum of a column of water in motion is the prime mover, effected 60 per cent. and a machine called the Danaide invented in France reached the high estimate of 75 per cent. But nothing yet executed has gone near these values. The majority of steam engines, for instance, waste half their power.

"I shall repeat the experiment when the apparatus is entirely finished, and let you know the result. The preceding was by no means a fair trial; in particular, one of the cranks only was applied, and this caused an irregular action and, consequently, waste of power. Imperfect as it is, it will bear a comparison with Professor Robison's account of the man's work. In that experiment, it is evident, that (supposing no friction) the man descended and ascended about 46 feet per minute. Taking this performance, we shall have 15 strokes of the beam in the same time, each stroke being made by a descent of the weight through 37 feet; and as the actuating weight is 6 times what we have supposed it, (110 : 18. 67 : : 6 : 1 very nearly) we shall have 112 × 15 × 6 × 10,080lbs. raised through 7 inches every minute, by the same degree of exertion as raised 7 C. ft. $11\frac{1}{2}$ft. This is the same thing as 512lbs. raised through $11\frac{1}{2}$ feet. Now 512lbs. = $8\frac{1}{8}$ C. Ft. instead of 7. Therefore the performance of this beam, *loaded as it is with friction*, has an advantage over the solid one in the proportion of 8 to 7.

Explanation of the Sketch, (Fig 3, Pl. I.)

a Wheel of first mover on which the rope passing over the sheave *h*, and carrying the weight *g* = 18lbs, is wound.

b a twelve-inch sheave on same axis.

c c Two wheels actuated by *(a)* and carrying each the eccentrically fixed crank rods *(ff)*.

i i Weights on the equal-arm levers *(ll)* which turn on the fulcra *(oo)*; each arm measures 2 *ft.* from the fulcrum.

r r Rollers attached to the levers for greater facility of action.

A a compound fluid mass consisting of .... 35lbs. loam
85lbs. water

Total 120

B The hollow beam containing it, worked by the crank rods *(ff)*, and raising by means of the lever *(mn)* = 2 *ft.* the weights *(ii)* hung to the equal-arm levers *(ll)*."

The above gives a favourable idea of the advantages of the principle which has suggested this invention. Yet as we have a great jealousy of all contrivances for distributing more power than has been impressed upon them, we hesitated to receive implicitly all that the inventor has advanced in favour of this machine. In fact, we feared some oversight, though we could detect none; and we therefore suggested, as a means of obviating every objection the substitution within the hollow beam of an equal weight of solid matter for the fluid. If the performance of the machine were greater with the fluid in the beam than with the solid matter; there could be no doubt of the real merit of the invention, or of its advantages in practice. The following is the result of this experiment, as detailed in the inventor's letter.

"The hollow beam was loaded with dry loam 60lbs. and sanl-wood 120lbs. the beam itself being 120lbs. total 300lbs. Actuating weight 28lbs. The weight on the levers was increased from 50 to 73lbs. The last was the maximum that could be overcome after many patient trials.

"The wood was removed and replaced by its weight of water, actuating weight 23lbs. Weight on the levers 120lbs.

2d Experiment.

"The actuating weight was made 26lbs. the levers raised 141. It was impossible to carry the experiment further, on account of the strain which any increase of the weight would have occasioned.

Performance of solid beam,	28 : 73
Fluid,	26 : 141
or,	28 : 152
" In favour of the fluid more than,	2 : 1

" The height through which the weights were raised, was in each experiment the same, and as near as could be measured 8 inches."

We feel very curious to witness a repetition of this experiment.

V.—*Proceedings of Societies.*

1.—Asiatic Society.

A meeting of this society was held on the 5th December, the president in the chair.

The following gentlemen proposed at the last meeting were balloted for and unanimously elected: Colonel Watson, Mr. Dobbs, Captain Gowan, Lieutenant Wilcox. Mr. Hurry, Mr. Bagshaw.

The following office-bearers for 1829 were re-elected.

Vice-Presidents.—Honorable W. B. Bayley, Esq. Honorable Sir J. Franks, Honorable Sir E. Ryan, Honorable Sir C. T. Metcalfe, Bart.

Committee of Papers.—Dr. Adam, Dr. Grant, J. Calder, Esq. Dr. Carey, Lieutenant Colonel Hodgson, H. Macnaghten, Esq. J. Tytler, Esq. A. Stirling, Esq.

After the private business of the evening was gone through, the subject of the admission of Honorary Members was discussed, agreeably to the notice given at the preceding meeting. It was resolved, that in future any ordinary member should be at liberty to submit to the committee of papers, either directly or through the secretary, the name or names of such gentlemen as he might consider entitled to this mark of attention on the part of the society. It should then be at the option of the committee to nominate such person or persons for the ballot, or to decline doing so, and this without assigning any reason.

It was further resolved, that the members should, after the practice of the Fellows of the Royal Society, dine together on the anniversary of the institution of the society.

The following donations were made to the library and museum.

The Koran, as recently printed in Calcutta, with an interlinear version into Hindustani. A sheet showing the " Balance politique du Monde," by Mons. Balbi. Copy of an oration delivered before the Medico-Botanical Society of London, by the Society. Visconti, a tragedy by Dr. Kennedy, by the author. Anniversary Address delivered at the Liverpool Royal Institution, by Dr. Traill. Sir William Dugdale's History of Embanking, by Dr. Strong. The Meteorological Register for November and December, by Lieutenant Colonel Hodgson.

For the Museum.—The head of a woodcock, killed near Currah in the Dooab, presented by Captain Hewitt. A very full collection of organic remains from the Yorkshire strata, by Mr. Turner.

Thanks were voted to the several contributors.

Committee of Natural History and Physics.

Wednesday, 24th December, 1828.

Honorable Sir E. Ryan, in the chair.

After the private business of the evening, a paper by Mr. Piddington on the soil in which the Cinchona grows, was read.

A supplement to the paper by Dr. Hardie, on the Geology of Udipúr, was also read.

The following papers were transferred to the Committee by the General Secretary. On the Origin of Ærolites, by Dr. Butler. Account of a hail storm at Kota, by Major Caulfield. Analysis of some iron ores from Burdwan, by Mr. Piddington. Remarks on the Meteorological Journals of 1819, 1820, 1821, by Captain P. Gerard. Account of peat earth found in the vicinity of Calcutta, by Dr. Strong. Description of a new species of Buceros, by H. Hodgson, Esq.

Specimens of organic remains, from the bed of the Satluj, forwarded by Dr. Govan, were presented by J. Calder, Esq.

Thanks were voted to the several contributors, and the meeting adjourned.

2.—Medical and Physical Society.

A meeting of this society took place on the 3rd instant, H. H. Wilson, Esq. in the chair.

A ballot was taken agreeably to the regulations, for the office bearers of the ensuing year, when the election was declared by the scrutineers to have fallen on the following gentlemen:

Vice-President, H. H. Wilson, Esq. *Secretary and Treasurer*, J. Adam, M. D. *Assistant Secretary*. W. Twining. Esq. *Committee of Management*, J. Grant, Esq. P. Breton, Esq. G. Waddell, M. D C. G. Eger on, Esq.

Committee of Papers.—J Grant, Esq. J. Mellis, M.D. P. Breton, Esq. G. Waddel, M. D. W. Cameron, Esq. C G Egerton, Esq.

It was proposed and unanimously agreed to, that the senior members of the Madras and Bombay Medical Boards, be elected HONORARY Vice-Presidents, and that these Boards collectively be requested to become PATRONS of the society.

The following gentlemen were elected members of the society, Messrs. Train, McKinnon, and J. Turner.

The secretary laid on the table the papers which had been received since the preceding meeting, viz a communication from Mr. H Clark; a drawing of a tumour on the face of a native, and an account of Calculus in a native boy, by the same gentleman; a case of popliteal aneurism in a native, successfully operated on by Mr Lawrence of the Madras establishment.

A letter was read from Mr. H. Piddington, submitting to the society two manuscript indexes, the first to the Linnæan genera, and the second to the native and Linnæan names in the Hortus Bengalensis, forming a complete key to that work.

Some private business being then dispatched, Mr. Raleigh's paper on the use of Belladonna in extraction of the lens was read and discussed; after which the meeting adjourned.

VI.—*Scientific Intelligence, &c.*

In the translation of Baron Cuvier's Regne Animal, now in course of publication, a very full review of the order Ruminantia by Major C. H. Smith, F. R. S is inserted as a supplement to that order.—It has been presented to the Asiatic Society by the author, who has at the same time addressed a letter to the gentleman through whom it was presented, from which the following is an extract. We doubt not our Moofussil readers will be glad to see what are the obscure or doubtful points in this branch of Indian Zoology; nor are we without hopes, that it may prompt some valuable communications, and we need hardly say how happy we shall be to find any of them destined for our pages.

"My request to you and to all gentlemen residing in India, who take an interest in zoological studies, is to favour me with their criticisms on any or every view I have taken of the question; also if you or any other gentleman would honour me by the transmission of drawings, measurements, descriptions, and manners of animals of any class, but particularly of mammalia, birds or fish, and above all of ruminating species, I, and it may be added, zoologists in general, will feel the obligation. Although I am aware, that my friend General Hardwick has done much and is now extending his inquiries on Indian subjects, still there is room for a vast deal more than one man can do. I am particularly solicitous about the Sanscrit, Hindustani, and Persian nomenclature, which you will perceive I have adverted to in my observations, but which, I am confident, are far from complete or even exact. It is surprising that we know so much more of the animals of Africa than of those of India. I wish to have my views considered relative to the two distinct races, if not species of buffalo; the distinctness of species of the hunched domestic ox of India, whether this species does not derive from the gayal. I wish to have more light on the Jemlah goat of the Himalaya mountain. I wish to have more information on the great stags of India, their species, manners, and habitat; on the Nilgau, or the strepsicerotine antelope, which was supposed to be exclusively African, but which, from a scull in the possession of General Hardwick, appears to reside also in India; on the two (if two) species of quadricornis; on the antelope (unicorn) of Bhotan; on the Bara Sinha of India; the Amon Chekora, the Jerrial, &c. I wish for information on the several axines of India, on the stag of the Mariannas, on the Ubi of Raffles, the *cerf noir* of Blainville, the [illegible] of Nepaul, on the several species of musks, &c.

I shall also be happy to have a particular account of the hand, &c of the Gigantic Oran-Ootan mentioned by my late friend Dr. Abel; and finally I am desirous to have some observations on the existence of the Papua or Ethiopian race of man in some of the mountains of central India."

GLEANINGS

IN

SCIENCE.

No. 3.—*March*, 1829.

I.—*On Malaria.* By J. Macculloch, M. D., F. R. S., &c.

[From the Journal of Science, N. S. Vol. II.]

It is a familiar fact, not merely to physicians, but to the people at large, that there is produced from marshy lands a peculiar substance, called *marsh miasma*, in physic, and to which Italy has given the term *Malaria*; from the effect of which on the human body, there are excited fevers of different characters, but generally divided under the two great leading heads of *intermittent* and *remittent*. What the chemical nature of this substance is, has not been discovered, though numerous experiments have been instituted for this purpose; yet with respect to its properties, we know enough to believe that it is a compound gas, decomposable by certain agencies, and also capable of being conducted to certain distances from the place of its production, by the winds. Further, it is ascertained, that it can be condensed or accumulated in particular places; that it can form a certain attachment to the soil, or to peculiar solid bodies, although this is not permanent, as happens with respect to the matter of contagions; and lastly, that it is particularly affected, both in respect to its propagation and production, by certain qualities of the atmosphere, consisting in its conditions as to temperature and hygrometrical moisture.

Thus, although ignorant of its nature, we are in possession of certain facts appertaining to its natural history, which we can convert to use in warding off its evil influences, or preventing the attacks of the diseases which it produces; while, if one class of precautions depend on this knowledge, the other consists in that remaining branch of its natural history, which relates to the causes through which it is, in the first instance, produced, and in the next, propagated.

Being, as I have remarked, produced by marshy lands very especially, it has been naturally concluded that its immediate cause was the mutual action of vegetables and water, though it has been disputed in what that precise action consisted. To ascertain this is, however, of some importance, as on the nature of that action must, in a great measure, depend under what particular circumstances it is produced; consequently, what places ought to be suspected, and, in the view to prevention, avoided.

That it is the produce of vegetable decomposition cannot, apparently, be questioned; because, if it were extricated from living plants, it should be found in thousands of situations where it is unknown; and that it belongs to wet soils, and also to hot countries, or to a high temperature, further proves that it depends on that process of decomposition which notedly occurs, in these circumstances, most rapidly and extensively. But a difficulty remains in attempting to determine what is the peculiar quality or stage of this decomposition under which it is produced; whether it is the process commonly called putrefaction, or some change among the elements of plants of a different nature.

One point is at least provided; viz. that it is not necessarily accompanied by any smell, or that it is not an odorous substance in itself. It may exist in abundance and virulence, without being sensible in this manner; and hence it had been thought this arose from some mutual action of vegetables and water, which was independent of proper decomposition. This, however, must be an incorrect opinion; or the living vegetable is not required for its production: since it is fully proved to be generated in abundance by vegetable fragments long dead and detached from the plant, by mere mud impregnated with unassignable vegetable matter, and also by such chemical substances as contain some of the elements of plants, without belonging to them; of which sugar, as I shall presently notice, offers a conspicuous instance.

To ascertain these facts is important, because they form the groundwork for precautionary measures as to disease, as will speedily appear; while I may here make one remark as an example of the utility of this knowledge; it is that while there is a popular fear with respect to putrid and stagnant waters, there is none respecting clear waters, under whatever form, producing living vegetables, and free of smell, while the danger, in reality, may be scarcely less in the one case than in the other.

Generally, therefore, we must conclude, that wherever plants in contact with water undergo decomposition, even as to their expended and dead portions; or wherever fragments of plants are exposed to moisture; or wherever mud impregnated with invisible or dissolved vegetable matter, exists; or, finally, wherever any chemical compound of the vegetable elements is wetted or held in solution in water, there the poison in question may be, or will be produced, provided the temperature be sufficiently high: and it is this necessity for a certain temperature, which is the cause why that peculiar decomposition of vegetables which forms peat, does not produce malaria, however it may occur in peaty lands; because, generally speaking, the formation of peat is limited to climates or regions of a low temperature. The nature and cause of the exception now made, will be explained immediately.

I need not attempt to inquire further into the nature of this decomposition, since there are no facts on which to found an inquiry. What we must conclude, is, that some of the vegetable elements are let loose and re-combined into a new gaseous compound, while experiments carefully conducted (as ought not to be doubted when Vauquelin has been engaged in them) have not detected even the presence of such a new substance in the atmosphere of marshes, far less its nature. It is, however, evident that it cannot be any of the hydrocarburetted or other chemical gases which it has, at different times, been supposed; while, remaining thus in darkness, the only test of its presence continues to be the effects as to disease which it produces on the human body.

But, as I remarked, we possess enough knowledge of its natural history to assist us in guarding ourselves against its effects; and as that question, utility, is the object of the present paper, I will proceed to describe as much of that as is admissible in so limited a space. To know all the classes or kinds of places which are capable of producing malaria, is the first and main object as to prevention: to know by what means it is propagated is the next, while the contingent circumstances necessary to its production, or to its effects on the body, consisting chiefly in temperature and moisture under various modes, will also demand attention. All these things being known, we have laid the preliminary knowledge, requisite to prevention, as far as that is in our power; without them, we are subject to those hazards which arise from ignorance; and what those hazards and their effects are, cannot be very obscure when it is computed, and, perhaps, truly, that half the entire mortality of the world is the consequence of malaria: of the fevers chiefly which are its produce.

But one circumstance requires to be premised; and this is, to prove that our own country can produce malaria, and is subject to its diseases; because, were this not the fact, the statements of this paper could possess but very little interest to general readers, whatever it might to physicians or philosophers. I think I shall prove that we are all interested in them, and that also, to a considerable degree.

The chief disorders produced by malaria are intermittent fevers, and what are called by physicians, remittent fevers: writing to general readers, we should call them fevers simply: while further, it is usual among the people, for them to receive the term typhus; a name implying, strictly, contagious fever, and, as thus misapplied, a source, not of confusion merely, but of considerable evil in various ways. Not to enter into medical discussions, which would here be inadmissible, I must be content with simply saying what is easily proved; that the fevers, generally, which appear in summer, or from June to November, are of this class, and the produce of malaria; while, that agues, as they are popularly called, are produced by the same cause, is admitted on all hands.

Now, every one knows that such fevers do occur in summer, in many parts of England, and in certain districts, very conspicuously; and further, that while they have been peculiarly abundant and severe during the three or four last years, they were in 1826 unusually destructive and numerous, having been the cause of a mortality, which, in England, must be deemed considerable. Every one also knows that agues occur in England, and very especially in certain districts, so well known as not to require being named: whence it cannot be doubted, that although, compared to France or Italy, we may be considered, comparatively, exempt from this cause of disease—we are very far indeed from not suffering by it; and that also to an extent, which, the more it is investigated, will appear the more serious.

But to put this out of all doubt, while it serves to prove also the power which we possess over this cause of mortality and disease, it is sufficient to review the history of our own country, or even of London, to be convinced that our climate is capable of producing malaria wherever the other necessary circumstances are present; and that, in as far as we are now exempted, it has been the consequence of an attention directed to these circumstances. Hence the reasons, and the inducements also, to further attention of the same nature, for which it is primarily necessary that we should possess an accurate knowledge of them.

How enormous and nearly incredible the diminution of agues and fevers has been in Lincolnshire, Essex, Kent, or generally in the fenny and marshy counties of England, since the agricultural improvements of those lands, is matter of notoriety, since much of it has even occurred in our own days: but even in London, on Burnett's authority, the intermittent raged like a plague in the reign of Mary: while, from Sydenham, Morton, and others, to whom we may add the testimony of Short, and more recently, the facts collected by Heberden, the years 1658, 1664, and those from 1667 to 1692, were years of a mortality from this cause, which could that act now as it then did, would, probably, in any one year, destroy twenty or thirty thousand persons in London alone. And this national mortality from fevers, from the fevers of malaria, extended even down to the year 1729, according to Short's testimony—so recent are the improvements by which it has been diminished.

Here, then, it is at last proved, that the climate of England did not then exempt it from malaria; and as that climate remains the same, so must this poison be still produced; whatever exemption we do possess, being the consequence of having corrected or destroyed some of those places or soils by which it is generated; but we have not corrected them all, nor have we exterminated the disorders which we have diminished. This is the task which yet remains for us, and for that end it is, that I shall here endeavour to point out the circumstances which do produce malaria; since I have, I trust, proved that it does exist among us; too generally overlooked, and not unfrequently denied or even ridiculed, while the chief cause of this oversight is the error to which I here allude of misstating the cause and nature of the summer fevers.

It is this error chiefly, and further, that of looking for the evidences of malaria in the production of intermittent solely, which prevents a correct judgment from being formed of the existence of this poison, and of the places producing it, though, in addition to the fevers of summer, I could, had I here sufficient space, and were this a proper place for it, easily show that a great many other disorders, often little suspected as consequences of malaria, are equally produced by it, and are equally the evidences of pernicious soils. But as I have no room for detailing all this evidence, I must be content with saying, that wherever I here mark any particular mode of land or water as productive of malaria, it is, that in some one or more cases of that variety, I have ascertained, by this very evidence, that it was present, and the cause of disease; while nearly all, perhaps the whole, are further admitted or proved to be truly pernicious, by the various French and Italian physicians, who have bestowed on this subject an attention which has been utterly withdrawn from it in England. And if this detail is general, rather than special, it is not for want of special instances and proofs in abundance, that I have not noticed these; but from the recollection, that to point out the insalubrity of certain spots, might injure the properties, by affecting the value of those, should that which is here adduced succeed in commanding belief.

It is plain, to commence, that there is no mystery or charm in the term marsh; and that if such a tract of land can produce disease, it is because it contains vegetables growing and decomposing in a moist soil: this is the general analysis of the cause; and wherever, therefore, the same circumstances occur, that spot must be, as to all the objects in view, a marsh, or a source of malaria. A minuter analysis of the facts can scarcely be necessary, either as to the plants that grow in marshes, or to the mode in which the moisture exists: since, generally, the plants are similar, while all vegetables appear indiscriminately to produce the poison in question.

In the next place, a marsh, in the popular sense of the term, possesses a certain extent; and in the popular view also, this extent is judged necessary to the production of disease. Facts without number prove, that space, bulk, or extent is not essential; but that the smallest spot of the same character, or of the proper character is sufficient to produce disease, though the extent of its influence must be expected to vary in consequence: and even without these facts, this would be judged true from the following argument.

It is ascertained that a marshy spot can influence to disease a place situated at a considerable distance from it; and this extent, in Italy, has been ascertained to amount to at least three miles. Now it is plain, that whatever the original body of malaria was, it must have been diluted by the atmosphere during such a course to a very great degree, or that the quantity reaching any individual must be very minute. It must be indifferent, therefore, whether that minute quantity which thus acts, is a portion of a great body of the same substance, or whether it is the whole which was generated; and this must, in reality, be indifferent in any case, even without transportation. It is ascertained, that a single inspiration will produce the disease; and therefore, whether in this case or in that of dilution under transportation, the exposed person receives the whole produce of a square yard, or any other given space, or the same quantity out of a mass produced by twenty thousand or million of square yards, the consequence will be the same. It is a mere question of arithmetic; and moreover, as the whole malaria of any marsh is the collective sum of the portions produced by each plant or fragment, whatever can act on the exposed individual will be sufficient to the effect, though it were the entire quantity in existence, or the single part will act as effectually as if it was accompanied by a thousand similar ones.

Thus it is of no consequence how small the marshy or pernicious spot is, provided it can act; and the only difference is, that a smaller spot will act on fewer persons; and also, that from the greater dilution which it must undergo on transportation, it must act at shorter distances, or may require absolute proximity or contact.

Having premised these two necessary considerations, it is sufficient now to inquire what are the modifications and forms of soil or water, or of certain spots and places generally, which contain the elements of a marsh, or are distinguished by a vegetation accompanied by water. Whatever these may be, let their names be what they may, or their appearances as deceptive, they are at least suspicious, and in very many cases they will be found to be real causes of the diseases of malaria, wherever the temperature, or the season and climate, are favourable, while that these are so with us I have just shown.

In a general enumeration, I may name the following places or soils, before proceeding to such particulars as may be necessary in explanation. Besides proper marshes, fresh and salt, meadows, or wet pasture-lands generally, however situated, there are woods, coppices, and thickets, plashy and limited spots of grounds, sea walls and river embankments; and, as falling better under the head of water, lakes, rivers, and ponds, including mill-dams, ornamental waters, canals, and the pools of insignificant dimensions which occupy commons, gravel-pits, or other similar places, together with agricultural ditches and drains of all kinds. To these, I must also add, as coming under the heads of vegetable matter without vegetation, sewers and town-drains, dunghills, tide-harbours, flax-ponds, or other receptacles of putrefying vegetables, and lastly, at sea, bilge water.

With respect to the most common of causes, though it is often thought, in England, that salt marshes are not insalubrious, and, very particularly, that security is obtained whenever they are daily washed by the tide: an ample experience shows that they are as pernicious as fresh-water ones, and that a daily tide is no security. Some of the most poisonous tracts in Europe, indeed, in France, Spain, Italy, and Greece, are salt marshes; and of how little value or protection the sea is, is fully proved by the tide-rivers of the tropical climates, most notoriously the most pestilential places that exist, even in those destructive regions. Where such tracts are found in England I need not say, as they are always both obvious and well known.

It is not believed with us, that meadows can produce malaria: this is to be questioned, in the first place, on general principles, and from other European experience; and secondly, it can be proved by domestic experience, that they actually are the causes of fevers with us; and even independently of their ditches or drains, which, it must be observed, may often produce disease, where the soil which they intersect may be incapable of that. In Italy, in France, in Spain, and in Greece, and almost invariably in the American states, they are not only causes of malaria, but among the most common ones. The great seats of that poison in those countries, are the valleys and plains which give passage to rivers, whether on entering the sea or otherwise; and out of these, there is a very small proportion marshy, compared to those which are merely meadow and pasture, and which, very often also, are the seats of cultivation. Did meadow lands not produce malaria, there could be few of those tracts abandoned in summer, compared to those which it is nearly impossible to inhabit after the heats are once established, while, to refer to individual places, would be to form a catalogue of no small length.

With respect to our own country, whoever shall please to inquire, will find that fevers occur in autumn in those situations, particularly in hot seasons, when they are unknown in the drier lands, and more especially in the cultivated ones; nor can there be any reason to doubt that it should be so, when our climate has been proved capable, like the southern countries of Europe, of producing this poison from the same class of soils. And whoever also will inquire, will often find great surprise expressed by country practitioners, at the occurrence of fevers in rural and detached situations, where the commonly esteemed cause, contagion, cannot be suspected; while he who may pursue his inquiries on this principle, will be able to explain the causes without difficulty, from the presence of some spot or tract of this nature. And I must add, that it is not even necessary that such pasture-lands should be flat meadows, as I could easily produce instances of endemic fevers, almost amounting to epidemics in the last summer, where the lands, being wet and poachy however, were not only elevated, but formed the declivities of hills.

There is one case of land, which, as to ourselves, is not worth noticing, as it does not occur among us—I mean rice-fields; and if I here enumerate them, it is for the sake of preserving the integrity of the subject. But it gives me an opportunity of introducing one or two remarks which do concern us: the most important of which is, that meadow-lands will be pernicious in summer, in proportion as they have been wet the preceding winter, and that the danger will be especially considerable, should they have been inundated, as is often the case in some of our flatter countries. The other is, that the act of breaking up such moist pasture-lands for cultivation, is often hazardous, as it is amply proved that malaria is thus produced or extricated in unusual quantity or virulence: this effect, in the hotter climates, being often such as to produce the almost immediate death of the labourers employed in it. Hence, to note a precaution as to the prevention of disease, for which no opportunity will here occur in noticing the ordinary remedies against malaria, it is a matter of prudence in all such cases to break up meadow-lands in winter, when malaria is not produced; or, if this cannot be done, in the early part of the summer, rather than in spring or autumn, the two seasons in which the disorders produced by malaria are most active.

Woods, including coppices and thickets of whatever nature, comprise the next class of soils or places which I may notice as productive of malaria. Of the pernicious properties of those in warm climates, the evidence is too abundant to admit of doubt. In Africa, in the East Indies, and generally in the torrid climates, woods, forests, jungles, bamboo and reed thickets, and many more varieties than I need distinguish, are the most noted causes of the fevers which have so generally been the sources of disease, whether to permanent residents, or to armies engaged in such territories. They are, in fact, proverbial: the question is, whether they are similarly pernicious in our own country. On general principles, they ought to be so, though, in the same proportion as any other pernicious soils are, less extensively and severely; and that they are in reality injurious, is proved by a variety of experience. Sussex is one of the counties well known to be productive of autumnal fevers, and even of intermittents, if less popularly celebrated than the fens of Lincolnshire or the marshes of Essex. The inhabitants, at least, know this full well; and he who may examine into the localities, will easily find, that if they not produced by the woods, there is no other cause; while, to confirm the reality of this cause, I could easily point out, in various parts of England, endemic diseases the produce of such woods and coppices, even in districts that would be less suspected than Sussex. There cannot be much stronger language on this subject, than that of a medical friend in Wales; who, after a long residence in India, complains that he is still vexed by the jungle-fever, in the woody districts in which he practises, and who, without system on this subject, entertains no doubt that the cause lies in the close and damp coppices of his neighbourhood.

If I have noted, in the preceding catalogue, plashes of moist ground, meaning under that term to comprise all kinds of insignificant wet or marshy spots, I need not specify more minutely what they consist in ; while their abundance every where renders all reference to places unnecessary. They are in reality marshes, though small ones ; and if what I have said respecting space or bulk as to a marsh is valid, then are they sufficient to produce disease, provided the proximity is sufficient. And if it would be fruitless to quote examples of disease produced by them, from the impossibility of reference, I must trust that my own experience of such events, combined with the general argument, will be a sufficient warranty.

The last variety of mere-land production of malaria, and requiring enumeration, comprises sea-walls and river embankments ; and the reasons for pointing it out are, chiefly, that being very often implicated with the remedy for marshy lands, it is the more apt to be unsuspected. Yet it is a species of land extremely productive of malaria ; and not unfrequently the sole or principal cause of that which continues to be generated after the reformation of such lands, while seldom viewed as such. Such walls are, in truth, very generally petty marshes in themselves, as is easily seen along the banks of the Thames ; and there is abundant proof that the reeds and other rank vegetation which occupy them, are a frequent source of the same disorders as those lands were accustomed to produce before their drainage and embankment. To know this cannot but fail to be useful ; since it is possible, at least, to avoid such places, and often not difficult to manage those necessary works so as to diminish the hazard of evil.

I may pass to the localities or situations which have a nearer reference to water than to land. What relates to lakes is simple ; and if it little concerns us, inasmuch as our own lakes lie in a climate scarcely productive of malaria, the demonstration of their pernicious qualities in Italy and Switzerland is ample. The causes, immediately, are various ; and I need not do more than name them. Their deltas, whether at the exits or entrances of the rivers, are meadows of the worst kind, because subject to inundation ; their borders are often marshy ; and, in addition, they frequently contain creeks or shallow places, productive of putrefying aquatic vegetables and stagnant water ; while, in some situations, as diminished by the heat of summer, they often also expose offensive mud, generative of the same poison.

With respect to rivers, I may commence by one general remark. It is the observation of Volney, that there is not a river in America that does not produce malaria ; and when, after enumerating in addition, valleys, woods, mill-ponds, meadows, and lakes, he says that, through his whole circuit, he did not find a dozen houses without fevers, it is a testimony which is too pointed not to be quoted as to this and many other of the facts and places which have here been enumerated. And in France and Italy, the same opinion prevails ; while, if we consider the reasons, it is apparent in the nature of their banks, producing a marshy vegetation ; in the diminution, during summer, of their waters, exposing mud ; in their casual inundations, rendering the enclosing lands wet ; and in the general character of the soil and vegetation by which they are bounded. How these circumstances may exist and act in our own country, I must now leave to the judgment of others to determine ; and it will be easy to see what variety of river may be suspicious or insalubrious. We need not look for danger in a rapid stream, a northern climate, or a hilly region ; but the Ouse, the Lea, and similar rivers, cannot possibly be exempt, unless England claims an exemption on some principle different from that which exists in France and Italy.

But to this I must add one remark. It is an universal prejudice in England that there is no hazard from running water ; as it is, that clear water cannot be insalubrious, and that stagnation and putrefaction are essential to the production of malaria. It would be fortunate, indeed, for France and Italy were this true ; and fortunate for all the tropical climates, even in a greater degree. I have already said that putrefaction is not necessary ; while the fact that the most pernicious situations in the world are the banks of the tropical rivers, and the lands perpetually washed by a clear green sea, shows convincingly the little truth of this popular judgment. The rivers which I have just named, and many more among us, including the Thames in many places, are decidedly productive of disease, though running streams ; but the evidences are too notorious every where to need urging. And therefore, when, with regard to mill-dams or other ponds, forming the class of waters next to be noticed, it is popularly said that they cannot be pernicious, because the water is constantly refreshed by a stream, it is a false as well as a dangerous opinion. Should such a receptacle admit a stream that replaced the whole water in a few hours, which is rarely the case, it would even then be no other than a slow river, and therefore no more free of danger than the worst of those.

In proportion as any species of soil or place is least suspected and most common, it seems the more necessary to be careful in pointing it out and dwelling on it; and hence it is, that I am more desirous of urging the dangers arising from what will, I know, be despised, as I have experienced it to be. I allude here to a great variety of apparently insignificant waters, comprising mill-ponds for various purposes and on all scales; ornamental waters and fish-ponds, canals, and the endless pools and ponds found all over the country, in deserted gravel pits, and in various other casual situations not worth detailing

It is always useful to examine any case of this nature by reducing it to its principles, or by a philosophical analysis, and to inquire whether it *ought not* to be insalubrious, before inquiring whether it actually is so. Now all these waters, be they what they may, include the elements of a marsh or of a water productive of malaria. Their margins are marshes, they contain aquatic and putrefying plants; and under the heat of summer, they disclose mud impregnated with vegetable matter. And this is true of the whole, whatever their extent or character may be; while, if plainly shown that bulk or space is not necessary even to a marsh, and that our climate is adequate to the production of malaria, there is no reason, *à priori*, why they should not produce it.

Now let us see how the experience confirms this; and if it does, then are we in possession of a very extensive class of causes of disease, and of one which very especially we have in our power as to correction, which, and not the excitement of useless alarm, is the object of this paper.

Volney's testimony as to the pernicious nature of mill-ponds, is decided as to America; and I have just quoted it. Monfalcon is equally decided as to the poisonous qualities of all such waters in France; and all the Italian writers agree that it is the same in their own country. And the evil does not depend on the name by which they are known, or upon their physiognomy or uses, any more than it does on their dimensions Let me mark a few of specific kinds which are condemned in foreign countries, as a guide to our judgments respecting our own.

There are many extensive districts in France occupied by ponds rather than lakes, maintained and farmed for an inland fishery, where the diseases from malaria prevail to such an extent, that the average of life does not exceed twenty years; where the people are decimated in every year; where absolute old age, in those who survive so long, takes place at forty; and where the aspect of twenty is that of fifty or sixty in countries such as our own; where even the children are diseased from their births, becoming subject to unceasing fevers if they live to seven, and thus continuing till the not far distant period of death arrives to terminate the literally long disease of life. I can here refer to Monfalcon for details that I dare not enlarge on, and for authority that no one will question.

Here is proof of the effect of such waters: and I will use the same writer's authority as to condemnation of all canals, ponds, and ornamental waters, of whatever nature; besides which, as to those who desire facts more specific, his word and that of other French physicians, will probably be taken for the fact, that the "canal" at Versailles, and the similar water at the Chantilly, which are mere ponds scarcely exceeding that in St. James's Square, are the common causes of severe intermittent and remittent fevers.

I need not quote further foreign authorities. They who doubt, may not, perhaps, take my word for the fact; but while I could point out, in this country of ours, the same occurrences in abundance everywhere, from many waters of this nature, I shall prefer trusting to the examination of others hereafter: since a conviction derived from such a source will be of much more value than a belief founded on any evidence which I could produce; marked as that is, and easy as it would be, to point out the very places, often familiar to the inhabitants even of this capital.

But that I may make these remarks of the more utility, let me note one or two other particulars; since, as far as reformation or avoidance of the cause is attainable, the value of these remarks must depend on their specific nature.

If any one will be at the trouble of examining the condition of health and the characters of the disorders, and further, the time of the year, and the particular kind of seasons in which these prevail, as these relate to the inhabitants of such spots, more particularly if he will compare the results with what occurs among the same classes of people in dry situations, I cannot doubt that he will find everywhere such proofs as I might easily have brought before him, did I not dislike to name the places. Such are the houses of the opulent in such districts as those which border the Thames, the Ouse, or other slow rivers; houses where ornamental water has been introduced, and more especially where these are confined by woods; ancient castles surrounded by moats, and, among the poorer classes, those placed by canals, mill-ponds, and in other analogous places: to which I might add what, however, is rare in our own country, if common on the continent of Europe, fortifications; the diseases of which, when the ditch is wet, are notorious everywhere. And to these I may subjoin, what will excite more surprise from their apparent insignificance, the ponds of gravel pits, which will be very often found the causes of those fevers that occur in such situations.

What will be observed in all such cases is, that the inhabitants, even where opulent, are subject to what is called vaguely ill health; while, to use a common, if a vulgar phrase, they are places where "the apothecary is never out of the house." And this ill health, where least marked, will be found to consist in a succession of petty and almost indescribable fevers, being in reality, the very condition which torments the inhabitants of the pestilential parts of France and Italy, from their cradles to their graves, in a variety of painful disorders, including rheumatism and sciatica; and in what, if difficult to ascertain absolutely, is well known to those familiar with Italy and France, namely, visceral obstructions, and very particularly, disordered spleen: well marked to those who know these countries, in the peculiar sullen complexions and physiognomies of the individuals.

If such is the general character of this ill health, I might easily explain its action at greater length: while it will be remarked by any one who will make the inquiries that, very frequently, whole families which were formerly healthy, have become thus disordered on taking up such a situation; and that others have, reversely, recovered health by leaving it for a drier one. But if all this is too little marked to attract ordinary notice, particularly where the cause is unsuspected, (though even popular opinion agrees in the insalubrity of low and damp situations,) there is disease enough produced by all the class of places which I have been enumerating, to satisfy any one who is really acquainted with the disorders arising from malaria; I allude to the fevers, the dysenteries, and diarrhœas, and the choleras of autumn, which will be always found peculiarly attached to situations of this kind, and often in so marked a manner, that it is wonderful the fact has not excited attention long ago. Let any one attempt to recollect where it was that, in the last summer, he has seen a whole street, a whole village, or the whole of the inhabitants of one house suffering under fever, and he will as surely find that such street, village, or house was situated near water in some shape; and that water, perhaps, not more than the pond belonging to the gold fishes, or the gravel-pit on the common.

But to remove one doubt which will naturally arise, let me make one remark on gravel-pits which will explain every other objection of a similar nature.

It is objected that waters or wet lands cannot be productive of malaria, because they do not excite ague; since this, unfortunately, in England, is too commonly reckoned the sole test of its presence. So far is this from being true, that in the most pestiferous parts of France and Italy, simple and original intermittent is a rare disease; so little is this the proof and the sole one of an insalubrious soil. To omit the various other disorders which malaria excites, it is the summer fevers which are the great tests, as they are the produce of this poison; and but for which the malaria of Italy or any other pestilential country would be of little moment indeed. And it is in these, therefore, that we must chiefly look for the evidences of insalubrious spots among ourselves: while unfortunately, as they are too generally termed typhus with us, or else attributed to heat, fruit, or other fanciful causes, the real malaria which produced them is overlooked or denied, as the place by which it was generated remains unsuspected.

And though with us, malaria does produce intermittents in spring, as it excites common or remittent fevers in autumn, there is a reason why the same waters or places do not excite the former disorders while they are the causes of the latter, and hence also why they remain unsuspected, or why their pernicious properties are denied: this is, that from the winter rains many of these receptacles of water, and gravel-pits very particularly, are filled to the margins, and are often also void of vegetation in spring; while from the heats of summer, the shrinking of the water and the growth of aquatic vegetables converts them into so many petty marshes—often also exposing to the sun their noxious mud.

Thus have I explained what will be found applicable to numerous cases of a similar nature: while it must be remembered in addition, that heat is necessary to the generation of malaria, and therefore, that many places and lands will produce it in autumn, which would not have done so in spring. And if English physicians, and the people also, forget or deny that their autumnal and summer fevers are the produce of this poison, it is not wonderful that they doubt or deny its existence; while this dangerous and destructive error is confirmed by their similarly overlooking the visceral disorders, and the remainder of that long train of affections which arise from the same cause, and which are most fully proved to arise from it by the unquestionable, and no less wide and severe experience of France and Italy.

To proceed with similarly unsuspected places, I may name all kinds of ditches and drains, as these are constructed for agricultural or other objects. These are, in their very essence, marshes, and often of the worst quality; since often in a state of putrefaction which is more rare in a real marsh of any extent. And very often they are the real cause of the fevers which continue in a country after those operations by which it has been drained; as would easily be proved by a corresponding examination. To know this fact is, as in all other cases, to know the remedy, as far as that is in our power; avoidance as the one generally most easy, and further attention to keeping them clean and free from weeds,—an attention not less required for agricultural reasons. And that such is their power in exciting disease is proved, not merely by the experience of modern France and Italy, but by the remarkable fact that, in the times of ancient Rome, perfectly informed at all times on this subject, such regulations formed a part of the laws, both as to these receptacles, and as to all canals. I need scarcely say that, in such cases, a clean earthern bank is the easy remedy; as, in the case of ornamental waters, a stone margin is an effectual security—as far, at least, as the margins are concerned—since it is equally necessary to avoid the growth of sub-aquatic plants and the exposure of mud to the sun.

I have thus described as minutely as I dare within these narrow limits, the chief places or forms of land and water which, as producing a living vegetation, are the sources of malaria; reserving what relates to remedial processes, to a future paper on this subject; and I may now point out what remains, consisting in vegetable decomposition independently of vegetation.

Among these, the sewers of towns are assuredly to be reckoned: and as I can take a proof from France without the hazard of offence, it is sufficient to mention, that the salpetrière was formerly subject to intermittent fevers, which attacked the inmates within the house; and that these being suspected to arise from the drains, these were closed up, with the immediate consequence of exterminating the disease. It is to be suspected that the object of the great cloacæ of ancient Rome was the same, though their history has not reached us; and it would be easy to confirm the same opinion, by the history of the diseases of the towns without end, and by that of their reforms on this point. Our own capital offers a striking example of the improvement of its health from this cause; while the history of Fleet-ditch is familiar. It had indeed been thought, and is still, that the fevers thus produced were typhus, or contagious fever: but while it is obvious that remittent or marsh fever will explain the effects equally, so must it be remembered that these fevers occurred in summer; that they were peculiar to those particular vicinities; and that, from the reports of Sydenham and Morton, the fevers of London were of this very character. And the whole analogy of fevers produced by such repositories of putrefying vegetable matter, not altered as to its effects because mixed with animal matter, seems to prove, as clearly as any thing can be proved, that these town-fevers, from this cause, are truly fevers from malaria, and not from contagion; while the deception which considered them such, from occurring in the same houses or streets especially, is easily explained. It is just the same now as to rural situations; and the errors are the same. The whole inmates of a house are affected with a fever, not because it is contagious, but because they have all been exposed to the same cause: while, unluckily, the occurrence of petechiæ and so forth, in bad cases, assists in perpetuating the error; as if this was not a common symptom in the marsh fevers of Italy, Holland, and France.

If I cannot prove, in this country, that the dunghills and pools so often found in farmyards and before the doors of cottages, are productive of malaria, I can at least quote the very decided and numerous testimonies of French physicians to this purpose: nor, indeed, would it often be easy to account for the fevers occurring in this country, and among the lower orders, without having recourse to this cause; while the whole analogy of the subject leaves no reason to doubt that it ought to be one.

And let me make one or two remarks here which apply to all the cases of mud and putrefaction of whatever nature. I have already noticed that odour was not necessary to the existence of malaria; and the proof is, that while all such putrefying matters, even to marshes, possess as much smell in winter as in summer, the poison is produced only in hot weather. The other is, as to mud, that, without any visible vegetation, not only does its exposure to the sun in summer produce malaria and fevers, but that this can be disengaged, even through the water, provided that it is not very deep, and without absolute exposure. Thus, in the former case, have some of the most severe epidemics on record been produced by the sinking of lakes and rivers, and the exposure of their mud; as also happens often, and in the tropical climates very especially, from that brief exposure that follows the recess of the tide: while, in the West Indies, it is observed that fevers are invariably produced by certain pools, as soon as they are so far diminished in depth as to allow the air of the bottoms to escape through the water. Now, though our climate is less active in this evil, these are facts of value to us in the way of precaution, while they are among the least suspected causes of evil; since it has been ascertained, that fevers are produced where no other causes than these are present. And if I notice another fact, that, in the West Indies, the mere exposure of the naked soil, by removing stones which covered it, has produced a sudden and deadly fever; so have I, in this country, seen agues produced, immediately and decidedly, by the simple and little suspected accident of the inundation and drying of a celler.

But of fevers produced by the exposure of mud containing vegetable matter, the most frequent instances in our own country are those which occur in tide harbours. In the warmer climates, this is as notorious and deadly a cause as any one that exists: and under this head, nothing can be more notorious than that which occurs in the tide rivers of Africa and the East, or within the tropics generally; since this is the very analysis of those most pestilential of all spots, mangrove sea-beaches and rivers. Here, there is no proper vegetation in contact with the water, whether that be fresh or salt, as the stems of the trees rise out of the naked mud: while it is ascertained by endless experience, that such a river or beach is safe as long as the mud is covered, but that the fever seizes on the boats' crews at the very instant of the recess of the tide; while its cause is rendered immediately sensible, by a peculiar earthy smell, well known even many miles at sea, as brought off by the land-winds, and equally exciting fevers among the ships' crews.

Now, it is familiar that in our own country, fevers, and in summer as usual, are common in our seaports; while, from the persistent errors on this subject, always attributed to poverty, filth, confinement, and, of course, to contagion. That this is not the real cause, is proved partly by the season to which they are confined, if it was not by the fact that they are really not contagious, but mistaken for such by being attached to particular spots or houses; and partly by that other fact, that they will be found to appertain to the very kind of seaports which I have described, and not to occur in deep harbours or on clean and rocky shores. It is easy for any one to conjecture where they ought to happen; and any one who will inquire, will find that the experience corresponds to the theory as derived from the present view.

It is so important, for many reasons obvious to medical men, to distinguish between contagious fevers and those which are not such, and the evils which arise from misstating marsh-fever for typhus are so numerous, that I must be excused for urging this subject as to all these last cases, and as to one also of very great importance which is about to follow. And, independently of this medical importance, it is particularly necessary in all that relates to prevention: since, as long as these fevers shall be believed to be typhus, or to arise from any other cause than what they really do, it is plain that it will be impossible to apply any means of diminishing their numbers, whether by avoidance of the causes or by directly removing or checking the influence of these. And I am sure that a more accurate attention, recently bestowed on fevers by practitioners, will confirm what I have said respecting the comparative rarity of proper contagious fevers ; if these persons have not yet agreed to refer them to malaria, and have not learned to discover the various sources whence this is produced, and which I have here been attempting to point out and explain.

There is yet one important source of malaria, arising in rural situations, which I am bound to explain, and which will complete what I have to say of the causes of its diseases on shore: this is the steeping of flax and of hemp ; though with us the latter is rare, and the former not every common ; except in Ireland and in Scotland, where, from the nature of the climate, it is probably not very pernicious. In Germany, France, and Italy, this has been fully proved to be one of the most active and severe causes of fevers of this character ; as, in New York and elsewhere, it has been similarly proved that the same consequences, productive also of mortal epidemics, have arisen from the putrefaction of coffee, potatoes, the refuse of the indigo manufacture, and other similar causes.

It remains to notice but one case, and that an unsuspected one,—a case also which will probably be disputed—while I cannot help thinking that it is one of very extensive and serious importance : this is, bilge-water. It is remarked, in the first place, that sugar-ships are peculiarly subject to bad fevers, and it is well known that the bilge-water from this substance is the most offensive that exists. Further, when a ship is termed sickly at sea, it is always found, that she is what is called a foul ship ; of which it would be easy to produce specific examples in abundance from the history of the navy, were it not from the hazard of personal offence. And to confirm this, when it has been found that the same ship was sickly under one commander, and healthy under another, the cause has been equally traced to the neglect of cleanliness. And how destructive fevers have often been at sea, both in the naval and the commercial service, needs not be said ; as it is unnecessary to point out the great loss of lives, the serious inconveniences, and, what is here especially necessary, the absence of every other cause of fever in such a situation as the open sea.

Now, if this class of fever is not the marsh fever, or the produce of a vegetable malaria, it would form a singular exception to all the analogies recently pointed out ; while, further, if it is not so, whence does it occur in exact proportion to the presence of vegetable matter acted on by water, and whence especially does it occur in so marked a manner in sugar-ships ? But it is generally, perhaps always, considered as a contagious fever, and perhaps not unnaturally, under the neglect of this cause ; since amidst such a crowd, it is an easy conclusion that the disease is communicated from one to another. Yet, if it were so, the cleanliness to which I have alluded would not be a prevention ; and in which, were it a contagious fever, it would far more often spread to the people on shore, after reaching harbour, than it is found to do. That there are sometimes contagious fevers on shipboard is most certain ; but such cases and their consequences must not be allowed to mislead us on so important a point as this,—since this would be to deprive us of the means of prevention which are in our power, and which are as simple as they are efficacious.

But not to dwell any longer on this case, important as it is, I cannot help thinking, that when naval surgeons shall have duly considered this subject, they will learn to coincide in the same opinion ; and if it should prove so, then may we learn to prevent at sea, that which has been a most grievous evil in more ways than it is necessary to point out. And if ships were to be duly fumigated with sulphurous acid before receiving fresh crews in harbour, with such further precautions as to the people themselves are well understood ; and if, in addition, care was taken daily to wash the ship, so as that the pump should bring up water as clear as the sea without, it is almost impossible that diseases of this character should ever make their appearance, or impossible, at least, that they should ever again be a cause of serious suffering.

But I must terminate a paper which is rapidly exceeding its stipulated bounds ; intending, in a future communication, to complete the subject, by describing what belongs to the propagation of malaria ; what relates to the effects of climate ; and what further may be done as concerns what is the real object of this paper,—the prevention or diminution of disease.

II.—*Account of a New Method of projecting Shot.* From the Proceedings at the Royal Institution.

[Journal of Science and the Arts, vol. III. N. S.]

Mr. Brockedon gave some account of a new method of projecting shot, which had been discovered by Mr. Sieviere the sculptor. Mr. Sieviere had furnished Mr. Brockedon with a report of his earliest experiments, and to some of a later date Mr. Brockedon was an eye-witness. The discovery was accidentally made many years since by Mr. Sieviere, who was one evening amusing himself with a pewter syringe, which he had converted into a cannon, having closed the discharging end of the syringe, and made a touch-hole. Into this cannon he put some pinches of gunpowder, and discharged the piston from it, which fell harmless at a short distance; happening to invert the order of firing by holding the piston, the syringe wa. discharged with so much violence, as to pass through the ceiling and floor into the chamber above that in which he sat. He was struck with the prodigious difference of effect produced, and immediately had a shot cast, which in form was like a mortar: this he fired from a solid mandrel, or bar swung upon trunnions, and capable of elevation and adjustment. His experiment succeeded so entirely, that he was induced to make a shot with radiant bars, which, though they added little to its weight, added much to its power of destruction to rigging, &c. The weight of this shot, which was of cast iron, was 15 pounds; this was discharged through a bank of clay 6 feet thick, and fell 20 yards beyond it. When fired again, it hit point blank at distance of 175 yards, and was buried above 3 feet in the bank; the chamber of this shot, with which a touch-hole communicated, was precisely like that of a mortar, and when it was placed for firing upon the mandrel, the shoulder of the chamber at the bottom of the calibre rested upon the end of the mandrel. The chamber contained a charge of 1¼ ounces of gunpowder. An experiment was made with a shot which weighed 25 pounds, but a charge of 2¼ ounces of gunpowder was so great as to burst it, and to throw a fragment of 5½ pounds' weight to a distance of more than a quarter of a mile. Subsequent experiments with shot of wrought and cast iron of different forms, confirmed the fact, that shots discharged with the magazine within them were projected with a force greatly exceeding that which the same quantity of gunpowder applied in the usual way would effect.

Mr. Brockedon attempted to account for this greater force by supposing, that the power usually wasted in the recoil of the gun, was added to the force by which the shot and mandrel were separated. He stated, that no recoil in common gunnery took place, until the shot had left the cannon; and offered the following proofs of this fact. It is a common practice to fire a cannon suspended from triangles: the mark against which it is directed being hit, if any recoil had taken place before the ball left the cannon, the ball must have struck some other point tangential to the circle which its point of suspension would describe. Mr. Brockedon mentioned that Mr. Perkins, in the course of some experiments upon recoil, had fastened a loaded rifle barrel to the edge of a horizontal wheel, which moved freely upon a vertical axis; the rifle was directed, and hit the mark, though the recoil whirled the rifle and wheel round with great velocity. Mr. Brockedon illustrated this further by supposing a boat on still water, and imagining a plank placed from stem to stern, and a man on it pushing with a pole a bundle of hay from him along the plank, the separation of the hay from the man could not affect the situation of the boat on the water, whilst the hay was on board; but if the hay were thrust over, the moment it became independent of the boat, the man and boat would separate from the hay with forces proportioned to their densities.

The space required to contain the products of the combustion of gunpowder, has not been determined by careful experiments, but vaguely stated by some at 500, by others at 1000 times the volume of the powder; taking the lowest statement, and supposing a cartridge to be six inches long, and the length of the gun to be five feet, not more than about $\frac{10}{500}$ of the force generated by combustion operate upon the ball: the remaining $\frac{490}{500}$ths are wasted upon a recoil, which, overcoming the vis inertiæ of the gun with its carriage, (which weighs between three and four tons for a 24-pounder,) will drive it back, in garrison service, against an inclined plane 18 inches or two feet. The instant the ball leaves the gun the recoil takes place, and the products of combustion, which remain within the gun in a highly condensed and heated state, are opposed in their escape by the vis inertiæ of the external air from which the gun recoils. If recoil were attempted to be explained simply upon action and reaction, by the intervention of a force separating two bodies, whose resistances were as their densities, then the recoil would prevent any certainty of striking the point or mark against which the cannon is directed; contrary to the effect proved by firing from triangles, Mr. Perkins's experiments, and the practice of every sportsman. The recoil too takes place, whether the firing be with blank or ball cartridge; if the recoil be greater with a ball, it arises merely from the resistance which this offers to the discharge, allowing time for the more complete ignition of the gunpowder, and the generation of a greater force; hence also the effect of a rifled barrel, and the recoil of a foul gun.

The sky-rocket illustrates also this theory of recoil. To increase the surface of the composition exposed to combustion, the rocket is bored conically, nearly to its entire depth; the products of this combustion are met by the atmosphere as they rush from the neck of the rocket, and recoil from the resistance; as the cone enlarges, the force increases, and accelerates the ascent of the rocket, poised and directed as it is by the rod. The difference between the effects of the rockets and the shot is, that in the former the force increases from the gradual, but increasing surface in combustion; in the latter, the force is at once generated, and the aid to the force which separates the shot from the mandrel is greatest at first, and though gradually lessening, always adds something to the force of the discharge, until the air within the calibre is equalized with the atmosphere.

The father of the late Sir Wm. Congreve tried some experiments with shot fired from a mandrel; but as he bored the mandrel into which the discharge was put, and did not put the magazine in the chamber of the shot, they failed.

As the expense of trying experiments with Mr. Sieviere's engines is too great for an individual to incur, the probability of its becoming a most destructive engine in warfare ought to recommend it to the serions attention of Government. The advantages of the lightness of the mandrel and the unlimited weight of the projectile are immense. When the experiment was made with the 25-pound shot, an invalid watchman carried the cannon shot and ammunition upon his head to Primrose Hill, before breakfast; and the safety of the engine to those employed in its use may be shown in the fact, that the shot which burst did no injury to the gunner; and no mischief could happen, for if the shot burst without advancing from the mandrel, the fragments dispersed at right angles, and if with any projection, in lines resulting from the united forces, leaving the gunner in safety. The recoil of the mandrel is very small, and arises only at the moment of separation from the pressure of the gases, which, escaping from the calibre, presses upon the end of the mandrel with effect proportioned to its surface.

III.—*Notice of the Theory of* M. Dutrochet *on the immediate or proximate Cause of vital Action in Animals and Plants.*

[From No. IX. of the Gardener's Magazine.]

The name of Dutrochet is well known in the scientific world, as connected with anatomical and physiological researches. (*Gard. Mag.*, vol. i. p. 76. and vol. ii. p. 254.) In the present work he is considered to have surpassed all his predecessors in illustrating the laws of vital motion in plants; and, as far as we are able to form a decisive opinion on the subject, we think he has discovered the immediate cause of the ascent of the sap. The various phytologists who have preceded Dutrochet, and especially those of the latter end of the last century, have afforded us a very correct knowledge of the organs of plants; but it seems to have been reserved for this philosopher, to have determined their use in a more precise manner than has hitherto been done.

The organs by which the sap ascends, M. Dutrochet has demonstrated to be those simple tubes, utterly devoid of valves, and without any lateral intercommunication, which are situated both in the soft and hard wood. They are the lymphatic tubes of Decandolle, the false tracheæ of Mirbel, and the corpusculiferous tubes of our author. They are not found in the bark, nor in the pith; and it is well known that the sap does not rise through these parts of the stem.

The proper juice, or sap, after it has been changed by the leaves into a nutritious fluid, according to M. Dutrochet, descends both by the bark and the alburnum or soft wood, through tubular oblong cells. These oblong cells give out the nutritive juice contained in them, through their sides; and in spring, when the sap ascends, it takes up a portion of this juice for the developement of the leaves and the growth of the plant. The pith has neither the power of conducting the ascending, nor the descending sap. M. Dutrochet agrees with Linnæus, Dr. Darwin, and others, considering it to be, to the vegetable, what the brain and spinal marrow are to the animal. Dr. A. T. Thomson (*Lectures on Botany*, vol. i. p. 379.) conjectures that the pith is intended chiefly to give bulk and stability to the young shoot; because, whenever this becomes ligneous and able to support itself, the pith dries up and diminishes in volume rather than increases.

Besides the vessels for the ascent of the sap, and those for the descent of the proper juice, there are a third description of vessels, which radiate from the centre of stems to their circumference, and are commonly called medullary rays. These rays are composed of oblong tubes or cells, extending from the centre of the wood to the bark, where they are met by similar tubes, in apparent but not real continuity in the bark. The radiated tracheæ of the wood give out the ascending sap from the lymphatic tubes, and the radiating vessels of the bark give out the prepared sap, or proper juice, from the descending tracheæ, or oblong cells, or the bark. The juice and the sap, thus poured out between the wood and the bark, unite in forming a nutritive fluid which, consolidating, adds to the bulk of the plant, in the form of alburnum or soft wood, and liber or inner bark. This, it must be acknowledged, is a very simple and beautiful theory, and altogether consistent with matter of fact.

The next point which M. Dutrochet proceeds to determine is, the cause of the progression of the sap in the lymphatic tubes, oblong cells, and radiating tracheæ. It is unnecessary to trace the proofs, that there is no actual circulation of the sap in plants but merely an ascending and descending current, and a lateral diffusion and union. The condition of a plant requisite to admit of the exercise of these functions is, the susceptibility of becoming turgid by the application of water; in other words, that which distinguishes a dead plant from a living one is, the turgidity of its cellular parts. A dead plant may have its tubes, cells, and tracheæ filled with fluid, but these will never become turgid: a living plant, on the contrary, which has been apparently dead, when one extremity is placed in fluid, becomes filled with it throughout to an excess productive of turgidity. In a separated part of a plant, the ascension of the sap depends on the susceptibility of turgidity of all the parts of the section. In a plant growing in the soil, the cause of turgidity, or of the rise of the sap, is to be found in the minute conical bodies which terminate each radicle. M. Dutrochet, by careful examination with a microscope, found that the minute conical termination of the radicle was furnished with other projecting bodies, like sponges, which perform the office of the piston of a syringe, and have the power of introducing into their cavity, and through their sides, the water which comes in contact with their exterior surface, and which, at the same time, opposes the exit of any fluid these spongioles imbibe. To this power M. Dutrochet has applied the term endosmose (*endon*, inward, *osmos*, impulse); and he has proved its existence, on a larger scale, in the cœcum or blind gut of a young chicken, which he filled half full of milk, firmly closed at the open extremity, and then immersed in water. At the end of twenty-four hours the cœcum had imbibed seventy-three grains, and at the end of thirty-six hours, one hundred and seventeen grains of water, and become very turgid. From this time its weight diminished; and, at the end of thirty-six hours, it had lost fifty-four grains of the water which it had absorbed, and the milk had become putrid. This experiment M. Dutrochet considers as demonstrating, that the absorption of the water depends on the fluid in the cavity being denser than that which surrounds the organ; and that, as long as this dense fluid remains undecomposed, the endosmose, or absorption continues; while, as soon as it becomes putrid, the endosmose ceases, and the water passes out of the organic cavity as rapidly as it had entered it. Farther experiment proved to M. Dutrochet, that when the cœcum was filled with a thinner fluid than that in which it was immersed, this thin fluid passed out of it into the other. This action he call exosmose (*ex*, out, *osmos*, impulse). He farther proved that fluids of a less density than water, when the solution contained in the cœcum is alkaline, produce endosmose; and, when it is acid, exosmose.

It may readily be supposed that, if the end of the cœcum, instead of being firmly closed, had been furnished with a small tube, the absorbed fluid, or endosmose, instead of producing an excess of turgidity, would have mounted in the tube. This M. Dutrochet found to be the case. He fixed the open end of a glass tube into the cœcum of a chicken filled with a solution of gum and water; and having immersed the cœcum in rain water, and supported the tube in a vertical position, he found that in twenty-four hours the fluid had ascended to the top of the tube, and that it continued to ascend and overflow at the top for three days. On the third day the water began to sink in the tube; and on the fourth day, the cœcum being opened, the fluid was found to be putrid. The experiment was repeated with the bladder of the carp, and with the inflated pod of the common bladder senna (Colutea arborescens), with equal success.

The next thing that M. Dutrochet endeavoured to discover was, the cause of fluids passing through organic substances possessing the action of endosmose and exosmose; and, as the contact of bodies of different densities, as zinc and copper, is a well known cause of electricity, that power naturally occurred to him as sufficient to account for the phenomena related. He proved this by an experiment, which it would occupy too much room to relate, at sufficient length, to convey useful ideas to the general reader. The man of leisure and science will have recourse to M. Dutrochet's book; and the gardener may safely adopt it as a proved fact, that that immense power, in a bleeding vine with a bladder tied round the extremity, which Mr. Braddick (*Hort. Trans.*, v. p. 202) found distended with the rising sap till it became as hard as a cricket ball, and which burst at the end of forty-eight hours, has been clearly traced to the difference between the specific gravity of the water of the soil, and that of the nutritious fluid contained in the almost invisible points, or spongioles, which form the extremities of the fibres of all plants.

The effect of temperature on endosmose was, to increase the process, which is a proof of the influence exerted by electricity on the process; it being a well known fact, that by increasing the temperature of two metals which produce electricity, the electrical current is increased in intensity.

In applying his observations to the vital statics of plants, M. Dutrochet's turgidity is shown to be produced and maintained by endosmose, and the accumulated sap reacted on by the natural elasticity of the sides of the minute organs which contain it. Endosmose in the leaves takes place, to supply the vacuum which is created by the transpiration of water from their surfaces. This explains the reason why cut plants absorb water, and why the roots and stem of a plant supply what sap they have to the leaves, after being taken out of the ground. The difference of plants, in regard to the temperature they require for the flowing or rise of the sap, is known to vary materially in different plants; and M. Dutrochet, with great apparent reason, thinks this is attributable to their different physical capacities for producing electrical currents.

The substance of M. Dutrochet's theory may thus be summed up. The sap of plants does not circulate; it ascends from the root in the wood or comparatively woody parts of stems and branches, and, being elaborated into nutritive juice, descends by the vessels of the bark. The sap in ascending, and the juice in descending, are diffused laterally by horizontal vessels, which meet in common in the space between the bark and the wood. The sap and proper juice are poured out in this space, and these unite and form increments both of bark and wood. All the motions of the sap and juice in plants take place in consequence of the operations of two distinct currents of electricity: the one negative, by which the vessels have the power of absorption, which M. Dutrochet calls endosmose, and by which the vessels become turgid; and the other positive, by which the vessels exude or secrete, which power M. Dutrochet calls exosmose.

It is gratifying to find, that none of the results of M. Dutrochet's experiments are materially at variance with the opinions of Mr. Knight: in some minor points their hypotheses differ; but all the important doctrines of Mr. Knight are confirmed and established by M. Dutrochet, on a basis which will not be easily moved.

IV.—*Observations on the Manufacture of writing Ink.*—By Dr. Ure.

[From the 3d vol. of the Mechanic's Magazine.]

Common ink for writing is made by adding an infusion or decoction of the nut-gall to sulphate of iron, dissolved in water. A very fine black precipitate is thrown down, the speedy subsidence of which is prevented by the addition of a proper quantity of gumarabic. This is usually accounted for by the superior affinity of the gallic acid, which, combining with the iron, takes it from the sulphuric, and falls down. But it appears as if this were not the simple state of the facts; for the sulphuric acid in ink is not so far disengaged as to act speedily upon fresh iron, or give other manifestations of its presence in an uncombined state. According to Deveux, the iron in ink is partly in the state of a gallate.

M. Ribaucourt paid particular attention to the process for making black ink, and from his experiments he draws the following inferences. That log-wood is a useful ingredient in ink, because its colouring matter is disposed to unite with the oxyde of iron, and renders it not only of a very dark colour, but less capable of change from the action of acids, or of the air. Sulphate of copper, in a certain proportion, gives depth and firmness to the colour of the ink. Gumarabic or any other pure gum, is of service, by retarding the precipitation of the feculæ; by preventing the ink from spreading or sinking into the paper; and by affording it a kind of compact varnish or defence from the air when dry. Sugar appears to have some bad qualities, but is of use in giving a degree of fluidity to the ink, which permits the dose of gum to be enlarged beyond what the ink would bear without it Water is the best solvent.

Lewis had supposed that the defects of ink arise chiefly from a want of colouring matter. But the theory grounded on the fact discovered by M. Ribaucourt requires, that none of the principles should be in excess.

It is doubtful whether the principles of the galls be well extracted by maceration; and it is certain, that inks made in this way flow pale from the pen, and are not of so deep a black as those wherein strong boiling is recurred to.

From all the foregoing considerations M. R. gives these directions for the composition of good ink:—

Take eight ounces of Aleppo galls (in coarse powder); four ounces of *log*-wood (in thin chips); four ounces of sulphate of iron; three ounces of gumarabic (in powder); one ounce of sulphate of copper; and one ounce of sugar candy. Boil the galls and logwood together in twelve pounds of water for one hour, or till half the liquor has evaporated Strain the decoction through a hair sieve or linen cloth, and then add the other ingredients. Stir the mixture till the whole is dissolved, more especially the gum; after which, leave it to subside for twenty-four hours. Then decant the ink, and preserve it in bottles of glass or stone ware, well corked.

Many recommend, that the sulphate of iron should be calcined to whiteness. Mr. Desormeaux, jun. and ink manufacturer in Spittalfields, has given the following in the Philosophical Magazine, as the result of much experience:—Boil four ounces of log-wood about an hour in six beer quarts of water, adding boiling water from time to time; strain while hot; and when cold, add water enough to make the liquor five quarts. Into this put one pound avoirdup. of blue galls coarsely bruised; four ounces of sulphate of iron calcined to whiteness; three ounces of coarse brown sugar; six ounces of gumarabic; and one fourth of an ounce of acetate of copper, triturated with a little of the decoction to a paste, and then thoroughly mixed with the rest. This is to be kept in a bottle uncorked about a fortnight, shaking it twice a day, after which it may be poured from the dregs, and corked up for use.

Dr. Lewis uses vinegar for his menstruum; and M. Ribaucourt has sulphate of copper among his ingredients. I have found an inconvenience from the use of either, which, though it does not relate to the goodness of the ink, is sufficiently great, in their practical exhibition, to forbid their use The acid of the vinegar acts so strongly upon the pen, that it very frequently requires mending; and the sulphate of copper has a still more unpleasant effect on the penknife. It seldom happens, when a pen requires mending, that the ink is wiped very perfectly from it; and often, when the nib only is to be taken off, it is done without wiping at all Whenever this is the case, the ink immediately deposits a film of copper upon the knife, and by superior elective attraction of the sulphuric acid, a correspondent portion of the edge of the knife is dissolved, and is, by this means, rendered incapable of cutting till it has been again set upon the hone.

If a little sugar be added to ink, a copy of the writing may easily be taken off, by laying a sheet of thin unsized paper, damped with a sponge, on the written paper, and passing lightly over it a flat iron very moderately heated.

Inks of other colours may be made from a strong decoction of the ingredients used in dyeing, mixed with a little alum and gumarabic. For example, a strong decoction of Brazil wood, with as much alum as it can dissolve, and a little gum, forms a good red ink. These processes consist in forming a lake, and retarding its precipitation by the gum.

Original Communications.

I.—*On the Hygrometric Scale of the wet-bulb Thermometer.*

On a former occasion*, I ventured to give an approximate table of the values of the depressions of the wet-bulb thermometer; but as I had not then a sufficient series of comparisons with the dew point hygrometer, to establish the soundness of my views, I now return to the subject prepared to supply this defect, and more confident, at the same time, that no instrument can so well be applied to the meteorological branch of hygrometry.

There are two methods of expressing the hygrometric state of the air; either by the absolute quantity of aqueous vapour existing in a given space; or by the proportional tension of the vapour compared to the maximum moisture, capable of being sustained by the air at any temperature: the latter is in every respect the most convenient expression, since the quantity of moisture varies for every temperature, and it may be readily obtained from the usual tables, when the tension and the heat are registered.

As in all other hygrometers, then, two points, namely, those of extreme moisture and extreme drought, require to be ascertained, the space between which may be divided into one hundred commensurable parts, to be called the scale of tensions.

The first of these is self-evident; for, when the air is fully saturated with moisture, evaporation, and consequently the cold produced by it, is = 0. This point is, however, generally termed 100 on the scale, whence it may be premised, that in finding a formula for the tensions depending on the depression of temperature of an evaporating surface, the *complement of the tensions* will be the most convenient index.

To find the other fixed point of the scale, or the diminution of temperature which would attend evaporation in an atmosphere perfectly dry, i. e. the maximum depression, is by no means so easy a problem, since the air is never altogether devoid of moisture in the driest weather, neither is it readily deprived thereof by artificial means.

M. Gay-Lussac at one time turned his attention to the solution of the wet-bulb problem, and it is to be regretted, that this able philosopher should have quitted the field, (no doubt for others of vaster interest and importance,) before his investigations were completed. He has, however, left us, in the Annales de Chimie, a valuable table of depressions obtained by passing a current of perfectly dry air over a thermometer with a moistened bulb. They are as follows, converted into Farenheit degrees.

Temperature.	Depression.
32.°	9.°98
33.8	10.96
40.	12.7
50.	16.1
55.	18.0
60.	19.9
65.	21.8.
70.	24.0
75.	25.7

As is usual with experiments made in Europe, this scale is unfortunately not extensive enough to be adapted to our climate; at any rate, it appeared desirable to confirm, by actual trial, the rate of progression which might be deduced from it for higher temperatures. This object was pursued in the following manner.

* Or. Quart. Mag.

A large gasometer containing 120 pints of air was well dried, and the reservoir thereof filled with pure cocoanut oil instead of water, it having been previously ascertained under the pump that this oil did not give out the least moisture. Above the oil and, consequently, within the air chamber, was suspended a large flat dish of concentrated sulphuric acid, which has the property of abstracting moisture more powerfully than any other substance. The gasometer was connected with the receiver of an air pump by tubes, so arranged that air could not pass into the receivor, without circulating through a large quantity of muriate of lime, which had been previously heated to redness. A thermometer and a hair hygrometer were placed within to note the heat and drought; and a stopcock at the top opened a communication with a small chamber containing the minute thermometer with a bulb covered with moistened cotton. By applying weights to the gasometer, a steady draught could be maintained through the apparatus, without the slightest variation of the hygrometer, although the exterior air, from the season, was loaded with vapour.

A certain velocity of the air was found to produce the greatest depression;—from 70 to 80 inches in the minute, which is hardly a perceptible breeze. The air was never completely dry, for muriate of lime does not appear capable of carrying the desiccation lower than 1. 5, per cent. of aqueous tension or 9 degrees of the hair hygrometer, at which point it continued from August to October without alteration.

Upon opening the stopcock the wet thermometer began instantly to fall; and as the minutest circumstances in experiments conducted with tolerable care may always turn to some use, the rate of cooling was several times noted down during equal periods of time. It was thus ascertained that the curve which would nearly represent the rate of cooling, approached to the form of a quadrantal arc; for, calling the time in which the maximum depression was attained, radius, the intermediate depressions were nearly as the sines or ordinates, to the abscisses of the radius, or the intermediate times: in other words $D = m\sqrt{ab}$ where $b = 2$ radius $- a$
and $a =$ the time, in parts of the radius.

Time in minutes and seconds.	Observed depression wet-bulb in degrees.	Calculated depression in degrees.
0 30	10°	10°7
1 00	16	15.0
1 30	19.2	17.7
2 00	21.3	20.0
2 30	22.6	21.7
3 00	23.6	23.0
3 30	24.3	23.9
4 00	24.5	24.5
4 30	24.9	24.9
5 00	25.0	25.0

The temperature of the wet-bulb at the beginning of these experiments was 80°, that of the exterior air 85°, and the maximum depression a little more than 30 degrees.

It will be remarked in the following table, that the depressions obtained by my experiments are a degree in excess of M. Gay-Lussac's. This may either be owing to a greater draught of air, or the employment of a very minute thermometer. Over concentrated sulphuric acid where the air was perfectly still, the depression was three degrees less at 90°, than in the current of dried gas, and in the open air at 75°, when the wet-bulb thermometer stood at 64.5. I have found that brisk agitation would lower it one degree, which proves that regard must be always paid to this cause of irregularity. The following is an abstract of the experiments thus conducted.

Wet-bulb Thermometer in a current of dry Air.

Barometer.	Thermometer in receiver.	Wet-bulb. Therm.	AqueousTension by Hygrometer.	Depression.
29.43	72°5	47°2	.015	25°3
.52	75.0	48.2	.015	26.8
.30	78.5	52.1	.015	26.4
.26	82.6	54.8	.015	27.8
.25	83.5	54.5	.013	29.0
.30	84.7	55.0	.015	29.7
.30	85.0	55.0	.015	30.0
.20	85.0	54.8	.012	30.2
.15	90.2	56.8	.015	33.4
.15	90.3	56.7	.015	33.6

Wet-bulb Thermometer suspended over concentrated sulphuric Acid in a close Vessel.

Barometer.	Thermometer in receiver.	Wet-bulb. Therm.	AqueousTension by Hygrometer.	Depression.
29.10	90.0	59.7		30.3
	109 0	66.7		42 5
	116 0	70.0		46 0
	125 0	73 9		51.1
	134.	76 0		58.0
	138.	77 0		61 0

To compensate the want of perfect dryness in the air, the depressions in the last column must be increased from 0.5 to 0 7 of a degree. Another correction is necessary for the height of the barometer. The amount may be estimated from a table given in Mr. Daniel's Experiments on Evaporation, (Journ. Arts, XVII.) wherein that gentleman makes the amount of evaporation as nearly as possible in the inverse proportion of the pressure, while the temperature of the water on the wetted bulb decreases arithmetically for geometrical decrements of pressure.

Pressure Inches.	Ratio.	Evaporation. Grains.	Ratio nearly	Depression of Temperature.	Ratio.	Depression in Brewster's Encyclopædia, Art. Hygrometry.	Ratio.
30.4	1	1 24	1	9	0	5	0
15.2	$\frac{1}{2}$	2.97	2	12	*a*	9	*a*
7.6	$\frac{1}{4}$	5.68	4	15	2*a*	13	2*a*
3.8	$\frac{1}{8}$	9.12	8	18	3*a*	18	3*a*
1.9	$\frac{1}{16}$	15.92	16	21	4*a*		
.9	$\frac{1}{32}$	29.33	32	24.5	5*a*		
.5	$\frac{1}{64}$	50.74	64	26	6*a*		

Applying the rule, which seems to be pretty well established by these experiments, we shall find the correction due to a change of pressure from 30 to 29 inches, is at 90° only one-third of a degree; and therefore, for trifling variations of the barometer, this correction may be neglected. It appears from the experiments above, that the depressions of the wet-bulb thermometer in a perfectly dry air form a geometric series, increasing with the temperature. They may be nearly represented by the formula

$\text{Log. } D = \text{Log. } T \times 1.275 - 0.950$, but it is better to adhere to the experimental numbers, where practical uses are alone contemplated, and I therefore prefer forming, by means of a diagram, a system of numbers which shall approximate as nearly as possible to them: this is easily done by varying the difference gradually, to render the curve uniform, as is shown in the fourth and fifth columns.

Table of maximum Depressions.

Temperature.	Calculated.	Observed.	Nearest approx.	Diff.
30°	8.6	9.0	9.0	3.6
40	12.4	12.7	12.6	3.7
50	16.4	16.1	16.3	3.8
60	20 7	19.9	20.1	4.0
70	25.2	24.0	24.2	4.3
80	29 9	28.4	28.5	4.6
90	34 7	34.0	33.0	4.9
100	39 7	——	38 0	5.3
110	44.9	43.5	43.3	5.8
120	50.2	49.2	49.1	6.4
130	55.6	55.4	55.5	7.1
140	61.0	62.0	62.6	

Having then established the two extreme points of the required hygrometric scale, we may proceed to fill up the interval by means of series of comparative observations with the dew point hygrometer of which notice was made in the commencement. Unfortunately, the temperatures at which these comparisons were made, were almost entirely between 80° and 95° Farenheit, but it is reasonable to suppose that the same law which will accommodate this range of observations would hold good for other temperatures; and it will, therefore, be sufficient to settle with accuracy a single line of hygrometric tensions; and that for the temperature of 90 degrees, is the one to which our register can be most conveniently adapted.

I shall not attempt to copy at length the whole series, which were made between March and September, embracing, therefore, the driest and dampest state of the air. Each observation was inserted in a general diagram, comprehending the temperature, the depression, and the aqueous tension as given by the dew point hygrometer. This diagram presented to the eye several *groups* of the most prevalent states of the air, with regard to moisture; and from each of these could be derived an average, as a fixed point in the required table of values.

The figures in these groups varied among themselves four or five per cent. and frequently more, which proves the amount of uncertainty due to determinations by the wet-bulb: but it must be remembered, that this variation also includes the errors of the dew point process, which every observer knows to be liable to a considerable range of uncertainty: in fact, it is very probable, that the differential or wet-bulb thermometer kept constantly in action, may give a fairer average state of the air, than the momentary and somewhat troublesome experiment of finding the dew point, even by the rudest table which we may construct.

I have thought it right to confine myself in the following table to *dew point comparisons:* I had collected others with the hair hygrometer for the period of a year and a half, and they in a great measure agree with these; but the deductions from premises in a measure doubtful will always be looked upon with suspicion, whereas of the infallible *principle* at least of the dew point experiment there can be no doubt whatever.

Table of Aqueous Tensions observed.

Number Obs.	Temp.	Wet-bulb. Depression.	Aqueons Tension.	Depression reduced to temp. 90°
7	85°	3°5	83.1	3°7
12	87.5	5.7	76.1	5.9
12	90.	9.5	65.4	9 53
6	94.	13.0	52.3	12 3
6	92.5	17.0	41.0	16.8
13	87.0	20.	26.0	20.
8	92.	24.	17.9	23.3
by former table.	90.	33.1	0	33.1

The numbers in the last column are seen at once to be of geometrical increment; that is, while the quantity of vapour in the air diminishes, the depression increases as some power of this diminution. It is not worth while to seek an exact formula to represent the ratio*, (the exponent of which is, however, about 1.5); for by a diagram, or by trial and correction of the second differences, a series may be interpolated quite accurate enough to serve all practical purposes : and the depression being thus found, say for every 10 per cent. of aqueous tension, (whose maximum is called 100,) at the temperature of 90, the table may be extended to other temperatures by the proportions already discovered of the maxima depressions corresponding to them.

Depressions of the wet-bulb Thermometer in Degrees.

Temp.	*Tension of aqueous Vapour.*									
	90	80	70	60	50	40	30	20	10	0
30°	0°.6	1°.3	2°.0	2°.7	3°.5	4°.4	5°.3	6°.4	7° 6	9°.0
40	0.9	1.7	2.7	3.8	4.9	6.1	7.4	8.9	10.6	12.6
50	1.1	2.3	3.5	4.9	6.3	7 9	9.6	11.5	13.7	16.3
60	1.4	2.8	4.4	6.0	7.8	9.7	11.8	14.2	16.9	20.1
70	1.7	3.3	5.3	7.3	9.4	11.7	14.3	17.2	20.4	24.2
80	2 0	4.0	6.2	8.5	11.0	13 8	16.8	20.2	25 1	28.5
90	2.3	4.7	7.2	9.9	12.8	16.1	19.6	23.5	28 0	33.1
100	2.7	5.4	8 3	11.4	14.7	18.4	22.4	27.0	32 1	38.0
110	3.0	6.1	9.4	13.0	16.8	21.0	25.6	30.7	36.6	43 3
120	3.4	7.0	10.7	14.7	19.1	23.8	29.0	34 9	41.4	49.1

From the foregoing table, an inverted one may be formed, showing the tensions corresponding to given depressions, but it will answer sufficiently well as it is for the same purpose, with a littlo more trouble only in making the interpolations. I shall conclude with a few examples of its application to the observations of different individuals :— the data of the first five are taken from the article HYGROMETRY in Brewster's Encyclopædia ; the succeeding four, are from observations made by an officer on the river ; the remainder are from my own register (the tensions of the first five are derived from the hair hygrometer.)

* The author of the article HYGROMETRY in the Edinburgh Encyclopædia, gives a formula for the depression of the wet-bulb, dependant upon the tension of vapour at the given temperature, of which he argues that tho evaporation, and consequently the cold produced thereby, must be a function. Assuming F the maximum tension, f the actual tension by the dew point, B and b the standard and the actual pressures, and p and r quantities determined by experiment, he makes

$$D = \frac{B}{b}\left(p - \frac{D}{r}\right)(F - f)$$

This formula suits the temperature at which it seems to have been adopted (about 65°) but unfortunately no other ; for if we calculate the maxima depressions for the temperatures of 30, 60, 90, and 120, rejecting from the formula B b and f, and calling $p = 36$ and $r. = 10$ as determined by the writer ; we shall find D respectively equal to 7.0°, 28°, 45°, and 103°, instead of 9, 20, 33, and 49 as proved by experiment.

The table constructed by myself may be nearly represented by a formula depending on the temperature and pressure, the latter of which may generally be neglected.

$$d = (.001\, t^{1.5}) \div (.112\, f^{1.275}) \times \sqrt{\frac{P}{p}}$$

or where the depression is given, the tension t will be

$$t = \sqrt[1.5]{\frac{1000d}{.112\, f^{1.275}}} \times \sqrt{\frac{P}{p}}$$

in this expression t is the *complement* of the tension, (making the maximum tension 100,) f is the temperature, and d the depression of the wet bulb observed. The formula becomes less accurate as the depression approaches zero.

Temperature of the air.	Wet-bulb Depression.	Tension by dew point.	Tension by Table of Depressions.
53°7	6.2	50	48
54.4	6.9	52	50
60.0	8.5	48	46
72.0	5.0	74	72
68.2	12.2	45	38
92.5	11.5	59	57
94.3	12.8	55	52
90.3	7.8	70	69
89.	7.0	74	70
90.5	23.2	15.3	21
84.5	17.5	30	31
93.	24.6	18.4	19
87.5	21.3	20.	22
91.8	29.4	11.3	9
93.2	27.	11.4	14
96.5	30.3	11.	10
88.7	20.	26	28
85.	3.3	84	84
88.4	6 6	71.5	71.5
89.8	9·8	65	60
80.	12·8	50	44
81.3	5.3	77	75
94.	13.	51	52
95.6	8.1	54	53

For those who have frequent occasion to learn the aqueous tension of the atmosphere by the dew point experiment, it will save a troublesome calculation to have a table of reference for the value of the tensions in depressions of the dew point. I subjoin, therefore, a small table made on this principle. The distance of the dew point varies so little for the different temperatures, that one column would nearly answer for the whole, but in atmospheres nearly deprived of moisture, Daniel's method becomes nearly useless, from the great degree of cold required to produce deposition.

Temperature.	Depression of the point of deposition.	Tension of vapour.				
		75	50	25	10	0
30°		8°	20°	40°	60°	infinite.
50°		9	20	40	62	
70°		9	20,5	41	65,5	
90°		9,5	21,7	43	69	
110°		10	23,8	45	72	

II.—*On the Principles by which we ought to be guided in Shoeing and cutting the Hoof of the Horse.*

We may assume, in the first place, that the power of offering and of opposing resistance which exists in masses of horny substance, like those of which the walls of the horse's hoof are composed, is in some proportion to the actually existing quantity of the mass.

We may further assume, that nature, under the ordinary circumstances of unshod hoofs, exactly proportions the quantity of horny matter in the fore-part of the foot to the quantity in the hind part; and that, where the wear is likely to be greatest, there the growth of horn is likewise the greatest and *vice versa*.

And we may conclude from these considerations, (although perhaps the mode in which the effect is produced may not be palpable,) that while these natural proportions are preserved, there will be no change in the form of the hoof, from that which nature originally designed.

As the first step, then, towards ascertaining how we ought to treat the horse's hoof, while it is prevented by a shoe from being worn in those parts where wear would otherwise have taken place, let us learn, and keep stedfastly in mind, the provisions which nature has made for the preservation of these proportions in the quantities of horny matter, which are essential to the existence of a perfectly formed foot; and let us then consider, how these measures are counteracted, not only by the shoe itself, but by the knife of the farrier who, now that the necessity for expanded heels is acknowledged, proceeds to effect that object, in appearance at least, by paring out the inner side of the hinder part of the wall of the hoof, at that point where the weight of the animal principally rests, and where the walls turn forward to form what are denominated the bars.

Amongst a herd of colts running loose in extensive paddocks, we shall find, I believe, that in trimming the hoofs, there is little occasion, in general, for using the knife at the toe, that it is at the heel where the farrier has principally to work; this remark applies particularly to horses standing straight on their pasterns, whose hoofs, it is also believed, are generally remarkably wide at the heels.

Amongst horses in work, whose shoes have not been changed for a considerable length of time, we find the reverse to be the case; and that it is from the toe most horn requires to be pared away.

Amongst the colts, we meet with short toes, sound hoofs, high and expanded heels, the bars of the foot well defined, and the frog full and healthy.

Amongst the shod horses, we find long oval hoofs, narrow and low heels, diseased frogs, and the hinder part of the base removed to make room, as is supposed, for the development of the frog.

From the above it appears evident, that in shoeing horses a course, with regard to the removal of horn, is in general pursued, directly opposed to that which nature follows; and that, in place of being calculated to counteract the evils to be anticipated from stopping altogether the wear in certain parts, where nature has provided the means of meeting it, the ordinary system of shoeing does not even take this matter into consideration; and farther, that it effects a rapid destruction of the hoof in those parts, where nature has made no provision for meeting waste or wear.

From the circumstance of the feet of unshod colts being always found greatly worn at the toe, (the truth of which observation may be readily determined by reference to the officers in charge of the Company's Stud Depots,) I infer, that the wear at the toe, with young and restless unshod cattle is such, as more than to consume all that is provided by nature for meeting the consequent waste of horn; and that although the wear at the heel is, under these circumstances, comparatively trifling, still such proportions are maintained in the quantities of horn, as tend to keep the heels expanded. From the above, and from the circumstance of the hoofs of shod horses always requiring more cutting at the toe than at the heel, I again infer, that the growth of horn in that part of the hoof when shod with iron is much greater, than is required to maintain the equilibrium, although it is barely sufficient to meet the wear to which the unshod hoof would be subject, in a state of continual exercise; and I further infer, that if by any means we counteract this wear, and do not, by paring or otherwise, effect its removal, we most certainly ensure the accumulation of a preponderating mass of horn in the front part of the foot, to the utter destruction of those proportions, the continued existence of which we have assumed, as being necessary for the preservation of that form in the hoof, which nature originally designed.

If then the inevitable consequence of shoeing be, to cause the mass of horn at the toe to overpower the mass at the heel, and so (in some manner not perhaps understood by us) to produce contraction in the latter; what might the consequence be of still further reducing the mass of horn in the heel, by paring away the after part of the bars by cutting the heel low, so as to admit of the pressure of the frog upon the ground, and by rasping the quarters thin, under the idea that the frog will thus be enabled to expand, in consequence of its then meeting with less resistance from the walls of the hoof? If the hinder part of the wall of the hoof, and the inner part of the bars be cut away, or reduced, one might presume, that a contraction or falling in of the quarter would be inevitable; particularly, if the horn at the toe be, at the same time, permitted to spread unworn to an unnatural extent: and it appears a gratuitous assumption to suppose, that a tough elastic substance like the frog should, under the influence of any pressure from the superincumbent weight of the horse, be capable of forcing asunder piers of solid horn, such as exist at the heels of a hoof; particularly, when these very piers must sustain the principal weight of the horse, bearing too upon them in a false direction, as must be the case in all contracted feet.

Expecting to increase the growth of the frog, by cutting away the bars to make room for its development, is an attempt to undo what nature has already done,—to remedy, in fact, what has already been formed amiss; and is not an endeavour at correcting a false formation, while that formation is in actual progress. For, when the frog and bars of the hoof have grown down to be within reach of the farrier's knife, their formation has long since been complete; and no trimming, of either the one or the other, can be expected to produce any effect, beyond the mere reduction of size in that part, on which the knife has been brought to act. But if we take the same measures for ensuring open heels which nature follows, by preserving the due proportions of horn in different parts of the foot, we shall produce an effect, not on a substance already formed, but upon what may be termed an embryo substance, the vessels for secreting which will not fail to produce a sufficient quantity of proper matter, when the natural performance of their functions is no longer interfered with, by the undue pressure of the walls of the hoof.

To counteract as far as possible the preponderance, as I may say, of the mass of horn in the fore part of the hoof, which seems to be the inevitable consequence of using shoes, would appear then to be what ought principally to be held in view, in attaining the object of our pursuit; and I would recommend, therefore, at the time of fixing the shoes, first, that as much as possible should be taken from the length of the toe; then, that the shoe should be set well back on the horse's foot; that the knife should be used very sparingly in reducing the height of the heel; and that it never should be allowed to touch the hinder part of the wall, or the inner and after part of the bars. And as even by these means we may not altogether be able to preserve, during the whole interval between shifting shoes, the due proportion between the masses of horn existing in the fore and hind parts of the foot, I would further recommend the free use of the rasp on the front of the wall of the hoof.

It may be objected, that by leaving the heels so high we must place the foot in a position for which it was not designed by nature, and that we must thus run the risk of injury to the joints, and so forth. But I ask, do we not effect the same thing by permitting an undue growth of horn at the toe, and do we not thus produce an evil more to be dreaded than that which is objected to? We have, in interfering with nature, but a choice of evils allowed us; and I conceive, that in keeping on the side of high heels and short toes we err on the safe side; for it is against injuries and strains in the back sinew, that we require mostly to be on our guard; and I believe it will not be denied, that nothing brings that important tendon more frequently into jeopardy than low heels and long toes. Besides, if the objection holds against the short toe and high heel at the very time of shoeing, it is to be remembered that it will not be found to do so in a fortnight afterwards, on account of the more rapid growth of the horn which is proceeding in the fore part of the foot. Whereas, if the foot be cut into its proper form at the time of fastening the shoe, we ensure this growing evil, that with every successive day the foot will be departing more and more from its natural position, by the superior growth of the horn above pointed out.

If, for instance, we put a shoe upon a colt's foot, which we may suppose to be of the form nature designed, and if the shoes are to remain in their place during a month; the proportions of the masses of horn in the fore and hind part of the hoof will then be nearly just; but every day they will be becoming less and less so, owing to the superior growth of horn at the toe; during the second week the disproportion will have become greater, and during the third and fourth, the position of the foot will have been altogether false, thus ensuring during three of the four weeks an unnatural strain on the back sinew, while the mass of horn accumulated in the front of the foot will have become such as completely to destroy that equilibrium of the parts, which we are warranted, I think, in supposing nature designed to preserve.

If, on the contrary, we prepare this colt's foot with due allowance for the superior rapidity of growth of horn at the toe, if we put it in such a shape as will bring the foot to the natural position by the end of the second week; we shall have had, for the first and second week an irregularity on one side gradually correcting itself; we shall then have a perfect position of the foot; and during the third and fourth weeks, we shall have an irregularity on the dangerous side, only so great however as what would have taken place, at so early a period as the end of the second week, under the other system of management. If the existence of that due proportion between the masses of horn, in the different parts of the hoof, for which I contend, is destroyed by the existence of too small a quantity in the forepart of the foot, during the first fortnight, the evil consequences to be dreaded therefrom will be counteracted during the fortnight ensuing, and an equilibrium will thus in a manner be maintained.

But as every forced change, in the position of the foot, from that for which nature adapted the various joints and tendons, must be an evil dangerous in proportion to its extent; it will appear, that the greater the frequency with which the foot is brought into its natural position, and the oftener the equilibrium of the masses of horn is restored, the fewer will be the chances of injury, either to the joints, or the form of the hoof. In place then, of a change of shoes every month, as assumed above for illustration's sake, let us suppose them to be shifted every fortnight; and the hoof being trimmed each time, with reference to the change of form, from the unequal growth of horn, which the hoof must undergo during the interval, always aiming at having the foot in its proper shape and position during the middle of the term; and we make as near an approach to what appears, in theory at least, perfection in shoeing, as can be obtained; because by so doing we keep the relations of the different parts of the hoof more nearly in their natural state, and the extremity of the limb more nearly in its natural position, than we can do by any other mode of treatment.

The above are crude thoughts and conclusions, grounded upon what appear to be true and obvious principles. They are offered, not as the result of well conducted experiments, made with reference to the determination of the truth of the views submitted; but because the necessity for keeping the masses of horn in some sort of equilibrium does not appear to have occurred to any of those writers who have treated the subject scientifically, as being a desideratum in the treatment of the horse's hoof, and because it is still unfortunately found, that the majority of hoofs, after being in the hands of the farrier, do lose their natural shape, however smoothly they may be pared out and trimmed, with whatever skill the semblance of natural bars along the sole may be created by the dexterous use of the drawing knife, and with whatever art the heels may be made to assume the appearance of that width, which nature intended they should in reality possess.

III.—*On the Calculation of Heights, determined by Barometrical Measurement.*

To the Editor of Gleanings in Science.

Sir,

The method of measuring the heights of mountains by the barometer is well known. The formula generally employed when a book of logarithms is at hand is sufficiently short, and (to those acquainted with the principles on which it is founded) simple. Nevertheless I have found that to some it appears perplexing; add to which, a table of logarithms may not be immediately procurable in many cases. At all events I have thought that a simple method of deducing the results of barometric measurements, which, besides being independent of logarithms, should be entirely free from ambiguity, might prove acceptable to some of your readers. If you are of this opinion, you will oblige me by giving insertion to this communication in one of your early numbers.

I am,
Sir,
Yours obediently,
D.

Given the height of the column of mercury at two stations, with the temperature both of the air and of mercury; the former being shown by the detached, the latter by the attached thermometer. Required the difference of level of the two stations.

It is first necessary, if the columns of mercury be not of the same temperature, to reduce them to it. This is done by adding to the colder, or subtracting from the warmer $\frac{1}{10000}$ of its length for every degree of difference between the attached thermometers*.

The columns being reduced to the same temperature, the calculation of the difference of level is sufficiently easy, by attending to the following practical rule.

1st.—Take half the difference of the columns, and remove the decimal point five places to the right, adding as many ciphers as may be necessary. Divide by the sum of the columns †, the quotient is the approximate difference of level in English feet.

2dly.—Divide the sum of the columns by the difference, rejecting 100ths and 1000ths of inches. With the quotient, divide continually the approximate height found as above, reserving the alternate quotients, i. e. the 2d. 4th. 6th. &c. Then these quotients divided by the odd numbers 3. 5. 7. &c. give the 1st. 2d. 3d. &c. corrections which are in every case additive to the approximate height.

3dly.—Correct the result thus found for the temperature of the air in the folling manner. From the sum of the detached thermometers, subtract 16° 3, multiply the approximate height by this remainder, and divide by 1000 ‡. The quotient increased by $\frac{1}{10}$ of itself is the correction for the temperature of the air: it is additive. If we wish to be scrupulously accurate, we may subtract from this correction $\frac{3.5}{1000}$ of itself.

REMARKS.

This rule will give the difference of level with the same degree of precision as the logarithmic calculation, by attending to the corrections mentioned in the second part of it. In barometrical measurement, however, it would seem to be expecting more accuracy than the method is capable of, in the present state of our information, to be solicitous about such small differences as 10 or 12 feet. This being the case, the 2d part of the rule may be safely disregarded, in the calculation of heights that do not exceed 4 or 5000 feet; by which, the operation is reduced to a division by three places of figures. Even in differences of level amounting to 10000 feet, one correction will be found sufficient for every practical purpose.

As to the 3d part of the rule it is not peculiar to this method, being equally necessary when that by logarithms is resorted to. So that, upon the whole, to those not well versed in the use of those numbers, this method may be preferable. Certainly, for small elevations, it seems both shorter and easier.

Perhaps I should not omit to mention, that I have taken the rate of expansion for air as $\frac{1}{480}$ of its volume at 32° for each degree of Fahrenheit's thermometer. This is the determination of MM. Dulong and Petit, and, it is said, of Mr. Dalton and of M. Gay-Lussac. Puissant's formula is vitiated by his using a co-efficient, equal to 450 when reduced to Fahrenheit's scale.

I shall now take, as an example, the difference of level of the "Pic du Midi" above Tarbes, as measured trigonometrically by M. Ramond and barometrically by MM. Ramond and Dangos, the particulars of which may be seen in Puissant's Geodesie.

	Metres.	*Temp. Merc.*	*Temp. of air.*
Barometer summit of the Peak.	.537203	49.5 (F.)	39.2
Do. at M. Dangos.	.735581	65.6	66.4
		Diff. 16.1	Sum 105.6

* This operation is easily performed by prefixing two ciphers and the decimal point to the height of the barometer to be corrected, and multiplying by the difference of the thermometers. The product is the correction subtractive if the barometer be the warmer.

† When great accuracy is not required, it will tend to the materially shortening this division to reject 1000ths and 100ths of inches from the division. The error in heights of 10,000 feet can hardly exceed 10 feet.

‡ This is done by merely removing the decimal point, three places to the left.

The height of the barometers being expressed in metres makes no difficulty. It is of no consequence in what linear measure they are given, provided they be both given in the same. The rule requires, however, that the temperatures should be expressed in Fahrenheit's scale, and accordingly the indications of the centigrade thermometer, as given by Puissant, have been reduced to that scale as above.

1. To reduce the mercurial columns to the same temperature we have,

$$\frac{,735 \times 16,°1}{10,000} = ,0000735 + 16,1 = ,001176$$

And ,73558 —, 00118 = ,7344 the corrected length of the mercurial column. The correction is subtracted because the barometer ,73558 was the warmer.

The two barometers being ,7344
,5378

Their sum is 1,2716

Their difference ,1972

Half their diff. = ,0986 and removing decimal point 5 places to the right it will be 9860

Dividing by the sum 1,2716 we get 7754 feet, the approximate height or difference of level.

2. For the corrections.

Divide 1,2716 the sum by ,1972 the difference, the quotient is 6,45.

Now divide the approximate height 7754 continually by 6,45 the quotients are,

1047 1st.
163 2d.
25 3d.
4 4th.

Now divide the 2d. 4th. 6th. &c. quotients by 3.5.7. &c. for the 1st. 2d. and 3d. &c. corrections.

3)163 2nd quotient

54,3. 1st. correction.

5)4, 4th quotient

,8 2d. correction.

These corrections are always *additive*.

Approximate height	7754
1st. Correction	54.3
2d. Ditto	.8
Corrected height	7809.1

3. Correction for the temperature of the air.

From the sum of the detached thermometers, ...	105.6
Deduct, ...	16.3
Remains	89.3

Now the corrected height 7809, multiplied by 89°.3, and divided by 1000 gives 697,2, adding to this, 69,72 = $\frac{1}{10}$ it becomes 766,9 = the correction for the temperature of the air. Applying this correction to the approximate height 7809, the sum is 8575.9 the true difference of level by barometrical measurement. By geometrical methods it was found to be 8573,3.

IV.—*Remarks on Elasticity.*

When we say that a body is elastic, we simply mean, that on compressing it, if the pressure be removed, the body regains its original volume.

This is eminently the case with air. A correlative property of elasticity, is of course compressibility.

Air will bear considerable compression; in the state in which we breathe it, it is pressed by a column of mercury 30 inches in length.

Take a tube closed at one end, and at the other furnished with an air-tight piston. Suppose the piston loaded with a weight = to 30 inches of mercury. Then as there is also the weight of the atmosphere, the air is pressed by a double weight, and will be forced into one half the original space. Now supposing the temperature constant, the piston will rest say at *b*, *ab* being equal to *bc*. Now heat the cylinder, the pistion will rise to *a*. The weight then which a given bulk of air will endure without compression depends on the temperature.

The relation which the temperature bears to the weight is as follows: Under a pressure of 30 inches of mercury, air expands from 1 to 1.375 for 180° of Fahrenheit's thermometer, being 1 at 32 and 1.375 at 212. At 212° then, it will bear 42.2 inches of mercury, without being compressed; at 32°, 30 inches; at 148°—, (i e. 180° below 32°) it will bear 18,75. At 328°—, 7.5 only; and at 448°—. 0. That is to say, that if it be cooled down to 448—, air will not bear compression, so as to restore itself; will not support a weight; most probably it would liquefy.

What is then the cause of this curious property of heat, by which air may be made to support a greater weight?

We see that a weight condenses air into less space, by which it is enabled to support that weight without further condensation. In this case the particles are brought nearer to each other; a greater number are contained in a given space; that is, the specific gravity is increased. Now heat the air, and it will expand and force up the piston or weight, till the spring of the air be a precise balance to the pressure. We see then, that heat may neutralize the power of gravity acting on an elastic fluid; in other words, *that an elastic fluid heated, may produce the same effects as gravity.* Another conclusion we may draw is this, that the rarity of the fluid is no obstacle to its action. Take the instance before, suppose $a\ c = 1$ *ft* and the piston pressing with a force of 60 inches of mercury. Heat it to 212°, and it will expand to 1 375 *ft.* and still support 60 inches. The specific gravity is here only $\frac{8}{10}$ of what it was. Heat it again to 392, it will expand to 1.75 still the same pressure. Heat it to 572, it has doubled in volume and consequently is only $\frac{1}{2}$ the weight or density it was. So that by 540° of heat one half the quantity of matter is made to support the same weight. Again if we take hydrogen, which is $\frac{1}{20}$ of the specific gravity of air; the same conclusions will hold.

May we then proceed a little further and say.—As gravity may be counteracted by elastic force derived from heat; as the latter may in every case be substituted for the former; is it not probable that gravity is the effect of some elastic medium of extreme tenuity, of such tenuity as will readily enable it to penetrate through every substance? The reduction of two classes of phenomena to one cause, is a strong temptation to adopt such an opinion. May it not be owing to the action or elastic force of this fluid that all bodies gravitate or are pressed downwards; nay, even that the elastic force of a gas is owing? and this latter conclusion is greatly strengthened by the known fact, that all gases and even vapours expand after the same law. In the state of gas, the particles of the body are so widely separated as to afford room for this agent to exert itself. Thus, steam has in the same space only $\frac{1}{1800}$ of the number of the particles which water has.

But further: what if this elastic medium which we may suppose to fill all space, be the cause, also of heat? Thus we should account for equilibrium of temperature, commonly explained on the principle of radiation. Such an elastic substance, if it be also capable of entering solid bodies and combining with them as we see gases do, would account for what is called latent heat. There are, however, I think, some difficulties in this latter hypothesis difficult to be removed, in particular the phenomena of combustion, &c.

But again if we suppose the phenomena of heat to be something different from the mere motion of this elastic medium, we shall be then forced to admit, that temperature will modify the force of gravity: such a conclusion appears pretty well established by Mr. Herapath, who has ingeniously supported the above opinion.

TYRO.

V.—*Correction of Unequal Altitudes.*

It sometimes happens, in observing equal altitudes for the time, in rating a chronometer, that the afternoon observation is lost by unavoidable circumstances or forgetfulness. In such a case, if the error of the watch be required, there is no help, but that of observing another or unequal altitude in the afternoon. To save the trouble of calculating each of the observations at full length, the following formula was investigated; and as it may be useful to observers, we are happy to give it a place in our work. It is obviously applicable to other cases than the one above indicated.

Let ZS be the corrected morning zenith distance,
dz the difference of evening and morning,
P the horary distance from noon of morning observation,
dp the difference of the horary distances from noon,
PS the polar distance,
PZ the co-latitude;

Then $\text{Sin } \frac{1}{2} dp = \dfrac{\text{Sin } \frac{1}{2} dz \text{ Sin } (ZS + \frac{1}{2} dz)}{\text{Sin PZ Sin PS Sin } (P + \frac{1}{2} dp)}$

The investigation is as follows.

1. $\text{Cos P} = \dfrac{\text{Cos ZS}}{\text{Sin PZ Sin PS}} - \dfrac{\text{Cos PZ Cos PS}}{\text{Sin PZ Sin PS}}$

2. $\text{Cos P} + dp = \dfrac{\text{Cos } (ZS + dz)}{\text{Sin PZ Sin PS}} - \dfrac{\text{Cos PZ Cos PS}}{\text{Sin PZ Sin PS}}$

3. $\text{Cos P} - \text{Cos } (P + dp) = \dfrac{\text{Cos ZS} - \text{Cos } (ZS + dz)}{\text{Sin PZ Sin PS}}$

$= \dfrac{\text{Cos ZS} - \text{Cos ZS} + 2 \text{ Sin } \frac{1}{2} dz \text{ Sin } (Z + \frac{1}{2} dz)}{\text{Sin PZ Sin PS}}$

$= \dfrac{2 \text{ Sin } \frac{1}{2} dz \text{ Sin } (Z + \frac{1}{2} dz)}{\text{Sin PZ Sin PS}}$

But 4. $\text{Cos P} - \text{Cos } (P + dp) = 2 \text{ Sin } \frac{1}{2} dp \text{ Sin } (P + \frac{1}{2} dp)$

Therefore 5. $2 \text{ Sin } \frac{1}{2} dp = \dfrac{2 \text{ Sin } \frac{1}{2} dz \text{ Sin } (Z + \frac{1}{2} dz)}{\text{Sin PZ Sin PS Sin } (P + \frac{1}{2} dp)}$

$Z + \frac{1}{2} dz$ = evidently the mean of the zenith distances, and
$P + \frac{1}{2} dp$ = half the elapsed time.

EXAMPLE.

On the 27th May 1819, in latitude 31° 03′,4 the following altitudes of the sun were observed at the time set opposite them.

		H.	M.	S.
A.M.	58° 06′,9 at	21	37	45,1
P.M.	53. 57,4 ,,	2.	28	50,7

PS is found to be 68° 49′,6

Sine $\frac{1}{2} dz$ =	2°	04′	45″	8,55967
Sine $(Z + \frac{1}{2} dz)$	33.	57,9		9,74719
Ar. Co-sine $(P + \frac{1}{2} dp)$	36.	23,12		0,22678
Ar. Co-sine PZ	58.	56,6		0,06720
Ar. Co-sine PS	68.	49,6		0,03035

Sine 2° 27′05″ =8,63119

9^m 48,3s
Middle time, 0 . 3 17,9

23. 53 29 ,6 time of noon; to be corrected by equation answering to change of declination.

This formula also serves to find the change in altitude corresponding to a given interval of time and vice-versa.

VI.—*Proceedings of Societies.*

1. Horticultural Society.

The Society made their annual distribution of prizes at the Town Hall on Wednesday, January 14th, to the native gardeners who were able to produce the best show of vegetables. The number of competitors was less than usual, and the meeting but thinly attended. The samples of vegetables were inferior to those of a former occasion, a circumstance attribntable to the Society not having this year received their supply of seeds from England for distribution to the *malees.* This at least proves that the Society's exertions have been productive of benefit, and this truth, we hope, will not be lost sight of. A number of native gentlemen were present, a circumstance we record with pleasure.

In the evening the Society met at the Asiatic Society's appartments, for the purpose of receiving the Report of the Garden Committee, called for at the last meeting: The President in the chair.

The Secretary read an abstract of the minutes of last meeting.

The President read a paper, partly on the state of the funds of the Society, and partly on the state of the garden. Mr. Robison begged to be informed, whether the Society was to consider the paper now read, as the report which the Garden Committee had beea requested to submit to this special Meeting; or merely as an essay for which the Society was indebted to the president himself: because if it were a paper offered by the President individually, he (Mr. R.) would consider himself precluded from making any remarks upon it, however much it was obnoxious to animadversion. Mr. Leycester said, that the paper was entirely from himself; and Mr. Robison thereupon stated, that his object in making the motion at the previous meeting had not been obtained, and that this special extraordinary meeting had takeu place in vain. He therefore moved that a Committee be appointed, to take the present state of the Society into consideration, to report upon its finances, and laws. This was seconded by Mr. Calder and carried, when the following gentlemen were named as the members of the Committee—

Mr Bruce, Capt Jenkins, and Mr. Hurry, *Secretary.*

Mr Leycester laid on the table a List of Plants in the Society's Garden, which he designated "*Hortus Aliporensis.*"

The paper read by Mr Leycester having included some letters, to his address, from Mr Mitchell, head gardener, which were considered improper, it was proposed, and agreed to, that the Committee now named should particularly investigate Mr. Mitchell's conduct in regard to these letters.

The Secretary read a letter from Mr. Moore resigning the Society; and from Mr. Chester resigning his situation as member of the Garden Committee, on the ground, that he had been choson by the President, without the sanction of the Society.

Mr Robison proposed, that the meeting should come to a resolution of ascertaining, whether the Asiatic Society would receive the Agricultural and Horticultural Society as a branch of itself, in the same manner as the Physical Committee of that Society is now constituted; and this with the view of introducing more regularity, energy, and unanimity into its proceedings. The proposal, after some discussion, was agreed to be postponed; and the Committee of finance now appointed, was requested to take this matter into its consideration, and report its opinion to the Society, on the expeaiency and praticability of the measure.

Dr. Strong submitted six coffee plants reared in the shade, and six reared in the sun, with the view of showing, contrary to the received opinion, that the plants which had always been exposed to the sun's rays were both better wooded, and had finer roots than the others.

The meeting adjourned *sine die.*

VII. Scientific Intelligence, &c.

1.—*Transverse Strength of certain Woods.*

A valuable article has appeared in the Military Repository, embodying in a table the results of a series of experiments, performed on this subject at the Gun Carriage Agency, Kásipúr. The woods selected were Bengal Sundri, Múrang, Sál, Gorakpúr Sál, Rangoon Teak, Java Teak, Pegu Teak, Bombay Teak, Murang Tún, Bengal deal, Norway deal, and American ash. The pieces experimented on were each 72 inches in length, 2 inches square, and 60 inches between the points of support. Amongst other results it appears that the Bengal deal, though unseasoned, was equal to the Norway, each breaking with 720lbs. and a deflection of $2\frac{1}{2}$ inches. The great range in the strength of teak is one of the most curious and valuable of the results. The extremes are Rangoon and Bombay or Malabar, 1175 and 591 lbs. giving a ratio of 2 : 1. Both woods were seasoned. The highest value of Bombay teak was 889lbs. Bengal Sundri is the strongest wood tried, and required 1384lbs. to break it. Muraug Sal is the next, 1319. It appears in general, that the woods were stronger for being seasoned ; in the case of Sundri the difference was remarkable, 1384 and 992, or 7 : 5. The greatest range of Sal Chaokars and Daokars, was 1319 to 1179, when seasoned : unseasoned, it did not go below 1085. Murang Sal Battis gave only 787. Tún not seasoned 677, and American ash 483.

2.—*Comparative Value of Cements.*

A very valuable paper was published in the Oriental Quarterly Magazine, being an extract from the proceedings of the Benares Literary Society ; giving an analysis of the various limestones used in India for making lime, and the results of some experiments on the comparative value of the cements prepared from them.

The method of analysis was rather different from that usually employed The limestone in powder, "was exposed in small covered cupels to the regulated heat of an assay furnace. When the first heating was not sufficient to expel all the carbonic acid, they were again submitted to the fire ; and to prove that all the gas had been driven off, a few of the musters were further heated without loss of weight in a forge."

A very ingenious method was adopted to prove the correctness of this mode of analysis. The lime, rendered caustic by the preceding operation was converted into a hydrate. The increase of weight was found to correspond with the carbonic acid driven off.

The employment of this method of verification has afforded a very valuable hint towards the solution of the very difficult problem of separating lime from magnesia, or rather of estimating their respective quantities. It appears that magnesia does not form a hydrate, so that the increase of weight is an index to the quantity of lime in any magnesian limestone*.

The results of the analyses are given in a table, from which it appears, that the several kinds of Kunkur limestone vary from 54 per cent. carbonate of lime to 97. The Silhet limestone used in Calcutta, it appears, is pure carbonate of lime.

The following results were established by the experiments on cements.

1. That the half burnt brick-dust answers as well as the best baked, in mixture with lime for a dry or water cement.
2. For substantial work there should never be more than an equal weight of Soorkhee, even if the lime is thoroughly calcined.
3. That the hardest mortar, and that least pervious to water, was composed of one part of lime, and two parts pounded strong Kunkur.

We strongly recommend the original article to the attention of those of our readers who are interested in the subject.

3.—*On producing Early Grapes.*

The following extract is from a letter by a gentleman in the upper provinces, addressed to his friend in Calcutta.

"I see that you are now a member of the Horticultural Society ; in that I should delight were I an inhabitant of Calcutta.

In the practical part of gardening, I made some progress while I sojourned at Coel, for I succeeded in getting my grapes ripe during the first week of March, though a mistake was committed in the management of the vines during a temporary ab-

* The author has promised to follow up his views on this subject by some further experiments. Those who have read the papers of Dr. Daubeny and Mr. Phillips will acknowledge the value of the above hint.

sence of mine. The plan I adopted was merely to prune the vines on the 1st of October, and afterwards train them on bamboos, near the ground on the south side of a wall, protectiog them from the frost at night by screens of cloth stretched on bamboos. My intention was to train them on the north side of the wall, till I perceived the buds beginning to swell; then I should have changed the vines to the south side; they however, came out into flower a few days after the pruning, while I was away, aod consequently I could not move them there, for fear of destroying the flower. So I let them remain till the grapes were well formed, and then put them to the other side; this, however, killed a great many of the leaves, by putting their under surfaces to the sun, (I thought the leaves would have turned their upper surfaces to the sun): still the grapes were ripe in the beginning of March, and were as good as those which the same plants produced the former year, in June: had they been trained at first on the south side they would have been far better. I have found great effect in riogiog peach trees; the fruit of the ringed branches was more than double the size of that on the union of the same tree. Ringing did not in the least alter the apples or figs; this is directly contrary to the experieoce of Mr. Knight in England. Grapes I found much improved in size and flavour by ringing, besides beiog ripe a fortnight earlier than those of the unringed branches. At this place, I have unfortunately only a very small garden at present, and in it there are no trees of any soil."

"Mr. B. has a pear tree in his garden, which this year is bearing nine pears; I hope they will ripen. The tree itself thrives beautifully, also the Eoglish mulberry. The latter gives a great deal of fruit, the appearance of which is very fine, but there is very little flavour in it. What is the reason, that apple pips will not germinate in this country? I have sowed them frequently, and never succeeded in a single instance." B.

4.—*Size for damaged drawing paper.*

The usual receipts for the recovery of drawing paper that has lost its size, such as washing it with alum water, rubbing paste on the back of the sheet, have been acknowledged by all that have tried them to be, if not perfectly useless, yet by no means fully curative of the evil. Our readers will be glad to learn the following new, and it is asserted, perfectly successful process, discovered at the Behar Amateur Lithographic Press, and published io the John Bull newspaper under the sigoature of Lithos. It is stated to have the further advantage of making the water colours work with more freedom than they will even on good paper. The following is the receipt.

The paste.—Take a handful of Soojee, and having placed it in a soup tureen, pour a quart of a water over it, aud then work it well with the hand for half an hour, with a view to detach the gluten from the Soojee. Then strain the whole through a piece of cloth, throw away the residue, and allow the liquor to subside, which will take three hours. Then pour off the water, and the gluten* remains in the dish. Mix the gluten with a pint of cold water and pour it into a silver saucepan, and boil it to a proper consisteucy, which it appears should be that of very thin paste.

How to prepare the drawing paper with this paste.—Having placed the paper or drawing to be prepared, on a large sheet of common paper, wrap a piece of fine muslin about your fore-finger, and having dipt it in the paste, rub it first on the back, and agaio oo the front of the paper, until the paper takes the colour of the paste through and through. Then lay the paper to dry, lifting it once or twice, that it may not stick to the surface on which it is placed.

* The writer must mean the fecula or starch which has passed through the cloth. The gluten was the residue thrown away. Quære, If this be the writer's meaning, would not starch or hair powder, which are each of them pure fecula, answer as well? Ed.

Errata in No. 2.

Page 52 line 5 *for* "*f* or," *read* "for."
,, 59 ,, 11 from bottom, *for* "Dr. Butler," *read* "Dr. Butter."
,, 56 ,, 9 from bottom, *for* "economica," *read* "economical."

GLEANINGS

IN

SCIENCE.

No. 4.—April, 1829.

I.—*On the Irrigation of Land in India.*

To the most careless observer, the method employed by the natives of Hindustan to procure such a supply of water, as is not afforded by the season, or rather, which the intense heat of the climate, and the exsuccous nature of the soil demands, in an abundance that would be, not only useless, but hurtful, in many other countries remote from the tropics, must have suggested very naturally the reflection, that the labour so applied would constitute a very considerable proportion of the cost of the production of most of the land in cultivation; and that a less expensive mode of commanding such a benefit, might advantageously be borrowed from the practice that obtains in other parts of the world.

Water, and those vegetable *debris*, in the attenuation or extreme division to which the latter are subjected by the action of the sun, and of spontaneous decomposition, seem to be the chief pabula of plants.

Moisture and a certain degree of heat are alone sufficient, in many instances, to elicit rapid vegetation in a small way: while, on a more extended scale, the means of procuring stability, to enable the young plant to withstand the action of the wind and rain, are absolutely requisite: and the mechanical aid of pulverulent mineral matter becomes indispensably necessary, to enable the radical filaments to embrace the disintegrated particles of such matter, as is destined to favour alike the future development of the tender stem, and to maintain it by its mechanical properties in an erect position so as to become the intermedium of its subsequent subsistence and nourishment.

Soil, heat, and moisture, as well as the remains of former vegetation seem, then, to be necessary to vigorous and abundant production. The produce is generally found to be dependant upon the nature of the soil, as it regards the quality, as well as the quantity of the production. The degree of heat and its duration; the abundance, or deficiency of the moisture; the extent of the supply of vegetable exuviæ, aided by the peculiar nature of the mineral substances found on the surface of the earth, affect very materially the quality and quantity of products, whether of indigenous, or of exotic growth, repel one kind and naturalize another, without exciting more than the natural curiosity of man to observe the wonders of creation, or without eliciting a greater power of intellect, than the ability to record the facts, and to reason badly respecting them*.

But, of all the advantages most favourable to abundant vegetable production, the constant presence of moisture, even up to the period of fructuation, is one so universally felt and acknowledged, as to be considered the sine quâ non of vegetation†.

* Some modern author has said, "Man is a reasoning, but most often an irrational animal." Go to science, and ask her if the blunders of what the philosophers term, *par excellence*, "the exact sciences" are not the greatest errors that were ever the offspring of genius.

† Solar light is always known to be necessary to vigorous, as artificial light causes sickly vegetation. Electricity, which may account for many phenomena in the animal world not much noticed, is well known to affect the vegetable organization; and some experiments may be exhibited to demonstrate the mechanical effect of vegetation, the result of rapid development and growth produced by the agency of the electric fluid, by means of the intermedia or conductors, fluid and solid, surrounding the embryo.

Moisture is a contingency that affects the quantity of production so very remarkably, that there are observable on the same spot, at different periods of time, frequently, all the shades of difference between a third of a crop, and a double or treble crop; that is, a crop double or triple what is due to the unaided operation of the season*.

Let any one compare the produce of a kitchen garden, with that of an ordinary field; and he will immediately perceive the difference. Though much, no doubt, depends upon the quantity of labour, skill, and manure employed in the cultivation of the former; without the aid of a more abundant irrigation, the quantity of produce will not greatly exceed that of the latter. The suburbs of Paris sufficiently evince the truth of this observation; and the fact has not been overlooked by the writers on the subject.

The pressing necessity of France to manufacture sugar† from the beet root, compelled her to cultivate it in the late war. Four quintals answering to 400lbs. French weight, or nearly 4 cwts. of sugar were the ordinary annual returns of such a sort of cultivation obtained from one acre; and when the absence of a tropical sun is taken into the account, this amount of saccharine produce is surprising.

The astonishment which so large returns naturally excite will, however, be somewhat moderated, after our attentive consideration of the facts recorded by several writers‡ of respectability, who have treated the subject in a familiar and clear manner; whose testimony is very materially corroborated by the transactions of the Bath Agricultural Society, the annual reports of which must be necessarily associated with that degree of national improvement, which the future historian will not fail to hand down to posterity in terms of praise and of grateful acknowledgment.

Mr. Arthur Young, so long secretary to the Board of Agriculture in London, whose labours are felt by the people of Great Britain in the present day, has given a very interesting account of the profitable employment of extensive irrigation in the north of Italy.

The extensive knowledge of agricultural concerns which he has displayed, as well as his unequalled talents in this department of human science, attracted from all parts of the continent of Europe, and even from America, pupils of every description, not even to the exclusion of wealth and rank; since it is known that not only opulent foreigners, but even members of some of the noblest families, sought this extraordinary man to receive lessons of profitable instruction.

The unquestionable nature of such an authority as that of Mr. Young will render unnecessary any apology for the introduction of the following extract from his writings; and since the value of the information contained in the quotation was estimated highly by its author, it would be unpardonable to omit a passage that so directly tends to establish what is advanced in this essay.

Mr. Young, writing on the subject of this paper, gives the following interesting account of the estimation in which irrigation is held by the states and the people in the north of Italy.

" In the neighbourhood of Milan and Lodi, canals are not only numerous and uninterrupted, but conducted with great skill and expense.

" Along the public roads almost every where, there is one canal on the side of the road, and sometimes there are two. Cross ones are thrown over these, on arches, and pass in trunks of brick or stone under the road.

" A very considerable one, after passing for several miles, by the side of the highway, sinks under it, and also under two other canals carried in stone troughs one foot wide.

" The variety of directions in which the water is carried; the ease with which it is made to flow in opposite directions; and the obstacles which are overcome, are objects of admiration.

" The expense thus employed in the twenty miles from Milan to Lodi is immense; and *meritorious as many undertakings in England are, they sink to* NOTHING *in comparison with* THESE TRULY GREAT AND NOBLE WORKS.

" So well understood is the value of water in this country, that it is brought to the farmer (who has the power of conducting it through his neighbour's ground)

* See the Bath Agricultural Reports. Salt is there stated to have rendered fertile, land, which did yield a $\frac{1}{4}$th of a crop in the preceding year; and this manure was considered a very cheap one.

† Cultivated likewise in Prussia during the war, and one acre is said to have produced ordinarily 4 quintals of sugar from each crop.

‡ The Rev. Mr. White of Gloucestershire, and others.

for a stipulated sum, and under certain regulations, to any distance that may suit him.

"From a canal of a certain size*, at so much per hour (per week); and even from one hour down to a quarter. The usual price for an hour, per week, in perpetuity is 1500 francs, or livres."

It will be shown in the second part of this essay very satisfactorily, that the cost of irrigation in the northern states of Italy, does not exceed one-third of the expense incurred in India: putting the value of an ordinary labourer's day's exertions, or their worth in silver, on a scale of the exchangeable value of that metal in Europe and in Hindustan, at the same period.

In Egypt, the season of the Nile's annual fecundation is celebrated as a festival; while the artificial supply of water, to vast tracts of country, in some parts of Europe, is numbered among the most profitable objects of the state, and supplies a large revenue, that is derived from a source of prosperity totally unknown, and therefore unappreciated, in almost every other division of that quarter of the globe.

Little as that curious, shrewd, and suspicious people have permitted foreigners to glean from their national institutions; the Chinese are sufficiently well known, as cultivators of the soil, to be entitled to the highest respect, as a nation of husbandmen.

The sovereign himself holds the plough, on the occasion of a certain annual agricultural festival. Nor, semi-barbarous as they were termed by a late periodical writer †, can they be considered so well entitled to the epithet, as some people of more doubtful ancestry; since, to the art of agriculture, in which they must be confessed to be second to very few, they have added an improvement‡ which is practised by Europe, only in the solitary instance adduced by Mr. Young; as is amply evidenced in the number and extent of their canals, equally the source of the commercial, as of the agricultural, prosperity of that singular people.

If barbarism has destroyed some of the noblest monuments of the national grandeur, and public utility of former ages; it is but just to a fanatical religion to record the modern achievements of an African prince: who, regarding his own interest and the public benefit as inseparably united, has dared to falsify the sarcasm, that history§ had cast on the Moslems, in every century and in all countries.

The Pacha of Egypt, has opened a new view to his country, by the successful introduction of an efficient irrigation, that may well deserve the imitation of other states, whose territories are equally susceptible of a similar advantage. This improvement has now had the support of some years of probation in Egypt, and bids fair, to renew for it the title, of "one of the granaries of the world," which that division of Africa once enjoyed.

"Increase and multiply, and replenish the earth," is a command, that dates its origin as far back as the first days of the history of the human race. Then, shall the prince be held obedient who, possessing the power, has not encouraged the means, of maintaining the increase? It has always been esteemed, that he, who made two blades of grass to grow where only one grew before, deserved well of his country.

Mr. Malthus has taken much pains to show, that the tendency of population is constantly to press on the means of subsistence; and he has rung the changes on this position, for every degree of latitude and longitude on the globe, from China to Peru. But that gentleman does not appear to have suggested any practical mode, by which the evils of excessive population may be avoided.

Had he told us in what manner, a population of ten millions might be subsisted, where at present only five millions are but indifferently supplied with food, he would not merely have furnished the "medius terminus," but in so doing, he might also have availed himself of the occasion it would have afforded to descant upon the *mo-*

* That of one foot square with the assumed acclivity of 3.5 feet for the current; is the datum upon which the second part of this essay is founded, and from which all the inferences are drawn.

† Mr. Gifford, late Editor of the Quarterly Review.

‡ "Fas est et ab hoste doceri" is a maxim applied to war usually; and nations, being rivals some way or other, might apply it to civil affairs. We borrowed the bayonet from France, and porcelain from China, but are too proud to dig ditches to fill water for other than commercial advantages.

§ Gibbon says, "Islamism not only retards, but blasts every attempt at improvement."

ral improvements which must ever *follow* and not *lead* the physical*. He would have been able with much facility to demonstrate, that a people who possessed the ingenuity and enterprise to procure so notable an improvement of their physical condition, would not long stand in need of moral lessons; that the sagacity which was capable to procure so signal a benefit, would be equal to restrain the most deplorable acts of improvidence, alluded to in his treatise On Population, the converse of which would involve the absurdity of supposing the most provident, to be at the same time the most improvident, people in existence.

Of all the countries that may be found to be susceptible of a real amelioration, Hindustan presents one of the fairest and fittest subjects for such a contemplation. No doubt, the vague statements of such ephemeral authors, as the compilers of Gazetteers, are little to be relied on. But, although that account of the prosperity of India, some two centuries ago, which assigned a revenue of seventy crores, may have been exaggerated; it may admit of a reasonable doubt whether India Proper, or the Mogul Empire, as it used to be designated, did not yield a much larger revenue than it is found to afford in the present day†.

It has been said, without reference to official accuracy, that the population of British India, and its gross revenue are respectively 44 millions of souls, and 22 crores of rupees; or that an average of five rupees is the amount levied upon each native subject.

Hindustan formerly reckoned, in addition to the present territories held under the East India Company and the Mahratta states, many provinces in the north west, which are under the control of several disconnected sovereigns; forming an aggregate of about 930,000 square miles. On this extent of surface, a population of 100 to 110 millions was the reputed enumeration;—no great improbability, when it is considered, that this population assigns no larger a proportion, than 118 persons to every square mile; a number much under the estimate of European countries, known to be in a state of comfort and prosperity, little, if any thing superior to that of Hindustan at the period in question.

This extent of population, compared with a corresponding proportionate revenue of the present day, would indicate a gross revenue of 55 crores instead of 70, as assumed by some writers of the last century. And, if we are to credit the more accurate testimony of official opinion, we should be satisfied, that "considering the many civil and foreign wars, in which Hindustan has been engaged in former times, the present state of the revenue of British India may be reckoned, upon the whole, as flourishing as was ever witnessed in the time of the native government of the country‡."

This, by the way, contains a sort of admission, that the nominal revenue was greater under the native administration, but that civil discord occasionally produced notable defalcations.

At any rate, it goes to the corroboration of one of two facts: either, the power of production must have been greater than in the present day; or the people must have been most grievously oppressed.

The government then being a despotism, was no doubt conducted in the spirit of all other despotisms; by tyranny, to the nobles and dependants of the court, and protection to the multitude. A national revolt was impossible, since patriotism never felt, could never be acted upon; such a crime therefore found no place, in the criminal code of Mongolian dynasty.

It is reasonable to suppose, then, that the territories of Hindustan were more productive; since they could yield as large revenues in former days, as in the present; at a period when the precious metals were more scarce, than in the past and current centuries.

To have been more productive, the country must have enlisted the provident care of the government on its side. That this was especially called in, on more than one occasion, to the aid of the people, we may refer, among other proofs of the paternal wisdom of the Great Mogul, to the history of those monuments of imperial beneficence which now, after a lapse of nearly 50 years of neglect, have attracted the attention of the British Government.

* Dr. Johnson says somewhere, that a man composes but indifferently with a too empty stomach.

† The late Mr. Commissioner Deane's opinion was formed on the money amount, and not on the exchangeable value,—two very different views of the case.

‡ Mr. Deane's opinion in 1815.

Feroze's canal, which was re-opened a few years since, in the neighbourhood of Dehli, is said to be an object of no little importance; and that its value is well appreciated, in the prospect of ample remuneration, which the revival of such a source of universal benefit and utility to the neighbourhood holds forth. A tolerably accurate professional opinion, it is believed, warrants the deduction, that occasional disappointments are reasonably to be expected, both as they respect the extent of the benefit and the scale of physical difficulties, which are said to be renewed annually.

The Chevalier de Buat did not write, or construct formulæ for Indian hydraulics; nor could any one of his calculations embrace the peculiar case of a torrid sun, and a soil so thirsty, as to reduce a vast column of water at its nether extremity, to an evanescent quantity.

The English Engineer will be entitled to all the honour, as he must submit to all the labour, to be encountered under such unparallelled difficulties. The Italian engineer has to deal with streams more susceptible of control than those traversing the plains of Hindustan; which Major Rennel informs us, have sometimes deviated more than two miles, in ten times as many years.

The British engineers have two magnificent rivers, the Ganges and Jumna, on which to bestow their ingenuity, and their labour; and these engineers will have the more to boast, when they place themselves in a condition to assert, that they have turned to good account the two noblest means of enriching a country ever presented by nature, and have restored to her a situation, not merely equal to what she enjoyed in former times, but even of raising her to a condition of prosperity unknown to any country on the face of the globe, not excepting even China, or the northern states of Italy.

H. D. E.

II.—*Account of a Visit to the Biáns Pass in the Indo-Gangetic Range beyond the Head of the Kali* River.*

[In a Letter from a Correspondent.]

I left Almora on the 27th September, in company with * * * * and * * * * and we arrived at Petora to breakfast on the morning of the 30th, a little after 8 o'clock. We had three good long marches, encamping, the 3d evening, near the Goorung valley. * * * * was to accompany us, but not being able to leave the post till relieved, we were obliged to halt till the 3d October, when we marched to Kandali China, a little beyond Dhaj. From thence we proceeded to Askót, a very fatiguing march, being compelled to send our ponies by another road, the greater part of the way, and the sun was very hot—with a steep ascent. The fatigue and exposure was too great for * * * * who was obliged to leave us, and returned to Almora. The next day we had hard work in getting our baggage across the Gúri below Askót. The country about here is very pretty, but sadly infested by tigers, so much so, that the people bar their doors at night, and are afraid to venture out after nightfall. Some animal made a spring at a goat in our camp during the night, which roused us all up, and soon after we heard either a tiger or leopard at a short distance. We had chiefly limestone, till we approched the top of the range which is connected with Askót, where I found gneiss; in descending towards Askót, clay slate. I have said the next day we had hard work in getting our things across the river by a *laria*† (by which we had also to cross ourselves, as we did last year over the Rámganga); and if my companions and self had not put our hands to the rope, we should not have got over that day. We did in two minutes what the people took eight to perform, but it was hard labour. The 6th, we marched to Balúakot, our ponies having been left at Askót; the road through a thick jungle, about 500 feet above the Káli—rock not often visible—limestone—arrived at our ground between two and three o'clock, having breakfasted on the way, which detained us about two hours; the sun very powerful: 7th to Dobát a very long march, limestone, clay-slate, and si-

* One of the branches of the Gágra, and according to nomenclature ought to be the principal, being the name by which the Gágra within the mountains is known.

† A single rope stretched across the river, on which traverses a sliding block, to which the baggage or traveller is attached. From *laree*, string or rope.

liceous conglomerate*; breakfasted near Darchúla, and reached Dobát about sunset.

8th.—To Khélah, another long march; rock chiefly gneiss; arrived about sunset. 9th.—To Paliansí in Chaodáns, being the furthest village—a steep descent to the Dharma Gúri†; a violent torrent; Sanga carried away during the night by the rise of the river 3 or 4 ft. occasioned by the descent of an *avalanche* higher up. Halted from 7. 30 to 11 A. M. to allow of another being set up; reached our encamping ground by *torch light:* the latter part of the road being a very steep and contioued ascent from the river,—the rock always gneiss. Chaodáns is a fine country, and the inhabitants are a fine race, similar in fact to the people of Jowahir—very fair and of athletic make.

10th.—To Gálahgar; no village: rock small grained gneiss, mica-slate, and schistose quartz-rock. This was an unpleasant march, owing to heavy rain during the latter part; we marched after breakfast, and did not arrive till dark. 11th.—To Nijangar; halted to breakfast, and to allow our followers to cook a meal, as it was represented we should find no water on this side Nijungar; a very long but gradual ascent (three hours); very little forest; rocks as before mica-slate, gneiss, and quartz-rock; on the top of the ascent quartzose mica-slate. To the Nijungar a very long and steep descent (four hours) along the most frightful precipices I have seen; passed by flights of steps formed of small rough stones, 5 or 6000 ft. above the river, into which the slightest giddiness or uncertainty of the footing might have precipitated us. Encamped under a Wodar or overhanging rock about a mile above the Nijuogar torrent, water some little distance; it rained during the latter part of our descent, and we had a wet night.

12th.—To Budi the first village in Biáns, one of our longest stages. At the Nijungar we had mica-slate, but as we approached what I suppose the line of greatest elevation‡, gneiss occupied its place; granite also in blocks, but of small dimensions, and in no great number. Neither is the gneiss formation of much extent just here, and what is worthy of remark, we observed along the banks of the Káli in this neighbourhood secondary strata, consisting of sandstones generally soft and incoherent, some of a fine grain, others of the conglomerate structure, being composed of rounded pebbles imbedded in a basis. After passing what I considered the line of greatest elevation, we came to clay-slate again, with quartz-rock §, principally indeed varieties of the latter. These continue the whole way to the pass Lépú Dhúra.

13th.—To Kawalék. Passed the village of Garbia about half way (six miles) from Budi. At starting is a steep ascent of one hour, and we considered ourselves fortunate in having procured *yaks*‖, which were of no little service. On the top of the ridge we found mica-slate. From this to Garbia and onwards to Kawalék, the country is beautiful; between the two latter places, the road runs through a nearly level district beautiful wooded along the banks of the stream, which, as already noticed, are of secondary formation. Two species of fir, one of juniper, the yew, beech, and, towards Kuwahlék, gooseberry bushes were noticed. Here the Káli proper, which is, however, the smaller of the two, joins the main branch. The latter appears to originate to the westward, the people say at a distance of three days journey, and that the ridge from which it springs separates the districts of Dharma and Bíans. The view extends a considerable way up in that direction, the river appearing to have nearly a straight course.

14th.—To the pass Lépú Dhúra, whence Taklakót, the Chinese station or factory, is only three hours' journey; marched about half past six, stopping to breakfast at the place called Kálapáni, where a stream of clear water about 25 feet wide, but very shallow, issues from the neighbouring rocks, on the left bank of the larger stream. About a mile further on we left our baggage and servants, taking only a small *pawl*, our beds, and some firewood laden on *yaks*, not wishing to trust altogether to the information given us that we might return thence by nightfall. It was fortunate that we made this arrangement; for after leaving them at two and a half hours' journey further on, we did not reach them on our return from the pass till two hours after dark; snow falling all the way. The ascent is very gradual, and the road ex-

* Local, we suppose.—ED.

† One of the branches of the Káli or Gágra, which drains the Bhotia Pergunnah of Dharma, trans Imaum.

‡ Budi is to the north of the line of greatest elevation.—ED.

§ One of the most generally occurring rocks in these mountains might be called argillaceous quartz rock.

‖ The *chaori*-tailed bull of Thibet.

cellent; but owing to the great elevation the exhaustion and distress consequent on using the least exertion were so great that we were glad to mount our *yaks*, which however moved very slowly. We reached the pass about five P. M. and I almost feared at one time we should not arrive in time to have an observation of the barometer, which stood as follows:

	Inch.	Attd. Th.	Det. Th.
Oct. 14. 5 P. M.	15.820	34°.	24

We were very unfortunate in our weather; could see nothing but clouds, which indeed completely enveloped us: excepting the Bhotiahs only one native reached the pass with .

All the way from Kawalék we found nothing but varieties of quartz-rock and chert, with a small patch of clay-slate, the same in fact as I found at Uta Dhúra*. We had no beds of snow to cross excepting some new snow, which had just fallen on the pass†, about twelve or eighteen inches in thickness. I was surprised to find this pass so high, having understood 14 or 15000 feet to be the greatest elevation attained by Captain Webb in his journey.

The people, however, say that he did not visit the pass; so that his elevation must relate to Kalapani, beyond which they say he did not proceed. I have not the account to refer to; so must leave you to ascertain the matter correctly‡. M

Remarks by the Editor.

The above observation of the barometer is to be corrected for the capillarity of the tube which was a small one. The value of the correction was determined by comparison with another barometer, which again was compared with the standard one in the Surveyor General's Office. It was found to be ,281 inch additive to the instrument observed at the pass. Taking this corrected indication and the corresponding observation made in Calcutta, and published in the monthly register given in the Government Gazette, the elevation of the pass is found to be 16844 feet. Captain Webb's result is 17598 ft. In the above calculation we have not attempted to correct for the hygrometric condition of the air.

The writer of the foregoing letter has promised us his detailed journal of the excursion, from which we hope to make some interesting extracts.

III.—*On the Velocity of the Wind.*

It is the object of the present article to invite the attention of the numerous scientific navigators, who are continually traversing the wide ocean between Europe and India, to a subject well suited to furnish them with amusement for the many leisure hours which such a voyage affords.

Nothing can be less satisfactorily determined than the relation between the *force* and the *velocity* of the wind; the tables which are given thereof in works of physical science are almost entirely deduced from theory, and there is great reason to imagine, that they would not agree better with experiment than those of the resistance of fluids, for which new rules and theories have been frequently invented, founded upon elaborate experiments and inquiries. It has been often said that a ship could "vie with the wind in swiftness," but is not such an expression understood merely in a *poetical* sense, without the notion ever being considerately entertained that a ship could positively sail as fast as the wind. And yet there is nothing chimerical in such a supposition, and the facts which shall be presently stated, go far to prove, that with the wind on the beam a good sailer can even *outstrip* the subtle element in her course; but as such a proposition will hardly be received as a fact without the con-

* The pass in Jowahir at the head of the Garjia or Gúri, the main stream and most distant source of the Gágra.

† Compare this fact with European speculations on the elevation of the line of perpetual congelation. Professor Leslie, and the Quarterly Reviewers fixed it at 10.500 for the latitude of 30°, a statement which continues to be gravely copied into most works, without the slightest hint of its being contradicted by observation. See in particular Myer's Geography.

‡ In the 6th vol. Journal of Science and Arts, there is a table of Captain Webb's results, and some particulars of this journey, from which we would infer that he did not visit the pass; the height, however, is given in this table 17598 feet, we suppose from geometrical measurement.—Ed.

current testimony of several series of well conducted experiments, I hope to excite those who have a zeal for science, and particularly for the branches connected with their profession, to follow up the subject, and give the "Scientific Gleaner," the honour of establishing upon a surer basis, the laws of aërial motion and force.

The usual machines constructed for the purpose of measuring the *velocity* of the wind, are essentially faulty in principle; for they depend upon the measurement of the *force* exerted by the moving air upon a given surface, and the connection between these two properties is only assumed theoretically. In the course of a voyage from England, I registered daily the indications of an instrument on this principle, one which exposed a surface of six square inches perpendicularly to the action of the current; but, I soon began to doubt the accuracy of its indications, and to wish for an instrument capable of measuring directly the velocity itself.

With this view, I adapted a sort of small windmill to a train of watchwork, so as to register the number of revolutions made in a given time; the friction opposed hardly any resistance to the turning of the vanes, so that it might be judged beforehand, (as it afterwards proved,) that the number of rotations would be proportional to the velocity of the wind. This instrument had the additional advantage of not being affected by sudden gusts and irregularities, and of being exposed to the free current at the end of a long staff, whereas the common anemometers must be held near the eye, and are then difficult to read off from their continual vacillations.

The problem of determining the number of revolutions of the windmill which corresponded with given velocities of wind was necessarily left to be solved on terra firma; and the method pursued does not, I think, admit of cavil, as it will readily be granted that it is the same thing whether the instrument move through the air, or the air move past the instrument, the velocities being always equal.

The windmill then, furnished with a proper trigger to stop or set it in motion at given signals, was attached to the end of a long, wooden arm branching out from an upright axis, to which various degrees of rotatory force were imparted. The instrument thus described a circumference of such magnitude that, as regarded its own size, it might be considered a straight line.

The air of the apartment was also perfectly calm and undisturbed. It required a motion of 2¼ feet per second or one and half miles per hour, to overcome the friction of the machinery. The following table embraces the results deduced from the experiments, and shows that the revolutions are almost exactly in the ratio of the velocities, the only deviations being at both extremes, as might have been anticipated.

Velocity of the Wind.

Feet per minute.	Miles per hour.	Revolutions of mill anemometer.	Difference.
3	2.02	120	130
4	2.75	250	125
5	3.4	375	125
6	4.05	500	115
7	4.8	615	115
8	5.45	730	115
9	6.1	845	115
10	6.85	960	115
11	7.5	1075	115
12	8.15	1190	115
13	8.9	1305	105
14	9.55	1410	95
15	10.25	1505	

It now remains merely to fill up from hence the numbers registered on board the ship, adding the direction of the wind at the time, so as to obtain its actual, as well as its relative velocities. I have inserted too in the subjoined table, the force of the wind upon a square foot where it was measured, and the velocity according to the common tables thereof. The difference is very considerable. The ship's motion in miles is also inserted as measured by the log. But I omitted to note the quantity of sail she carried at the time.

Date.	Ships's motion in miles per hour.	Angle of the wind to the ship's course.	Force of wind on a sq. foot in lbs. aver.	Tabular velocity corresponding.	Revolutions of mill anemr. per minute.	Relative velocity of wind felt by the ship.	Actual velocity of wind independent of ship.
			corrected for ship's course.				
	by log.			miles p. hr.		miles.	miles.
Aug. 20	9.0	95.°	0.85	14.0	1080	7.4	7.0
21	9.4	100	0.90	14.2	1170	8.1	7.5
23	10.0	80	1.62	18.0	1515	10.3	12.5
24	8.6	105	——	——	1636	11.2	8.0
25	9.0	125	1.20	16.0	1660	11.4	6.0
28	9.0	105	0.85	14.0	1173	8.1	6.0
31	6.0 ?	90	0.17	5.4	459	3.8	3.8
Sep. 2	4.0 ?	only felt in	lee roll 90°.	——	110	2.0	2.0
5	5.0	80	0.30	7.6	325	3.1	4.0
6	5.4	60	——	——	505	4.1	6.8
8	7.2	60	0.41	9.0	614	4.7	8.0
9	8.2	50	0.48	10.0	680	5.1	10.5

The last column but one in this table is that upon which the chief confidence is to be placed; and if it be indeed allowed to warrant any conclusions, we may infer from it, that a ship sails with the least *relative* force of wind, when the latter is on the beam or the quarter; that with moderate breezes a-beam, the ship will move through the water *faster* than the wind which impels her; that even with a strong breeze of 10 knots a-beam, the ship will keep pace with the wind; with the wind before the beam, the experiments are at variance, but on an average the motion of the ship was even then equal to that of the absolute current of air, if not greater.

It may be remarked, that in theory there is nothing anomalous in these results, for supposing the directions of the wind and the ship to be at right angles with one another, and assuming that both were of an unyielding nature, the motion of the ship would depend upon the angle which the sails made with the wind, being as tangent of this angle, while the wind's velocity is radius; and consequently, theoretically speaking, it might increase *ad infinitum**. This, however, is very far from the real state of things in nature, and nothing but experiment can determine the variable influence of the wind and the water upon different forms of vessels, and under every different circumstance of position, form of sails, trim, tonnage, &c. The ship alluded to in the present notice was a good sailer, and carried but little freight when the experiments were made. Her greatest rate was 10½ knots with the wind on the quarter, and the velocity of the latter *by the tabular rates of the force* was 23 knots. In the strongest period of a gale near the Cape, the same instrument gave the velocity of the wind between 30 and 40 miles an hour, the force measured being 6 and 7 lbs. on the square foot; but from the subsequent comparisons with the two instruments, it becomes doubtful whether the velocity exceeded 30 miles at the utmost.

I have said quite sufficient to show how much uncertainty still dwells on all these points, and I conclude by inviting nautical men to give some aid to their elucidation. From the hints I have given, any maker of mathematical instruments can construct a windmill anemometer:—the simple wheel work of Wollaston's trochiameter would answer the purpose perfectly. The register of revolutions for 10 minutes or so, might be kept with the log daily; and any experimentalist at home or in India could prepare a just table of the value of the revolutions in rectilinear motion.

Q.

* The extremities of the vanes of a windmill, for instance, generally run over a much greater space in a given time than the current of air which gives them motion.

IV.—*Some Account of a Boring, made in Fort William, for the Purpose of procuring a Supply of Fresh Water, with Remarks on the Nature of that used in Calcutta, for domestic Consumption.*

To the Editor of Gleanings in Science.

SIR,

Having had occasion to look over some papers connected with the borings, recorded in the 12th Vol. of the As. Res., "in search of a spring of pure water," I found the accompanying analysis, by Dr. W. Hunter, of the water of a well, in the vicinity of the spot where the borings were made: the well was about 32 ft. deep, but borings had been carried down within the well to 75 ft. from the surface, and the springs from that depth raised the water in the well to 13 ft., from 4 ft. the average quantity of water previously in the well. The water of this well was described as by no means worse than that of other wells in the neighbourhood.

As no analysis of the kind has been published, that I am aware of, this may not be unworthy of a page in the GLEANINGS. For all common purposes, I presume Dr. Hunter's analysis sufficiently correct, and I shall be happy, if it draw attention to a subject of no small importance, and lead some of the able chemists we now have in Calcutta to give a further and more complete examination of the waters in daily use by the inhabitants of this city.

A high authority (Dr. Ure) speaking on the analysis of waters, says, "It cannot but be an interesting subject to ascertain the component parts and qualities of the waters daily consumed by the inhabitants of large towns and vicinities. A very minute portion of unwholesome matter daily taken may constitute the principal cause of the differences in salubrity which are observable in different places. And with regard to manufactures, it is well known to all artists of how much consequence it is to them that this fluid should either be pure, or at least not contaminated with such principles as tend to injure the qualities of the articles they make."

The water generally used by the European population of Calcutta is taken from the large tanks, and is in great measure free from the saline impregnations of well water, they being dependant on the rains for their supplies of water, and the salt springs below being prevented from rising into the tanks by the mud lining their sides and beds. The water from a fresh dug tank is not drinkable, and the precaution of puddling never being had recourse to in Bengal, it is only in the course of years, as the mud precipitated forms a coat impervious to the springs from the salt strata, that the tank waters become palatable. And in proof of this, I think it will be found, that the tanks most in repute amongst the natives for good water are such as have been longest dug. I know of two adjoining tanks, one of which has been dug about 50 years, and is about 15 ft. deep, the other was excavated two years ago, and carried down to the depth of nearly 40 feet. The strata were beds of sand and sandy loam, and clay intermixed with the usual decayed and charred vegetable matters; and all the springs were salt, and the tank continues now very brackish and useless, or rather, perhaps, pernicious, for the water is used by many of the poor, from their being no better water obtainable in the immediate neighbourhood. The water in the old tank was not affected by the inferior level of the new and larger tank, and though the salt, sandy strata must be common to both, its water is considered the best within a considerable distance, so much so, that the supply being insufficient for the numbers who came to the tank, its proprietor has been obliged to limit access to it. In some parts of Calcutta, and generally in the suburbs, there is a great deficiency of good clean tanks, and it is out of the power of the poorer classes to go any distance for water; and no person who has seen the filthy holes to which they are frequently forced to have recourse, but must apprehend, that much of the sickness they occasionally suffer may be attributable to the water they are in the constant habits of drinking, especially in the three months preceding the rains.

From the observations appended to the account of the borings before noticed, one is led to suppose, that the "primary object of getting at springs of fresh water entirely failed;" but, from a perusal of the papers connected with the undertaking, this conclusion appears to me by no means to be borne out.

I hope shortly to be able to submit to you several abstracts of borings made in Calcutta, from which I think it is to be inferred, that all the springs that are to be met with to 60 or 70 feet from the surface are salt, but that there are at no greater depth than 120 to 140 feet springs of fresh water, which may be obtained in an abundant supply within a very few feet of the surface by the method, now so common in England, of perforating to the springs with a borer, and keeping the hole thus made open with a cylinder of metal, terminating above in a shallow well of masonry.

From the borings above alluded to, it seems, that the rock is at Calcutta about 140 feet only from the surface, and that it contained much lime: it would be interesting to know, whether any other borings have ever been made, at no great distance E. or W. of Calcutta; as from the results of such experiments the inclination of the rock might be traced, and the practicability of procuring good water at any place in the vicinity might be surmised. There is no basset of limestone, that I am aware of, on the western edge of the plain of Bengal, and this would seem to indicate, that the rock in question is only a partial formation and perhaps a thin bed only, such as is met with in the clays of the London Basin, but the nature, extent, and depth of the rock are points of considerable importance, which, however, I fear are not likely soon to be ascertained.

I am, Sir,
Yours obediently,
A. N.

Calcutta,
March, 1829.

Analysis of the Water drawn from a Well in Fort Willam, at the Depth of 70 Feet from the Surface; by Dr. Wm. Hunter.

Agreeably to your desire, I have drawn up as good an account, as the materials now in my possession will admit, of the experiments I made on the water sent by you to me in 1804. For reasons which I shall presently assign, the analysis was left unfinished, with the intention of repeating the operation from the beginning, which I never had leisure to do; but I hope enough has been ascertained for purposes of real utility.

I.

1. The water was of a milky or opal colour; it had a muddy taste, and a slightly putrescent smell.
2. No change was produced on litmus or turmeric paper by dipping them in this water, but Fernambuco paper had a slight violet tinge induced on it, and the blue colour was in some degree restored to litmus paper reddened by vinegar.
3. Nitrate of silver produced a copious precipitate.
4. On the addition of pure potassa, a cloud was suspended in the middle of the glass.
5. Lime water struck a cloud.
6. Muriate of barytes produced no effect.

From these experiments it appears,

1st. That a minute quantity of some earth was suspended in the water, and (by exp. 4. 5.) it appears probable, that this was earth of magnesia, and (by No. 5.) most likely carbonated. A small proportion of clay is indicated by the opal colour.

2dly. That some putrid animal or vegetable particles had access by filtration to the water.

3dly. That it contained a very small proportion of disengaged alkali (by No. 2.), unless the change of Fernambuco paper was produced by the carbonate of magnesia.

4thly. (by No. 3.) That there was a considerable quantity of some muriate; and,
5thly. (by No. 5.) that there was no sulphuric acid, nor any of its combinations.

II.

1. Of this water 14lb. 1 oz. avoird. were evaporated to dryness, and left a residuum of 220 grs.
2. This being digested in rectified spirit of wine, filtered and evaporated to dryness, gave a residuum of 14½ grains.
3. On the residuum, insoluble in spirit of wine, were poured eight times its weight of distilled water; this solution being slowly evaporated for several days without depositing any crystals was evaporated to dryness, and yielded 118½ grains of a salt in dry powder.
4. The residuum, not soluble in spirit of wine nor in eight parts of distilled water, being well dried weighed 56½ grains.

The sum of these three portions falling short by 30½ grains of the weight obtained by the first operation led me, as above stated, to suspect inaccuracy and to break off the process, but not having had an opportunity of resuming it, I shall endeavour to assign the several ingredients as nearly as the imperfect state of the experiments will admit.

III.

1. The salts soluble in spirit of wine (II. 2.) amount to 103 grains in a pint of water.

2. A little of this salt was dissolved in water, and a particle of oxalic acid added to the solution; it dissolved without forming any cloud.

3. Pure ammonia and lime water respectively added to solutions of this salt gave no cloud, but one was formed instantly and precipitated by pure potassa.

4. Two grains and a half of this salt being dissolved in distilled water, and a little dilute sulphuric acid added to the solution, a slight cloud was formed, but no precipitate; being evaporated to dryness, it weighed two grains which tasted bitter.

5. Hence it appears, that this salt consists chiefly of muriate or nitrate of magnesia, with probably a very small proportion of muriate of barytes.

IV.

The salt obtained by solution in cold water, appeared to be almost entirely muriate of soda. It amounted to 8.42 grains in a pint of water.

V.

The residuum, insoluble in spirit of wine, and eight parts of distilled water, after keeping some days, weighed 57 grains; one-fourth part of this quantity was boiled in a pint of distilled water. This solution being evaporated, yielded 5½ grains of dry residuum, which gives 1.56 grains to a pint of water.

As it appears (from I. 6.) that the water contained no vitriolic acid, consequently no gypsum, this residuum must probably have been carbonate of magnesia, of which two parts are soluble in 100 of cold water.

VI.

1. The residuum, insoluble in 500 parts of water, weighed 8½ grains or 2.50 grains in a pint of water.

2. I dissolved half a grain of this in muriatic acid, and dropped into the solution some Prussiate of potassa; no precipitate took place; no iron is therefore contained in the water, for I had found by experiment that half a grain of sulphate of iron (which contains no more than 0.115 grains of oxyde of iron, according to Bergman) in a pint of water, gave a considerable precipitate of Prussian blue on adding the Prussiate of potassa.

3. On the remaining eight grains I poured distilled vinegar and filtered the solution, which, when evaporated to dryness, gave 9½ grains of salt. The earth dissolved by vinegar might have been carbonate of lime of or magnesia, and what it actually was might have been ascertained by sulphuric acid, but this was hardly necessary, for it appeared (by III. 2.) that the water contained no muriate of lime; now the muriatic acid having a stronger attraction for lime than for magnesia, would have preferably united with the former had any existed in the water, eight grains having furnished 9½ grains of this acetate of magnesia, 8½ would have furnished 10.09, which contain, according to Wenzel, 3.43 grains of magnesia, or 0. 98 in a pint of water.

4. The residue not soluble in vinegar was not weighed, but it may be nearly estimated (by deducting the magnesia from the whole mass insoluble in water) at 5.07 or 1.44 in a pint of water. This was shown (by VI. 2.) to be free from iron, and therefore it consists of clay, with perhaps a minute proportion of silex, but the existence of this last was not ascertained by experiment.

Conclusion.

From the above experiments, it appears that a pint of the water in question contains the following ingredients.

	grains.
Muriate of soda	8.42
Muriate of magnesia	1.03
Carbonate of magnesia	2.54
Clay, with perhaps a little silex	1.44
	13.43

If we make allowance for the quantity of residuum lost, by increasing these in the proportion of 200 to 189.5, the whole extraneous matters in a pint of the water will be 15.59 grains; but I cannot say in what proportions this augmentation ought to fall on the several ingredients.

From this statement it will appear, that the water can neither be palatable nor salutary. It contains of common salt nearly three times as much as the draw well at Upsal, which Bergmann says is seldom used unless for some mean purpose. A person who should drink two quarts of it daily would swallow 34 grains of common salt, 4 grains of muriate of magnesia, (which is said to be greatly superior in purgative power to Epsom salt *,) and 5¾ grains of clay. All this can hardly fail to affect the bowels, but I cannot pretend to say in what degree, nor how long, in case of necessity, such water might be used with safety.

I regret that the above account is so imperfect, but should you find it in any degree useful, it will be a satisfaction to,

My dear Sir, &c. &c.

(Signed) Wm. Hunter.

Calcutta,
18*th Aug*. 1806.

V.—*On the Native Method of raising Water by Baling.*

To the Editor of Gleanings in Science.

Sir,

There is a method of raising water in India by baling which, where the quantity is limited and no repetition of the operation likely to occur, is perhaps the best we can resort to, I mean on account of the simplicity and low cost of the apparatus. Many people imagine it to be more than this, and think that it is not only the simplest but the most effectual method of employing human labour; and of this opinion was a friend with whom, a few mornings ago, I went to examine the new canal that is now in progress, and in which the method in question was in operation for draining the small tanks that happen to lie in the line of the canal. A few simple measurements and calculations soon convinced him of his error; and as the result was unexpected in that instance, it may be so in others. I therefore send it you for publication, in case you think it likely to throw any light on the subject.

The method in question is, that of tossing the water by means of basket ladles or scoops held by ropes, from one level to another. If the difference of level be at all considerable, there must be a number of them, as the rise due to each scoop can seldom with advantage be extended much beyond the height of the men working it. The arrangement is doubtless familiar to most people in India, as it is frequently employed in irrigation in different parts of the country, and I shall therefore spare myself and your readers any further description of it.

Six men were employed at the one in question, two at the lower step of three feet, and four at the higher one of seven feet, the total height raised being ten feet. The scoops held in both cases as near as could be estimated about ⅓ of a cubic foot of water each, (the people themselves said 10 to 12 seers.) The lower scoop must have kept the upper one in full supply though only worked by two men, for it was found, that only 19 strokes of the latter were made in the same time, as 25 strokes of the former; add to which that, owing to the height of the upper step (seven feet), much water was spilled in throwing it up. We see then, that small as is the quantity of water which these scoops contained, they could not be increased in size with advantage; for the same sized scoop required four men to work it to a little more than double the height, and yet the delivery was not equal to that of the lower step.

We may from the preceding data easily calculate the quantity of water raised; and as great exactness is not required, we may assume that $22 = 19 + 25 \div 2$ was the number of strokes made in a minute by six men, each stroke delivering ⅓ of a cubic foot, 10 feet high. This is equal to 7⅓ c. ft. ($= 22 \times \frac{1}{3}$) raised 10 feet high, by the labour of six men, or to 73.3 c. ft. raised one foot high.

Now, in one of the preceding Numbers of the Gleanings, it is stated, (p.56,) in the account of the pump described by Professor Robison, that a "feeble old man working eight to ten hours a day" did with that pump raise 7 c. ft. of water, 11½ feet high per minute. This is equivalent to 80½ c. ft. raised one foot. Whereas by the scoop arrangement, it appears that six average Bengalees could only raise 73.3 c. ft. The difference here is enormous, and, if real, is certainly worth pointing out. An arrangement that will not permit six average natives to do the work of one feeble old man,

* Gmelin, App. Med. P. II. vol. i. p. 108.

weighing only 110 lbs.*, working too with a machine that depends chiefly on the man's *weight* for its efficiency, is mischievous; inasmuch as it wastes more than $\frac{5}{8}$ of the power, and to make this fact generally known, is, I conceive, one of the legitimate uses of your work. With every wish for its success,

I am, Mr. Editor,
Your obedient servant,
Ω.

VI.—*On the Application of Steam to the Purposes of destroying all Kinds of Vermin on Board Ships.*

The destructive ravages of white ants, when once they find their way on board the vessels in India, have long been the bane of that description of property, aggravated too by the secrecy with which their operations are frequently carried on, and by the absence of all means of prevention. Property of acknowledged value, to the extent at times of above a lac of rupees, has become, on the presence of this destructive animal being discovered, almost valueless; since hitherto, when once known to have infested a vessel, no instance, we believe, has occurred of their ever having been wholly extirpated; thus not only attaching a suspicious character to the vessel, but occasioning continued, and sometimes very heavy and expensive repairs. Indeed it is scarcely possible even to trace the extent of the evil with any degree of certainty. A ship may undergo a very heavy repair of damages occasioned by the ants, and every possible means may be adopted with a view to ascertain the existence of further damage, without success; yet a very few weeks may show another part of the vessel to be infested to a great extent, rendering necessary a yet further repair.

It may reasonably be supposed, that such destruction of property would not be permitted to continue, without some attempts at a remedy: of these, the most effectual have hitherto been the application of extreme cold, or sinking.

The former of course could only be carried into execution by sending the vessel infested to a cold climate, there to be laid up for a winter. Independent of the loss occasioned by the non-employment of the vessel, the remedy has never been, we believe, complete. A stop has been put to their ravages for a time, but a return to a warm country has shown that the animals have not been effectually destroyed; either they have merely been reduced to a state of torpidity, or if the living animal has been destroyed, the eggs have not been deprived of their power of production. The same remark may be made in regard to sinking, independent of which the expense and difficulty attending the operation render it little better than submitting to the evil itself.

That so obvious a remedy as that of filling a ship with steam, should, in these times, when its employment may be said to be almost universal, have been so long unthought of, is not a little remarkable, particularly when the practice of smoking ships, for the purpose of destroying rats and other vermin, has long been adopted, and with partial success. The destruction caused by rats on board ship is only second to that effected by the white ants. Instances have been known of their eating through a vessel's bottom and decks; while their ravages on the stores, provisions, and cargo are almost incredible. Nor are these the only vermin, with which ships in this country are infested. The cockroach and black ant, centipede, &c. if not destructive of the vessel itself, are so of the comfort of every person on board. The first find their way more or less on board every ship in India; the second prevail at times to an extent almost surpassing belief, on vessels trading to the Eastward, which supply themselves with wood in the Straits. The application of steam to the destruction of these latter animals is of itself an advantage almost incalculable. It is obvious, that nothing but the most searching, and, at the same time, powerful agent could extirpate an animal like the common ant. The experiment was first tried in England, at the suggestion of Captain Ford, late in command of the ship Providence in this port, on a ship belonging to him, and we understand with success. We be-

* A strong and active young man loaded with 30 lbs. raised 9 c. ft. the same height (= to 103.5 × 1 feet) being the work of 8½ Bengalees with the baling machine, according to the preceding data and calculations.

lieve, that the steam was not applied to the utmost extent of its power on that occasion. It has, however, since, on the representation made by Captain Ford of the success of the experiment in England, been applied to perhaps as great an extent as it could be with safety; and certainly to sufficient extent for all practical purposes, with the most complete success : since that experiment too it has been applied on several other occasions.

The first trial in this country was on the Honorable Company's ship Investigator. The experiment was conducted by Capt. Forbes, of the Bengal Engineers, and Mr. Kyd, the Honorable Company's Master Builder, and, as might be expected in such hands, would appear to have been managed in the detail, with the utmost care and attention, affording a secure guide for future operations. The following is an extract from their able and interesting Report.

"1. We had the Honorable Company's Steamer Irrawaddy moored alongside the Investigator; and having fitted two lead pipes furnished with stopcocks to the head of the Irrawaddy's boiler, by means of a new manhole cover; we led the pipes into the Investigator, and put them down the fore and after hatchways into the hold.

2. We had, in the mean time, closed the scuttles of the Investigator's sides, as well as all the hatches; moreover, the stern and gallery windows, and the entire front of the poop; boring at the same time a hole in each gallery cell, to allow the steam to come up from the hold into the cuddy.

3. We also fitted a pipe, having a stopcock on it, to the main hatchway, which was opened occasionally to observe the state of the steam, in case of danger, from its overpressure.

4. These preparations being made, we had the fires of the Irrawaddy's boiler lighted at 11 A. M. on the 7th ultimo, so as to let on the steam at noon the same day; by six o'clock the same evening, the steam began to show itself at the scuttles, and at the hatches; and the poop and upper deck began to feel hot. We continued the steaming for 48 hours, by which time the whole of the decks and sides even to the out side copper, close to the water's edge was so heated, as to be scarcely touchable by the hand.

5. On opening the hatches to ascertain the result of the operation, we were pleased to see the effectual manner in which the penetrating heat of the steam had destroyed the vermin. The white ants appeared reduced to a substance like soap, and the cockroaches and rats, to a soft pulp, capable of being washed down into the limbers.

6. The putrid smell of animal decomposition came on at the end of 24 hours, but did not continue above a day.

7. The paint on the beams and sides was shrivelled, and peeled off, and the leather which covered the ring bolts in the cuddy, was converted into charcoal.

8. We have purposely delayed sending in our Report, till we could ascertain the effect of the steaming on the caulking, a matter regarding which we were anxious, inasmuch as, if that had been disturbed, the operation would in future have had to be confined to a ship about to undergo repair in dock. We have, however, satisfaction in being able to report, that we can discover no injurious effect on the caulking; further, that the steaming a ship for the destruction of vermin seems perfectly feasible, either afloat or in dock, whether about to undergo repair, or to proceed to sea. The only circumstance demanding attention in the latter case is, that the ship will require new painting.

9. Although the destruction of vermin by steaming may be resorted to under all circumstances, yet the steaming of vessels in dock, previous to their undergoing their usual quinquennial repair of caulking and coppering, will be the most desirable.

10. In addition to advantages already noticed, the facility of introducing the steam from below, and the absence of condensation by the water, in contact with the whole surface of the immersed bottom, when afloat, will enable the steam to effect its object in one-third less time.

11. The present experiment having enabled us to ascertain an efficient and simple method of steaming ships, to destroy vermin, we beg here to record our opinion, that in all moderately large ships about to be steamed, the masts and bowsprit ought to be taken out, as also all projecting boomkins, davits, and cat heads. The whole of the hammock stantions and external birthing should further be taken away, and the ship be cleared of all lumber, and articles likely to sustain injury from the steam.

12. For large ships, where the unmasting would be laborious, we conceive that long bags made of painted canvas, might be put over the mast heads, and nailed to deck, and the steam admitted into them. Painted canvas also might be tacked with wooden battens to the deck, and to the outside, enclosing the sides all round, and this might be extended to hawse chocks, quarter galleries, and to all parts which it would be inconvenient to remove.

13. By lifting the ship's pumps about 3 feet, one of them may be fitted as a safety steam valve, and the other as a safety air valve, and thus a communication be made quickly with the lower part of the hold. The steam pipes should be long enough to introduce the steam into the bottom of the hold, as otherwise the steam and heat would be for a long time intercepted from the lower parts of the vessel, by a stratum of air.

14. Such of the steamers as may be intended to be used for steaming ships, might conveniently, and at small expense, be provided with a spare boiler man-hole cover.

15. The whole apparatus for steaming could easily be transferred to any one of the steamers, and would then be available for any ship. Independent of the man-hole cover, the parts would merely consist of two pipes of copper (fitted with stop-cocks) of 5 inches diameter, together with a steam safety valve pipe, and an air safety valve pipe, for the ship about to undergo the process*.

16. In steaming ships afloat, it will obviously occur to hang the steamer on to the vessel to be steamed, and then so to secure the two, as to prevent the cross motion their being separately moored would cause, to the injury of the steam pipes. For steaming ships in dock it will be requisite to have a boiler set so near to the dock, as to admit of having pipes fitted for the conveyance of the steam to the ship.

17. It will be requisite, when the steam has been admitted into a ship, whether it be afloat or in dock, to have a cauldron of boiling water ready to kill insects which may try to escape; and it will be requisite to have a few persons in attendance, to shut up places where steam shows itself, as well as to attend to the state of the pipes, and of the operation.

18. We come now to the consideration of the vast importance to shipping in tropical climates, which this successful experiment of steaming of ships, to destroy white ants, has indicated. The speedy riddance of rats, cockroaches, centipedes, and scorpions would alone be of importance. The waste of property by the two first is very considerable, and fumigation is frequently employed to get rid of them: smoking is dangerous, inasmuch as many ships have been burned in the process, but although smoking kills rats, it will not kill cock-roaches nor ants; neither has it the slightest destructive effect on their eggs, so that while the larger tribe of noxious animals may be got rid of by this means, the smaller and much more dangerous ones, the white ants, are left to destroy the ship.

19. Sinking is no doubt an effectual measure for the extirpation of those insects, but it is one which can be resorted to only in small ships, and in them even at a considerable risk of entire loss, and at considerable expense, a great waste of time in the employment of the vessel, and the disadvantage of laying a foundation, by the introduction of mud, for a future, more successful attack. In fact, it has invariably been found, that vessels which had been sunk to kill white ants, were speedily infested afterwards, and rapidly destroyed.

20. The being enabled to eradicate white ants from Indian ships, must have the effect of giving an enhanced value to this description of property. It is on record, as well as a truth familiar to the officers of the Marine Department, that several Government vessels have been entirely destroyed by white ants; and further, that by their ravages great public loss has been sustained: under such circumstances, too much cannot be said in favour of such an application of steam.

21. The success of the present experiment may form an era in the history of Indian shipping. The steaming of vessels, to destroy vermin, must speedily come into general use. Then the only wonder will be that, seeing the common application of steam to almost every purpose, its excellence as a substitute for fumigation was not *in this country* sooner suggested."

* Partial condensation, such as in the case of the Investigator led to the fracture of the upper deck pillars, would by these valves be effectually guarded against.

It is scarcely necessary to add a word to the above clear detail. The expense of the operation, including the requisite pipes, &c. did not amount to Sicca Rupees 800, and the subsequent charge for cleaning the ship was about 100. A complete apparatus to be attached to the boiler, it appears would not cost above Sa. Rs. 1500, after which the expense would be confined to the expenditure of the coals, and the necessary artificers and contingent charges. One precaution, however, would appear to be necessary to be adopted in the steam vessel, which is, to take care that none of the vermin find their way from the vessel steamed to the steamer. Such appears to have been the case with the Irrawaddy.

VII.—*Letter from a Correspondent in the Himmalaya.*

I am only lately arrived from a trip through the old tract, viz. Kunáwar, which I had hoped would reward me with some consoling recompence for the sacrifice I made for its accomplishment; but I failed entirely in my object of establishing the vaccine, owing to the folly and timidity of the Besáher Rajah. However, I have obtained some particulars in my journey, which, if not equivalent to the pecuniary losses I suffered, are at least interesting. The fossils and shells which occurred in my route are very strange objects. They are chiefly valuable from having myself seen them in situ. They comprise cockles, muscles, and pearl fish, univalves, and long cylindrical productions which are most singular objects. I found them lying upon the high land at 15,500 feet, in a bed of granite and pulverized slate; the adjacent rocks being at the same time of shell limestone. All the shells are turned into carbonate of lime*, and many are crystallized like marble. I came upon a village at a height of 14,700 feet;—are you not surprised that human beings could exist at such an elevation? It was yet the middle of October, and the thermometer on two mornings was 17; what it is at *this* season of the year, I cannot guess; yet the suns ray's felt oppressive, and all the streams and lakes which were sheeted with ice during the night, were free and running by 2 o'clock. The finest crops of barley are reared here, and to irrigation and solar heat are the people indebted for a crop. The barometer gave for the highest field 14,900 feet of elevation; this verifies the observations, or rather inferences, on the limit of cultivation in the upper course of the Sutluj; and I think it quite possible, and even probable, that crops may vegetate at 16 and 17,000 ft. The *yaks* and shawl goats at this village seemed finer than at any other spot within my observation. In fact, both men and animals appear to live on and thrive luxuriantly, in spite of Quarterly Reviewers, and Professor Buckland, who had calmly consigned those lofty regions, and those myriads of living beings to perpetual ice and oblivion. What would have become of the beautiful shawl goats which furnish those superb tissues, that adorn the ivory shoulders of our fair countrywomen, had the Professor and the Quarterly the management of these matters their own way!

On the North Eastern frontier of Kunáwar, close to the stone bridge, I attained a height of more than 20,000 feet, without crossing snow, the barometer showing 14,320, thermometer 27 at 1 P. M†. Notwithstanding this elevation, I felt oppressed by the sun's rays, though the air in the shade was freezing. The view from this spot was grand and terrific beyond the power of language to describe. I had anticipated a peep into China itself, but I only beheld its lofty frontier all arid, and bare, and desolate. It was a line of naked peaks, scarce a stripe of snow appearing; yet every point had an angle of altitude of a few minutes, some half a degree, and at a very considerable distance; this argues at least 21,000 feet.

I found Chinese guards stationed at all the passes, partly in consequence of Lord Amherst's visit to Simla, but chiefly on account of some Mandarines from China itself, who were moving slowly along the table land, and taxing the whole country. They have been settling the affairs of Ludak, and I fancy not much to the advantage of the Rajah who sought their advice. At Dankur, I had a most friendly interview with the Ludak Wuzeer, who gave me a dinner, accompanied by buttered tea in the Tartar

* All shells are composed of carbonate of lime, principally. In the case of the porcellanous division, it is combined with a little, and in that of the mother of pearl shells, with about one-fourth, of animal matter.—Ed.

† The date is not mentioned. Using the mean result for October observed in Calcutta, this gives 20,419 ft. as the elevation.—Ed.

fashion, stampt biscuits, and dried fruits. The tea, as you may imagine, was not very grateful, and I had much apprehension for its fate, after I thought it was safely lodged. On leaving the Ludak (Spíti) territory, I was most agreeably surprised by a visit from a Chinese officer, who had travelled day and night to meet me. He was a very strange figure, dressed out in a cloak of woollen broad cloth, trimmed with fur, a head dress crowned by a trident, a knife in his girdle, and boots of Bulgar or Russian leather; he was a man of medicine, and received many drugs from me the uses of which he wrote down, and a pair of lancets which he seemed to know how to use: there was much of character in this personage. He drank a liquor which tasted to me like bad beer, and each time he emptied his silver cup he filled up my tumbler much against my own wishes. There was a great deal of apparent openness manifested in all his actions; but I cannot view so unusual a departure from their accustomed suspicious vigilance, without some doubts of the sincerity of the part which this man performed. On taking leave of me he shook both my hands, and assured me of an invitation to Lake Mansarawur next season. He had heard of my searching for fossils and curiosities, and presented me with a petrifaction from Lake Mansarawur; it seems a species of Medusa.

But perhaps the most interesting circumstance of my tour was my meeting with the Hungarian traveller, M. Csoma de Körös, at the monastery of Kánam in Kunáwar. I found him with his learned associate the Lama, surrounded with books, and in the best health. He has made great progress with his literary studies, having nearly finished his Grammar and Dictionary of the Thibetian language which he has pledged himself to Government to fulfil; but his objects are vast and comprehensive, and the works he is now engaged in will form but a prelude to further researches. He wishes to invite learned men from Teshoo Loompoo and Lahassa, and by their assistance study the Mongol language, which he considers the key to Chinese literature, and through it get access to Mongolia, where he expects to discover much interesting knowledge; but unfortunately, he wants resources. The Lama receives 25 Rs. a month: a servant costs him four, his house-rent one, and his writing materials a proportion, so that he has not actually 20 Rupees left for the necessaries and comforts of life in that cold region of the mountains. It would be liberality well bestowed to render him the little aid he stands in need of; but he is so tenacious of his independance, and carries his nicety of feeling to such a degree, that he will accept of nothing but from a public source, and from that only, because he finds himself able to make a suitable return in his works. The only things he has ever accepted from me are a Latin dictionary and a Greek lexicon, which are useful in the arrangement of his materials. I offered him some rice and sugar, which I knew he was in want of; but he returned them, and sent me 16 rupees to purchase some articles at this place, which I have done and despatched to him. He is very anxious to see the numbers of the Oriental Quarterly which refer to his studies. Mr. Wilson sent him up two, and they have excited his curiosity to see the others which are referred to: I think No. 8 or 7 is one he wishes to see. He is much in want of ancient authors to consult, for instance, Pliny, Ptolemy, Quintus Curtius, Diodorus Siculus, &c. The Asiatic Society might perhaps supply his wants, and this small boon could not possibily be bestowed upon a brighter object of their patronage: indeed my humble opinion is, that if his allowance were made up to 100 rupees a month, either by the Society or by Government, it would be a well earned tribute, and one which would be amply repaid. M. Csoma showed me his labours with eagerness and pride. He has read through 44 volumes of the Thibetian Encyclopædia, and they have fully rewarded his perseverance. He has discovered part of the Mahabharat or poetical work which (at least great part of it) is supposed to be lost*. He pointed with great animation to a poem translated, from the Sanscrit edition, in one of the numbers of the Oriental Quarterly, holding in his hand, at the same time, the original in Thibetian. Mr. Wilson will be no doubt interested in this, and perhaps you might mention it to him. His learned companion, the Lama, has informed him that lithographic printing has flourished for ages in the ancient cities of Teshoo Loompoo and Lahassa; and that, at the former place, the anatomy of the human body is represented in 60 different positions in cuts or prints. The Kanjur, or work in Thibetian which treats of sciences and arts, has five volumes devoted to medicine. The geography of Thibet promises to receive very considerable illustration from the printed records deposited in the monasteries. Mansarawur being considered the central source of several great rivers is a mere

* There is some mistake here, either of the writer or of M. De Körös. The comparison, however, of the Thibetian version with the Sanscrit original would doubtless be interesting.—H. H. W.

figurative position, as indicating the highest level or point from which the waters are thrown off in all directions; for the Hindoos, as well as the Thibetians, know as well as we do, that two rivers cannot flow out of the same lake in opposite directions.

On the retrogression of literature in India and before it, learning fled to Thibet, and there found an asylum: and on this account we are warranted to look to that country for literary riches. The very fact of printing and printed works of gigantic magnitude argues favourably, and M. Csoma's discoveries are far from the least estimable part of this vast terra incognita. M. Csoma's abode in Kanáwur is particularly favourable to any object of enterprise; and if I could but once establish vaccination amongst the Lamas, I might get access to new and strange countries. M. Csoma's intelligent companion, being superior to prejudice, and possessing a modest confidence of this superiority, even offered to be vaccinated; but as I could hardly depend upon the effect, and could not have stopped to abide the result, the Lama considerately thought it better to forego the operation than risk a failure which, in my absence, would likely have proved fatal to the cause. I should wish to make another trip this year, but I will not undertake it without some encouragement.

Súbhatu, 13th Jan. 1829. J. G. G.

VIII.—*Proceedings of Societies.*

1.—Asiatic Society.

There was a meeting of the Society held on Wednesday, the 4th Instant, the Honorable Mr. Bayley, Vice-President, in the Chair.

The following gentlemen proposed at last meeting, were elected Members of the Society:

Mr. H. Wood, Mr. Martin, Baboo Prosonna Kumar Thakur, Baboo Dwarkanath Thakur, Baboo Sibchunder Das, Baboo Haromoy Dutt, Baboo Ram Comol Sen.

Two specimens of the large Bamboo from Martaban, various specimens of ore of antimony from the same place, and two mother of pearl oyster shells from Mergui, were presented by the Honorable Mr. Bayley.

A conglomerate of siliceous sand, mica, alumine, and peroxyde of iron from the Aracan Coast, was presented by Commodore Hayes.

A copy of Memoires de l'Expedition Anglaise de l'Inde en Egypt, by Mons. le Comte de Loc, was presented by the author.

The first volume of the Transactions of the Agricultural and Horticultural Society of India, was presented by the Society.

Scriptural terms, with their proposed rendering into Sanscrit, by the Reverend Dr. Mill, with remarks by Mr. H. H. Wilson, presented by the Reverend Mr. Holmes, Acting Principal of Bishop's College.

A copy of the 3d volume of the Asiatic Researches, was presented by Dr. Burlini.

The thanks of the Society were voted to the donors of the above.

The Meteorological Register, kept at the Surveyor General's Office, for December and January, was presented by Captain Herbert.

Read a letter from Mr. Hodgson, forwarding a duplicate Index in Persian and Nagari, of the contents of the Kanjur.

Read extracts from an account of a visit to the highlands of Punduah, and to the great cave of Buban, by Mr. Walters; and also a communication by Lieutenant Wilcox, detailing the progress of geographical discovery in Asam.

Resolved, that the thanks of the Society be presented to the authors of the above communications.

2.—Medical and Physical Society.

At the meeting of the Society held on the 7th instant, Dr. Don of the Bombay Medical establishment, was elected a member.

A letter was read from Sir James McGregor, Director General of the Medical Department, London, acknowledging the receipt of the 3d volume of the Society's Transactions.

Mr. Breton presented to the Society a variety of publications, drawn up by himself for the use of the Native Medical Institution.

Mr. Tytler's paper On the Effects of Mercury in Febrile Diseases, was then read and discussed by the meeting. Adjourned.

IX.—Scientific Intelligence, Miscellaneous Notices, &c.

1. *The New Canal.*

A canal of large dimensions is now in rapid progress, promising great and immediate advantage to the inland navigation of the Delta, and its connection with the capital,—a branch in which there appears to be great room for improvement, notwithstanding the great natural advantages of which the locality of Calcutta has to boast. The work has been some time under contemplation, but commenced only in February last. The line we understand commences from the Hooghly river, into which it will open with tide-gates, immediately north of the Chitpore bridge, over the Marhatta ditch. After crossing the Barrackpore and Dum Dum roads, it pursues a course parallel to the Circular road, at the average distance of something less than half a mile to the eastward of that road, until it intersects the Balyaghat road, when, after a slight curvature to the South East, it falls into the existing canal known by the name of the Eastward or Lake Canal, the route by which a considerable proportion of the craft navigating the Soondurbuns approaches Calcutta. The scheme of the canal allows a constant breadth of water exceeding 80 feet, and a depth of water never less than 6 feet. The part between the tide-gates near the mouth and the first bridge across the Barrackpore road is to be excavated to an additional breadth, in order to serve the purpose of a harbour for craft, that otherwise lie exposed before the different ghats of the town.

The work is at present entitled the Circular canal, and is connected with a series of works which have been in progress for some time, for the improvement of what is called the Upper Soondurbun passage, in which there exist many inconveniences and delays, if not real perils. Indeed, we understand, that the idea of this Circular canal, as one of communication with the river, originated entirely during certain discussions before a special committee of officers appointed by Government to consider the feasibility and advantages of a scheme for improving and shortening that route, at a time when much attention was directed to the possibility, by the failing of the Jellingee and Bhágirattee, of this circuitous route remaining the only practicable approach to Calcutta for the trade of the Gangetic provinces.

It is, I suppose, known to most of our eastern readers, that the lower Delta, called the Soondurbuns, is so intersected in its surface as to form a perfect net-work of tide-creeks, most of which are navigable for boats of considerable burden, and have been so used from time immemorial by the natives of the country. Before the year 1775, the only available communication between these creeks and the river Hooghly emerged into Channel creek, while the rest of the trade, then insignificant, which did not require to pass into the Hooghly, landed at Balyaghat, situated two miles east of Calcutta, on the margin of the great salt water lake or marsh. The passage excavated by Major Tolley, now bearing his name, at first a private adventure under a grant for so many years, and excavated with very insignificant dimensions, soon became both a much frequented passage and source of considerable revenue. The dimensions have been increased at several successive periods, with the increase of its importance, to their present standard. The depth and other local circumstances are still much against this canal being, in its present constitution, rendered a perfect navigable communication, or in any way sufficient for the magnitude of the trade which enters it. The bed is not sufficiently low to prevent a great portion being left dry during the ebb tides of November to May; and the whole canal is besides left exposed to the great variations of surface daily* and yearly† which take place in the Hooghly river.

The creeks of the Soondurbuns are subject to but little periodical influence, while the surface of the Hooghly is raised considerably by the influx of the fresh water. The daily rise and fall of tide in the creeks which approach Calcutta by the lake, partly from the great length of course in a straitened channel by which the tide has to travel, and partly from the influence of a vast marsh at the termination of its course in dissipating the force of the tide, is also very small, when compared with the daily variation in the Hooghly. The application of some kind of tide-gate at the junction of waters which are so widely different in their phenomena, appears to be the most economical and judicious plan that could have been devised. The canal will be kept by its ebb-gates from falling to the great depression of the river in the

* 7 to 15 feet, daily tide.

† 20 $\frac{1}{3}$ feet, extreme variation.

dry months, and will also be protected by its outer or flood gates from the violent influx of river water, which would otherwise obtain when the river is at its highest, or 11 feet above the waters of the lake. A constant passage will be available for purposes of navigation of several hours during each tide throughout the year, and sufficient influx of water from the river will be allowed to prevent the new canal from stagnating and presenting an appearance so disgusting and insalubrious as the present eastern Canal. The Circular canal is not immediately connected with any new system of town drainage in contemplation; but the subject appears not to have been left unconsidered, and much facility in such respects may be confidently expected from the position and proposed management of its water level.

About 3,000 labourers are now employed daily on the excavations, and the depth already reached, is in many places 18 feet. The final completion of the work may be looked for in 1831.

2. *Baron Cuvier's New Work on Fishes.*

A very full prospectus has reached us of a new work on fishes, preparing for publication by the above celebrated naturalist. To judge from the details given of the assistance and contributions that have been offered to the work—as well as of the general plan which it is proposed to follow in the execution—we would say that it is likely to increase, if possible, the high reputation due to the name of Cuvier in every civilized country of the globe. The Royal Cabinet which furnishes the groundwork of his labours is said to consist of 5,000 species and 15,000 specimens. A great part of these have been arranged so as to give a clear idea of the comparative anatomy of this class of animals, and being ticketed and deposited in the Museum of Comparative Anatomy with references to the present work, they will form an easy means of verifying any details, and of ascertaining clearly the identity of any species. The names of those who have given their assistance or promised it, would form a list of the most distinguished men in Europe, in this branch of natural history. In particular, however, M. Valenciennes, assistant naturalist in the Royal Museum, is stated to be his principal coadjutor, and the work is announced as their joint production. A very full history of Ichthyology is promised, comprising a critical analysis of all the works hitherto published on the subject of fishes.

The synonymes, which are not unfrequently the most puzzling branch of a science, have had particular attention paid to them; and from the large means of verifying descriptions placed at his disposal, it is probable that every justice will be done to this division of the work.

MM. Werner and Laurillard have undertaken to furnish drawings for the plates. One plate at least will be given for each groupe of species, and when called for by any peculiarity in the species, two or more.

For the terms of subscription and other particulars, we refer to our cover.

3. *Woollaston's Thermometrical Barometer.*

The thermometrical barometer, if its construction were so simplified that it might be afforded at one or two guineas, might, I think, be brought into general use. It is a very useful instrument, and might be employed occasionally as a substitute for the barometer, but always with advantage as an accompaniment and auxiliary; and the comparison of the results of the two instruments made on very elevated mountains, such as the Himmalaya, would be very desirable and interesting. The principle of the thermometrical barometer is good, and it is independent of the specific gravity of the mercury, which is an advantage; but on the other hand the boiling point would be shown a little differently according to the quality of the water boiled; rain water, spring, river, and snow water* would, I suppose, give different points in an instrument of so much sensibility. The instruments which have been sent out would be useless in a survey: it requires a steady hand to put them into the curious tubes of copper and tin, of which the cases are composed, without breaking, and a fall would be fatal to them;—indeed they are most fragile instruments, far more so than the

* These two, with distilled water, would most probably have the same boiling point, though the others would be likely to differ. There appear, however, to be some uncertainties, in the determination of the boiling point, independent of the nature of the water, the cause of which is not very clearly ascertained. It has been found, for instance, that water boils in platina vessels at 2° lower temperature than in glass; and that vessels of different materials have different boiling points. Also that a few pinches of metallic filings, or bits of metallic wire being thrown into a liquid, occasion it to boil at a lower temperature than it otherwise would.—Ed.

mountain barometer. The makers in England except Troughton and Dollond seem to think that it is a great advantage to put their instruments in small compass. For those intended to be used in India, it is just the reverse. Here we can always carry any thing worth carrying, be it ever so heavy. If I were to order any of these thermometrical barometers to be made, I would require nothing except the thermometer part and scale made as simply and substantially as possible, and the scale should be capable of taking in 4,000 feet at once, by being made longer. It should be in a case by itself. Nothing more is required, except a tea kettle or shaving pot, and the apparatus of thermometers, matches, bottles, &c. which are stuffed into the case might be dispensed with, but there should be three or four spare tubes ready filled in a separate case, to be fitted into the scale in the event of accidents.

J. A. H.

4. *Application of Lithography to the Printing of Oriental Works.*

It has been announced, in one of the French papers, that a M. Semelet has published an edition of the Gulistan lithographed, which bears the most perfect appearance of manuscript. This is said to be the first attempt of the kind, and additional praise as to the difficulty of the undertaking, and beauty of the execution, &c. is given. It is but justice, however, to the several parties to mention, that printing oriental works from stone has been practised in Calcutta for years, and not less than two editions of the Gulistan have appeared from one of the Lithographic presses of that city, —the last, published in 1827.

5. *Fallacy of the Mechanical Arrangement described in No. 2. of this Work.*

At page 56, of No. 2, of Gleanings in Science, there is some account of a new invented beam &c. in which article the editor remarks, that he can detect no fallacy in the experiment. I have to request him to observe, that the two weights marked *i i* counterbalance each other, as the machine will remain at rest in any position in which it is placed; and consequently the descent of the weight *g* performs no other work than to overcome the friction and inertia of the several parts of the contrivance.

N. N.

6. *Notice of a Storm, which occurred at Rewah, in February,* 1823.

Lat. 24° 32′ 32″ N. Long. 81° 20′ 8″ E.

As the occurrence of such storms, during the above month, is not frequent in this country, I have been induced to offer the following description of it, copied from notes taken during the time it lasted. It appears to be a fact rather remarkable in Indian meteorology, that violent storms of several days' duration, do invariably terminate in dense fogs, when they happen, which is very seldom, during the dry months of the year. On no occasion that I recollect have I ever remarked a fog after a storm, during the rainy season in the provinces above Bengal.

Rewah, Sunday, 23d February, 1823.—Wind variable, and squally weather. Large *cumuli*, forming in all directions, and occasionally heavy showers, with *nimbi* from the east.

Monday, 24th.—Sky entirely overcast by *cumulo-strati nimbi*, flying from east to west, the upper mass of *cumuli* having a contrary direction. About 3 P. M. heavy *nimbi* flying to the west, where a *cumulus* of great blackness and density was forming from south to north. It advanced with greater and greater rapidity, until its line crossed the ground where my tent was pitched, discharging torrents of rain, accompanied by thunder and lightening, and strong gusts of wind from all points of the compass. It rained without intermission until near midnight, and storms of thunder, lightening, wind, and rain continued all night.

Tuesday, 25th.—The same yesterday, but somewhat less violent. The sky occasionally opened, and *cirri* were visible in the higher parts. The *nimbi*, which still continued to fly from the eastward, assumed more the appearance of *cirro-cumuli*.

Wednesday, 26th.—The storm ended with a fog of great density, which did not clear up until past 11 A. M.

G.

I.—*On natural and artificial Puzzolanas.* By M. Girard, *Engineer of Bridges and Highways.*

[From Annales de Chimie et de Physique, xxxv. 140.]

The chemical researches undertaken by many distinguished philosophers, to discover the cause of the properties possessed by volcanic and artificial puzzolanas, have not hitherto ended in giving a tolerable theory of a phenomenon so common in the arts: perhaps this proceeds from the circumstances of this phenomenon not having been appreciated at their just value. What in fact distinguishes puzzolanas from other earthy substances, is only the property of acquiring a certain degree of hardness when they are intimately mixed with hydrate of lime, and the compound is kept under water for a longer or shorter time. Those are called bad puzzolanas which, in these circumstances, give a product which never acquires more than a very moderate hardness, or rather those which require a month or more to *become solid.* The whole phenomenon requiring explanation consists, therefore, as is manifest, in the *degree of hardness obtained at the end of a given time.* Now, it is known that hardness is not in the number of properties which, in nature, can generically distinguish a substance. The same quantities of the same elements give rise to a multitude of bodies, the hardness of which varies indefinitely. Thus, from chalk to marble we may mark more than twenty gradations in the resistance of the almost pure calcareous carbonate. It should, therefore, be expected that analyses, by only proving the quantities of silica, alumina, and oxyde of iron contained in clays, would teach nothing, or at least very little, respecting their puzzolanic properties. This is confirmed by experience, and it is now necessary to seek, in less essential circumstances, the causes of these properties.

Chemical facts not having cleared up the question, Messrs. John and Berthier appear to agree in attributing the puzzolanic properties to the *cohesion* and the *absorbing power* alone which the matter acquires by the action of fire.

But the properties of argillaceous fossil sand or gravel, which I was the first to point out, and those which Messrs. Meril and Payen discovered at the same period although in a degree weaker, in the grauwacks and decomposed granites of Bretagne, do not allow the opinion of Messrs. John and Berthier to be adopted, at least without restriction And I think that I am prepared to establish now, that in fact the *cohesion* and *absorbent power* are not, in any degree, the causes of puzzolanic properties.

M. Vicat has examined (Ann. de Chim. for June, 1826) what was the influence of calcination on each of the elements of a clay which, calcined itself, gave a good puzzolana. This inquiry should seem to throw a great deal of light on the theory; yet its only result was to prove that silica, separated by acids from unbaked clay, is an excellent puzzolana, and loses part of this property by calcination, whilst alumina, which is only a bad puzzolana, gains a little by calcination, but too little to compensate for the loss of the silica; so that M. Vicat is led to conclude from this experiment, that it is not correct to assimilate what passes in an intimate mixture of silica, alumina, and oxyde of iron, submitted to a weak calcination, with what takes place when the same oxydes are calcined separately. The question, then, remains entire; and the following is succinctly the result of the experiments which I have tried with a view to resolve it, and which every body can easily repeat.

If the clays to which sands or gravels owe their puzzolanic properties be separated by washing, and the most energetic and the least so are chosen; if the same operation be performed on certain argillaceous sands *of the colour of dark wine-grounds*, which have not, as puzzolanas, any but negative properties; if to these samples are joined pure clays, namely, free from sand, and more or less ochreous, such as are found abundantly every where; if each of these clays, dried in the air and powdered, be mixed with half its volume of fat hydrate of lime, and the different mortars thus obtained be immersed, in the consistence of a firm paste, we shall be naturally induced to divide the clays made use of into three classes, calling them:

Good puzzolana clays, those which shall have given mortars which, at the expiration of ten or fifteen days at most, shall resist the strongest pressure of the finger without receiving any impression, or which, loaded on a surface of 0,00005 metre with a weight of 2 kilogrammes, experience no perceptible depression;

Middling puzzolana clays, those for which it is necessary to wait a month or six weeks for a similar result;

Lastly, *null puzzolana clays*, those which give mortars remaining soft indefinitely, and which the finger penetrates with ease. I remark that I have found clays of this class only among earths strongly ochreous and of the colour of wine-lees, of which I have spoken above; but others may exist.

In the first place, I should remark that, in the first class are comprised, not only the clays extracted from the *energetic sands*, of which I have spoken in my first note, but also the *yellowish brown* and other clays which are met with in nature without mixture of sand. As much may be said of the second class, and hence it must be concluded that the *clays extracted from sand or gravel do not possess peculiar properties*, but that these properties are common to them in the same degree with many other clays of different colours; and that the properties of sands (*arenes*) have been remarked on account of the mixture of siliceous fragments only, which, being found ready made, renders these properties much more striking, as I shall explain elsewhere.

Having classed, by these first trials, the substances which I intended to examine, I submitted them, *in a state of dust*, to very nearly a dull red heat for fifteen minutes only, in an open crucible. The following are the phenomena which appeared: clays of the two classes speedily underwent a sort of ebullition; at the same time their colour changed rapidly, passing from yellowish red, yellow, yellowish brown, &c. to deep brown red, to bright red, to blackish red, &c.; and weighing the substance with care before and after this operation, I found that these calcined clays had lost variable quantities of their weights, amounting in some to a fifth part of their original weight.

Clays of the last class, on the contrary, did not perceptibly change colour, nor lose generally above two or three hundredths of their weight.

By forming mortars with clays thus prepared, in the same proportions and with the same lime that I had employed for trying the natural clays, I found, first, that clays of the first class all became, without exception, excellent puzzolanas, in other words, the mortars obtained as above-mentioned, and immersed, acquired in the course of two days sufficient consistency to resist completely an impression from the finger; that these mortars tried at the end of fifteen days by the penetration of a point, exhibited a degree of hardness equal to that of mortars of the same raw clay at the end of *four months*; and that by pursuing this comparison till a more distant period, the progress of the calcined clay mortars being afterward slower than that of the mortars of raw clay, there was no longer any appreciable difference, at the end of a year, between them, it being understood that care was taken first to scrape the surfaces in contact with the water to the depth of *one or two centimetres* (=.4 to 0.8 inch.), an operation, the necessity of which I have remarked in my first note inserted in the *Annales* on this subject. Secondly, that clays of the second class exhibited nearly the same phenomena, with this difference, that puzzolanas obtained by calcination were generally less energetic, and afforded mortars less hard than the preceding. There was besides a much greater difference between these mortars and those of raw clay, than for clays of the first class. The latter required more than eight months to attain the degree of hardness, which the others acquired in fifteen days. Thirdly, clays of the third class did not appear to have gained any thing, or gained very little by calcination, and gave mortars which remained constantly soft under water as before.

Although it appeared to me very probable, that the effect of a calcination so slight and of so short a continuance as that to which I had submitted the different clays, could only have been to decompose a hydrate, and that the remarkable diminution of the weight of the substance, the kind of ebullition and remarkable change of colour must be attributed to the disengagement of water retained in combination: yet I thought it necessary to satisfy myself directly that there was neither a disengagement nor an absorption of gas in that operation. To do this, I distilled a determined quantity of clay of the first class, in the state of dust, in a retort connected with a balloon by a tube; I found it sufficient to keep the retort at nearly a dull red heat for fifteen or twenty minutes. The change of colour took place as when exposed to the air, and aqueous vapour condensed in the balloon in drops. When the apparatus had cooled, and the water collected together, the weight of the calcined clay and the weight of the water were together almost exactly equal to the weight of the clay employed*.

It may then be considered as demonstrated, at least for all clays more or less ochreous, the only ones I have had at hand, and which are certainly diffused in very great quantity in nature, that the only effect of a light calcination, sufficient to change them into the state of excellent puzzolanas, is to decompose, at least in great measure, the hydrate formed by the different oxydes which compose the clay in the natural state.

This conclusion was, it must be confessed, strikingly in agreement with the opinion of Messrs. John and Berthier, since it is very evident that, by the decomposition of the hydrate, the absorbent property was considerably augmented in the product; but it remained to explain why certain clays were only in different puzzolanas, and why others were null, as after calcination.

I thought to find, in the chemical analysis, the solution of this problem; but I arrived at results which were insufficient. In the first place, among the clays of the first and second classes some were found composed, as it were, of the same quantity of the same elements. Afterwards the clays, which I have named *null puzzolanas*, contain generally more than 80 parts in 100 of silica, very little alumina, and a great deal of oxyde of iron. Must then the development of puzzolanic properties be attributed solely to the presence of alumina and to calcination? This fact would have been in direct opposition to the experiments of M. Vicat, which I have mentioned above.

Thus I found myself placed in the midst of the same uncertainties, that had accompanied the researches of those, who had engaged in the same inquiry before me. At length it occurred to me to form mortars with the elements of each clay separately, and to compare the results. For this purpose, I separated the various natural clays, which I had tried, into *silica*, on the one hand, and into *alumina and oxyde of iron*, on the other, by means of hydro-chloric (muriatic) acid and ammonia. I washed the residues on the filter carefully, and after drying them slowly in the sunshine, or over a gentle fire, avoiding every thing that could be considered a calcination, I mixed them in equal portions with fat hydrate of lime, and immersed the mortars. It had been better to employ one-half of hydrate of lime, as in the preceding experiments, but I preferred the other proportion, because I operated on small quantities of each substance.

Here follow the results which I was soon enabled to perceive. 1. All the mortars, with the silica of the clays of the first class, had solidified at the end of six-and-thirty hours, so that the strongest pressure of the finger did not even make the slightest inequalities of the surface disappear. At the end of eight days, these mortars had acquired a great consistency, and by submitting them to proof, by means of penetration with a point, I found *that* consistency superior to that of mortars of calcined clay, after fifteen days of immersion. 2. The mortars formed with the silica, extracted from clays of the second class, acquired a less considerable hardness than the preceding. 3. In fine, those obtained with the silica, extracted from clays of the third class, remained soft indefinitely. 4. Mortars formed in each class, with the compound of alumina and oxyde of iron slightly dried, and which consequently still retained a considerable quantity of water, became solid after an immersion of fifteen or twenty hours at most. I do not think that any puzzolana exists more rapidly energetical, and I have not remarked any perceptible difference, although the compound of alumina and oxyde of iron varied considerably in the proportions. Nevertheless, it is important to remark, that this rapid consolidation is not followed by a corresponding progress, and that after eight or fifteen days the mortars of silaca of the first two classes are harder than these. 5. Lastly, the mortars formed by all the elements of the clay at once, offer nearly the same results, as those of silica, for the first two classes. However, and I insist on this point, these elements contain then at least as much water as the raw clay, and are not more *absorbent*: they form, nevertheless, excellent puzzolanas.

Several important conclusions may, I think, be deduced from these facts. They prove, first, that clays differ from one another generically in the state in which the silica is found; secondly, that the presence of water, more or less com-

* When clay contains carbonate of lime or vegetable substances, gases are indeed disengaged, but always in small quantity. This disengagement has not besides any relation with the calcination of puzzolanas purely argillaceous.

bined with the elements of the clay, does not impair the puzzolanic properties, which appear especially to result from the state of *isolation* in which its elements are placed. I do not pretend, however, to establish that the silica can take a great number of different states, but rather that they are reduced to two, namely, that in which it is combined with the other oxydes, and that wherein it has been originally free, and in which its molecules have been of a nature to yield to cohesion, and to agglomerate. It is sufficient to suppose, then, that, in clays of the first class, the silica is very principally found in the first state, which is favourable to its combination with lime; that, in clays of the second class, the part of the silica, which is found in the first state, is less considerable; and that, lastly, in clays of the last class, it is null, or at least very inferior to the part which is met with in it in the free state.

I shall remark, that the clays of the first two classes being strongly *hydrated*, it may be regarded as almost certain, that the part of the silica, in combination with the alumina and oxyde of iron in the clays, is found united with these two bodies *in the state of hydrate.*

Now, I have proved above, that the only effect of a slight calcination on hydrated clays, was the decomposition of the hydrate; and the facts which I have just stated prove that it is sufficient that the silica, and the compound of alumina and oxyde of iron cease to be in combination, in order that the body shall become an excellent puzzolana. I think, therefore, that I proceed on sure ground in explaining the development of the puzzolanic properties in clays by a slight calcination, by this simple hypothesis alone, that the *hydrate* being decomposed by heat, *the silica is found to b separated by that means from the combination*, and that things take place then in the mortar of calcined clay, as in that wher in all the elements of the raw clay were united, after having been separated by chemical analysis.

A fact, for me very unexpected, will complete the proof of this theory, already very sufficiently demonstrated by what precedes. I have said that a slight calcination was insufficient for converting clays of the third class into the state of puzzolana, even of a middling kind, and I had therefore believed that the effect of that calcination was null, or almost so, on the elements of those clays; but, in order to leave nothing to chance, I analyzed one of those clays, combined as I have before explained, and I was greatly surprised on forming a mortar with the silica which I had so procured, to see that it had become solid more speedily and completely after its immersion, than the mortars obtained with the silica of the raw clays of the first class. There is but one method of explaining this sufficiently remarkable chemical fact, which is, by admitting that, by the assistance of heat, the oxyde of iron is made to enter into combination with the silica, effecting by that means the division of the agglomerated particles of the latter. This fact is, moreover, quite similar to that observed with silica and with lime, and remarked by Descotils. But what is important to remark is, that the clay thus converted, and in which the elements are found combined *by the dry way*, and are separately fit for making excellent puzzolanas, is, notwithstanding, still but a *null puzzolana.* Thus, it remains demonstrated, that the condition necessary to the existence of a good argillaceous puzzolana, is, *that the silica shall be found isolated* from the other oxydes, and, *nevertheless, in a fit state to form new combinations.*

It hence appears very evident, that certain clays, *strongly hydrated*, are good puzzolanas in the natural state, only because the combinations of the oxydes, in the state of hydrate, can be readily dissolved, in order to give place to new combinations determined by the presence of hydrate of lime: it is therefore probable, that the silica combines separately with a part of the lime, and that the rest unites with the alumina and the oxyde of iron. It is, besides, an opinion generally admitted, that when several bodies, such as silica, alumina, oxyde of iron, lime, and water are found in presence of each other, they do not all combine together, but rather two and two, or three and three. Hence, as the decomposition of the natural hydrate ought to precede the consolidation of the mortar, it is not surprising that this consolidation should make less rapid progress in raw than in calcined clay, in which the decomposition of the hydrate is done ready.

It may be asked what part the oxyde of iron acts in puzzolanas, and whether its presence is useful. I believe that the answer to this question is found in the approximation of the two following facts:—M. Vicat has observed that pure alumina, calcined or not, was but a very indifferent puzzolana; and I have found that the compound of alumina and oxyde of iron, separated by analysis from the different clays, is, on the contrary, a puzzolana rapidly energetical, and, in this respect, very valuable; I am inclined to believe, also, that the presence of the oxyde of iron promotes the decomposition *of the natural hydrates*. This is a point which comparative experiments, undertaken at the same time on white and on coloured clays, would soon have cleared up.

Science is still in want of a good monography of clays, which, however, would be useful to the numerous arts in which clays are employed. The earthy compounds to which this name is given appear to be formed in a very variable manner. Chance has greatly assisted me by offering two of their most important modifications in the *hydrated clays*, rich, nevertheless, in silica, and in the clays which are not hydrated. I have found but a small number of the latter, but I can point out where they are to be found, and show specimens of them.

The preceding considerations appear to me to contain the *rational theory* of argillaceous puzzolanas, which may be stated very generally as follows.—*The consolidation of puzzolana-mortars under water, depends on the combination which takes place between the lime and the silica, on the one hand, and between the lime, alumina and oxyde of iron, on the other. It is known, moreover, from direct experiment, that these two combinations possess very rapidly the property of hardness under water, or, what is tantamount, of forming a solid hydrate in determinate proporions.*

More than one useful application of the experiments above mentioned may be made in a large way. First, since fifteen minutes of a heat not exceeding a dull red are sufficient to effect the conversion of *hydrated clays* into excellent puzzolanas when these clays are in the *state of powder*, I believe there would be generally a vast economy in preparing artificial puzzolanas in this manner *in open air*, as recommended with reason by General Treussart: thus we should avoid having to pulverize it, and we should abridge at least nine-tenths of the time and expenses of calcination; in a word, we should thus calcine all clays as readily as sands *(arenes)* themselves are calcined*. Might we not also render useful as puzzolanas the residues of the alum manufactories, which must be rich in excellent silica ready prepared, and the price of which is little or nothing? I give these ideas to those who, like myself, may have more than one occasion to make a useful application.

II.—*On Boring for Water.*

[From the Encyclopædia Metropolitana, Art. BORING.]

The practice of boring for water, and the frequent success that has lately attended the operation, in producing a great supply without the actual sinking of a well, render the subject one of great importance; we conceive, therefore, that our readers will be gratified with the following description of the process, for which we are indebted to the London Journal of Science, XXXIII.

The situation of the intended well being determined on, a circular hole is generally dug in the ground, about six or eight feet deep, and five or six feet wide. In the centre of this hole, the boring is carried on by two workmen, assisted by a labourer above, as shown in the plate, Fig. 1. The handle, Fig. 2. having a female screw in the bottom of its iron shank, and a ring at top, is the general agent to which all the boring implements are to be attached. A chisel, Fig. 3. is first employed, and connected to this handle by its screw at top. If the ground is tolerably soft, the weight of the two workmen bearing upon the cross bar, and occasionally

* I have executed these calcinations in a large way, in a manner as convenient as it is rapid, in small furnaces, supporting a kind of evaporating basin, the bottom of which, in strong sheet iron, was kept at a temperature near to a dull red, by the reversed flame of a fire-place suitably disposed. I shall give elsewhere a detailed description of this apparatus.

" Puzzolana, a kind of substance formed of volcanic ashes, more or less compacted together, and so called from Puzzuolo, and pulvis Puteolanus, from Puteoli, situated near Mount Vesuvius, from which these ashes are ejected, and in the vicinity of which they abound."—*Rees's Cyclopedia.*

forcing it round, will soon cause the chisel to penetrate; but if the ground is hard or strong, the workmen strike the chisel down with repeated blows, so as to peck their way, often changing their situation by walking round, which breaks the stones, or other hard substances, that may happen to obtruct its progress.

The labour is very considerably reduced by means of an elastic wooden pole, placed horizontally over the well, from which a chain is brought down and attached to the ring of the handle. This pole is usually made fast at one end as a fulcrum, by being set into a heap of heavy loose stones; at the other end the labourer gives it a slight up and down vibrating motion, corresponding to the beating motion of the workmen below, by which means the elasticity of the pole, in rising, lifts the handle and pecker, and thereby very considerably diminishes the labour of the workmen.

When the hole has been thus opened by a chisel, as far as its length will permit, the chisel is withdrawn, and a sort of cylindrical auger, Fig. 4, is attached to the handle, Fig. 2. for the purpose of drawing up the dirt or broken stones which have been disturbed by the chisel. A section of this auger is shown in Fig. 5. by which the internal valve will be seen. The auger being introduced into the hole, and turned round by the workmen; the dirt or broken stones will pass through the aperture at bottom (shown at Fig. 6,) and fill the cylinder, which is then drawn up, and discharged at the top of the auger, the valve preventing its escape at bottom.

In order to penetrate deeper into the ground, an iron rod, as in Fig. 7, is now to be attached to the chisel, Fig. 3, by screwing it to its upper end, and the rod is also fastened to the handle, Fig. 2, by screwing into its socket. The chisel having thus become lengthened, by the addition of the rod, it is again introduced into the hole, and the operation of pecking or forcing it down, is carried on by the workmen as before. When the ground has been thus perforated, as far as the chisel and its rod will reach, they must be withdrawn, in order again to introduce the auger, Fig. 4, to collect and bring up the rubbish, which is done by attaching it to the iron rod, in place of the chisel. Thus, as the hole becomes deepened, other lengths of iron rods are added, by connecting them together, as in b, Fig. 8. The necessity of frequently withdrawing the rods from the hole, in order to collect the mud, stones, or rubbish, and the great friction produced by the rubbing of the tools against its sides, as well as the lengths of the rods, augmenting in the progress of the operation, sometimes to the extent of several hundred feet, render it extremely inconvenient, if not impossible, to raise them by hand. A tripedal standard is therefore generally constructed, by three scaffolding poles tied together, over the hole, as shown in the plate, from the centre of which a wheel and axle, or a pair of pulley blocks are suspended, for the purpose of hauling up the rods, and from which hangs the fork; Fig. 9. This fork is to be brought down under the shoulder, near the top of each rod, and made fast to it by passing a pin through two little holes in the claws. The rods are thus drawn up, about seven feet at a time, which is the usual distance between each joint, and at every haul a fork, Fig. 10, is laid horizontally over the hole, with the shoulders of the lower rod resting between its claws, by which means the rods are prevented from sinking down into the bore again, while the upper length is unscrewed, and removed. In attaching and detaching these lengths of rod, a wrench, Fig. 11, is employed, by which they are turned round, and the screws forced up to their firm bearing.

The boring is sometimes performed for the first sixty or a hundred feet, by a chisel of two and a half inches wide, and cleared out by a gouge of two and a quarter diameter, and then the hole is widened by a tool, such as is shown at Fig. 12. This is merely a chisel, as Fig. 3, four inches wide, but with a guide put on at its lower part, for the purpose of keeping it in a perpendicular direction; the lower part is not intended to peck, but to pass down the hole previously made, while the sides of the chisel operate in enlarging the hole to four inches. The process, however, is generally performed at one operation by a chisel of four inches wide, as Fig. 3, and a gouge of three inches and three quarters, as at Fig. 4.

It is obvious, that placing and displacing the lengths of rod, which is done every time that the auger is required to be introduced or withdrawn, must of itself be extremely troublesome, independent of the labour of boring; but yet the operation proceeds, when no unpropitious circumstances attend it, with a facility almost incredible. Sometimes, however, rocks intercept the way, which require great labour to penetrate; but this is always effected by pecking, which slowly pulverizes the stone.

The most unpleasant circumstance attendant upon this business, is the occasional breaking of a rod into the bole, which sometimes creates a delay of many days, and an incalculable labour in drawing up the lower portion.

When the water is obtained in such quantities and of such quality as may be required, the hole is dressed or finished by passing down it the diamond chisel, Fig. 13; this is to make the sides smooth previous to putting in the pipe. This chisel is attached to rods and to the handle, as before described, and in its descent, the workmen continually walk round, by which the hole is made smooth and cylindrical. In the progress of the boring, frequent veins of water are passed through; but as these are small streams, and perhaps impregnated with mineral substances, the operation is carried on until an aperture is made into a main spring, which will sometimes flow up to the surface of the earth. This must of course depend upon the level of its source, which, if in a neighbouring hill, will frequently cause the water to rise up, and produce a constant fountain. But if the altitude of the distant spring happens to be below the level of the surface of the ground, where the boring is effected, it sometimes happens that a well of considerable capacity is obliged to be dug down to that level in order to form a reservoir, into which the water may flow, and from whence it must be raised by a pump; while in the former instance, a continued fountain may be obtained. Hence, it will always be a matter of doubt, in level countries, whether water can be procured which will flow near to or over the surface; if this cannot be effected, the process of boring will be of little or no advantage, except as an experiment to ascertain the fact.

In order to keep the water pure and uncontaminated iwth mineral springs, the hole is cased for a considerable depth with a metallic pipe, about a quarter of an inch smaller than the bore. This is generally made of tin, (though sometimes of copper or lead,) in convenient lengths; and as each length is let down, it is held by a shoulder resting in a fork, while another length is soldered to it; by which means a continued pipe is carried through the pipe, as far as may be found necessary, to exclude land springs, and to prevent loose earth or sand from falling in and choking the aperture.

III.—*Miscellaneous Notices.*

1.—*New Oxyde of Iron.*—M. Berthier considers the scales which fall at the smith's anvil as a new oxyde of iron. They do not unite with acids simply, but are resolved into protoxyde and peroxyde, which then form salts. They are not constant in their composition; but, when most so, consist of iron 100, oxygen 34.2: hence they are the deutoxide.—*Dub. Phil. Journ.* I. 281.

2.—*Manner of Bronzing Statues, Medals, and Ornaments, made of Copper or Bronze.*—The receipts for communicating to newly cast bronze a colour which gives it the appearance of old bronze, vary more or less. We shall here give the method employed by Jacob, one of the best artists of Paris. Take two gross of sal ammoniac, half a gross of salt of sorrel, which dissolve in a demi-setier (400 grammes) of white vinegar: after cleaning the metal well, dip a pencil slightly in the solution, and rub it continually on the same place, until the colour is dry, and the tint has acquired the desired intensity. That the drying may take place more quickly, this operation is performed by exposing the object to the sun or in a stove. The bronze colour becomes deeper, in proportion to the length of time occupied in passing the brush over the same place.—*Journ. des Conaiss. Usuel. et Pratiq.* 1827.

3.—*Method of discovering Potassa by the Blow-pipe Flame.*—M. Harkort, of Freyberg says, that, in consequence of an observation made by Kirwan, namely, that oxyde of nickel with potash, gave a blue glass before the blow-pipe, whilst soda with the same oxide produced a brown glass, he was led to examine whether the distinction might not be made to afford a useful test. On making the experiment with potash, he obtained an excellent result; the blue produced is not likely to be confounded with that produced by cobalt, because it inclines to a milky appearance. So sensible is this test, that the presence of potash was readily discovered in the *periclinite* (a new variety of felspar, distinguished by Professor Breithaupt,) although existing there in very small quantity. The experiment relative to soda was not so successful, the glass acquiring only a weak brown colour.—*Jahr. der Chem.* 1827.

4.—*Use of Chloride of Lime in Gardening.*—Chloride of lime, it is said, will destroy insects on trees, and prevent effluvia from arising from vegetables or other matter in a state of decomposition. It was used in France to preserve the bodies of those unfortunate persons who had destroyed themselves, until owned by their friends. A body, if washed with the preparation, will keep for weeks without alteration or offensive odour. Timber washed with it will be preserved from the effects of damp and confined air, and it would also prevent the spread of dry-rot, and destroy insects on plants.—*Gard. Mag.* III. 354.

5.—*Succession of Crops.*—The alternation or succession of crops, found to be of so much importance both in gardening and agriculture, has been proved to be a general law of nature by M. Dureau de la Malle, in the *Ann. des Scienc. Nat.*, tom. v. p. 553., Août, 1825. The facts which he brings forward are perfectly consistent with the experience and observation of various naturalists and cultivators. The botanist Ray observed, after the great fire of London in 1666, that Sisymbrium Irio sprung up among the ruins, where it had never been seen before; and Professor Pallas, in the end of last century, observed in Russia that, when pine forests were destroyed by fire, they were not succeeded by the pine or fir tribe, but by wild service, birch, lime, poplar, and analogous trees. Dr. Dwight also, in the beginning of the present century, found that his grandfather's field near Northampton in Pennsylvania, which a century before was covered with oaks and chestnuts, after being under the plough for two generations, and then left to itself, brought forth a thick grove of white pines, without a single oak or chestnut tree. From the various instances, both of herbaceous and ligneous vegetables, adduced by M. Dureau, he concludes that the succession of plants is a fundamental law of vegetation in a state of nature; and that its imitation by art, in our fields, gardens, and woods, is of the first importance.—*Gard. Mag.* III. 208.

6.—*On the Preparation of artificial Ultramarine.*—M. Gmelin, of Tubingen, has published the following process, (which he says succeeds infallibly,) for preparing ultramarine. Procure hydrate of silica and of alumina, the first, by melting together well-pulverized quartz with four times as much carbonate of potash, and by dissolving the melted mass in water, and precipitating by muriatic acid; the second, by precipitating a solution of pure alum by ammonia. These two earths should be carefully washed with boiling water. After that, determine the quantity of dry earth which remains, after having heated a certain quantity of the moist earth to redness. The hydrate of silica which I made use of in my experiments contained 56 parts in 100, and the hydrate of alumina 3.24 parts of anhydrous earth.

Afterwards dissolve with heat, in a solution of caustic soda, as much of that hydrate of silica as it will dissolve, and determine the quantity of earth dissolved. Then take for 72 parts of this anhydrous silica, a quantity of hydrate of alomina containing 70 parts of dry alumina. Add to it the solution of silica, and evaporate the whole together, constantly stirring the mixture, till there remains only a moist powder.

This combination of silica, alumina, and soda, is the basis of the ultramarine, which is now to be coloured by sulphuret of sodium, which is done in the following manner.

Into a Hesian crucible furnished with a cover fitting close, put a mixture of two parts of sulphur and one of an hydrous carbonate of soda. Heat gradually till, at a dull red heat, the mass is well melted; then project this mixture, in very small portions at once, into the midst of the melted mass. As soon as the effervescence, owing to the vapour of the water, ceases, throw in a fresh portion. Having kept the crucible for an hour moderately red hot, remove it from the fire, and let it cool. If there is an excess of sulphur, drive it off by a moderate heat. In case all parts of the ultramarine are not coloured equally, the finest parts may be separated, after reducing them to very fine powder, by washing with water.—*Ann. de Chimie*, xxxvii. 409.

7.—*Height of Mont Blanc.*—The height of Mont Blanc and of the Lake of Geneva has lately been carefully ascertained by M. Roger, an officer of engineers in the service of the Swiss Confederation. The summit of the mountain appears to be 4435 metres, or 14,542 English feet above the lake of Geneva, and the surface of the lake 367 metres, or 1233 English feet above the sea. The mountain is, therefore, 15,775 feet above the level of the sea.—*Quart. Jour. Sc.* iii. N. S. 502.

ERRATUM in No. 3. P. 92. l. 15, *for* union, *read* unringed.

GLEANINGS

IN

SCIENCE.

No. 5.—May, 1829.

I.—*Results of a Series of Experiments on the Elasticity and transverse Strength of different Kinds of Timber. By Captain* H. C. Baker, *Superintendent of Suspension Chain Bridges.*

The experiments of which the results only are below recorded were conducted, as nearly as circumstances admitted, with similar apparatus to that used by Mr. Barlow, and described in his Treatise on the Strength and Stress of Timber. The extreme precision aimed at by Mr. Barlow, for the purpose of establishing a particular theory, was neither attainable with the means I had at command now, nor, had it been so, would it, after Mr. Barlow's researches had so satisfactorily established the accuracy of his premises, have been attended with any consequences at all commensurate to the difficulty and labour necessary to secure it.

The specimens, many of them, were obtained from the Commissariat Timber Depôt, some very fine ones from the Cossipore Gun Carriage Agency, and others were the produce of the Calcutta market; those of three and two feet in length were, generally speaking, cut from the sound fragments of the larger specimens.

The central deflections of the seven and six feet specimens, were usually noted immediately after the application of each 50 lbs. of fresh load, at which time, the set taken by each appeared more regular than after some interval had elapsed; the first deflections were commonly recorded with 150 lbs. weight, in some cases with only 50 lbs.; sometimes the specimen was turned upon the tressels and subjected to two or more trials, and in a few instances the load was left suspended for many hours, deflections being noted at different intervals of time.

The depth of the neutral axis was occasionally observed, but this has been found to vary so very little (being generally 5-8ths of the depth) as to render its insertion in the table of results of little practical utility.

The direct cohesive strength of the woods having also been subjected to experiment, the calculation of it from Mr. Barlow's formula has not been thought necessary; but the curious in such investigations are here presented with abundant materials for pursuing the inquiry.

It is much to be regretted, that from the circumstances under which most of the specimens were obtained, so little accurate information respecting the timber could be procured. The age, size, time of felling, and circumstances under which the trees had been respectively placed subsequently to their being cut down, would all have materially enhanced the value of this statement of results to the man of research, but unluckily no such information was accessible; indeed, in few cases was it even precisely known from what part of the timber the specimen itself was cut out, or what number were off the same trunk. The year of importation, where known, has been however inserted.

The following were the woods experimented on:

Saul*. *Shorea Robusta.* Rox. This timber is too well known to render a lengthened description of it requisite, but its value for building purposes does not yet

* The Saul and Sissooh are obtained from the forests north of the Ganges line, between the 25th and 31st N. Lat. 74th and 88th E. Long. but chiefly from the tract lying north of Poorneah and Goruckpore.

yet seem to have been fully appreciated. The great mechanical resistance it affords in cases of strain, however applied, render it unquestionably the most valuable of Indian timber yet generally known for engineering purposes. The regularity of its deflections is indeed such as to render calculations of the requisite scantling for any particular stress at all times simple and sure.

The general appearance of its fracture beautifully illustrates Mr. Barlow's theory of the axis of motion, or rotation being centrically situated, the upper or compressed fibres being smooth as though cut with a sharp knife, those in a state of tension, so fine and intimately blended as to resemble those of hemp rope when violently torn asunder.

The saul of the Calcutta market is seldom above 30 feet in length, but the trees grow to a much greater height. From the injudicious practice of squaring it, after felling, its mean girt is only about six to seven feet, but must be naturally much greater.

Saul has lately been very successfully substituted for teak, in many of the component parts of the gun carriages, cheeks, beams, and transoms, poles, and framing of gun and ammunition boxes; occasionally spokes, naves, and fellies Its toughness in cases of percussion must obviously render it a much safer material than teak to be near in action. It has also been used for door and window pannels. It however shrinks more from its greater density.

Soondree. *Heritiera Minor.* Rox. An aquatic timber from the Soondurbuns. A very tough and elastic wood, commonly used for boats, boat masts, poles, buggy shafts, and the spokes of wheels; but it is a very perishable wood, and shrinks a good deal in seasoning.

Teak.—To both the above woods, the teak *(Tectona grandis)* in point of strength and elasticity is decidedly inferior; its brittleness renders it indeed rather hazardous to stand near the specimens when subjected to heavy strain, as the pieces, sometimes several of them, fly with considerable impetus in different directions.

A reference to the tables will show that, of a great number of specimens tried, three only, 80, 81, 88, at all approximate in results to those of Mr. Barlow: the mean S. of my experiments is about 2078; of Mr. Barlow's 2462, a number which exceeds that of the saul tried by me. Mr. Barlow's specimens must therefore have been vastly superior to any in common use throughout India, for the comparative superiority of saul in point of strength is, I believe, indisputable.

The teak grows to a great height, 70 to 80 feet, and more, but cannot be easily obtained good of that size, the heart being frequently very much decayed. It is a durable wood, when exposed and not subject to the depredations of white ants, until it has been very long in use. The uses to which teak is applied are too generally known to render much remark necessary: planking, boxes, pannelling, doors, windows, venetians, furniture, beams of houses, are amongst the most common.

Sissooh*. *Dalbergia Sissooh.* Rox.

In structure somewhat resembles the finer species of teak, but it is tougher and more elastic; the Sissooh grows to the height of about 30 feet, but it is generally rather crooked, and therefore not so well adapted for beams; Sissooh is said to get harder with age.

It is by the natives employed for house furniture, beams, cheeks, spokes, naves, and spokes and fellies of wheels, keels and frames of boats, blocks, printing presses, and generally in all work where crooked timber is required.

Jarool *red.* A fine even wood in structure, and grows to great size in the Chittagong district, but that brought to the Calcutta market is too small to be of much use, except for picture frames, and other similar purposes.

The Chittagong forests are said to be nearly cleared of the best, or thorny species of Jarool, the others are of little value; it is considered a treacherous wood in ship building.

Gomar or Gumbhar Wood. Specific gravity 500; a light and easily frangible wood, produced both in the Morung and Chittagong forests; the latter of a pale straw colour, the other darker, much heavier, and to appearance finer, but not stronger. This wood (though not to be trusted in stress of any kind) is well calculated for light planking, pannelling, blinds and venetians, and is in much estimation for picture frames, organ pipes, sounding boards, and other such work where shrinkage is to be avoided.

* See note, preceding page.

PUSSOOH or Pussur. A jungle wood of a deep purple colour; specific gravity about 800; extremely dry, brittle, and liable to warp. Native boats built of the best species of Pussooh, are said to last about three years; when of good quality it stands against the brackish water better than Saul.

SATIN WOOD. Of a bright yellow colour; the produce of Ceylon.

BLACK ROSE. A superior species of Sissooh; from the Malabar Coast.

TOON. *Cedrela Toona.* Chittagong.

MANGO WOOD. *Mangifera Indica.* Common Indian fruit tree.

JUNGLE WOOD.

TRINCOMALEE WOOD. A neat looking, even wood, in structure like mahogany; would probably make good furniture.

In the following tables, it is necessary to bear in recollection, that the letters represent the following quantities.

l The length in inches.
a The breadth or thickness.
d The depth.
W The breaking weight.
Δ The last deflection.
W′ Greatest weight whilst the elasticity continued unimpaired.
δ Deflection ditto ditto.

The Specimens were all as far as No. 250, 84 inches in length, and 2 inches square, with a distance between the supports of 72 inches, excepting Nos. 110, 111, 112 and 113, which were 66 inches between the supports.

Those from No. 250 to the end were 72 inches in length, 2 inches square, and 66 inches between the supports.

TABLE I.

Number of the experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect. Weight in lbs.	Greatest weight and deflection while the elasticity remained perfect. Deflection in inches.	Breaking weight lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of E from the formula, $E=\frac{l^3W'}{ad^3\delta}$	Value of S from the formula, $S=\frac{lW}{4ad^2}$
	MORUNG SAUL.								
6	Imported 1825,	905	450	.925	1050	3.0			
7	Chowker*, cut from	884	450	1.00	1170	4.8			
8	the side.	905	450	1.05	1110	3.7			
9	Chowker, 2d size,	935	450	1.4	990	4.6			
10	clear of the heart,	941	450	1.5	1068	3.5			
11	1825.	930	450	1.2	1170	4.7			
12		915	450	1.05	1170	4.6			
13		1007	450	.9	1158	5.0			
	Mean of the 8 results	928	450	1.128	1121	4.34	602	9306382	2522
31	Imported 1822,	1059	450	1.2	990	4.0			
32	Neem Chowker,	1049	450	1.15	930	3.0			
33	from near the out-	1059	450	1.1	1104	4.0			
34	side.	1049	450	1.25	990	3.9			
	Mean results	1054	450	1.175	1003	3.72	696	8934121	2267

* Chowker is a large tree squared on all four sides; Dowker of smaller growth, and squared only on two sides.

Number of experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect. Weight in lbs.	Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of E from the formula, $E=\frac{l^3W'}{aa^3c}$	Value of S from the formula, $S=\frac{lW.}{4ad^2}$
53	MORUNG SAUL.	894	450	1.5	990	3.7			
54	1814, Chowker,	966	450	.9	1442	3.9			
55	seasoned prime, in Cossipore Gun-carriage yard.	925	450	.95	1146	2.9			
	Mean results	928	450	1.116	1192	3.50	740	9406451	2684
65	Beam of Tolly	1060	300	1.075	850	3.4			
66	Gunge Bridge built	1050	300	.9	850	3.2			
109	in 1812-13, rebuilt 1819-20.	1047	300	1.075	890	3.4			
	Mean results	1052	300	1.016	863	3.33	778	6888188	1942
110	Neem Saul Chow-	1050	450	1.075	750	3.4		Transferred to the 6 feet specimen.	
111	ker, sap-wood, im-	1052	450	1.525	950	3.4			
112	ported 1826, inferior	1040	450	1.425	850	3.7			
113	specimens.	1040	450	1.225	750	3.0			
	Mean results	1045	450	1.312	825	3.37	646	6162976	1700
14	Dowker, 1825,	935	450	1.0	1170	4.6			
15	clear of the heart;	827	450	1.6	858	3.8			
16	young timber.	925	450	1.05	1254	3.0			
51		853	450	1.5	996	4.6			
26		977	450	1.1	1152	3.4			
27		1054	450	1.35	930	3.6			
28		1007	450	1.1	900	2.75			
29		946	450	1.0	1230	4.4			
30		935	450	1.2	788	2.9			
	Mean results	934	450	1.21	1040	3.7	704	8675702	2340
2	1825, cut near the	853	300	.725	940	3.3			
3	heart, young.	833	300	1.0	918	4.7			
4		838	300	1.0	1008	4.2			
5		802	300	.9	918	3.3			
	Mean results	842	300	.906	946	3.87	670	7724503	2128
42	SOONDREE.	1022	500	1.35	1300	6.5			
43	Seasoned, cut from	1030	500	1.625	1350	5.2			
44	large plank, im-	1022	500	1.55	1338	4.7			
45	ported 1824.	1047	500	1.125	1260	5.8			
	Mean results	1030	500	1.362	1312	5.5	471	8075080	2952
21	TEAK.	683	300	.85	1020	3.9			
22	Duggy, Rangoon,	725	300	.95	654	2.1			
23	seasoned; suppos-	634	300	.95	912	3.6			
24	ed to have been im-	682	300	.90	816	2.75			
25	ported some years.	714	450	1.225	984	3.3			
	Mean results	687	330	1.006	87[illegible]	3.13	828	7652326	1964

Number of experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect. Weight in lbs.	Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of E from the formula, $E=\frac{l^3W'}{ad^3\delta}$	Value of S from the formula, $S=\frac{l W.}{4\ ad^2}$
56	From Bombay, re-	714	300	.9	1000	5.5			
57	ceived at Cossipore	683	450	1.5	750	5.15			
58	April 1825.	680	300	.95	900	5.1			
59		690	450	1.475	966	3.6			
60		683	300	1.1	800	4.9			
61		690	300	1.025	800	3.85			
	Mean results	690	550	1.158	869	4.68	553	7050777	1955
38	SISSOO.	683	300	1.1	792	3.9			
39	Inferior specimens, seasoned, cut near the root.	700	300	1.225	816	4.0			
	Mean results	691	300	1.162	804	3.95	668	6022719	1809
62	Received at Cossi-	730	300	1.05	1000	4.1			
63	pore yard 1819.	714	300	1.05	1240	7.0			
64		700	300	1.125	850	3.4			
	Mean results	714	300	1.675	1030	4.841	535	4178149	2317
49	PUSSOOH.	770	100	.8	810	4.2			
52	Large, 1825; cut near heart, prime.	880	100	.90	450	3.0			
	Mean results	825	100	.85	630	3.6	724	2744470	1417
47	Small 1826, near	700	100	.6	660	3.0			
48	heart.	710	100	.5	630	2.8			
	Mean results	705	100	.55	645	2.9	893	4241454	1451
	GOMAR.								
1	Imported 1826; in-	440	100	.4	509	6.05			
36	ferior specimens,	514	100	.8	450	4.0			
37	cut from side; from Sunderbuns.	444	100	.7	540	3.5			
	Mean results	466	100	.633	499	4.5	574	3685308	1124
250	MORUNG SAUL.	1028	500	1.0	1150	3.5			
*251	Chowker, 1st size,	1100	500	1.05	954	2.8			
90	imported 1826, old	1080	450	.925	1124	3 4			
253	timber, from the	1015	500	1.0	1150	2.7			
267	side; the specimens	1100	550	1.1	1210	3.3			
268	thus marked* were	1090	500	1.0	1124	3.2			
*296	of rather inferior	1110	500	1.225	974	4.0			
*297	quality, cut from	1100	500	1.375	1000	4.6			
*298	wood in which the	1100	450	.9	1012	3 5			
*299	sap predominated.	1080	500	.825	1096	3.6			

Number of experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect. Weight in lbs.	Deflection in ins.	Breaking weight in lbs.	Ultimate deflection in ins.	Value of U from the formula, $U=\frac{l^2}{d\,\Delta}$	Value of E from the formula, $U=\frac{l^3W}{aa^3\delta}$	Value of S from the formula, $S=\frac{lW}{4ad^2}$
254		1030	450	.925	1100	3.2			
255	Chowker, 2d size,	1040	450	.925	1100	3.1			
256	imported 1826,	1050	500	1 0	1100	3.2			
257	prime timber, cut	1080	500	1.0	1200	3.0			
339	clear of the heart.	1110	550	.9	1220	3.0			
340		1050	500	1.	1256	3.4			
341		1052	550	1.0	1192	3.0			
342		1050	500	1.025	1222	3.4			
343		1050	550	1.0	1258	3.0			
	Mean of the 20 results	1069	500	1.009	1134	3.3	660	8904112	2339
67	Seasoned, received	1008	500	1 36	1000	2.95			
68	at Cossipore in	950	500	1.075	1250	3.55			
69	1818.	1005	500	1 0	1250	3.6			
	Mean results	987	500	1.141	1166	3.37	646	7874014	2405
70	Seasoned, in store	1050	500	1.05	1000	4.2			
71	at Cossipore since	1005	500	.875	1100	2.6			
72	1826.	1000	400	1.0	1050	6.0			
	Mean results	1018	483	.975	1050	4.3	506	8901318	2165
350	A picked specimen, Gurreah Haut bridge, 1819-20.	1005	550	.925	1366	3.4	640	10683973	2817
269	Neem Chowker,	947	500	1.1	1050	3.0			
270	near the heart, not	940	500	.9	1050	3.5			
271	prime specimens,	947	500	.9	1250	3.5			
272	imported about	1000	500	.9	1350	4.2			
273	1825.	918	500	.975	1170	3.5			
274		918	500	.95	1174	4 0			
	Mean results	945	500	.954	1174	3.6	605	9417452	2421
50		1000	500		1072	3 0			
73		1005	550		1234	3.4			
264		1050	500	.925	1050	2.5			
265		1040	500	.9	1220	4.0			
266		1040	500	.9	1000	3.6			
246		1000	550	9	1000	3.0			
247		1010	500	.9	1210	3.0			
92	Young timber,	1000	500	2.1	1100	3.85			
96	Dowker, 1826, cut	1000	500	1.125	1000	2.7			
97	clear of the heart.	1015	500	1.025	1250	4.2			
100		1020	500	2 3	1200	4.5			
248		1000	550	1.1	1210	3 3			
249		1000	450	.8	1288	3.7			
330		1010	500	1.0	1256	3.4			
331		1000	450	.925	1184	3.4			
349		991	500	.9	1200	3.4			
	Mean results	1011	500	1.033	1155	3.4	640	8784213	2382

Number of experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect.		Breaking weigt in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of E from the formula, $E=\frac{l^3W'}{ad^3\delta}$	Value of S from the formula, $S=\frac{lW}{4\ ad^2}$
			Weight in lbs.	Deflection in inches.					
242	Batty, 1826.	1005	500	1.1	1122	4. 5			
243		1000	500	.90	1220	4. 6			
244		1000	500	.97½	1210	4.65			
336		1005	500	1.175	1138	4. 0			
337		947	500	1.2	1102	4. 2			
338		1005	500	1.125	1090	3. 2			
	Mean results	993	500	1.079	1147	4. 2	518	8326459	2366
	SOONDREE.								
74	*The two last inferior specimens,	1000	500	.9	1250	5. 5			
75	received at Cossipore Gun-carriage	991	500	1.325	800	3. 2			
76	agency 1825.	990	500	1.3	800	3. 2			
	Mean results	997	500	1.075	950	4. 0	544	8022336	1960
	TEAK.								
77	Pegue; seasoned,	750	450	1.6	800	3. 2			
78	received at Cossi-	742	500	1.05	1128	3. 2			
79	pore from 1825.	700	300	1.15	700	3. 5			
	Mean results	730	450	1.266	876	3. 3	660	6386907	1806
80	From Malabar; sea-	718	500	1.025	1100	3. 8			
81	soned, received at	720	500	1.05	1162	3. 5			
88	Cossipore in 1825.	736	500	1.275	1150	3. 0			
	Mean results	724	500	1.116	1137	3. 4	640	8050403	2345
89	Luzzar; seasoned	688	450	1.125	1050	3. 0			
102	1826.	688	500	1.525	1150	3. 8			
105		688	500	1.375	1100	4. 3			
	Mean results	688	400	1.341	1100	3. 7	588	6471875	2175
	TEAK.								
284	From Malabar; sea-	792	500	1.075	1120	3. 6			
285	soned 1826.	747	500	1.05	1036	2. 6			
286		792	500	1.1	850	2. 3			
	Mean results	743	500	1.075	1002	2.83	769	8357011	2066
	TEAK.								
344	Mug Gun-carriage	750	500	1.0	1036	4. 5			
345	plank; unseasoned, 1827.	736	500	1.0	950	2. 2			
	Mean results	743	500	1.05	993	3.35	650	8556428	2048

* See the results with Goruckpore Saul at the end of this Table.

Number of experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect. Weight in lbs.	Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of E from the formula, $U=\frac{l^3W'}{ad^3\delta}$	Value of S from the formula, $S=\frac{lW}{4ad^2}$
82	SISSOOH.	736	450	1.15	1174	4. 3			
83	Seasoned; Cossi-	710	450	1.55	900	3. 5			
84	pore 1819; very fine	740	500	1.125	1300	4. 8			
85	specimens.	736	350	1.4	1042	5. 0			
86		740	350	1.15	1050	5. 7			
87		683	300	1.025	1150	3. 0			
	Mean results	724	400	1.233	1102	4. 4	495	5829197	2272
91	FIR.	714	300	.85	750	2.65			
98		690	150	.425	750	2. 4			
	Mean results	702	225	.637	750	2.525	862	6346801	1547
346	GOMAR.	526	150	0.5	668	4. 3			
93	From side, 1826.	440	300	1.975	600	5. 5			
94		440	150	.85	550	4. 1			
107		400	150	.95	500	5. 4			
	Mean results	450	187	1.069	579	4. 8	453	3143226	1194
95	PUSSOOH.	780	300	1.75	750	3. 2			
101	Large, near the heart, 1826.	700	300	1.3	750	4. 0			
104	Small, 1826.	750	300	1.4	782	4. 7	463	3833280	1613
	Mean results	743	300	1.092	760	4. 0	544	4936401	1567
278	SATIN WOOD.	1061	450	.925	950	2. 8			
279	From Madras 1827,	1025	500	1.125	1116	2. 8			
280	where said to have been long in store.	1043	550	1.125	1180	3. 1			
	Mean results	1043	500	1.058	1082	2. 9	751	8491729	2219
281	BLACK ROSE.	875	300	.7	1240	3. 7			
282	A very superior	875	300	.7	1300	4. 1			
283	species of Sissoo.	875	300	.7	1050	3. 3			
	Mean results	875	300	.7	1196	3. 7	588	7700785	2466
275	TOON.	643	300	.75	920	5.			
276	From Chittagong.	553	300	.85	600	2. 9			
277		734	300	.85	880	5.			
	Mean results	643	300	.816	800	4. 3	506	6606066	1650
288	MANGO.	654	150	.45	674	3. 0			
289		656	150	.55	698	3. 0			
	Mean results	655	150	.5	686	3. 0	726	5390550	1415

Number of experiment.	Names of the woods.	Specific gravity.	Greatest weight and deflection while the elasticity remained perfect: Weight in lbs.	Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of E from the formula, $E\frac{l^3W'}{ad^3\zeta}$	Value of S from the formula, $S=\frac{lW}{4ad^2}$
293	JUNGLE WOOD,	797	300	1.15	6[illegible]6	3.5			
294		815	300	.875	874	3.4			
295		780	300	.70	1016	5.0			
	Mean results	797	300	.908	852	3.96	550	5930190	1757
290	TRINCOMALEE WOOD.	8[illegible]4	500	1.45	938	4.8			
291		785	3[illegible]0	.625	10[illegible]	4.5			
292		81[illegible]	300	.65	1050	5.3			
	Mean result	814	366	.908	1018	4.8	453	7242809	2099
259	GORUCKPORE SAUL.	1000	500	.975	1172	3.2			
260		1000	500	1.1	1076	3.5			
261	Imported 1823.	1015	5[illegible]0	1.0	1140	4.5			
99		99[illegible]	500	1.175	1304	4.2			
262		9[illegible]	500	1.0	1180	4.0			
263		1000	500	1.	1198	4.0			
347		1000	500	.9	1168	3.8			
348		99[illegible]	5[illegible]0	.8	1350	3.6			
106		91	500	1.25	1210	4.6			
258		1040	500	.975	1066	2.4			
	Mean results	99[illegible]	500	1.075	1238	4.1	544	8357011	2419

TABLE II.

Result of Experiments with 3 and 2 feet Specimens.

Number of Experiments.	Names of the woods and dimensions.	Specific gravity.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of S from the formula, $S=\frac{lW}{4ad^2}$.
232	SAUL.		720	1.7		
239	Specimens, 3 feet long.		744	1.3		
300	Distance of supports 33 inches.		600	1.15		
312			642	1.2		
313	Depth $1\frac{1}{2}$ inches.		612	1.4		
314	Breadth 1 inch.		750	1.5		
233			500	1.0		
236			750	1.8		
240			500	.875		
301			566	1.0		
302			852	1.525		
304			726	1.2		
305			630	1.025		
306			780	1.30		
307			780	1.45		

Number of Experiments.	Names of the woods and dimensions.	Specific gravity.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\,\Delta}$	Value of S from the formula, $S=\frac{lW}{4\,ad^2}$
308	SAUL.		660	1.45		
310	Specimens, 3 feet long.		774	1.4		
311	Distance of supports 33.		596	1.5		
234	Depth 1½ in.		400	.8		
235	Breadth 1.		624	1 3		
237			600	1.6		
238			750	1 65		
303			714	2 1		
309			642	1.0		
	Mean results		663	1.34	536	2431*
241	SOONDREE.		792	2.0		
315	3 feet long.		720	1.7		
316	Bearing dist. 33 inches.		720	1.9		
317	Depth,......... 1½		720	1.5		
318	Breadth,....... 1		628	1 8		
319			846	2.0		
320			744	1.7		
321			600	2.5		
327			554	2.0		
	Mean results		702	1.9	383	3574†
220	TEAK.		500	.8		A very fair mean this, for Teak.
225	3 feet long.		618	1 3		
226	Bearing d st. 33 inches.		524	1.2		
227	Depth, ...1½		672	1.35		
228	Breadth,.........1		350	1.0		
221			768	1.25		
222			624	1.05		
223			548	1.2		
224			400	.875		
229			606	1.1		
230			600	1.0		
231			596	1.3		
	Mean results		567	1.2	605	2080
322	SISSOOH.		750	1.5		Very fair:
325	3 feet long.		536	1.45		
326	Bearing,...... 33 inches.		828	1.825		
328	Depth, ...1½		430	1.0		
329	Breadth,........1		536	1.7		
332			450	1.5		
323			560	1.8		
324			548	1.5		
	Mean results		579	1.534	473	2123
334	LARGE PUSSOOH.		348	1 2		
355	Dimensions as above.		474	1.1		
	Mean results		411	1.15	631	1507

* A very good mean this for Saul. Compare the results of large specimens of medium quality.

† Good Soondree is above this in mean result.

Number of Experiments.	Names of the woods and dimensions.	Specific gravity.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U = \frac{l^2}{d\Delta}$	Value of S from the formula, $U = \frac{lW}{4ad^2}$
351	WHITE FIR.		396	1.0		
352	Dimensions as above.		486	1.5		
353			390	1.0		
354			402	1.2		
	Mean results		418	1.2	605	1532
152	SAUL.		522	1.2		A very fair mean.
155	2 feet long.		348	1 3		
195	Distance of supports,		468	1.1		
196	22 inches.		468	1.05		
197	1 inch square.		502	1.2		
198			430	.9		
199			300	.6		
200			496	.85		
201			502	1.2		
202			580	1.4		
203			538	1.2		
204			496	.85		
205			460	1.1		
206			484	1.2		
154			372	.8		
218			400	1.0		
153			300	.6		
156			420	1.2		
	Mean results		450	1.04	465	2475
183	SOONDREE.		330	.9		Rather below par, perhaps 20 per cent.
184	Dimensions as above.		322	1.2		
185			438	.8		
186			408	1.1		
187			420	1.3		
188			514	1.3		
189			470	1.8		
190			392	.9		
191			524	1.7		
192			466	1.35		
193			448	1 5		
194			250	.9		
	Mean results		415	1.23	393	2282
157	TEAK.		378	1.1		
159	Dimensions as above.		396	.8		
161			390	.7		
162			366	.8		
163			339	.95		
164			360	.85		
165			366	.85		
166			378	.775		
167			384	.75		
168			324	.85		
169			348	.8		
170			312	.725		
171			384	.8		
172			390	.85		
	Mean result		363	.83	583	1996

Number of experiments.	Names of the woods and dimensions.	Specific gravity.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U from the formula, $U=\frac{l^2}{d\Delta}$	Value of S from the formula, $S=\frac{lW}{4ad^2}$
158	SISSOOH.		372	1.3		
160	Dimensions as above.		348	1.325		
173			348	1.3		
174			298	1.25		
175			286	1.2		
176			286	1.5		
177			262	.9		
178			336	1.075		
179			346	1.25		
180			350	1.2		
181			[illegible]	1.1		
182			348	1.1		
219			438	.9		
	Mean results		330	1.2	403	1815
207	JARROOL.		538	1.2		
208	Dimensions as above.		460	.95		
209			478	1.0		
210			526	1.1		
211			496	1.1		
212			420	1.1		
213			466	1.1		
214			532	1.0		
215			484	1.0		
216			520	1.0		
217			428	.85		
	Mean results		486	1.03	470	2673

II.—*Description of the Drill Plough of Tirhoot, used in the Cultivation of Indigo, with comparative Remarks on the Drill and Broadcast Systems of cultivating Indigo.*

> "Straight on the packhorse track we keep the road,
> True to the jingling of our leader's bells."—COWPER.

It has often been remarked, but assuredly without much consideration; that to wean the natives of India from their deep rooted habits, their inveterate prejudices, and their pertinacious adherence to the customs of their forefathers; is a hopeless task.

With respect to the tender point of religious belief, the assertion may be admitted: but in the ordinary affairs of life, the economical and the effective are as much appreciated, and *cæteris paribus* as much acted on; as in countries which have attained a greater pitch of social refinement.

That a native artisan works creditably, considering his means, will not be denied: and that under European superintendence, he will turn out almost as finished a piece of workmanship, as his brother operator of the west, can hardly be disputed: but he is deficient avowedly in what may be termed the organ of *positive* inventions, though in the *comparative* and *superlative* degrees of that attribute, we all know from daily experience, he is highly gifted.

That he will readily adapt himself to implements of novel construction, and entirely dissimilar to those he has been accustomed to work with; is proved from the circumstance, that the machine which forms the subject of this paper, is entirely managed by natives; and performs its functions with a celerity and precision, not to be surpassed by the more costly and complex, though more elegant manufacture of the European artisan.

The great ends of all mechanical contrivances, simplicity combined with economy and efficiency, is as much provided for in this homely looking machine, as could be desired or devised. It is far from being insinuated that this machine cannot be improved: on the contrary, there is ample room to invite improvement: one man invents, but others refine on the invention; and thus the honour of the original discovery, is frequently wrested from the right owner.

The author of this essay lays no claim to the invention of this machine; which he believes, was first introduced by the proprietors of the works hereinafter alluded to: but having been early engaged in the indigo culture, and being firmly convinced that the general system of cultivating that plant can be practically improved; he has no hesitation, in making the use of this machine more generally known: in order, that those whose custom it is "to try all things, and hold fast that which is good;" may indulge their useful penchant, at little cost.

One moving power (that of the bullocks employed in drawing it) effects as much, and to as much purpose as the mighty steam engine itself. By the following description, aided by a reference to the plate; the simplicity, yet perfect efficiency of this machine, may be readily understood.

A is a semicircular trough, in which the indigo seed is deposited. Horizontally through this trough the iron axle B of the wheels C C is inserted; being kept in true revolve by an iron plate or shoulder C D firmly rivetted to it, and fitting close to each inner end of the trough. On this iron axle, within side the trough; are fixed two small circular wheels D D, having at the distance of every inch, holes $\frac{1}{8}$th of an inch deep, bored in a slanting direction, and made very smooth inside. E E are two straight iron ploughshares, which form the drills: these shares are of solid iron in front, but hollow for about one half of their side dimensions; so that they form at their back part a kind of channel down which the seed is conducted, from the hoppers. This, not only ensures the seed being conveyed straight into the furrow; but protects it from the wind; which might otherwise blow it aside, immediately it left the hopper.

F F are the hoppers, cut through a solid piece of wood, in the point of the machine to which it serves as a girder: the outlet of the hoppers, and the grooved sides of the ploughshares; are immediately perpendicular to each other.

G G are the rollers, which cover up the drills after the seed has been deposited: their mechanism is sufficiently obvious.

H is the pole, having near its upper extremity, two or three grooves or notches III. By fixing the small rest J of the yoke K into these notches or grooves; the ploughshares, being consequently elevated or depressed, make deeper or shallower furrows as may be deemed requisite. In this part of the machine, an improvement might be effected: either, by making the ploughshares to be set higher or lower at pleasure, by means of the elevating screw; or at less expense, by means of a third wheel, in the middle of the fore part of the machine, immediately under the stock of the pole; which wheel, running in an iron bed on each side, drilled with holes at various distances, might have a stout iron pin as an axle, the shifting of which higher or lower, would cause the shares to cut a deeper or shallower furrow. At present this effect is produced, by causing the yoke to rest heavier or lighter on the necks of the cattle engaged in the work. Amongst other improvements of this machine; that of a ploughtail or handle to ensure straight drilling, might advantageously be adopted. At present, one man walks by the side of the machine; to clear the hoppers in the event of any seed adhering, in its progress through them. The same might act as ploughman, which part is now taken by the driver: and as the regularity of the drills is only provided for, by the accuracy with which this man can guide his cattle; the hoppers might be made to clear themselves, by means of a spring wire kept in motion by small cog wheels, or other simple contrivance.

The ploughshares are fixed into the fore part of the machine, by means of an iron wedge; which is strongly hammered through a hole in their upper part: by this means, the ploughshares are removeable at pleasure.

The land intended to be sown, having been previously rendered fine by repeated ploughings and harrowings, with the common native plough and *hanga*; and all clods, weeds, &c. removed; the trough is filled with well dried seed to about the level of the iron axle, and every thing being adjusted, the bullocks are urged on. The shares cut the furrow; the wheels of the machine turn those of the trough: the slanting holes bored in the wheels of the trough, during their passage through the seed, take up each one or more seeds (seldom more than one), and in the downward part of their revolution unload themselves with precision into the hoppers; which lead them into the hollow of the ploughshares; which last deposit the seed in the furrow they have just made; and the rollers following, cover up the furrow and the inclosed seed, in an instant.

Let us compare this method of cultivating the indigo plant, with the old system; or in fact the general system; for this method has, only of late years, been introduced, by the spirited exertions and forethought of the proprietors of one of the finest and most extensive set of works in India. I allude to B****a and T******a in Tirhoot: which in the late disastrous season, while neighbouring concerns made but a trifle; manufactured upwards of 2000 maunds of excellent indigo, which brought a high price.

In common with other parts of the country, Tirhoot suffered from the severe drought, in the early part of 1828; and of the little plant that did ensue from the sowings of that period, the best part of that sown broadcast wore a sickly appearance, while many fields totally failed: on the coutrary, such portions as had been sown by the drill machine, wore a good appearance, in consequence of having been laid well in the ground. In this crisis, the drill machine was called into play: over fields which had entirely failed, as well as over those in which a scanty crop only prevented itself, the drill was speedily passed: and in lieu of a dreary blank, the cheering prospect of interminable files of vigorous plants soon presented themselves. Timely showers soon after descending; the crop reached maturity, and yielded a good produce.

Look on this picture and on that. On the one side is a planter with his hand despondingly thrust into empty pockets; ruminating on the blue looks, with which his faithful friends will greet him, on his arrival with some half hundred chests of "fair copper;" not to mention the appalling balance on the wrong side of his annual sheet: on the other, a more enterprising and well judging individual, rightly deeming there is a remedy for every evil under the sun, puts his shoulder to the wheel, and makes a fortune, where another loses one. So much for management.

In the saving of seed, this machine has a particular recommendation; and at a time, when indigo seed has attained the high price of 10 rupees per maund (nearly double what it was in 1821), it behoves planters to look about them, for any means to effect a saving in this respect.

In the broad cast system about 10 seers of seed are allowed to the beegha*, varying a little more or less according to the measurement of the lands in various districts—in the drill method only 7 seers are consumed; so that a saving of 30 per cent. takes place in a most essential part of the business.

If this were its only recommendation, it would be valuable; but there are other points connected with the new method to which brief advertence may be made.

It is well known, that the more lateral room, the indigo plant has to tiller in; the more productive it is, of those minute branches, or ramifications; and consequently of the leaves, in which the *Indigogene* is lodged.

In the broadcast system, each plant may be considered in the light of an individual in a dense crowd, where the want of air and space renders each anxious to overtop the others to inhale a fair portion of the breath of heaven. So it is with the indigo plant, sown broadcast; having no lateral room, a kind of race takes place for the superiority of altitude, and the consequence is long, lanky, woody plants, with few side shoots or leaves except at their tops. In the drill system, there is elbow room and free ingress to the sun and air, and whatever else excites the healthy vitality of vegetation; consequently it will be observed, that in drill reared plants, the tillering commences almost from the root, and is comparatively lateral; in the broadcast, the forking of the branches only commences high up; and the produce of leaves is scanty; while the whole bears a perpendicular tendency, as if the plant had really no alternative but to struggle upwards, to retain existence, amidst a host of closely jammed and equally struggling coupeers.

I am aware, that in a subject like this, I shall be met with many objections. I shall be told the machine is not suited to all districts or soils—true; but it will suit all but deep clays, which are not frequently resorted to for indigo when loam can be had; and there is an old adage, that the proof of the pudding is in the eating. Let any planter but try a few beeghas sown by the drill machine, and as many upon the old system; let him manufacture the plant produced from each, and satisfy himself as to the quantity, and quality, resulting therefrom, before he condemn what he has not

* This beegha contains 57600 square feet.

tried. The experiment is not costly; on the contrary, a positive saving will be immediately effected in the consumption of seed; while the expense of making the machine is a trifle, and the ordinary artisans attached to an indigo work, can easily put together a machine of this kind in a few days.

I should hope, that this essay may fall into the hands of many, who have the means and the leisure, to make the experiment to satisfy themselves.

The non-appearance of the usual fall of rain at this season of sowing in the present year, offers a fair opportunity to those who may be willing to put to the test the new method for other purposes than those of mere experiment. Should any of your readers be desirous of inspecting a model of the machine in question, you can refer them to Your obedient servant,

ONCE A PLANTER.

III.—*On the Expediency of introducing Machinery into India.*

To the Editor of Gleanings in Science.

SIR,

To those who take notice of the progress of the arts and manufactures of Europe; it must be evident, that the period is fast approaching, when the skill that has hitherto been confined to Great Britain, will be diffused over the whole of the nations of the continent: and since it is a well ascertained fact, that there is an excess of capital in England which has for some time past been seeking foreign employment; it most assuredly behoves the Government to give the enterprise of the nation a direction, such, as will have a tendency to prevent that crisis in the affairs of many of our manufactures, which threaten ere long to become rivalled in the other nations of Europe.

In the single, but vast manufacture of cotton, which has so long been one of unrivalled beauty, among our other excellent productions of art, it is known, that its superiority over that of other countries is entirely dependant upon the mechanical perfection of all its manipulations, and that this perfection is based upon a greater tact which we possess, in rendering the machinery as highly perfect as it is surprisingly complex. Skill, capital, and enterprise, have been the parents of this excellence of workmanship. And it is well known, that there are many countries which have already, and for some years past, entered the list of competition; and that will, in a few years more, arrive at the same pitch of perfection.

The finest sort of twist, it is also well known, is exported in vast quantities, to supply the looms of France, Germany, and Russia. These countries will however in time make their preparation for themselves; and then perhaps three-fourths of the fixed capital in Great Britain, expended in this branch of the manufacture, will become useless.

It is true, that the change may be a gradual one; but, it is not the less necessary, that a gradual remedy should be applied to meet the change, which the silent operation of such a cause, will necessarily produce on the manufacturing classes.

The miseries of the Luddites and Manchester Meetings, may be again renewed: while a contemporaneous anarchy in the sister kingdom; cannot fail to open apparently bright prospects to a starving population; having nothing to lose, and much to gain; from a state of things, that will have been brought about, by the improvidence of those who might have directed its energies, to the proper objects of national prosperity.

In Great Britain, three-fourths of the cotton twist manufactured, was said several years ago, to be designed for foreign consumption; and the finest sort exceeded 320 hanks, of seven skeins to the hank, per pound!

Russia, at that period, had several cotton manufactories, in which the fineness of 70 hanks had been attained; while France and Germany had reached 150!

From this, it would seem, that the foregoing reflections have some foundation in reason. They well deserve the serious attention of the Government, and the Legislature of the country; and should not be neglected even amidst other more pressing calls.

It would be wise, then, to encourage the exportation of machinery to Hindustan, and by thus acting, not only gently to meet the revulsion or re-action in this branch of industry in our own country; but, in some sort to repair the mischief, which our commercial cupidity had long since inflicted, upon a simple people: thousands of whom have been rendered destitute and wretched, by a policy, scarcely compensated for, by our prodigal protection.

In the name of common sense, what is it that stands in the way of such an improvement to both countries; but, want of confidence, and of clear-sighted views?

If one-tenth part of the enormous capital, that was lately embarked in the wildest projects that ever entered the brain of man; had been invited to India 25 years ago; Great Britain would have commanded the commerce of the world, in the vast article of clothing alone (now become universal). And she could well have exchanged the hundreds of effeminate, and demoralising cotton mills, for thousands of such establishments as those of Boulton and Watt; since all the machinery to be worth any thing must have been made by English artists in their own country; as well as that of numerous other sister arts, which would have sprung up with the progress of general improvement in a country, which is stored with uncounted mines of incalculable wealth.

Should Englishmen be induced even, eventually, to abandon the country, they would do so, after creating tastes and wants, that could not be so well gratified as by that people who had given them existence; and thus, India, totally disconnected with us in every other point of relation, would be ten times as valuable to Great Britain, as America is at this moment.

But, in order to render the importation of machinery, extensively beneficial; a preparatory step would, in some sort, be requisite. The cotton of the country, is of a quality so inferior (although very fine) to that required for manufacture by machinery*; that it is totally unsuited to being employed in the present neglect of the culture of this great staple commodity.

Its inferiority is so great, that it sells for less in the Liverpool market sometimes, than it can be procured for in the country of its growth!

Is cotton then not worth cultivation? That it is worth cultivation, is proved by the fact of the improvement which proper care effected, having been ascertained to exceed the ordinary length of staple, fourfold, and upwards! This is a fact, of which any one may satisfy himself, by making the experiment in his own garden.

It would be no idle speculation on the part of those engaged in indigo cultivation, to raise cotton also; and to these, sugar, and coffee, could be demonstrated to be no unprofitable additions. The consequence of such an arrangement, or method of commercial farming, would be, that a planter might have the whole of the crops nearly in a ring fence; instead of being compelled, to visit distant patches of indigo cultivation, to the great injury often of his health, and not unfrequently of his fortune.

It can be shown, that such a system, even in an unfavourable season, would reduce his gross loss to a fraction of what is often experienced by the present system. It is not very creditable to the understandings of Europeans, that they should consider such a country as India, to be capable of producing little more than four or five small ship loads, of a dyeing drug! And to complain of the dullness of trade, and the narrowness of the commercial channel, without making even an infant exertion to extend, and expand this channel by seeking new objects; is to plead guilty to the existence of an apathy, that can have no other foundation than in a total want of all energy and enterprise.

Much blame is attempted to be cast on the anomalous relations of Europeans in India: these are certainly singular and peculiar; but the peculiarity can frame but a very slender apology, for that extreme caution, of which, glaring facts demonstrate the total inexpediency. A great change is perhaps about to be made in many of the relations of Europeans in India; since it is shrewdly imagined, that the "good old way" of making the most of the country, must be exchanged for a better.

The task of enlightening 80 millions of people, must require two or three centuries; for we may reasonably suppose, that they will not advance much faster than the European nations, who were little raised above barbarism three centuries ago. But let any man take a survey of Europe since Watt, and Arkwright, and Smeaton added a new impulse; and he will soon perceive, that the last half century has effected more than was accomplished in the two preceding.

The philosophers and chemists, are undoubtedly very assisting, in the vast improvement of knowledge; but their labours, though equally valuable, did not form so powerful an appeal to the common sense and feelings of mankind, as the first great artificers; the benefit derived from whose labours, bring them into our presence, in almost every imaginable comfort we enjoy. The philosophers, rendered navigation more expeditious, certain, and secure: but, the artists quadrupled British capital in half a century; and made England what she is, and could not have been without them. It was well said, that "the Steam Engine had fought the battles of Europe."

* The writer knows this practically to be the case.

Machinery for the manufacture of webs, has already been imported, and more is finding its way out! The first importation was about four years ago; and this, as an experiment, should have satisfied speculators, that the like speculation might be reasonably repeated. These importations have been made chiefly, it is believed, by those who are new in the field of Indian improvement; and their speculations being of a very different cast to those of more established commercial undertakings, have not excited much sensation.

The success of the project must convince the most sceptical: want of information can alone account for its tardy adoption by others.

The government of the country has set the first example, in the creation of a magnificent manufactory; which is filled with the best specimens of the skill and genius of Watt: need commercial intelligence require more encouragement, to follow such an auspicious example? If a large capital can be profitably laid out, to procure, what the ignorant may deem, a trifling advantage; in how many other objects, might capital not seek the most profitable application?

It may be objected by some, that in order to execute a project of the kind, a considerable capital would necessarily be required: and, that its temporary inactivity, would be very discouraging, to those engaged in the ordinary routine of commerce; which always desires quick returns.

Yet, limited as may be the command of capital, that might be found to be engaged in transactions of small profit; it is imagined, that two or three lakhs of rupees might be available for such an undertaking: and, that there are wealthy persons among the natives of the country, as well as the European community, who could be prevailed upon to embark in an enterprise of this sort.

The thing requires little more than to be stated, to be known, and acted upon: and the first step, after forming the resolution to adopt a project of this nature, must necessarily be, that of procuring persons to teach the natives, the manufacturing processes, as well as the means of keeping the machinery in proper order.

The manufacture of paper, such as is in use with the natives, even, promises to be one of no mean importance; and might well deserve attention.

It is conceived, perhaps not unreasonably, that, the machinery would replace the original outlay, in three years; yielding 8 or 10 per cent. interest, in the intermediate time; after which, the profits would be superadded.

Saw-mills, would likewise offer no inconsiderable advantage, in the investment of capital.

I need not, Mr. Editor, enlarge the list, as I feel assured, that much good must result from the attempt of any one of these projects: which, by the way, you should recollect are not schemes; nay, they are not so much as projects; and you may safely assure your readers, that they may, more reasonably, consider such things as certain in their operations, than even the seasons themselves. In this last conviction I beg to subscribe myself,

Sir,
Your much edified and constant reader,
H. D. E.

IV.—*Proceedings of Societies.*

I.—Asiatic Society.

Committee of Natural History and Physics. Wednesday, February 4th, 1829.
The Hon. Sir E. Ryan, in the chair.

Mr. Calder presented, in the name of Mr. R. Rose, a collection of Geological specimens; made, during a survey of the roads, from Midnapore, to Sumbulpoor; and from thence, to Cuttack, and Balasore, with a descriptive Catalogue of their localities.

The Committee having in their possession a very extensive collection of geological specimens from various parts of India, it was proposed by the President, that a selection of duplicates should be made for the purpose of being presented to the Geological Society of London.

The motion was seconded by Mr. Calder, and unanimously agreed to.

It was part of this motion, that the Committee should, through their secretary, request in return from the Geological Society of London a series of specimens illustrative of the English strata, and generally such communications or suggestions as might tend to direct inquiry in India, and assist the progress of geological science.

An addition to Dr. GOVAN's collection of specimens was presented by Mr. CALDER.

A short account of the native process of making iron at Amdia near Mednipoor, by Mr. R. ROSE, was presented and read.

A Paper by Dr. GOVAN, on the Geology of the range on which Simla and Tarba are situated, was read.

Thanks were voted to the several contributors, and the meeting adjourned.

Wednesday, April 8th, 1829.

Sir E. RYAN, President, in the chair.

Read a letter from Mr. SMITH, proposing to print a Geological Map of India; on condition of the Society's taking sixty copies: Referred to the general meeting.

An Extract from a letter of Mr. HODGSON, to Mr. CALDER, on the subject of the Chiru, or Unicorn, was read. The substance of this will be found amongst our notices.

A notice of Tin Ore from the coast of Tenasserim, by Mr. ROSS, was read. This is given amongst our notices.

A Paper on the Geology of central India, by Dr. HARDIE, was read in part.

Specimens of the clay obtained by boring near the Salt Water Lake, were presented by Mr. STRONG.

Thanks were voted to the several contributors, and the meeting adjourned.

2.—HORTICULTURAL SOCIETY.

Wednesday, February 11th, 1829.

W. LEYCESTER Esq. President, in the chair.

The President delivered an account of the expenditure in the Garden, during November, December, and January preceding: from which it appeared, that the sums laid out upon it, amounted to Rupees 660 13; while the produce sold during the same period, amounted to Rupees 240 9 9; leaving a balance against the Garden, of Rupees 420 4 3.

The estimate for the months of February, March, and April, 1829, amounted to Rupees 767.

A letter was read from Mr. J. W. PAXTON, which accompanied a dozen oranges that he had brought from Ceylon; and which he considered of so fine a quality, that they ought to be imported into Calcutta regularly, and propagated in our gardens.

The oranges were made trial of by the meeting, but found to be spoiled; so that no opinion could be formed, upon their good or bad qualities.

The thanks of the meeting were voted to Mr. PAXTON.

Mr. ROBISON presented, in the name of Mr. BLACQUIERE, a large beautiful orange; raised by him in his garden here, from the seed of one which he had received direct from Mozambique eight years ago; and which was so very fine, as to induce him to plant the seeds. The orange was made trial of by the Members present; who all agreed, that it was the finest orange which any of them had seen reared in or about Calcutta: it was considered by the meeting particularly desirable, that a species of orange, which seemed so well adapted to the soil and climate of Calcutta, should be immediately introduced.

The thanks of the Society were voted to Mr. BLACQUIERE for his interesting present; and it was resolved to ask Mr. BLACQUIERE's permission, when the proper season came, to take grafts from the tree.

It was proposed by Captain JENKINS, that so soon as the funds of the Society permitted, medals and premiums should be offered to native cultivators for the best mangoes, peaches, leachees, guavas, pine-apples, plantains, and other native fruits: this was seconded by the President, and agreed to.

It was resolved that the 1st volume of the Society's Transactions be distributed to all the Members of the Society, and that the translations into the Bengalee and Hindee be made as soon as possible.

The President announced, that the seeds ordered in December, 1827, and which were intended for distribution in the month of August, 1828, had very lately reached Calcutta from Liverpool.

It was resolved, that Dr. CAREY should be permitted to take the flower and fruit seeds, least likely to remain good, for the purpose of his sowing these in his own garden immediately; and that the vegetable, and larger kinds of seeds should remain unopened till the season of 1829.

Resolved, that a copy of the Transactions be sent to learned Societies in Europe, Africa, Asia, and America.

Resolved, that a copy of the Transactions, handsomely bound, be transmitted to the MARCHIONESS OF HASTINGS, the first Patroness of the Society.

Resolved, that the remaining copies be sold at Six Rupees a copy.

Resolved, that the President be desired to prepare and communicate to LORD WILLIAM BENTINCK, the request of the Society, that His Lordship and LADY BENTINCK would become Patron and Patroness of the Society.

The Meeting adjourned.

Wednesday, April 15th, 1829.

W. LEYCESTER, Esq. President, in the chair.

Mr. ALEXANDER informed the Meeting, that he had the honour to be requested to communicate the intention of the GOVERNOR GENERAL to become a Member of the Society.

Mr. ROBISON presented a paper by Baboo RADAKANTH DEB, containing the history, properties, culture, and propagation of the Bere Tree of India.

A letter was read from Mr. DAVID SCOTT, at Gowahatty, forwarding the seed of a new vegetable, called by the Assamese, *Kamti Lai,* and which he considered "worthy of introduction by the Society, as producing, early in November, leaves of considerable size, and which, in the absence, at that season, of more delicate vegetables, are very palatable."

The thanks of the Meeting were voted to Mr. SCOTT for this, in addition to his numerous and valuable communications.

The Secretary to the Committee, appointed on 14th January last, for the purpose of investigating and reporting upon the present state of the Society, presented the report which had been prepared by them, and the same was read by the Secretary of the Society.

The report states, that the books and accounts of the Society were in such a state, that the Committee would derive little or no information from them, and that important documents were wholly omitted. That on examining the accounts of the Treasurers, it appeared that the funds of the Society, which, in May 1827, amounted to Sa. Rs. 11,000, were now exhausted; and that the expenditure appeared to have been conducted, not only without the sanction of the Society; but in contravention of rules laid down for that purpose. That independent of all contingent expenses, the annual expenditure of the Society amounted to Rs. 5,200; while the income amounted only to 3,400, including the donation of Government; and that the present state of expenditure had only been supported by a wasteful call upon funds of which the interest alone ought to have been touched. The Committee therefore recommended an immediate retrenchment; 1st, by giving up altogether the Poosa Garden, and 2dly, by reducing the expenditure of the Allipore Garden to Rs. 100 per month. That an incorporation of the Society with the Asiatic Society, did not appear expedient, unless it should be found that the Society cannot otherwise be kept up; which the Committee did not apprehend to be the case, as they knew of many gentlemen who wished to become members, and were only deterred by not being able to procure information of the Society's proceedings. That an effectual means of increasing the utility of the Society, and keeping up an interest in its proceedings, would be secured by a frequent publication of its proceedings, and translation of its transactions into the native languages. That it appeared highly expedient to elect the officers of the Society annually, and that they could not conclude without recommending an early day being immediately appointed for electing all the officers of the Society, and fixing rules for their subsequent re-election and rotation, as the past experience of the Society had proved too plainly the necessity of a greater degree of vigilance and exertion, than could be expected from the present system of management.

It was moved and carried, that the report of the Committee be received and recorded.

It was moved and carried, that the thanks of the Society be offered to the Committee.

It was moved and carried, that the Poosah Garden be immediately given up, and that the Secretary be requested to express to Government the regret of the Society, that the state of its funds did not permit it to continue the cultivation of the garden.

The state and expense of the Allipore Garden, were next taken into consideration; but the subject was deferred till a future meeting, as some difficulties arose from the nature of Mr. MITCHELL, the head gardener's engagement. It was resolved, however, immediately, to reduce the expenditure of it to the lowest possible scale.

Mr. ALEXANDER was of opinion, that the attention and funds of the Society had been too much devoted to Horticultural objects, thereby losing sight, in a great degree, of agriculture, which he considered the main business of the Society; and most of the Members present appeared to be of the same opinion, but no specific motion was made, and the subject was postponed to a future day.

In terms of the concluding clause of the report, it was moved and seconded, that an extraordinary Meeting of the Society should be called on Wednesday evening, the 29th current, for the purpose of electing all the officers of the Society for the ensuing year; but the Reverend Secretary, Dr. CAREY, expressed his inability, from distance and various avocations, to meet the Society so frequently; and it was therefore suggested and agreed to, that as the present Meeting was a numerous one, the election should then take place, by each Member giving in to the Secretary a written list of the persons whom he voted for.

The Meeting, being decidedly of opinion, that it was essential to the welfare of the Society that it should possess the power of either re-electing or changing its officers annually, proceeded to chose a President, two Vice-Presidents, and two Secretaries, for the following year; deferring the election of the Members of the Committee till it should be known upon whom the choice of the principal officers fell. Upon the lists being handed to the Secretary, and examined, the following gentlemen appeared to be chosen by a large majority, viz.

The Honorable Sir EDWARD RYAN, President.

The Rev. Dr. WILLAM CAREY and Baboo RADAKANTH DEB, Vice-Presidents, and C. K. ROBISON, Esq. and Baboo RAM COMUL SEN, Secretaries and Collectors.

The above gentlemen were declared duty elected, and the Secretary was requested to communicate to Sir EDWARD RYAN, the choice which had fallen upon him by the Society.

Dr. CAREY was of opinion, that many considerations ought to have induced the Society not to press the proposed measure, and however much he was obliged to the Society for the confidence reposed in himself, he was constrained to decline the honour of being Vice-President; indeed, his feelings were of that nature as to lead him to withdraw from the Society.

It was moved and carried, that the thanks of the Society be offered to Mr. LEYCESTER, for his long continued services as President.

Mr. LEYCESTER returned thanks.

It was moved and carried, that the thanks of the Society be offered to Dr. CAREY, and its expression of extreme regret, that any circumstances should deprive it of his highly valued services.

Dr. CAREY returned thanks.

It was agreed to, that Dr. CAREY'S resignation should be accepted, and another Vice-President chosen in his room. On examining the lists, Mr. NATHANIEL ALEXANDER was found to be chosen, and was declared elected.

Mr. ALEXANDER, in returning thanks, stated, that he had also several gentlemen to propose as members, who, he had no doubt, would be very efficient; but as the evening was far advanced, he would defer his proposal till the next Meeting of the Society.

According to the resolution expressed above, an extraordinary Meeting of the Society will take place at the Asiatic Society's Apartments, on the evening of the 29th Instant, to elect the Members of the Committees, and to determine on some very important business.

SCIENTIFIC INTELLIGENCE, MISCELLANEOUS NOTICES, &c.

1. *Library of Useful Knowledge.*

A Society has been formed in London, of which Mr. BROUGHAM is president, for the diffusion of useful knowledge, and particularly for undertaking or assisting in the publication of popular, and generally useful treatises, and for affording such works, to those desirous of purchasing, at the most economical rate possible. A fund has been raised by subscription to effect these various objects, and they have commenced their operations by planning the library of useful knowledge. This is a kind of popular corpus scientiarum, or circle of arts and sciences—an encyclopædia in fact, but without the alphabetical arrangement. A number is published every fortnight, consisting of 32 pages, 8vo. close print (equal to nearly 100 ordinary 8vo.), the price being 6d. To mechanics' institutions, or in fact to any society or person wishing to use them for the diffusion of knowledge, they are sold at a still cheaper rate, we understand.

We have been favoured with a sight of the first 28 numbers of this publication, and of a discourse, which accompanies it, on the objects, advantages, and pleasures of science: we shall give the titles of the subjects on which they treat for the information of our readers, adding our strongest recommendations of the work as one got up in a very creditable style, and giving a very full and correct view of each subject and of the latest improvements in it. We refer to our cover for further particulars.

1	Hydrostatics	
2	Hydraulics	
3	Pneumatics	
4	Heat	Part I.
5	Heat	Part II.
6	Mechanics 1. On the Mechanical Agents, or Prime Movers	
7	Do. 2. Elements of Machinery	Part I.
8	Do. Do. Do.	Part II.
9	Mechanics	
10	Account of Lord Bacon's Organum Scientiarum	
11	Mechanics 3. On Friction and the Rigidity of Cordage	
12	Optics. Treatise I.	
13	Optical Instruments	Part I.
14	Vegetable Physiology	Part I.
15	Electricity	Part I.
16	Mathematical Geography	
17	Arithmetic and Algebra	Part I.
18	An account of Lord Bacon's Novum Organum Scientiarum,	Part II.
19	Optics	Part II.
20	Life of Cardinal Wolsey	
21	Optical Instruments	Part II.
22	Electricity	Part II.
23	Physical Geography	Part I.
24	Life of Sir Christopher Wren	
25	Arithmetic and Algebra	Part II.
26	Thermometer and Pyrometer	Part I.
27	Outlines of General History	Part I.
28	History of Greece	Part I.

The first number ran through 5 Editions, the 2d through 4. The circulation of the work has already attained the number 20,000.

We consider this project as one calculated to exercise a very great influence, over the character and happiness of the present, as well as the rising generation.

2. *Notice of some Tin Ore from the Coast of Tenasserim*.*

It appears from a communication from Major BURNEY to Mr. Secretary SWINTON, that some Chinese declare these deposits of tin to be superior to those of Junkseylon; and that the Burmese with their rude means obtained 55 per cent. of metal from this ore, but that the Chinese have obtained 60 per cent. This appears highly probable, as from the sample sent to me for examination, of which the greater portion is here produced, I obtained upon an average of three assays 47 per cent. of pure tin; but it will be observed, that the ore in this sample is but very partially separated from its matrix, in the constituents of the bed or deposit in which it occurs, which is rather a tedious and laborious process, but generally a well executed task among the Cornish tinners in England, to whom the present sample would appear what they would term *half dressed*, or but partially separated from the siliceous and other earths, with which it was originally intermixed. Hence it may be concluded that, had the heterogeneous particles been carefully detached from this ore, it ought to yield 70 per cent. of metal smelted upon a large scale; and even as much as 76 per cent. in a crucible with a powerful flux, such as borax and anthracite; about an eighth of the former, and a third of the latter in proportion to the weight of the sample assayed, but, where anthracite is not procurable, a double portion of close grained charcoal finely pounded, will be a tolerable substitute.

It may be observed that this tin ore is what the Cornishmen term *stream tin;* which is distinguished from mine tin by being found in the bottoms of streams, or other alluvial deposits, where streams have been, which have washed it away from the pa-

* This communication by D. Ross, Esq. joint Secretary of the Class of Natural History and Physics of the Asiatic Society, was read at the last meeting, as noticed in the account of their proceedings, p. 140.

rent rock, in which it originally occurred: hence this kind of ore called *Stream tin* is much finer than what is broken immediately from the vein, because the action of the atmosphere and water decomposes, and carries off, the sulphurets and arseniates with which the ore was originally associated in the vein; therefore, the tedious and pernicious process of calcination, by which the sulphur and arsenic are sublimed from mine tin ore, is not at all required for stream tin ore such as this, but it may be immediately put into the reverberatory flowing furnace.

The ore is found at a place called Ch,hando, near Páluk, about two days' journey from the sea, halfway between Mergui and Tavoy.

Calcutta Mint, 8th April, 1829. D. R.

3. *Captain Franklin.*

Captain FRANKLIN who, our readers will remember, obtained the permission of Government, at the earnest recommendation of the Class of Natural History and Physics of the Asiatic Society, to proceed to Baudelkhand, the scene of his former labours, for the purpose of completing his geological survey of that province, had reached Munghér, on the 14th Jan. He found the rocks near Rájmahl, Sikrigali, and Pír Painti of the trap formation. He was not therefore surprised to find Granite at Pathar Ghàta and at Callgàon. He had made a pretty full collection of shells, and found amongst others Helix globosus, Vivipara fluviatilis, Planorbis, Helix Nautili, Anodonta, Iridina, and some Bulimi and Scalariæ. These being crag shells, prove beyond a doubt, he thinks, that the banks of the Ganges are posterior to the London clay. The season was adverse to his making any entomological collections, most of the insects being under ground, and the cold so intense as to have destroyed the butterfly; but in ornithology he had been more successful, and had, at that early period of his journey, collected 40 specimens stuffed, and accurately drawn and coloured after nature. This department of natural history is as yet untouched* in India, and offers a fine field to those who will devote themselves to it. Captain Franklin proposes to leave no means neglected of forming a complete collection of Indian birds.

We shall be glad to learn further particulars of the researches of this able and zealous officer.

4. *Antilope Hodgsonii (the Chiru, or Unicorn.)*

We do not think we are disposed to overvalue the utility of our work, when we say that by affording a means of ready communication between the several cultivators of Science—not only in India, but between those of India and of Europe—it may often diminish the labour of research, or dispense with it altogether. An instance has just occurred to us of this, in noticing which we shall have the further gratification of ensuring to the gentleman, to whose assiduous zeal in the pursuit of science we owe so many interesting facts, that meed of fame which, as it is the principal, is also the most pleasing, reward the naturalist can look to.

In the second number of our work we published an extract from the letter of a distinguished zoologist, pointing out what appeared in Europe to be the principal desiderata in that branch of zoology, to which he had particularly devoted himself. Amongst these, is mentioned the Chiru or Antilope Unicorn of Bootan (Thibet?) It appears, that some years ago a description of this animal was furnished by Mr. HODGSON, of the Bengal Civil Service, at present Resident at the Court of Catmandu. The description was accompanied by a skin of the animal, from the inspection of which the late Dr. ABEL was enabled to render the description more technically complete,—who, justly appreciating what was due to Mr. HODGSON, named the species, till then undescribed, *A. Hodgsonii*. Circumstances, of which we are not prepared to give any account, and for which no particular person is answerable, have interfered with the publication of this paper hitherto; but we trust, it will not any longer be allowed to slumber in forgetfulness. Our neighbours on the continent are too much alive to the interest attending any new and important contribution

* We mean by English hands; but our neighbours, the French, have we suspect been far from idle. With scarcely any establishments in the country, and dependant entirely on the exertions of a single individual, we suspect they know more of Indian ornithology than we who have been masters of the country for 70 years. M. Duvaucel, whose death was an irreparable loss to science, we believe, made a very full collection of Indian birds, which were sent to France, and must be, we suppose, now in the Cabinet of the Royal Museum. While on the subject we may also mention, that a pretty full collection of preserved (not stuffed) specimens of mountain birds was forwarded to the Honorable the Court of Directors for their Museum, by Captain Manson, late of the Mineralogical Survey Himmalaya Mountains.

to our knowledge of the animal kingdom, to allow us to follow Horace's maxim in this instance, and keep our descriptions under review and correction " in nonum annum."

5. *Geology of the Himmalaya.*

A correspondent wishes to have some further account of the Slate clays and Limestones described by Dr. GOVAN, in his communication to the Asiatic Society. He is said by the newspapers to have stated that gypsum is found in the former, thereby assimilating them with the Newer red sandstone; and bivalve shells in the latter. Our correspondent wishes to know of what genera these shells are. He would also be thankful for a description of the two rocks, and a fuller account of their relations.

6. *Geology of Udayapúr.*

Dr. HARDIE, whose communications on the Geology of Central India, have been so often noticed in the Report of proceedings of the Asiatic Society, is employed we understand on a contiuuation of his paper on the Geology of the valley of Udayapúr. He is in particular preparing very complete sections of the strata. We wait with interest the result of his labours.

ANALYSES OF BOOKS.

Transactions of the Royal Society of London. For the year 1828, *Part* 1.

The first part of the Transactions of the Royal Society have reached this country; and, having been favoured with a sight of them, we hasten to lay before our readers a pretty full abstract of their contents. The papers appear to us to be all communications of great interest, and fully support the high character that attaches to the Transactions of this learned body.

I. *Experiments to ascertain the ratio of the Magnetic forces, acting on a needle suspended horizontally in Paris and in London. By Captain* Edward Sabine *of the Royal Artillery, Sec. R. S. pp.* 1 *to* 14.

In making experiments on the intensity of Terrestrial Magnetism, at different places on the earth's surface; the principal precautions necessary are, to take care, that the power of the needle to be affected by the magnetic influence residing in the earth, does not alter, in the intervals of the several experiments; and that the temperatures in which the experiments are performed, be as nearly as possible, the same. Both these objects were attained in the present series; as the same needles were employed, at Paris, and London: and on returning to London, the experiments were again repeated, and found to give the same results. The like precautions were taken, in determining the magnetic influences at London, and Edinburgh.—With regard to the temperature, it was endeavoured, as far as was possible, to perform the experiments in the same temperature, at each place; and to show how far this effort was successful, the following table of the mean temperatures of the experiments compared, is given.

at London 54°,5	at Paris 62°
50	52
40,5	59
54,5	66,5
55	54
54,5	53

As there is still a preponderance of temperature at Paris, the results were corrected, by using a formula of Professor Hansteen's. Supposing the time of n vibrations in the temperature of t to be T seconds and in the temperature t' to be T′ seconds then $T=T' (1-0,000165 (t'-t)$ the temperature being expressed in degrees of Fahrenheit.

The object was, to determine at each place, the time in which the horizontal needle performed a certain number of vibrations (100). The following extracts will give an idea of the instruments used, and the methods of proceeding.

" The magnetic needle employed in these experiments were cylinders of 0,16 inch diameter, and 2,4 inches in length, pointed at the ends: they were suspended by a single silk fibre, of rather more than 5 inches in length. The box in which they were enclosed (as a protection from the weather) was of wood, having at the bottom, a graduated circle in ivory; rather exceeding in diameter the length of the needles, and over the centre of which, the silk fibre was suspended. The bottom of the box, being rendered horizontal, by means of foot screws; and shown to be so, by an attached spirit level: the zeros of the circle, were placed in the direction of the magnetic meridian; and a needle was suspended in a horizontal position. Another needle was then employed, to draw it 50 or 60 degrees from its natural direction; on the removal of which, the suspended needle resumed its direction, in the ordinary process of vibration. The registry of the vibrations was commenced, when the arc had diminished to 30°, and continued, until it was reduced to *below* 5°; the method of registering the vibration will be best understood, by a reference to the close, and is too simple to require further explanation. The number of vibrations made by each needle, between the arcs of 30°, and 5°. was usually from 300 to 400, and the times in which these were performed, varied, in the different needles, from 12 to 16 minutes; the mean time of performing 100 vibrations, between the specified arcs, is the result deduced for each experiment."

The results by different needles were as follows: Supposing the intensity at London to be 1, it was found to be at Paris

	1.0732
	1.0675
	1.0726
	1.0723
	1.0709
	1.0717
Mean	1.07137

"From the very careful observations which are regularly made, on the dip of the needle, at the Royal observatory at Paris; the mean dip, corresponding to the period when these experiments were made, is known to have been 67°.58′. In assuming the dip, at the same time in London, to have been 69°.45′ (which is allowing a diminution of 3′, per annum, since it was observed in 1821 to be 70°.04′ (Ph. Tr. 1822 Art. 1) we cannot fail to be sufficiently near the truth, for the present purpose. The horizontal intensity at Paris, being then as 1,0714 to 1 at London; it results, that the absolute intensity of terrestrial magnetism, was greater at London, than at Paris, at the period of these experiments by about 11 parts in 1000."

"The horizontal intensity at London and Edinburgh, were as 1 : 0,9505."

II. *On the resistance of fluids to bodies passing through them. By* James Walker, *Esq. F. R. S. E. communicated by* Davis Gilbert, *Esq. M. P. V. P. R. S. pp.* 15 *to* 23.

This paper we shall reprint at length in our next number.

III. *On the corrections in the Elements of* DeLambre's *Solar Tables required by the observations made at the Royal Observatory, Greenwich. By* George Biddel Airy, *Esq. M. A. Fellow of Trinity College, Cambridge, and Lucasian Professor of Mathematics in the University of Cambridge; communicated by* John Frederic William Herschel, *Esq. V. P. R. S. pp.* 23. *to* 34.

This appears to be a valuable paper, but scarcely capable of abridgment. Mr. Airy's object was to compare a series of 10 years' observations of the sun's right ascension; with his place as determined by calculation, from the tables of DeLambre. He finds the following corrections in the several elements required to make the calculated and observed places agree.

"The epoch for 1821,5 ought to be increased by 5″,061."

"The epoch of the perigee ought to be increased by 46″,3. These epochs are to be measured from the equinoxial point, adopted by Mr. Pond, in his catalogue of 1826."

"The greatest equation of the centre ought to be diminished by 0″,84"

"The mass of Venus ought to be reduced in the proportion of 10000 to 8911 or 9 : 8 nearly."

"The mass of Mars ought to be reduced in the proportion of 10000 to 6813 or 22 : 15 nearly."

"The coefficient of the lunar equation ought to be diminished by 1,″04. The coefficient in DeLambre's tables is 7,″5; hence, if the Moon's parallax be not altered, the quotient of the Moon's mass by the sum of the Moon's and Earth's masses is to be diminished in the ratio of 29 : 25 nearly.

If these deductions could be relied on, we should have

Mass of Venus $= \frac{1}{401211} \times$ that of the sun,

Mass of Mars $= \frac{1}{3734602} \times$ that of the sun, Mass of the Moon$= \frac{1}{80,4}\times$that of the Earth; and the limits of the errors of DeLambre s tables, roughly estimated, would be as follows;

Error in epoch for 1830.	... ± 5″,6
Grearest error, from error in place of perigee, ...	± 1,5
Greatest error, from error in greatest equation of centre, ...	± 0,8
Greatest error, from the combination of these, ...	... ± 1,7
Greatest error, from error in mass of Venus, ...	... ± 1,5
Greatest error, from error in mass of Mars, ...	... ± 1,9
Greatest error, from error in mass of the Moon, ...	... ± 1,0
Greatest possible negative error, ...	— 11″,7
Greatest possible positive error, ...	... + 0″,5

These conclusions agree, upon the whole, with the results of a similar comparison made by M. Burkhardt, founded on Maskelyne's observations from 1774 to 1810. The principal difference, is in the diminution due to the mass of Mars, which by M. Burkhardt is $\frac{1}{20}$; but Mr. Airy thinks that his conclusion is sufficiently well established, to be entitled to confidence. In the motion of the perigee too, there is some difference: and upon the whole it appears, that this latter is of such an irregular character, as to require the introduction of some yet undiscovered inequality, of the form $a. \sin (b\,\theta + c)$, where θ is the sun's mean longitude, and b a coefficient differing very little from unity. In a Postscript to the paper, he finds that in consequence of the action of Venus, the Earth's motion in longitude is affected with an inequality for which the expression, taking the mass of Venus as determined in this paper, is

2″,6 × Sin (8 × mean Long. of Venus—13 × mean Long. of Earth + 39°. 57′)

The period of this inequality is about 240 years. "This term," he says, "completely accounts for the difference in the secular motions, given by the comparison of the epochs of 1783 and 1821, and by that of the epochs of 1801 and 1821." We must refer to the paper for further details.

IV. *Experiments to determine the difference in the length of the seconds Pendulum in London and in Paris. By Captain* Edward Sabine, *R. A. and Sec. R. S. communicated by* Thomas Young, *M. D. For. Sec. R. S. and Sec. Bd. Long. pp.* 35 *to* 77.

"The length of the pendulum vibrating seconds, having been measured in London by the method and apparatus of Kater, and in Paris by those of Borda and Biot; and the standards of linear measure of the two countries, having been referred respectively to those measurements for future verification; an endeavour was made by M. Arago, in 1817 and 1818, at the instance of the "Bureau des Longitudes," to bring the lengths so measured into direct comparison with each other, by ascertaining, by means of invariable penduhims conveyed intermediately between Paris and London, the difference of length that actually exists between the pendulums at those places; which difference ought also to be that between the absolute measurements."

Captain Sabine made use of an invariable pendulum, which had been prepared under his directions for M. Schumacher, and which that gentleman had placed at his disposal for the purpose. The Board of Longitude added a second, which had been made for them, at the same time as M. Schumacher's, and has been since supplied at the request of the Russian Government to Captain Lütke of the Russian Navy, employed on a voyage to the Pacific ocean. He was assisted in Paris, by M. Mathieu, M. Nicollet, M. Savary, and Captains Freycinet and Duperry; in London by M. Quetelet and Captain Chapman, R. A.

The result of all the experiments is, that one of these pendulums performed 85933,83 vibrations in a mean solar day in Paris, and 85945,80 in London. The other 85921,22 and 85933,30 in the temperature of 60° and at the level of the sea. The acceleration is in one case 12.03, in the other 12.08 vibrations; mean 12.05: by selection the acceleration may be reduced to 11.93 vibrations.

Now the length of the seconds pendulum in Mr. Browne's house, London, by Kater's measurement is 39.13908 inches; and in the Salle de la Meridienne in the Observatory of Paris, by Biot's measurement 39.12843 inches. The difference corresponds to an acceleration of 11.76 seconds.

The difference in length of the pendulums corresponding with an acceleration of 12 seconds applied to Captain Kater's measurement, would give for the length of the seconds pendulum in Paris 39,12820 instead of 39,12843, as found by Biot. The difference is ,00023$=\frac{1}{4000}$ of an inch, a proof of the extreme accuracy with which each of the observers must have conducted his labours. Borda's determination in 1792 was 39,12776, a result differing ,00067 inch from Biot's, and ,00044 from Kater's. The meau of Borda's and Biot's is 39,1281, differing only $\frac{1}{10000}$ of an inch from Kater's.

In a postscript to this paper, Captain Sabine notices some trifling errors which had been pointed out to him by Captain Kater in his former paper on the length of the seconds pendulum, (Ph. Tr. 1827, Art. ix); he gives the amount of the error, and a revised list of his determinations of the length of the pendulum in various latitudes, which is as follows:

	Lat.	Length of Pend.
St. Thomas,	0°. 24′, 7 N	39.02074
Maranham,	2. 31, 6 S	39.01213
Ascension,	7. 55, 2 S	39.02410
Sierra Leone,	8. 29, 6 N	39.01997
Trinidad,	10. 38, 9 N	39.01888
Bahia,	12. 59, 3 S	39.02433
Jamaica,	17. 56, 1 N	39.03503
New York,	40. 42, 7 N	39 10120
London,	51. 31, 1 N	39.13929
Drontheim,	63. 26, 0 N	39.17456
Hammerfest,	70. 40, 1 N	39.19475
Greenland,	74. 32, 3 N	39.20335
Spitzbergen,	79. 49, 9 N	39.21469

V. *On the measurement of high temperatures. By* James Prinsep, *Esq. Assay Master of the Mint at Benares. Communicated by* Peter Mark Roget, *M.D. Sec. R.S.*

The author of this paper justly comments on the deductions in which the chemists and natural philosophers of Europe appear to have hitherto reposed more confidence than they deserved. We mean the table of melting points, deduced from the indications of Wedgewood's pyrometer, which continues to be gravely copied from one system into another. We will venture to say, that no inquiring student ever received this table of results as other than purely imaginary, notwithstanding the high names that appear to have sanctioned it by adoption, and the reputation which the very ingenious author of the pyrometer so justly enjoyed. In England Mr. Daniel, it appears, had the merit of first attracting attention to the subject, and he succeeded in showing, that the numbers till then received as expressing the degree of heat at which some of the metals entered inso a state of fusion, were so erroneous, as to prevent any confidence being placed in the indications of Wedgewood's pyrometer.

Daniel's pyrometer, through a great improvement on the former, was still liable to some objections. Platina, the metal, the expansion of which is used to measure the degree of heat, has so low a rate of expansion that a very minute error in its estimated length would cause a considerable one in the deduced temperature. Add to which, that this little is diminished by the expansibility of the enclosed case of black lead. It may be doubted too, whether the expansibility of either the platina or black lead follows any law so regular, as to allow us to extend the scale, fixed by comparisons made at low temperatures, to very high ones. Indeed this objection is uot founded on mere conjecture; for MM. Dulong and Petit have shown very satisfactorily, that no metal has the same rate of expansion at the higher temperatures as at the lower. It is further objected to Daniel's pyrometer, that plumbago of which the case is formed is a very bad conductor of heat, and is also liable to lose its shape.

There are two objects to which a pyrometer may be supposed to be applicable; that of indicating, by similar phenomena, equal degrees of temperature, and that of referring these temperatures to the more familiar and better known scale of Fahrenheit's thermometer. The first constitutes the real value of the pyrometer; the second is a question of curiosity. We think each of these problems has been resolved very happily by the author of the paper before us.

To have a pyromcter that shall be universally comparable, that is, that shall in similar temperatures develop similar phenomeua; we have only to provide ourselves with small portions of metallic alloys of different degrees of fusibility, which being exposed in a cupel in any part of the furnace where we wish to know the temperature, the particular alloy that melts will indicate, so far, the value of the temperature; that we shall be sure always to have the same temperature (withiu certain limits) where this alloy melts. But we will allow the author to speak for himself.

"The fusing points of pure metals are determinate, and unchangeable, they also comprehend nearly the whole scale of temperature; the non-oxydable metals alone embrace a range from the low melting point of silver to the high ignition of platina. There are, it is true, only three fixed points in this scale, but as many intermediate links may be made as are required, by allowing the three metals together in different proportions. When such a series has been once prepared, the heat of any furnace may be expressed by the alloy of least fusibility which it is capable of melting. Besides the uniformity of the determinations which such a pyrometer would give, several other advantages might be enumerated:—the smallness of the apparatus, nothing more being necessary than a little cupel containing, in separate cells, eight or ten pyrometric alloys, each of the size of a pin's head; the indestructibility of the specimens, since those melted in one experiment would need only to be flattened under the hammer to be again ready for action; and the facility of notation, since three letters, with the decimal of the alloy, would express the maximum heat. Thus Pyrom. S. ,3, G. might be used for an alloy of 0,7 silver with 0,3 gold, and G. ,23 P. would express gold containing 23 per cent. platina.

"As gold melts at a heat not very much above silver, I assumed only 10 degrees between them, measuring each by a successive addition in the alloy of 10 per cent. of gold to the pure silver, the tenth degree being of course measured by pure gold. These alloys are easily made, and require no comment: in accurate researches, they may be further subdivided, using always the decimal notation.

"From the fusion of pure gold to that of pure platina, I assumed 100 degrees, adding 1 per cent. of the latter metal to the alloy, which measured each successive degree. Now it is hardly to be supposed, that the progress of these hypothetical degrees represent equal increments of heat; they will, however, as I before observed, always indicate the same intensity; and their absolute value, as a matter rather of speculative than of practical interest, is to be sought by other expedient."

The foregoing extract sufficiently explains the principle of Mr. Prinsep's pyrometer. We are sorry, we can only find room for the naked results, and must pass over a great deal of interesting discussion on points of collateral interest. We strongly recommend the paper to the perusal of those at all interested in pyrometrical questions.

	Max. Alloy Melted
Muffle of an assay furnace, front,	S ,0 G.
Muffle of an assay furnace, middle,	S. ,3 G.
Muffle of an assay furnace,	S. ,5 G.
The Calcutta charcoal is better than the Benares, and frequently heats the muffle to	G. ,03 P.
Melting point of copper by two trials under the muffles,	G. ,04 P.
Calcutta silver melting furnaces of the English construction,	G. ,075 P.
Black lead table furnace, without chimney,	G. ,08 P.
Calcutta open native furnace,	G. ,06 P.
Calcutta blast furnace for melting musters,	G. ,20 P.
Apex of condensed air blow-pipe flame,	G. ,20 P.
Melting point of cast iron, about	G. ,30 P.
Highest heat of a forge with the Benares charcoal,	G. ,55 P.

The second object of pyrometrical inquiries, the referring any fixed point of high temperature to Fahrenheit's scale has, we think, been equally happily effected by this experimentalist. The air thermometer has been proposed as a means of measuring high temperature, and if made with a bulb of platina it would embrace the whole of the scale of heat, and include the highest temperatures we can command. Dr. Ure proposed such a thermometer in his Chemical Dictionary, and we believe it has been executed in England. In India there was available neither a sufficient quantity of the metal, nor had that been at hand were there any facilities for working it: gold was then the only alternative*, and this necessarily confined the experiments to indications below the fusing point of that metal, and consequently but a little above the melting point of silver. For the arrangement of the apparatus, we must refer to the paper, as our extracts have been already more than our limits can justify. They appear to us to have been every way unexceptionable: the scale of the thermometer was verified occasionally by immersing it in boiling water, so that there can be scarcely any uncertainty in the results established by these experiments. The method of calculating the indications of the scale from the bulk of air displaced,

* The bulb of this thermometer contained 1000 Rs. worth of gold.

will be sufficieutly obvious to those acquainted with the general principles of such physical inquiries. It may be sufficient to state that it has been assumed, as generally received, that air having a bulk of I at any temperature t becomes 1,375 at any temperature $t+180°$. A very full detail of all the experiments, and of the methods of calculation, is given in the paper, to which we must now finally refer, and conclude with the table of results deduced.

Average Results.

Full red heat,	1200°	
Orange Heat,	1650	
Silver melts,	1830*	Daniel 2233° Wedgewood 4777°
S.,1 G.	1920	
S.,25 G.	2050	

VI. *On Captain* Parry's *and Lieutenant* Forster's *experiments on the velocity of sound. By Dr.* Gerard Moll, *Prof. Nat. Ph. University Utrecht, communicated by Captain* Henry Kater, *V. P. R. S.*

It has been long known, that Newton's expression for the velocity of sound does not agree with observation. For while the former gives the velocity about 980 feet in the second, the latter shows it to be nearly 1100. Laplace has endeavoured to improve the formula, by introducing a coefficient having reference to the elasticity developed by the latent heat which is set free at each undulation of the air, by its cousequent condensation. The value of this coefficient has been taken from the experiments of Messrs Gay Lussac and Welter. Even with this correction, experiment and theory differ 4 to 6 metres or 13 to 19 feet. This difference has been attributed, partly to the uncertainty of the above coefficient, partly to the errors of experiment.

The object of the present paper is to show, that the latter error can in no way be considered to account for this difference, and that the source of the error, if not in the uncertainty of the coefficient, the value of which is derived from M. Gay Lussac and Welter's experiments, must be sought elsewhere†. Dr. Moll had, in concert with Dr. Overbeck, carried on a series of the most elaborate experiments on this subject yet performed, an account of which was published in the Ph. Trans. for 1824.—Captain Parry and Mr. Forster had also conducted a series of similar experiments at Port Bowen, in lat 73° 13′ 39″. On reducing the results of each series to the same conditions of gravity of pressure and temperature, he finds the followiug values established, as the number of metres travelled by sound in 1 second of time.

			Metre
Captain Parry and	Lieut. Forster,	4th June, 1825,	333. 15
Do.	Do.	17th and 21st February, 1825,	333. 71
Do.	Do.	22d March, and 3d June,	332. 85
Dr. Moll and Dr. Van Beek's,			332. 05

Consequently the difference amongst experiments, conducted under such very different circumstances, is here much less than the discrepancy of theory and observation.

The following results may also be compared.

Messrs. Stampfer and Von Myrbach, in 1822,	333.25
MM. Arago, Mathieu, and Biot,	331.05
M. Benzenberg, in Germany,	333.7
MM. Epinoza and Bauza in Chili,	356.14
Dr. Olinthus Gregory, in England,	335.14
The French Academicians, in 1738,	332.93

* Guyton Morveau (Ann. de Ch.) gives 1822°,7. His pyrometer was a bar of platina, the expansion of which was measured by a multiplying index. The melting point of gold he fixes at 2517° 6.

† In a very ingenious paper by a Mr. Meikle, in a late number of Brewster's Journal, it is remarked, that while philosophers have affected to correct for the increase of elasticity, occasioned by the liberated caloric in the condensation of the pulses, they have altogether neglected the larger and more obvious source of error, occasioned by the increase of elasticity due to the heat liberated by the firing of the gunpowder.

VII. *An account of a series of experiments made with a view to the construction of an achromatic telescope, with a fluid concave lens, instead of the usual lens of flint glass. In a letter addressed to* Davies Gilbert, *Esq. M. P. Pres. R. S. By* Peter Barlow, *Esq. F. R. S.*

The subject of fluid object glasses has now been long before the public, though as yet the practical trials do not appear to have been sufficiently encouraging to have prompted the construction of any really effective instruments. Mr. Barlow has been for some time engaged in a series of experiments on this subject, which originated in an attempt to apply to practice the rules given by Mr. Herschel in the Phil. Trans. 1821, and a conviction that flint glass of the requisite size and purity could not be procured without great difficulty. He has succeeded in constructing two instruments of that description, the one of 3 inches aperture, the other of 6 inches. Mr. Barlow's views have been so far different from Dr. Blair's, (the first proposer of fluid object glasses,) that in the telescopes of the latter it was still necessary to retain the flint lens, his only object being to destroy what has been named the secondary spectrum, due to a want of proportionality between the coloured spaces of the spectra of flint and plate or crown glass, as compared with their respective refractive indices, whereas the design of the latter was to dispense altogether with the flint glass, by substituting in its place a fluid medium of the requisite refractive and dispersive power.

Mr. Barlow was led after many experiments to select sulphuret of carbon, which has a refractive index about equal to that of the best flint glass with a dispersive power more than double. "Perfectly colourless, beautifully transparent, and possessing the same optical properties, when hermetically sealed, under all temperatures, to which it is likely to be exposed for astronomical purposes, (unless indeed it should be found that direct observations on the solar disc are inadmissible,) its high dispersive power gives it an advantage which no glass ever made or likely to be made can possess, though the fixed nature of the latter may probably always give it a preference in the construction of telescopes."

Having succeeded, after many trials, in confining the fluid, Mr. Barlow attempted a telescope of 6 inches aperture; but after some unsuccessful trials he was induced to confine himself to 3 inches aperture. With this instrument, though by no means perfect, he separated a great number of Sir W. Herschel's stars, which have been noted as tests of a good 3½ inch refractor. Thus the "small star in Polaris is visible, as also 70 p Ophiuchi, 39 Bootis, the quadruple star ε Lyræ, ζ Aquarii, α Herculis, &c. Encouraged by this success he was led again to attempt the 6 inch aperture object glass, and the result of his endeavours has been highly satisfactory With this instrument the small star in Polaris is so distinct and brilliant with a power of 143, that its transit might be taken with the utmost certainty. The small stars in α Lyræ, Aldebaran, Rigel, ε Bootis, &c are very distinctly exhibited. Amongst the larger close double stars Castor and γ Leonis are well defined with a power of 300, and amongst the smaller double stars ω Aurigæ, 52 Orionis, ζ Orionis, &c. The belts and double ring of Saturn are well exhibited with a power of 150, and the belts and satellites of Jupiter are very tolerably defined with the same power."

"In the usual construction of achromatic telescopes, the two or three lenses composing the object glass are brought into immediate contact, and in the fluid telescope proposed by Dr. Blair, the construction was the same, the fluid having been enclosed in the object glass itself; nor could any change in this arrangement in either case be introduced with advantage, because the dispersive ratio between the glasses in the former instance, and between the glass and fluid in the latter, is too close to admit of bringing the concave correcting medium far enough back to be of any sensible advantage. The case, however, is very different with the sulphuret of carbon. The dispersive ratio here varies (according to the glass employed) between the limits 299 and 334, which circumstance has enabled me to place the fluid correcting lens at a distance from the plate lens equal to half its focal length, and I might carry it still further back, and yet possess sufficient dispersive power to render the object glass achromatic. Moreover, by this means the fluid lens, which is the most difficult part of the construction, is reduced to one half or to less than one half of the size of the plate lens, consequently to construct a telescope of ten or twelve inches aperture involves no greater difficulty in the manipulation, than in making a telescope of the usual description of 5 or 6 inches aperture, except in the single plate lens itself; and, what will be thought perhaps of greater importance, a telescope of this kind, of ten or twelve feet length, will be equivalent in its focal power to one of 16 or 20 feet.

The paper concludes with an analytical investigation, investigating the conditions required to correct the secondary spectrum.

VIII. *A catalogue of nebulæ and clusters of stars in the southern hemisphere, observed at Paramatta, in New South Wales, by* James Dunlop, *Esq. In a letter addressed to Sir* Thomas Makdougal Brisbane, *Bart. K. C. B. late Governor of New South Wales. Presented to the Royal Society, by* John Frederic William Herschel, *Esq. Vice-President.*

The title is all we can afford room for of this paper.

IX. *An account of trigonometrical operations in the years* 1821, 1822, *and* 1823, *for determining the difference of longitude between the Royal Observatories of Paris and Greenwich.* By Captain Henry Kater, *V. P. R. S.*

In this paper, though on a subject which many will think has been almost exhausted, we meet with those original views and ingenious suggestions which distinguish every thing that comes from the pen of Kater. We shall endeavour to give our readers an idea of them.

In the year 1790, a series of trigonometrical operations was carried on by General Roy, in concert with MM. De Cassini, Mechain, and Legendre, for connecting the observatories of Paris and Greenwich. In 1821, the Royal Academy of Sciences and the Board of Longitude at Paris communicated to the Royal Society of London their desire, that these operations should be repeated by both countries. On the part of England Captains Kater and Colby, and on that of France MM. Arago and Mathieu were appointed to conduct them. Mr. Gardner, from the ordnance survey of England, was attached to them.

" The signals used for connecting the stations upon the coasts of England and France, were lamps with compound lenses constructed under the direction of M. Fresnel. It will be sufficient here to mention, that the lens, composed of numerous pieces, was three feet in diameter, and that the light far exceeded that of any of our lighthouses, appearing at the distance of 48 miles like a star of the first magnitude."

It was in the course of these operations that the great improvement was suggested by Captain Kater of attaching numerous microscopes to a circle; in preference to changing the zero, when a second or third value of the angle was desirable for the purpose of diminishing the errors of divisions, as had been till then the practice. He had four additional microscopes applied to General Roy's theodolite, which, with one of the two that belonged to it, made five*. The other was allowed to remain, for the purpose of comparing the mean of two opposite readings, with that of five. We had an opportunity of hearing of this great improvement at the time or soon after the ingenious author suggested it; and we thought then, as we do now, that it is one of the greatest steps yet made towards improving small instruments Nor can we refrain from expressing our astonishment, that the instrument makers have not yet adopted it. Perhaps it is too searching a proof to subject their workmanship to; but this is an additional reason for the purchasers insisting on having it. Except the repeating property, we do not recollect an equally important improvement, and we doubt if this latter does not yield to it.

At the station at Severndroog Castle they " experienced considerable difficulty in obtaining the requsite angles with Hanger Hill, as the signal erected upon that tower was seen only once, in consequence of the intervening smoke of London. At length Col. Colby thought of a method by which this difficulty was overcome. Tin plates were nailed to the staff upon Hanger Hill tower, the plates being disposed above each other in certain angles so as to reflect the sun's rays to Severndroog. This contrivance, which answers the purpose in a certain degree of the heliostat of Professor Gauss was perfectly successful; each plate gave in succession a neat image of the sun resembling a fixed star, which was seen through a smoke so thick that even the hill was invisible."

Of the three known methods by which small triangles on the surface of an ellipsoid may be resolved; i. e. 1. Spherical trigonometry, 2. The method of chords, and 3. That of Legendre depending on the spherical excess; the latter is adopted. We cannot help thinking, that the first is very undeservedly neglected in general by geodests. DeLambre has shown its facilities and a method of converting the logarithm of a distance in feet into the logarithm of a sine. The question is of course merely one of facilities, as they are all equally accurate; Legendre's being perhaps the most elegant.

* It requires a prime number, or, in turning the instrument half round, the microscopes would come on the same divisions again.

In the course of the survey several determinations were made of distances measured by General Roy. The following table contains the particulars of the comparison. General Roy's distances are corrected for the difference of Captain Kater's and the imperial standard. It is further to be noted, that the differences are solely attributable to differences in the measurement of the angles; as the distance of Hanger Hill from Severndroog Castle, the first side of the present triangulation, is assumed to be as determined by General Roy. We have added a column giving in round numbers the ratio of the differences.

	General Roy.	Captain Kater.	Diff.	Ratio.
Fairlight to Frant,	113850.6	113857.3	6.7	$\frac{1}{17000}$
Fairlight to Tenterden,	71577.2	71580.8	3.5	$\frac{1}{20000}$
Fairlight to Folkstone,	154802.7	154807.0	4.3	$\frac{1}{36000}$
Dover to Notre Dame, Calais,	137459.4	137472.0	12.6	$\frac{1}{11000}$
Notre Dame, Calais, to Fiennes,	45222.7	45221.0	1.7	$\frac{1}{26000}$

The distances of the several stations from the meridian and perpendicular of Greenwich being calculated; Captain Kater proceeds to the determination of the difference of longitude and latitude. For this purpose he thinks the most convenient formulæ yet proposed, are those given by Oriani, in his Opusculi Astronomici, published at Milan in 1826. They are as follows.

Let a = semi major axis of the Earth.

b = semi minor axis.

e = the eccentricity $= \left(\frac{a^2-b^2}{a^2}\right)^{\frac{1}{2}}$

M = the distance in feet from the perpendicular to the meridian, at Greenwich.

P = the distance in feet from the meridian of Greenwich.

$m = \frac{M}{b \text{ Sin } 1''}$

$p = \frac{P}{b \text{ Sin } 1''}$

L = the latitude of Greenwich.

λ = the latitude of the foot of the perpendicular let fall from the given station on the meridian of Greenwich.

ϕ = the required latitude of the given station.

u = the required longitude of the given station.

Then 1. $\lambda = L \pm m \left(1-e^2+\frac{3}{2} e^3 \text{ Cos.}^2 (L \pm \frac{m}{2})\right)$

2. $\psi = p\,(1-e^2 \text{ Sin}^2\lambda)$

3. $\text{Sin } \phi = \text{Sin } \lambda \text{ Cos. } \psi$

4. $\text{Tan. } u = \frac{\text{Tan.} \psi}{\text{Cos. } \lambda}\left(1-\frac{e^2}{2}\text{Cos.}^2 \lambda\right)$

In attempting to determine the value of the perpendicular degree, by means of observed azimuths and co-latitudes; a result is obtained which "must be erroneous about one hundred fathoms." It is shown, that to account for such an error, it is only necessary to suppose the azimuths erroneous to 2″, a degree of accuracy which Captain Kater thinks cannot be predicated of any azimuth determined by observation of the pole star. Any error of the level would be increased; and besides the ordinary errors attributable to the level there may be irregularity of local attraction, such as at Arbury occasioned the plummet of the sector to be drawn 5½″ out of the vertical. Any slight uncertainty of polar distance would be doubled in the azimuth, to which if we add lateral refraction affecting the position of the object to which it is referred, we shall agree with Captain Kater, that observations of the pole star are totally unworthy of credit, at least in the latitude of England. He considers observations of a star when near the east or west point of the horizon as more eligible, using the time for the calculation of the azimuth. The alteration of the azimuth, however, from a variation in the refraction, should be taken into account; and therefore the altitude of the star must be obtained. It would not be "necessary to observe the star when *very* near the horizon, as the error in the azimuth arising from the level decreases as the tangent of the altitude; and at an elevation of 12°, is scarcely more than two tenths of the error in the horizontality of the axis of the telescope."

In determining the differences of level of the several stations, the refraction was found to vary from $\frac{1}{11}$ to $\frac{1}{13}$ of the contained arc the mean value being $\frac{1}{12}$.

The result of the whole is, that Calais is east of Greenwich 1°. 51′. 18″,7. and adding 0°. 28° 59″ the difference of Calais and Paris, we obtain 2°. 30′. 17″.7. or 9m. 21,18s. as the difference of longitude of Paris and Greenwich; "differing 28s. in defect, from the admirable results obtained by the operations with fire signals (Phil. Trans. 1826) by Mr. Herschel."

It is proposed, at some future period, to repeat the measurement of the base. At present, the truth of the above result depends on the accuracy with which General Roy's operations were conducted.

In an appendix, Captain Kater throws together such remarks, as could not very well be introduced in the body of the work.

On the subject of applying five microscopes instead of the former pair, he enters into considerable detail; and shows this method to be preferable to taking the angle on various parts of the limb. With one reversal, readings are obtained on 10 different parts of the limb; which would be equivalent to changing the zero (using 2 microscopes) five times. He states Mr. Pond to be the original proposer of this very important suggestion. As the second original microscope was not removed, it afforded an opportunity of comparing the results by the two methods. We shall note some of these differences. The greatest difference of the mean of 10 microscopes, direct and reversed observation, is 4″,1 that is, when confined to the same divisions; but if the divisions be changed, the difference goes to 7″,0. These differences are, it may be well supposed, generally much less; but it is the knowledge of the extreme differences that is useful, as enabling the observer to say what he can calculate on. The first difference 4″,1 must be due either to the imperfection of the observation, or to some uncertainty of the microscopes, or reading of them, or lastly, and this appears to be Captain Kater's view, to lateral refraction. The difference of 7″,0 and 4″,1 = 2″,9 must be, we suppose, the effect of errors of division, and must therefore represent the amount of the sum of 40 errors + and —. The greatest difference in the results of 5 and 2 microscopes (two observations each) is 7″,8. In general it is scarcely 2″ and when the observations are taken on different points of the limb, the mean result deduced from 5 microscopes and from 2 scarcely differ 1″ on an average, certainly not more. We may draw one conclusion from these details, if observations on different parts of the limb of this instrument, which is 3 feet diameter, on a mean of 10 microscopes, differ 7″,0; then on a circle 1 feet in diameter, with only 3 verniers, they may well differ 7″,0 × 3 = 21″ which as such a circle is divided to 720° = 42″.*

In the course of this work Captain Kater remarked a curious fact, which was new to him. "In hazy weather when the staff was so faint as to be only just visible, it disappeared upon bringing it to the intersection of the cross wires, so that the angle, could not be observed. A remedy for this inconvenience was suggested and put in practice by Mr. Gardner. The horizontal spider's web of the micrometer being moved about the centre, Mr. Gardner succeeded in lodging upon it a particle of dust. When the image of the staff was brought to this, it appeared as if planted upon a mole hill, and we were thus enabled to observe with great accuracy. I consider this as a very important improvement in the theodolite; and we availed ourselves of it upon all occasions, excepting in the observations of the pole star."

Some trials were made by the two methods of intersection, and it was found that with the dot the facility was nearly twice as great, as measured by the time taken to make ten observations. The precision too was greater, as with the dot the extreme difference from the mean was 4″, while with the cross wires it was 7." Captain Kater considers these quantities as the maximum values of the errors due to the telescope and microscopes.

Much stress has been laid on an objection, often made, as to the unequal expansion of the limb of a circle. It was satisfactorily determined, that though the readings of the different microscopes, one being assumed as a standard, were different on disturbing the equilibrium of temperature, yet the mean appeared to be always the same: this is an additional reason for using a number of microscopes.

* The difference has been found by actual examination to be about 40.″

When however the change of temperature affected the supports of any of the microscopes, the case was different; and this shows the importance of securing the microscopes in the most unexceptionable manner. Captain Kater suggests that in small instruments they should be fixed to a solid plate of metal; or if attached to radii or supports, that these should be covered with flannel, so as to prevent sudden and partial changes of temperature.

Another source of error is mentioned by Captain Kater, which is familiar to us, and which we have often had occasion to observe. It arises from the spring or resistance of the parts connected with the motion of the circle on its centre. It was found, for instance, that when the tangent screw was moved in opposite directions, a difference of 3″ was found in the readings. And it was only by a kind of shaking motion, the screw being moved backwards and forwards by little jerks, that a proper consistency in the readings was established. This is the error no doubt that has been attributed, in instruments of reflection, to the bending of the index—a thing we will venture to say impossible.

We have already noticed that Captain Kater considers the reality of lateral refraction as having been fully established. "The angle between the same objects would differ under the most favourable circumstances about 5″ on different days, and perhaps 1″½ or 2″ may be considered as the error which may affect an angle from lateral refraction in the ordinary state of the atmosphere."

The last source of error treated of is the same as that which occasions what is called the variation of "the run" of the microscopes. Captain Kater proposes, instead of moving the wire of the microscope, to have the wire itself fixed and the body of the microscope moved by the micrometer screw. In this case no error can arise from a variation of the distance between the limb of the instrument, and the object glass of the microscope. It possesses also this further advantage, that the object glass may be changed and the power of the microscope varied at pleasure without affecting the scale. Any error too, arising from the want of strict perpendicularity of the plane of the circle to the axis of motion, would be obviated.

The paper terminates with tables of all the observations both of angles and azimuths; and a plate is given showing the disposition of the triangles.

X. On the phenomena of volcanoes. By Sir Humphry Davy, *Bart F. R. S. pp.* 241 *to* 250.

The grand discovery which has immortalized the name of Davy—the metallic nature of the bases of the alkalis; must have suggested to many the great support which these views afford to the geological theory of Hutton. Combined with the beautiful results which Sir James Hall has established, we think the fact of the combustible nature of the bases of the earths and alkalis throws a powerful light on what were considered the obscure or the weak points of that theory. In particular, the difficulty of the central fire which in the usual spirit of controversy was so much exaggerated has now ceased to be urged as an objection; and when we see potassium burning on the surface of water, we can be at no loss to account for the heat, the existence of which, at some particular period, the geological appearances sufficiently proved. But neither this proof, nor the ocular demonstration which the phenomena of volcanoes afforded, could convince the devotees of system; and till it could be shown how and whence the heat should originate, they were determined to uphold their objections. Neither is the want of air to support such a combustion any longer a difficulty, when we know of metals which have such an attraction for oxygen as to take it from water, and with such rapidity, as to become intensely heated from the violent chemical action. These objections which, though not vital to the theory, (for it was sufficient to show that we had proofs of such intense igneous action having actually occurred, without being obliged to show *how* it originated) were yet so often and so imposingly brought forward, that we must feel it is a great step to have completely silenced them. We now find it received as an established position, that the oxydation of the several metalloids by contact with water is not only sufficient to account for the phenomena of volcanoes, but even for the formation of the trap rocks; supposing observation to have established the probability of their igneous origin. This great discovery, which we owe to Sir Humphry Davy, throws a beautiful light on the whole compass of geological phenomena, and lends probability and a coherence to what was till then supposed by many to be the mere wanderings of imagination.

"The metals of the alkalis and those of such of the earths as I had decomposed, were found to be highly combustible, and altered by air and water even at the usual temperatures of the atmosphere: it was not possible, consequently, that they should be found at the surface of the globe, but probable that they might exist in the interior; and allowing this hypothesis, it became easy to account for volcanic fires by exposure of the metals of earths and alkalis to air and water, and to explain, not only the formation of lavas, but likewise that of basalts and many other crystalline rocks, from the slow cooling of the products of combustion or oxydation of the newly discovered substances."

This opinion was given by Sir H. Davy in 1808, and he has since 1812 endeavoured to collect such evidence as might throw light on it. Vesuvius is the mountain which he has principally examined, and the present paper contains the parculars he has collected.

"One of the most important points to be ascertained was, whether any combustion was going on at the moment the lava issued from the mountain." To determine this point some of the fused lava was put into a glass bottle furnished with a ground stopper to exclude the external air; and on examining the air within the bottle, sometime after, no diminution was found to have taken place.

He threw upon the surface of the lava nitre in mass and in powder; no appearance occurred similar to what takes place when nitre is thrown on combustible matter raised to a high temperature. Some fused lava was thrown into water, and the disengaged gas collected. It was in very minute quantity, and when analysed proved to be common air almost pure. "A wire of copper $\frac{1}{20}$ of an inch in diameter and a wire of silver of $\frac{1}{30}$ inch introduced into the lava near its source were instantly fused. An iron rod of $\frac{1}{5}$ of an inch, and a piece of iron wire of about $\frac{1}{30}$, were kept for 5 minutes in the eddy in the stream of lava, but were not melted. They did not produce any smell of sulphuretted hydrogen when acted on by muriatic acid."

"A tin plate funnel filled with cold water, was held in the fumes disengaged with so much violence from the aperture through which the lava issued: fluid was immediately condensed upon it, which was of an acid and subastringent taste. It did not precipitate muriate of baryta, but copiously precipitated nitrate of silver and rendered the triple prussiate of potassa a bright blue. When the same funnel was held in the white fumes above the lava where it entered the bridge, no fluid was precipitated upon it, but it became coated with a white powder which had the taste and chemical qualities of common salt, and proved to be this substance absolutely pure."

"A bottle of water holding about $\frac{3}{4}$ pint, with a long narrow neck, was emptied immeditely in the aperture from which the vapours pressing out the lava issued, and the neck was immediately closed. This air examined on my return was found to give no absorption with solution of potassa, so that it contained no notable proportion of carbonic acid, and it consisted of 9 parts of oxygen and 91 of azote. There was not the least smell of sulphurous acid in the vapour from the aperture, nor were the fumes of muriatic acid so strong as to be unpleasant; but during the last quarter of an hour that I was engaged in these experiments the wind changed and blew the smoke from the crater upon the spot where I was standing: the sulphurous acid gas in the fumes was highly irritating to the organs of respiration, and I suffered so much from the exposure to them that I was obliged to descend."

In another visit Sir Humphry Davy made similar observations, with similar results. Neither pure silver nor platinum were at all changed in colour when exposed to the fused lava. The sublimations were collected from various parts of the cooled lava; they were of a white yellow and reddish colour, and consisted chiefly of common salt, chloride of iron, and sulphate of soda. There was also in small quantity sulphate and muriate of potassa, oxyde of copper, and muriate of cobalt. The same observations nearly are repeated in the details of other visits, with the addition of particulars relating to other phenomena, such as the subterraneous noises, the showers of stones, &c. which we regret we have not room to dilate on, and we must hasten at once to his conclusions.

"I shall now offer some observations on the theory of these phenomena. It appears almost demonstrable that none of the chemical causes anciently assigned for volcanic fires can be true. Amongst these the combustion of mineral coal is one of the most current, but it seems wholly inadequate to account for the phenomena. However large a stratum of pit coal, its combustion under the surface could never produce violent and excessive heat; for the production of carbonic acid gas, when there was no free circulation of air, must tend constantly to impede the process; and it is scarcely possible that carbonaceous matter, if such a cause existed, should not be found in the lava, and be disengaged with the saline or aqueous products from the bocca or craters. There are many instances in England of strata of mineral coal which have been long burning, but the results have been merely baked clay and schists, and it has produced no results similar to lava."

"If the idea of Lemery were correct, that the action of sulphur on iron may be a cause of volcanic fires, sulphate of iron ought to be the great product of the volcano, which is known not to be the case; and the heat produced, by the action of sulphur on the common metals, is quite inadequate to account for the appearances. When it is considered that volcanic fires occur and intermit with all the phenomena that indicate intense chemical action, it seems not unreasonable to refer them to chemical causes. But, for phenomena upon such a scale, an immense mass of matter must be in activity, and the products of the volcano ought to give an idea of the nature of the substances primarily active. Now what are these products? mixtures of the earths in an oxydated and fused state, and intensely ignited; water and saline substances such as might be furnished by the sea; and air altered in such a manner as might be expected from the formation of fixed oxydated matter: but it may be said, if the oxydation of the metals of the earths be the causes of the phenomena, some of these substances ought occasionally to be found in the lava, or the combustion ought to be increased at the moment the materials passed into the atmosphere. But the reply to this objection is, that it is evident that the changes which occasion volcanic fires take place in immense subterranean cavities; and that the access of air to the acting substances occurs long before they reach the exterior surface."

"The same circumstances, which would give alloys of the metals of the earths the power of producing volcanic phenomena, namely, their extreme facility of oxydation, must likewise prevent them from ever being found in a pure combustible state in the products of volcanic eruptions; for before they reach the external surface they must not only be exposed to the air in the subterranean cavities, but be propelled by steam, which must possess, under the circumstances, at least the same facility of oxydating them as air. Assuming the hypothesis of the existence of such alloys of the metals of the earths as may burn into lava in the interior, the whole phenomena may be easily explained from the action of the water of the sea and air on those metals; nor is there any fact, or any of the circumstances which I have mentioned in the preceding part of this paper, which cannot be easily explained according to that hypothesis."

The hypothesis of a central fire is alluded to in the conclusion, as also sufficient to account for the phenomena; and Sir Humphry Davy thinks the increasing temperature of the earth, as we descend, affords some probability of the existence of such a central heat.

XI. Abstract of a Meteorological Journal kept at Benares, during the years 1824, 1825, *and* 1826. *By* James Prinsep, *Esq. Assay Master of the Mint at Benares, communicated by* Peter Mark Roget, *M. D. Sec. R. S.*

A great deal of useless trash is annually, quarterly, monthly, and occasionally, inflicted on the public under the title of Meteorological Registers. Every one wishes to be a meteorologist, every one observes and records; but no one will submit to the labour of reducing his observations, and extracting from them any useful results.

The present paper is an example of what a meteorological register ought to be. In a single page are comprised the mean results of observations made twice a day for a whole year. Three pages then contain the three years' results.

From these we learn that the annual mean of the several particulars is as follows.

Year.	Barometer.	Thermometer.	Mean comp. tension.	Hair hygrometer.	Evaporation.	Rain.
	Inches.					Inches.
1824	29,593	77°,5		87	60,4	38,3
1825	29,620	78°,1		——	67	46,9

The observations, it is to be regretted, were not made at the same hours; they varied from 9 A. M. to 10¾, and from 5 P. M. to 6¾. The mean temperature of the atmosphere for those hours was in 1824, 2°,5, above the mean of the extremes; in 1825 only 1°.84. The greatest monthly excess is 5″,2. By Dr. Brewster's paper in the Transactions of the Royal Society of Edinburgh, in which he gives the registered temperatures for every hour during two years, as observed in Leith Fort, it appears that the mean of 10 A. M, and 6 P. M, for two years is 1°,4, above the mean temperature. This shows, what indeed was to be expected, that the hourly progress of the temperature will in different latitudes be very different, and require different curves to represent them.

The two following tables will we doubt not be deemed sufficiently interesting. They give great plausibility to an opinion which the author of this paper has hazarded, viz. that the daily and monthly oscillations of the barometer are referable to the effects of varying temperature only. The surprising correspondence in the march of the two instruments, exhibited by these two tables, must strike every one. It is rendered still more obvious in an engraving given, in which they are delineated. The night oscillations, which are known to occur at the same hours as the day ones, appear, as the author has observed in another place, to offer an objection to this theory; but may not the same explanation be here admitted as in the case of the tides of the ocean, which are known to occur on opposite sides of the earth at the same time, though the exciting cause is on one only? But however this be we cannot but consider the idea as exceedingly ingenious; and we recommend the subject to the study of meteorologists.

TABLE I. *Daily Oscillations.*

Month.	Barometer.				Thermometer.			
	1824	1825	1826	mean.	1824	1825	1826	mean.
	Inch.	Inch.	Inch.	Inch.	o	o	o	o
January, ..	,092	,091	,109	,097	19,7	14,0	16,1	17,8
February, ..	,109	,096	,103	,103	21,1	16,1	18,8	19,2
March, ..	,117	,124	,122	,121	19,8	20,3	18,6	20,7
April, ..	,127	,113	,135	,125	21,3	25,1	19,9	23,2
May, ..	,120	,113	,140	,124	22,7	23,6	18,0	21,9
June, ..	,119	,111	,109	,113	24,5	11,7	*7,9	16,1
July, ..	,071	,091	,070	,077	10,2	7,7	6,9	9,0
August, ..	,084	,099	,082	,088	9,4	8,6	6,3	8,3
September, ..	,094	,117	,098	,103	13,9	8,5	9,2	10,3
October, ..	,103	,094	,102	,100	13,7	9,7	13,2	18,1
November, ..	,095	,120		,107	18,8	13,3		16,8
December, ..	,085	,111		,098	17,9	15,4		16,3

* From this month the register thermometer stood in a new house, much less exposed than before; the ranges are consequently much smaller.

TABLE II. *Monthly Oscillations.*

Month.	Barometer at 32°.				Therm. (mean of extremes.)			
	1823	1824	1825	1826	1823	1821	1825	1826
January, ..	+,285	+,299	+,277	+,232	— 16,7	— 18,0	— 17,0	— 16,5
February, ..	+,233	+,165	+,184	+,119	— 10,7	— 11,5	— 13,1	— 10,9
March, ..	+,095	+,106	+,124	+,104	+ 0,5	— 1,3	— 4,3	— 0,9
April, ..	—,083	—,016	—,027	—,048	+ 11,2	+ 6,9	+ 9,5	+ 10,3
May, ..	—,137	—,192	—,104	—,113	+ 12,7	+ 14,8	+ 15,5	+ 12,7
June, ..	—,307	—,304	—,278	—,267	+ 15,7	+ 11,9	+ 15,4	+ 11,5
July, ..	—,311	—,327	—,277	—,318	+ 6,1	+ 7,8	+ 7,6	+ 6,1
August, ..	—,227	—,242	—,168	—,175	+ 5,0	+ 7,8	+ 7,9	+ 5,1
September, ..	—,130	—,041	—,108	—,111	+ 4,5	+ 6,9	+ 5,6	+ 6,3
October, ..	+,084	+,066	+,036	+,099	— 0,1	+ 1,6	+ 1,4	+ 2,5
November, ..	+,169	+,201	+,175	——	— 11,3	— 8,5	— 9,2	——
December, ..	+,309	+,290	+,240	——	— 17,0	— 18,5	— 17,4	——

Mean Barometrical Altitude 29,468. Mean Thermometric Altitude 77,81.

The volume concludes with monthly meteorological tables for the whole year.

GLEANINGS

IN

SCIENCE.

No. 6.—*June*, 1829.

I.—*On Colouring Matter.*

Circumstances having occasioned me to be engaged in the preparation of dyes, my attention was naturally turned towards the investigation of their properties; and as during my researches the result of many experiments was decidedly opposed to the notions which at present obtain in chemistry, a system entirely new was necessarily forced upon me. As facts could not be reconciled with theory, it was quite clear that the theory could not be just: it therefore became necessary to compare facts and theory together; to discover the cause of their disagreement; to make the theory conform to facts; and to endeavour to find out a number of laws, constant and uniform in their operation, which should constitute the science of those arts which colours have created. The observation and experience of any one individual will always be exceedingly limited; and the inferences which he may draw from them will often be erroneous: as therefore I profess to have no interest apart from that of truth, so I have no wish that any opinion of mine, resting only on my own unsupported experiments, should have any other influence than that of exciting inquiry. I could have wished preparatory to the enunciation of my own views, to examine some of the facts respecting colouring matter* recorded by Doctor Bancroft, in his Philosophy of Colours, as also his opinions on them; but as this would occupy too much space for my present purpose, I shall proceed to submit a few general principles, after which I shall adduce some of the chief facts upon which they are founded.

1st. Oxygen is the principle of colour in colouring matter, and might therefore be as appropriately called chromogen as oxygen.

2nd. Oxygen, in proportion as its combines with colouring matter, increases the† depth of its colour, and diminishes its solubility.

3rd. THE WHOLE THEORY OF DYING, *as regards colouring matter*, (excluding compound colours which will also be affected by it,) rests on considerations connected with the proportion of OXYGEN, as a constituent of the colouring matter, and upon proper views of chemical affinity.

4th. Oxygen, by entering largely into the composition of colouring matter, occasions a permanent and irrecoverable deterioration of its quality.

5th. Alkalis are oxygenating agents } relatively to colouring matter.
6th. Acids are disoxygenating agents }

7th. Many substances as they acquire oxygen have (whilst they continue below the point of saturation) their disoxygenating power increased.

8th. It cannot be calculated that two agents capable separately of attracting oxygen, from a third substance, will, if employed together, exercise their joint forces in the disoxygenation of this third substance; it is probable that they may act upon, and so counteract each other; and this is especially likely to happen where metallic acids are concerned.

9th. The atmosphere under one set of circumstances oxygenates; under another, disoxygenates, relatively to colouring matter. There are some other points connected with chemical affinity and the fixity or fugitiveness of colours, but as they would carry me too far, I suppress them.

* I have confined myself throughout this paper to speculations on colouring matter.

† I perhaps ought to have added " or changes one colour of a light shade to another of a darker shade."

I. That oxygen is the principle of colour in colouring matter, is I think established upon universal experience; upon the whole mass of facts which nature throughout time has accumulated, and made to bear upon this point. Almost all vegetable exudations are primarily of a white colour or transparent, and of a fluid consistence, till they acquire colour and solidity by exposure to the air*. The external parts of these substances will be found to be much harder and darker than the internal, and those which combine most intimately with, and with the largest portion of oxygen, can only be dissolved by agents the most powerfully disoxygenating. In short, after having carefully examined the whole of the phenomena recorded of colouring matter, I declare that I have not found them to yield a single exception or a solid objection, which can clash with the universality of this proposition. I shall, therefore, endeavour to reduce the proofs to as small a space as possible; although the received opinion respecting the modus operandi of chlorine on colour, and my inability, from having no chlorine in my possession, to refute it by experiments, will necessarily expand the argument, by obliging me to show by indirect means, that chlorine destroys colours by depriving them of oxygen.

1st. I shall assume that as chlorine acts alike upon all colours, all colours depend upon some common principle.

2nd. I shall endeavour to show that all colours are weakened by a partial loss, and destroyed by a total loss of oxygen.

3rd. I shall infer, that if this effect, brought about by other agents, proceeds from privation of oxygen, that chlorine acts in a similar manner; or, that the same effect is attributable to the same cause.

Assuming then that the principle of colour is one and the same, in all colouring matter, I now proceed to show, by as large an assemblage of facts as is compatible with the limits of an essay, that all colours are weakened by a partial loss, and destroyed by a total privation of oxygen†. The action of the rays of the sun upon minute particles of colouring matter is disoxygenating; so also is the action of the sulphurous acid gas; and to this property Doctor Bancroft ascribes the effects of these agents in bleaching. Here is one instance, if it be admitted, wherein agents, producing the same effect as that which is now produced by chlorine, accomplish it by depriving colouring matter of its oxygen. Indigo (at what, for the sake of distinction, I would call the maximum of oxydation) is an insoluble blue concrete: in proportion as it is deprived of oxygen, it loses its solid form; and passes from the darkest shade of green to the lightest; from thence to different shades of yellow; till at last it becomes colourless, and, as proved by an experiment of Doctor Bancroft's, is irrecoverably decomposed‡.

Lac dye is an insoluble, dark, dingy blue concrete; when the lac colouring matter is obtained from fresh stick-lac, it may be formed by means of an alkali and lodh§ into a beautiful red solution; if sulpherreted hydrogen gas be mixed with this solution, it will soon destroy its colours.

For the sake of analogy it might be mentioned, that considering lac dye as a peroxyde, the changes of colour by gradual disoxygenation are from blackish blue to purple; from purple to bluish red; from bluish red to red; and from thence through all the shades of red to yellow; till at last it becomes colourless, and the essence of the colour is irreparably destroyed‖.

Lac dye kept for some days in a highly oxydized solution of tin, will be deprived of its colour; and arguing *a fortiori*, other colours would experience the same fate.

* Doctor Bancroft states that the outer bark of the quercitron is full of colouring matter; but on account of its dark dingy colour he recommends it to be thrown away. I imagine that the only difference between this and the inner bark, is that the former is more highly oxygenated.

† Where an assertion is hazarded without proof, the proof must be sought for, in the sum of the evidence taken together. Thus if I say that the rays of the sun are disoxygenating, and do not prove it; but it appears that disoxygenation from other causes produces the same effect; it must be taken as evidence that the rays of the sun are disoxygenating.

‡ Oxygen seems to be the bond of union between the constituents of the basis of colouring matter, since the slightest portion appears to be sufficient to prevent their separation, and to hinder total decompositions: but as soon as oxygen is totally removed, the constituents of the basis enter into new combinations, and the colour can no longer be regenerated.

§ The bark of a tree which in India is used to produce the same effects in dying as in Europe, are obtained by tin solution.

‖ Combustible matter generally, if gradually carbonized, passes through a series of brown colours to black, passing through the lightest shades to the darkest.

Doctor Bancroft affirms, that in madder there are two kinds of colouring matter, red and brown; I should interpret the facts differently. All colouring matter derived from roots and woods is likely, aud that from various causes, to be united with oxygen in varying proportions; which will of course alter their properties without effecting their essence: thus indigo, is indigo whether in the form of a blue concrete, or a green solution; its properties are changed, it essence is unaltered.

All the phenomena mentioned by Doctor Bancroft, convince me, that the difference between the red and fawn coloured particles of the colouring matter of madder, is owing to their being combined with different proportions of oxygen.

Thus, in an account which he gives of a pink, bran is used to disoxygenate the colouring matter; and he observes, if the proportion of bran is too large, the colour will be weak, (i. e. too much disoxygenated;) and if it be too small, the colour will be less rosy, (viz. not sufficiently disoxygenated.) Again he observes: "Glue, with madder, manifestly obstructed the combination of colouring matter with alum, since only a kind of salmon colour was produced." In my view of the case, the glue was too disoxygenating, or used in too large a proportion. Further he says: "Boiling extracts the fawn coloured particles;" that is, promotes oxygenation: "acids prevent their extraction;" i. e. disoxygenate them. The colonr of madder infusion or decoction is destroyed by sulphuretted hydrogen gas.

The red colour of this dye, by disoxygenation, is first rendered yellow; then colourless; and finally annihilated. What is here observed respecting madder, will apply to other woods and roots, yielding colouring matter; but as Brazil wood and logwood present a few anomalies, I shall as shortly as I can explain them.

Acids produce precipitates in decoctions of Brazil wood; an addition of acid, or a stronger acid, increases the colours. Brazil wood contains gum, which takes oxygen from the acid, and is thereby precipitated, carrying with it much colouring matter; an addition of acid, or a stronger acid, separates and dissolves the colouring matter, which deepens the colour of the decoction. If the colouring matter existed in combination with different proportions of oxygen, the most oxygenated would be precipitated, and its solution would add much colour to the decoction.

The other auomaly is presented by logwood. Solution of tin, instead of lightening, deepens its colour. Logwood, besides its colouring matter, possesses an astringent principle, on which account it is employed in medicine; to this last is owing the anomaly in question. Its power with different agents in producing dark colours, and its use in the black dye arise from the same cause.

This colour, as well as that of Brazil wood, is destroyed by sulphuretted hydrogen gas. Litmus tincture and violet syrup, secluded from air, lose their colour; which oxygen, timely administered, will restore*.

Doctor Bancroft insists chiefly on his experiments on Buccinum, or the Tyrian purple, to prove that colours are sometimes owing to an abstraction of oxygen. If my opinion, respecting the agency on colouring matter of alkalis and acids, be well founded, the better half of his experiments would prove deserters and range themselves under the banner of the enemy. But waving any advantage derivable from this source, is it not probable that phosphorous acid (which the Doctor says is given out by the Buccinum while it gains its colour) may, by its disoxygenating effects, render the colour latent?

The principal fact, however, which is brought forward in proof of the truth of the opinion that the Tyrian purple owes its colour to an abstraction of oxygen, is, that the white mucous matter which is the subject of this colour acquires its colour equally, whether it be in vacuo, or in the open air, provided only that it be exposed to the light. I think an experiment on Prussian blue, which I shall afterwards have occasion to adduce, will prove highly illustrativeof this matter. That Buccinum does not differ with regard to its colouring principle from other colouring matter may, I think, be inferred, both from its passing when acquiring its colour from the lightest shades or colours to the darkest, and from its colour being, as all other colours are, destroyed by chlorine†.

* Vegetable juices are strongly disposed to undergo decomposition; hence perhaps this loss of colour. Litmus, it is said, retains its colour longer in spirits than in water; the one checks, the other promotes decomposition. The colour obtained from dry red cabbage leaves is much more permanent, than that extracted from the same plant when recent.

† Chlorine, in a diluted form, causes the same changes in the shades of colours as are effected by other deoxygenating agents; if it be undiluted, the effect may be too sudden and powerful to admit of these changes being observed; in the same

The facts here mentioned may be augmented ad infinitum, and I think they prove, that the principle of colour is the same in all colouring matter, and that colours are darkened or lightened in proportion as they combine with more or less of this principle; that agents which are known to be powerfully disoxygenating, lighten colours and ultimately destroy them; that therefore the principle of colour in colouring matter is oxygen; and, judging from analogy, that chlorine destroys colours by depriving them of oxygen.

The IId proposition, that oxygen, in proportion as it combines with colouring matter, increases the depth of its colour, and diminishes its solubility, has received much support from the facts already exhibited. Lac dye is much darker than lac colouring matter in stick lac, and it is not soluble by means of an alkali and lodh, as is the latter. In the red and yellow baths from woods or roots, if the colouring matter be equally oxygenated, any cotton or woollen stuff immersed in them, will remove the particles most highly oxydated; those particles being insoluble, attach themselves to the cloth and are of a much darker and more degraded colour than the other less oxygenated colouring matter in solution. Indigo is a powerful example of the truth of this proposition, but as its phenomena have already been mentioned it is not necessary to repeat them. It may be proper to notice in this place an ambiguity, accompanying many resinous colouring matters; an addition of oxygen appears to destroy the colour, and reduce the resin, which is generally of a brownish yellow colour, to a solid state. As these colours are destroyed, and acted upon by disoxygenating agents in the same manner as other colours, I do not suspect that they differ from them at all in their nature. But I conclude that the resin with which they are combined attracts oxygen, by means of which (as will be explained and illustrated when examining the VIIth proposition) it deprives the colouring matter of its oxygen, and thus decomposes it. With this apparent exception, the rule here laid down applies to all colouring matter.

The IIIrd proposition may be permitted to rest upon the facts which have been or may be brought forward in the course of the essay*; I will now pass to the consideration of the IVth general principle.

IVth. The fact stated in this proposition is of very considerable importance, especially as relates to the preparation of dyes. Such experiments as I have had an opportunity of instituting convince me, that all dyes suffer an irreparable injury by combining with a large portion of oxygen; and this may probably be owing to some derangement which this excess of oxygen may occasion in the relation and proportions of their several constituents. I shall confine myself here to the adduction of two instances.

1st. I took a portion of dried indigo leaves, and a piece of the finest indigo I could procure, and having made from them two separate solutions, I dyed a bit of calico in each; the colour obtained from the leaves was incomparably stronger, fresher, and more vivid, than that produced by the indigo: the experiment was many times repeated, and always with the same result.

2d. I took some fresh stick lac and some lac dye, and dyed with the colouring matter of each a bit of flannel; the woollen dyed with the fresh stick lac colouring matter was fully equal to the finest cochineal scarlet, and many degrees superior to that produced by the lac dye. The inferiority of lac dye to cochineal, I am disposed to attribute to this cause: perhaps age may in this manner work its ill effects on colouring matter. The practical utility of this fact it is foreign from the object of this paper to discuss.

manner as the copious and speedy union of oxygen with indigo solution or the Tyrian purple basis, will produce a full purple or blue colour without the intermediate shades being perceptible. Charcoal, on many occasions, exhibits effects similar to chlorine, and charcoal is known to be powerfully disoxygenating. I attribute to this its antiseptic property; its power of destroying colour and smell, &c. Sulphur when precipitated from alkalis is white; as I conceive by the loss of oxygen: by heat or melting, the colour is restored; in my opinion by regaining its oxygen. Sulphur at a certain heat changes its consistency, and is formed into a substance used for taking tho impressions of medals, &c.: this has been attributed, and as I think justly, to an acquisition of oxygen. Charcoal under certain management is dissolved, and combined with sulphur into a fluid, which has been called carburetted sulphur. I imagine the charcoal must have been deprived of its oxygen by the sulphur previous to their combination taking place. I should like to ascertain whether chlorine would produce any change on charcoal, and what?

* The characteristics of alkalis and acids are, their effects on colouring matter, and these effects proceed from their oxygenating or disoxygenating properties.

Vth*. This proposition and the next, being at variance with received opinions, will perhaps at first seem a little startling; I think, however, that upoo strict examination they will be found to rest upon sufficient and satisfactory evidence. In this country, it is well known that a steeping vat at an indigo factory is when in use filled with indigo plant aud water; that fermentation speedily takes place, and a solutiou of indigo is the coosequence: the fermented liquor of this vat is what, in future, I design when I speak of indigo solution. I made the following experiment. I took two phials, one empty, and the other containing a little pure potass. I dipped them into the vat, and when they were nearly filled with indigo solution I closed their mouths with my thumbs and withdrew them; a little time and agitation rendered the indigo solution with potass of a dark blue colour, and by rest a dark blue precipitate was deposited; whereas no time or agitation effected any change in the pure indigo solutiou.

Alkalis employed in the manufacturing of indigo always deepen the colour, and carry it towards a black.

It† is well known that alkalis act powerfully in precipitating indigo. If a solution of pure alkali be sprinkled over indigo solution, it immediately produces upon those places where it happens to fall, a beautiful blue colour. Indeed its power of regenerating indigo was so efficient, that I employed it as a test to inform me of the state of the water (technically called dirty water) which remaios above the indigo after its precipitation or subsidence.

By mixing a little caustic ley with some of this water, and then dipping iuto it a bit of white cotton, it was easy to discover whether it contaioed indigo, and in what proportion. Alkalis are almost, if not quite, indispensable in dying with indigo; I could not obtain‡ a colour of any body from indigo solution, but with caustic leys it dyed very speedily and effectually. I conceive that the following is the explication of the modus operandi of alkalis in promoting the solution of indigo and other dyes. Alkalis promote the oxygenation of the disoxygenating agents, by which their action on the indigo or other dye is increased. If the alkali be present in too small a proportion, the indigo is not deoxydated; and if in too large a proportion, the agents may get saturated, and thus lose their effect. Or the equilibrium between them and the indigo may be destroyed, and a tendency be excited, in the primary principles of both, to enter into new combinations; thus producing decomposition. In fine, the general effect of alkalis is to deepen colour, by, as I believe, occasioning an additional portion of oxygen to enter into the composition of the colouring matter. And although this property may be modified by the facts stated in the VIIth proposition, yet I believe it will be found, that the effects ordinarily produced by alkalies on colouring matter, are in nature the saoie with those which a combination of oxygen with colouring matter produces, in whatever way that coombination may be effected. Of course reference will always be had to any other specific properties possessed by the agents employed. The reverse of what I have said of alkalis will very exactly apply to acids.

VIth. Acids mixed with indigo solution render it incapable of dying; they prevent the oxygenation of indigo§. And thus I conclude that in the manufacture of indigo, the use of beating is to expel the carbonic acid gas; since the indigo cannot combine with oxygen whilst this is present. Acidulous salts possess in a less degree this disoxygenating property of acids.

* Disoxygenating and oxygenating are merely relative terms. Alkalis, under the circumstances adduced in this article, appear to be oxygenating; and as, when employed in bleaching, they restore colour, I incline to believe that they favour the combination of oxygen with colouring matter; they are perhaps, however, subject to the qualification elicited by the examination of the VIIth proposition.

† Some of the effects here mentioned, are in part attributahle, when caustic alkalis are employed, to the neutralizing of the carbonic acid, but the deepening of the colour would seem to proceed from an addition of oxygen.

‡ Perhaps heat may have dissipated the carbonic acid, and promoted the combination of oxygen with the indigo, and thus supplied the place of an alkali. I did not try it.

§ It would be a wonderful improvement in the construction of indigo factories, in my opinion, especially where the manufacturing is protracted to a late period, to build the vats on arches, and to make flues through all the walls; for the purpose, of course, of making fires and assisting the manufacturing process by means of artificial heat. This would enable the manufacturer to ferment his plant, and obtain from it all the indigo in the coldest weather; it would also, I think, improve the quality of the indigo.

As no evidence is so good as that which is extorted by truth from those with whose opinions it is at variance, I will here adduce an experiment and observation of Doctor Bancroft. The Doctor wished to remove the alkali from the blue employed in calico printing. He first tried the muriatic acid; but, he says, he afterwards found that neither the sulphuric nor the nitric regenerated, as he expected they would do, the indigo: he states, as the cause of failure in effecting the object which he had in view, the speedy precipitation of the indigo. I however suppose that the neutralisation of alkalis must have operated prejudicially, by preventing the manifestation of a blue colour*. Doctor Bancroft is of opinion, that sulphuric acid, in the process for making Saxon blue, acts first by disoxygenating the indigo, and then dissolves it; the fact seems to me to be indisputable, and I have further been disposed to think, that the difference between the precipitate obtained from Saxon blue and indigo, may rather be owing to a combination of a sulphuret than to any destruction of the essence of the colouring matter. A solution of tin is said to throw indigo down from its solution in the form of a light blue precipitate; it is probable that this precipitate recently prepared as regards oxygenation may resemble the precipitate from Saxon blue, and like it be soluble in acids†. With regard to other colouring matter, the disoxygenating property of acids is equally marked and incontrovertible. It increases their solubility‡, reddens blues, yellows reds, and bleaches yellows.

The VIIth§ proposition rests upon a number of facts which, no doubt, observation will continually increase. Experiment as well as reason led me to doubt the accuracy of Doctor Bancroft's experiments respecting the effects || on the scarlet dye, of the nitric and sulphuric acids, when combined together in whatsoever proportion in solutions of tin.

In order therefore to satisfy myself on this point, I instituted a set of experiments, the result of which was, that no specific effect in the scarlet dye was produced by the union of these acids in tin solution; but that highly oxydated solutions, however produced, carried lac colouring matter towards a yellow or salmon colour; and I found that disoxygenating substances (I used sugar), by reducing the oxygenisation of tin solution, whether added to the solution, or to them, not only brought the lac colouring matter to a good scarlet, but that this was effected when nitric and sulphuric acids were combined in the solution, in a very large proportion.

In this instance the disoxygenating property of tin solution seems to be increased by an addition of oxygen and diminished by an abstraction of it. Should bran, flour, linseed meal, or other disoxygenating substances be added in excess to the scarlet dye bath, the disoxygenating property of the tin solution will be destroyed, and little or no effect be produced on the colouring matter: this may serve as a key to unlock an experiment of Doctor Bancroft.

After stating that sulphuretted hydrogen destroyed the colour of a decoction of Brazil wood, he adds, that this cannot be by disoxygenation, because potass restores

* According to the doctrine already laid down, alkalis although oxygenating, yet exerting this property on the agent or agents employed for disoxygenating the indigo, they promote its disoxygenation. In this view of the matter their neutralisation may regenerate and precipitate the indigo; if this effect were not produced, I should think the absence of an alkali would act as predicated, i. e. occasion the colour to be faint.

† In Doctor Bancroft's work "Philosopy of permanent Colours," it is astonishing the number of inconsistencies, and the perpetual warfare between theory and fact, which arise from acids being considered as oxygenating.

‡ This effect is produced, as is well known, by sulphuretted hydrogen. Sulphurous acid gas extinguishes combustion, whence it has been recommended, when chimnies are on fire, to burn sulphur in the grate or fire place. This property I imagine to proceed from the gas taking oxygen from the combustible matter, and would obtain in all incombustible substances having an equal affinity with oxygen. Chlorine extinguishes burning bodies, a collateral proof of the mode in which it acts in colour.

§ A number of chemical anomalies, regarding the elective attractions, are only to be accounted for, from the principles laid down in this proposition. Thus iron precipitates silver from its solution; when silver is highly oxydised, iron loses this property; brass pins, under certain circumstances, precipitate tin, and they acquire a coating; if the tin be highly oxydated, the brass pins cannot precipitate it. A hundred other instances must be familiar to every chemist.

|| Namely, producing a salmon colour, with colouring matter employed to dye scarlet.

the colour, and because a large portion of acid of tin (at a low degree of oxydation) with sugar, added to the same decoction, did not destroy the colour, although, as the Doctor affirms, they would have destroyed the colour of indigo.

Lodh, as I have before mentioned, will disoxygenate lac colouring matter in recent stick-lac, but it will produce no such effect on lac dye, which is much more oxydated.

This introduces some degree of perplexity into the subject, and prevents the establishing of any general rule with regard to disoxygenating agents. . Doctor Bancroft directed his attention towards the extraction and consolidation of colouring matter, but failed, probably from this propensity of colouring matter to combine, whilst in any bulk acquiring a dry solid form, with oxygen, and not, when using his preparations, adverting to this circumstance, and taking proper means to obviate it. Should it be necessary to concentrate colouring matter by means of heat, it should be done in vacuo.

It is astonishing how many circumstances may change the relations of oxygenating and disoxygenating as applied to the same substances. I will here transcribe an account from Nicholson's Chemical Dictionary, which will not only confirm the remark, but I think also throw considerable light on the phenomena displayed by the colouring matter of the Buccinum. "A French colourman having mixed some Prussian blue and white lead with neat oil, and set it by for some time covered with water, be found the surface only blue and all the rest white. On pouring it out on the stone, and beginning to grind it afresh, with intention to add more Prussian blue, he found the colour gradually returning of itself; here it might be supposed the oxyde of the Prussiate had parted with oxygen to the oil, or to the lead, or to cloth, thus becoming white, except that in the surface, which was supplied with oxygen from the superincumbent water; and that it recovered its colour by attracting oxygen from the air: but on this supposition, it would seem that light must contain oxygen, since the colour of this paint, spread on wood or paper, returned by exposure to light in vacuo as well as in the open air."

Here I imagine light enables the colouring matter to disoxygenate the oil, whereas, in the absence of light, the oil disoxygenated the colouring matter.

I suspect experiments would prove that the changes in the colour of the blood proceed from this change in the relation of substauces towards each other, with regard to their affinity for oxygen.

Thus* it seems not improbable that the animal matter of the blood, by absorbing oxygen, disoxygenates the colouring matter; and that in the course of circulation, by parting with oxygen, its disoxygenating power on the colouring matter is gradually lost; by which means the colouring matter regains its oxygen.

By what I have said, it will easily be understood that any attempt at classing oxygenating substances in the present state of our knowledge, must be attended with insuperable difficulties. Alkalis totally deprived of oxygen, take it from every thing: perhaps the disoxygenating property of chlorine † likewise is owing to a similar cause. On the other hand, many substances retain oxygen more strongly, the more they possess of it, and as they acquire oxygen have their disoxygenating power increased.

I will here subjoin a list of a few of the acids in the order in which they appeared to me to act in disoxygenating lac dye. Mineral acids; nitric; phosphoric; sulphuric; muriatic; vegetable acids; oxalic; a fruit supposed to contain oxalic, malic, and citric; citric; tartarous; malic; aceto-citric; lactic (whatever that may be); acetous. The gallic acid presents considerable difficulty, in other words, the phenomena which it occasions have not (so far as my knowledge extends) been examined with that care and attention, which they merit. The property of striking a black with highly oxydated salts of iron, is always (I believe) evinced by the gallic acid. M. Berthollet affirms that no black can be dyed in silk or cotton by means of this acid; and that this property is distinct, from the acid nature of this substance, appears to me to be proved by these two circumstances: 1st, gallates possess this property

* The colouring matter in fresh stick lac contains much animal matter, and alkalis seem to disoxygenise it: upon, as I conceive, this principle old stick lac has the animal matter changed and diminished.

† All antiputrescent substances are disoxygenating; the most powerful agent of this class is chlorine. Putrescent miasmata, animal and vegetable, would seem to owe their activity to oxygen. This is no place in which to discuss this point, but any one acquainted with the history of these substances will at once recognize both the truth and importance of this fact.

in a stronger degree than the pure acid; 2d, this acid seems to possess the same property which I have attributed to acids generally, namely, that of disoxygenating and increasing the solubility of colouring matter, since in dyeing printed goods, it tends to keep the grounds clean, and prevents them from being much discoloured*.

I cannot therefore help being of opinion, that in fact†, there exist two distinct substances—gallic acid, and the principle which with highly oxydated salts of iron forms a black, and generally adds to the depth and fixity of adjective colours. M. Berthollet is said further to have affirmed, that tannin in combination with salts of iron, much oxydated, dyed silks and cotton black; if this be true, (and it could not be owing to the presence of gallic acid, for gallic acid alone would not do it, and had this acid existed in a large proportion in the tannin, it would not have escaped the notice of this eminent chemist,) it evidently carries the opinion towards certainty; and as it also proves that this principle is not necessarily a constituent nor in inseparable union with gallic acid, so it affords reasonable grounds for expecting that it may be obtained in a separate form, and may admit of being accurately examined and analyzed.

The VIIIth proposition has been established whilst considering the VIIth: it has been shown that sugar renders tin in solution less disoxygenating, and that the two produce an effect the very reverse of what would be the effect of their joint forces. Spirits of wine, soap, and water, I found to produce good red solution with lac dye; but lodh did not serve to cooperate with these more powerful agents.

The IXth proposition might have been included in some of the preceding ones.

If colouring matter be exposed to the sun and air, in a divided state, (as when applied to stuffs as a dye), the sun and air disoxygenate it, producing a fading, and ultimately, a destruction of the colour.

Whilst on the other hand, colouring matter in masses, especially if it be moist, acquires, instead of losing, oxygen by exposure to the sun and air‡.

I perhaps ought to notice one other circumstance, which seems to be deserving of some attention from those who would desire, in acting upon these principles, to avoid a very obvious source of error. If colouring matter, in parting with oxygen, loses colour, it is evident that other matter may acquire colour by gaining oxygen, and that in some cases of this kind the disoxygenating agents, by combining with the colouring matter, may degrade the colour, as much as by its disoxygenating power it had improved it.

My apology for having written this memoir is derived from my opinion of its utility and necessity. Hitherto (as it appears to me) science has shed little or no light on those arts, whose subject is colour. Doctor Bancroft (who I believe is good authority, and whose work is the only one on the subject, with which I am acquainted,) describes very faithfully the general dyeing processes, and I dare say gives the best known instructions for producing and fixing the various colours from the several materials in use as dyes; but if we except indigo, the rationale of a single process is not, that I recollect, unfolded. He indeed tells us, that old fustic produced a muddy

* The circumstance of adjective colours being deepened and fixed, and as regards the grounds of printed stuffs, the colouring matter being lightened and removed, by gall or analogous substances, seems to be strongly in favour of the opinion, that there must be two distinct agents having even opposite properties, in order to occasion these apparently contradictory effects.

† Doctor Bancroft has promulgated the same idea, and conceives gallic acid to be a modification of some other acid: this does not seem possible; it is sublimeable, and I think many, if not all, acids having this property, produce marked effects on iron.

‡ No subject can be so immense, and I had almost said so appalling as that which concerns the relation which substances bear towards oxygen; almost all physical phenomena, seem to me to proceed from it—electricity, chemical affinity, meteoric stones, spontaneous combustion, &c. I have thought that fluids may exist having two constituents, the one absorbing from the other all the oxygen; thus, as chlorine and aqua regia dissolve gold, I have been disposed to conclude, that the nitric acid disoxygenates the muriatic, thereby converting it into chlorine. I have even been disposed to question whether metals are oxydated when dissolved by acids according to the received opinion; in tin solution metal is combined with hydrogen and partly escapes in the form of gas:—does not this fact evince the possibility of metals existing in the atmosphere, and under certain circumstances of being regenerated or precipitated from it? But I feel I am departing from my subject, and proceeding far beyond my depth.

colour, which M. Chaptal remedied by means of a little glue*, which precipitated the tannin; perhaps observations like these may be frequent, but it is plain, that their application is not general. Oxygen, he thought, generally was necessary as a constituent of colouring matter, but he also thought, that there were exceptions, and although he knew that the various changes and destruction of colour of which indigo is susceptible, depended on the proportion or total abstraction of its oxygen, he yet seems not to have had the slightest idea of referring the change and destruction of colour, in other colouring matter, to the same cause. He ascribes bleaching by exposure to the sun to disoxygenation, but the bran liquor produces the same effect, and is used with the same intention in clearing the grounds of printed goods; and though when used in the bath employed for dying those goods, the grounds are much less discoloured than they otherwise would be, yet it seems never to have occurred to him that those effects proceeded from the same cause†; on the contrary, he concludes that the bran bleaches the grounds by superior affinity, by attracting the colouring matter from the cotton‡.

Should the opinions I have hazarded, which have been hastily put together, and which circumstances unnecessary to mention prevent me from revising, attract to the subject any one more capable than myself to do it justice; I am not, I confess, without hopes, that the prosecution of the inquiry may be productive of some utility, in which case the end I proposed to myself, on venturing to put them on paper, will have been very completely answered. E. M.

II.—*Details of several Borings made in Calcutta, in search of a Spring of Fresh Water.*

To the Editor of the "Gleanings."

Sir,

I have now the pleasure to forward the abstracts of the Borings promised in my former communication.

It may not be unnecessary to mention, that the water of several wells, from 20 to 40 feet deep, having all proved so brackish as to be unfit for drinking, the borings were undertaken to ascertain whether fresh springs were to be met with at a greater depth; and the nature of the intervening strata. The borings were all made within a limited extent and adjoining to the wells; and the gentleman who conducted the operations was so satisfied with their results, that he was preparing to sink a well of masonry on the line of Section No. 5, when ill health compelled him to leave the country.

He was apparently unacquainted with the method since so extensively adopted of lining the hole made by the auger with a metal pipe, and thus superseding the necessity of sinking a well below the level to which the springs will rise of their own accord.

* I. E. disoxygenated the colouring matter. It is worthy of remark, that dyeing drugs relatively to each other are oxygenating and disoxygenating, the lighter colours are generally disoxygenating towards the darker; the most disoxygenating, according to my experience, is turmeric. A dissertation, however, on this subject would be necessary, thoroughly and satisfactorily to explain it.

† Bran liquor was sour and oxygen was the sour principle; therefore, I suppose it was concluded, that acids would afford to part with oxygen, and consequently were deemed oxygenating; but from whatever cause it proceeded, it is certain that chemistry held it as a sort of a fundamental principle, that acids were oxygenating, and alkalis disoxygenating; though, with regard to colouring matter, the reverse is universally the case, and innumerable chemical phenomena are wholly inexplicable upon this assumption.

‡ It is remarkable that although solution of tin is known to be generally disoxygenating, and although Dr. Bancroft himself successfully used it with this intention to dissolve indigo, yet that the effect produced by it on those colouring matters employed to dye scarlet was never attributed by Dr. Bancroft to this property; but to a peculiar chemical action, without any attempt to explain the nature of that action.

1. *Section afforded by borings from 3d December* 1804 *to 4th January* 1805.

Feet inclusive.

0 to	9	Surface mould, bricks, and rubbish.
10 —	11	A layer of sand.
12 —	57	Sandy blue clays; from 36 to 40 feet mixed with decayed wood; the last 3 feet light brown clay.
58 —	62	Hard sea green clay, mixed with small stones. (Qu. Kunkur?)
63 —	70	Yellow clays, hard, and latterly very hard.
71 —	75	Hard kunkur and sand.

Remarks. The borings for the first 32 feet were taken parallel to a well of that depth, and afterwards continued in it.

On the 29th of December, the water in the well stood at 5 feet 3 inches, and at noon at 2 feet 3 inches. On the 30th December the water began to rise, and continued to increase until it attained the height of 13 feet within the well.

This water, taken from the well on the 16th January 1805, was analysed by Dr. Hunter.

The water never rose beyond the height mentioned, and was then on a level with the water in two adjoining wells; showing that the salt waters of the three had their origin in the same springs.

2. *Section afforded by borings from August to September* 1805.

Feet inclusive.

0 to	11	Two feet of bricks and rubbish, and 4 feet of brown mould, were followed by 5 feet of yellowish clay, mixed with sand.
12 —	15	A stratum of soft white sand.
16 —	62	A series of hard blue and blackish clays, mixed with roots, rotten and charred wood, and latterly changing to whitish clays; at 56 feet kunkur.
63 —	71	Stiff, very hard, white and yellow clays, streaked occasionally with green, containing some kunkur.
72 —	96	Beds of yellow sand, and yellow clay mixed with sand.
97 —	118	Fine, hard, yellow-ochre coloured clay, intermixed with kunkur; the latter 3 feet, very hard.
119		Coarse grained sand, the soil very hard; the borer broke, and was not recovered.

Remarks. At 70 feet the water from the borer was brackish. At 81 feet and at 101 feet, the rods were brought up nearly dry. A well of tiles had been sunk, round the borer, for the first 30 feet, and the water rose in it to within 7 feet of the surface; fully 4 feet of the ground, at this place, was artificial, above the general level of the country. During the whole dry season the water only subsided, in this well, 3 feet, and was on the 3d June only 10 feet below the surface; whilst the water in wells at a little distance was 30 feet below it.

3. *Section from* 16*th December* 1805, *to* 11*th February* 1806.

Feet inclusive.

0 to	18	Brown garden mould, sandy blue mould, and very soft blue sand, mixed with clay.
19 —	31	Yellow sand, blue clay, coarse blue sand, with a little clay, very soft.
32 —	64	Hard blue and black clays, kunkur and charred wood.
65 —	93	Stiff greenish and yellow clays, occasional sand and talc. (Qu. Mica?)
94 —	107	Bluish and white clays, at 106 feet fine sand and white clay.
108 —	113	Hard white fine clay, and yellow clay, with small kunkur gravel.
114 to	127	Fine yellow sand, coarser ditto, and very hard: here the borers broke, and 91 feet of rod were lost.

Remarks. At 65 feet the waters were brackish. The waters rose to within four feet of the surface and were well tasted.

4. *Section from the* 15*th March to* 28*th May* 1806.

Feet inclusive.

0 to	15	Soft garden mould, bluish sandy clays.
16 —	57	Sandy blue clays, with kunkur and charred wood, latterly hard.
58 —	66	Very hard bluish clay, changing latterly to yellow.
67 —	91	Stiff greenish and yellow clays, with kunkur, occasional sand and talc. (Mica?)

92—119 Blue clay, yellow and white clays, blue and white, and white and yellow clays, all hard, with kunkur.

120—127½ Yellow clay aud sand, yellow sand, and fine yellow sand with a little clay; hard: here the auger was partly broken; the edges were found sharpened like a knife.

Remarks. The water at 66 feet appeared tasteless, probably owing to the heavy rains this season, in March, which had made the water in all the wells tolerable. The substance brought up with the auger from the greatest depth, 125½ feet, effervesced with acids, and the water was well tasted. After repeated trials, failed to penetrate or to break off a piece of the *rock* so as to bring it to-day.

N. B. It is not mentioned that the water rose in the auger hole; but the upper strata in this boring had fallen in several times, and this might have prevented the rise of the water.

5. *Section, the same as that published in the* 12*th Vol. As. Res. from* 19*th May*, 1804, *to* 15*th January*, 1815.

Feet inclusive.

0 to 21 Earth, rubbish, sandy blue clays.

22 — 57 Blue clays, rotten and charred wood, latterly mixed with kunkur, and stiff.

58 — 65 Very stiff bluish, greenish, and yellow clay, with kunkur.

66 —122 Stiff variegated clays, blue, reddish, yellow, lead coloured, &c. with sand and kunkur.

123 —141 Coarse greenish yellow sand, dark grey sand, red and grey, and dark grey sand, with a little talc (mica ?)

Remarks. The greatest depth attained was on the 11 July 1814, when the borer first broke. All subsequent attempts were ineffectual, the rods breaking before the depth of 140 feet had been regained; and ultimately, a part of the rods was left sticking as supposed in the rock. On the 15th July 1814, the water rose to near the surface of the ground, and was good and plentiful, but from the heavy rains at the time it was doubtful whether the rise was to be attributed to springs below. In the subsequent attempts, the auger came up dry, from the lowest hard stratum. From the wells first alluded to being all brackish, and from these borings, I think it may be concluded, that there are no springs but of salt water likely to be met with in the vicinity of Calcutta within 75 or 80 feet of the surface; and from the borings Nos. 2 and 3 it appears evident that there are fresh water springs at a depth not exceeding 130 feet, and that their source is of sufficient height to allow them to rise within 4 or 5 feet of the surface of the most elevated lands on the banks of the Húgli.

It of course matters not at what distance the source of these springs may be from Calcutta, so long as it be of sufficient height, and that they are prevented from rising by a continued bed of impervious clay.

This must be the case, or the fresh waters would have risen and combined with the springs in the salt strata, and have been rendered incapable of rising higher than the sources of the latter; but it appears by the borings, that the former rose 14 or 15 feet above them, and it is not improbable that they would have risen still more had they been secured from obstructions from the falling in of the upper strata by a cylinder of wood or metal.

Experience has shown, at home, that the springs obtained in this manner always afford an abundant supply of water; and many of the largest manufactories, in and about London, are now furnished with all the water they require from pipes, which have been sunk by boring, from 200 to 500 feet.

The expense in England attending the operations of boring and lining the aperture with metal, appears now to be very inconsiderable; the over-flowing well in the Gardens of the Horticultural Society at Chiswick cost only 130 £; and the borings were carried down 329 feet, before springs were met with that rose sufficiently to give an over-flowing fountain. In Cambridge, where the practice for boring for water is now common, and the usual depth of boring required is about 130 feet, the cost of obtaining a fountain that affords from 11 to 12 gallons of water a minute is only 25 £.

I am, &c.

A. N.

III.—*On the general Employment of Steam Tugs for Inland River Navigation.*

The practicability of navigating the river Ganges in steam boats, and the utility of their general employment were, about this time last year, considered something in the light of doubtful questions; questions on both sides of which much might be said. Many of our most sturdy oppugners of innovation indeed did not rest here; for they boldly asserted the scheme to be altogether visionary, and were loud in prophesying disappointment. The attempt has been made; and, as is generally the case, it has been found that the difficulties had been greatly exaggerated, as well as the advantages which the boats of the country were supposed to possess. A second voyage has been performed; and we now no longer hear of the practicability or utility of the scheme being called in question.

It is difficult to estimate correctly the value of this successful experiment, which, it is generally believed, we owe entirely to Lord Bentinck; or to calculate the consequences to which it may lead. The state of India is so wretchedly below what it might be; the capacity of the country is so inferior to its capability; that we might, were it necessary, safely assume the position that any change must be for the better,—every innovation an improvement. In the case of steam navigation, however, there is no necessity for thus begging the question; its advantages are too tangible (when the practicability of its employment has been shown) to allow of our resting its claims to notice on such an unsound basis.

The facilitating our communication with the north-western provinces is obviously a great advantage, whether we consider the question in a political point of view, as it may affect commerce, or as it concerns us individually. The power of transporting troops and military stores with such expedition along the whole line of river, we can easily see, would greatly strengthen the hands of Government, in the event of our being ever involved in another war. During the late contest with the Burmese, such a power would, at particular junctures, have been gladly purchased at any price. The advantages to commerce are scarcely less obvious, in ensuring quick returns, without which commerce must necessarily languish. The voyage to the upper provinces at present occupies a period scarcely less than that to England; and this delay, in the case of perishable articles particularly, must be a great drawback on any thing like an active interchange of commodities. To passengers, the convenience of superior expedition will be great. But in all these cases, it is not only in the diminution of delay that the advantage of steamers consists, but also in the diminution of risk; and this in two ways: first, by shortening the period over which the risk extends, and avoiding all those delays arising from unfavourable winds, which often prove fatal in their consequences; and secondly, by the greater power which a steamer bas, to contend with difficulties. Thus whether it be to stem a current, to avoid a lee shore, or falling bank, or a dangerous eddy; in any of these cases a steam boat will be safe when ordinary boats must be lost. But to enter fully into all the advantages of steam on such a river as the Ganges, would require limits far beyond those that can be looked for in the "Gleanings;" and even were they available, the writer of this is far from thinking himself capable of handling the subject so as to do justice to it. The foregoing crude remarks are rather meant to serve as an introduction to an extract from the Mechanics' Magazine: an account of the views of Captain McKonochie R. N. with regard to the employment of steam tugs, rather than steam passage or freight boats in some of the great ferries in Scotland.

One of the principal objections originally made to the introduction of steamers on the Ganges, was, their draught; and this objection appears to have had more foundation than many of the others. The Hoogly in going up, even during the rains, got aground several times, and on one occasion so obstinately, that in making way again she carried the best part of the sand-bank with her. In the latter voyage, which she made in April, she could not reach Mirzapoor, but stopped at a place called Cachwa, some miles below it. Even had she reached Mirzapoor, it was believed she would have had to wait the setting in of the rains for her return; and upon the whole, I believe it may be safely said, that this objection of their greater draught is even yet a serious one, and must be obviated before they can be generally introduced with that prospect of decided advantage expected from them. Now one consequence of employing steamers as tugs, is, that they, as well as the passage or freight boats, may be made of much less draught than when the engine is on board the same boat with the passengers or freight. It is, therefore, with great satisfaction we perceive, by the Government advertisements, that tenders are invited for building passage boats of certain dimensions and draught, and of two descriptions as regards accommodation, one for conveying European soldiers, the other for officers or other passengers. This was the only thing wanting, we believe, to ensure the successful and general establishment of steamers on the Ganges. To those who are not sufficiently familiar with the subject, Captain McKonochie's statement of the advantages of the tug system will be interesting.

"It has been ascertained by actual experiment in America, that to enable a vessel to stem a current with an absolute velocity equal to half the velocity of the current, it requires THREE TIMES the motive power, *if that power act on board a vessel*, that would be necessary *if the power were applied to a rope hauling her*."

"It has been found that steam paddles (on the common construction) act with greater effect when they dip no more than 18 or 20 inches into the water; but when a heavy cargo is embarked on board of the same vessel as the engine, the paddles are sure to be sunk so low as to have their power materially impaired, at the very time when power is most required. Steam tugs would be wholly exempt from this inconvenience."

"The propelling power of an engine depends also greatly on the proportion which the breadth of the paddles bears to her power; but in steam boats, which have to take on board passengers and freight, it is necessary to make the paddles of less than the most efficient breadth for two reasons: first, for the sake of convenience, that they may go close along side piers and quays; and second, because, carrying their cargo on deck, their centre of gravity, when laden, lies high, and the paddles must be light to suit this peculiarity. Steam tugs have no similar disadvantages to encounter. They may have their paddles precisely of that breadth which is best."

"A boat with only a powerful engine on board, may be made to swim in four feet water and under (few steamers draw less than six or seven*): while the engine, which is now such an incumbrance to steam boats that carry passengers, would, by its weight, be positively beneficial to the tug; because she must have substance as well as power (bone as well as blood) to fit her for draught. The engine, it might have been added, can always be placed in a tug, exactly in the centre of the vessel, where its power can be applied with best effect; while, in a boat fitted to receive passengers, this consideration has generally to yield to convenience."

"Passage vessels that are to be towed may of course be constructed of far less draught of water, than when they are to carry an engine. Flat in the floor, and buoyant with any cargo, they might, even in the worst weather, be conducted by a tug along side a shallow pier, and land passengers and goods in comfort and safety, long after approach was impracticable to a loaded steam or sailing boat of even the same draught of water. Flat bottomed boats are so buoyant that to superficial observers they appear dangerous craft; but in the smooth water of a tug they would be steady; and they are in all circumstances, as every experienced seaman knows, the safest of all boats. The Yarmouth keels which take stores and provisions out to the men of war, are open boats, sunk to the gunwale when their cargoes are on board, yet no accident ever occurs to them. The Campeche droguers are, in like manner, square boxes, with scarce a sharp end to go foremost, yet they too load gunwale deep, take cargoes out through heavy rolling seas to ships four leagues off, and survive all the apparent dangers of their passage."

"Passengers would be *much more comfortable*, and SAFER, than they can possibly be when embarked with a steam engine. The heat, smell, smoke, dirt, and jarring, unavoidably caused by an engine, are all serious evils, and aggravate, in no small degree, the pains of sea sickness. A small neglect of the machinery may at any time produce a great calamity: the chances of such neglect are greatly multiplied by the presence of passengers on board, and by their occasional curiosity; while the weight of the engine, in the event of collision with any external object, gives a great additional impetus to the shock, and causes the vessel, when a hole happens to be made in its bottom, to go down like a stone. Were tug boats adopted, all these drawbacks on steam navigation would be obviated. At a distance from the tug, and the means of anchorage on board, the passengers in a vessel in tow would be safe† in it whatever happened; and as every corner in the passage vessel might then be given up to accommodation, a thousand conveniences would be introduced which are at present unthought of."

* The Hoogly draws about four feet.

† "The exemption from danger would not however be so complete as is supposed; for a late accident at New Orleans shows that an explosion in a tug *may* do harm to vessels in tow. But it would be so great comparatively, as to form a consideration of the first importance." Ed. Mech. Mag.

"Upon the tug system, high pressure engines might be again introduced into steam navigation*."

And lastly, the tug system will be superior in economy to any thing yet devised. A large steam boat, with a powerful engine, cannot be constructed much under 4000£; but the best steam tugs need not cost above 2000£. nor steam vessels (for ferries) above 3000.£ each.

Remarks by the Editor of the Mechanics' Magazine.

"The saving, we apprehend, would be even greater than Captain McK. here anticipates. An engine of 12 or 14 horse power, if embarked in a tug, would be sufficient for almost any purpose; and a common condensing engine of that dimension may be obtained for less than 500£. and a boat to embark it in, clinker built and copper fastened, for little more than 300£. Such at least are the ordinary prices at Newcastle, where tugs are much employed to tow the trade up and down the Tyne; and they cannot differ greatly from the prices elsewhere."

"Captain McK professes himself unable to imagine what objection can be stated to the tug system, in opposition to so many advantages, except it be some supposed difficulty in managing two boats together in certain circumstances of tide, current, weather, &c. But to this he gives the following satisfactory answers: 1. The thing has been already done on the American rivers, at least as rapid and as strong as any of ours†: 2. where there are thus great advantages to be obtained, and only one small difficulty to be overcome, with common talent and energy, if there is a will, there will be found many ways."

Captain McK. describes the American method of connecting the tug with the passage boat as follows. "Two iron rods are secured one to each bow of the passage vessel, so as easily to play up and down; and their other extremities are brought together, as in a triangle and are jointed and pivoted on the taffrail of the tug. As general principles the nearer the two boats are kept together the smoother, the lighter, and more manageable, will be the draught; and inflexible rods, besides their convenience for backing and keeping the boats apart, transmit the impulse undiminished; whereas, ropes act like springs, and a considerable portion of the power is expended in merely stretching them."

Captain McK. also proposes the tug system for the open sea, and thinks the advantages of its employment would be equally great. "Should a gale of wind arise, the tug, relieved from its incumbrance, would probably be always able to command its own safety; while the passenger vessel, lying under jury masts, (with the means of erecting and rigging which she might be furnished,) or at anchor, would be much more lively without the engine."

Ω

IV. *On the Varnish used by the Burmese, and its probable Identity with that of the Chinese Lackered Ware.*

The following letter was addressed by a gentleman to a friend on the subject of ascertaining, through a correspondent in China, some points relating to the so generally admired Chinese varnish. We publish the letter at full length, inasmuch as it may furnish to others, having an opportunity of inquiring, an idea of the particulars to which they should direct their attention. The letter is followed by an enumeration of the facts which were in consequence ascertained. The principal point, however, the identity of the Burmese and Chinese varnishes, is not noticed. We hope our publication may, in the possible extension of its sphere of circulation, yet reach China; and that our countrymen in that quarter of the globe may be reminded by it, how many interesting facts and how much

* "We are particularly glad to observe the stand which Captain McK: makes in favour of these engines. It is disgraceful to the character of the nation that a few destructive explosions, occasioned solely by gross neglect and ignorance, should have had the effect of driving them so generally out of use. There is not another people in the civilised world who have been so frightened out of the use of a great mechanical power. Our own step-children [brethren ?] the Americans, seem to cry out shame upon us; for there is scarcely on all their rivers a single steam boat which is not propelled by a high pressure engine."—Ed. Mech. Mag.

† Or of ours.

useful knowledge they might, with their opportunities, colleet, and add to the common stock.

"I send you a bottle of the Burmese lacker, and shall be much obliged to you to forward it to your correspondent in China with a request, that he would endeavour to ascertain whether it be the same as that with which the Chinese make their beautiful black varnish. It would be very desirable to know the exact process by which the Chinese prepare it, from the raw state in which it flows from the tree, such as is now sent.

After ascertaining the mode of its preparation, the ingredients which may be mixed up with it, &c. the next thing to be inquired into, is the manner of laying it on wood, leather, and other articles. How many coats are laid on, what time is allowed for each coat to dry, and whether it is dried by artificial heat, exposure to the sun, or, as I have heard, in damp cellars under ground. Also how it is polished, and whether any coat of a transparent varnish is laid over the whole.

The implements, used in laying the lacker on, should also be described, such as brushes, &c. Perhaps the hand only is used.

If the Burmese lacker be the material from which the Chinese prepare their lacker, it is requested that a portion of what is now sent, may be so prepared and returned. If it be not the same, still the Chinese artist may be asked if it will answer as a substitute, or what are the objections to it. We know that the Burmese employ it as a lacker, though we do not know how it is prepared and used by them.

And, lastly, the Chinese artist should be asked from whence their best lacker is procured; and, if possible, a drawing of the tree and of the seed should be made. If we could get the seed itself, or some live plants, so much the better.

P. S. I should be glad to procure specimens of the real Chinese lacker in its rude and prepared state. If it be mixed with any kind of oil, some of the oil is also requested, and the proportions of oil to the varnish should be stated. If a transparent varnish is laid over the last coat of the lacker, the composition of that varnish should be ascertained, and specimens of it in its prepared state, and of the resin from which it is made, should be sent."

Reply to the above.

"The *Varnish tree* of China is called *Long shu*. The varnish, when fresh and unmixed with colouring matter, is of a reddish colour and transparent. In this state it is laid over bamboo pillows, baskets, &c*.

The pigments incorporated with the varnish are lamp black, vermilion, &c. as the colour may be wanted black, red, or other.

Common lackered ware has only one coat; the finer kinds three; from whence dealers, in recommending the ware, say 'it is three times lackered.'

The uncoloured lacker is called *tsüh* in Chinese. Du Halde tells us that it is not customary to draw it from trees less than 7 or 8 years old.

The Chinese lackered ware, when discoloured by the contact of hot fluids, is exposed to frost or the sun to restore its brilliancy. The lacker is laid on with a stiff and very wide bristle brush inserted into a wooden handle, and is suffered, as far as I have observed, to dry gradually in a cool shady place, and not in the sun, lest it blister and peel off.

V.—*Proceedings of Societies.*

I.—Asiatic Society.

A Meeting of this Society was held on Wednesday, the 6th instant. the Honorable Sir Charles E. Grey, President, in the Chair.—Maharajah Baidyanath Rai, Captain Hamilton, Mr. Seppings, and Captain Cowles, were elected Members of the Society.

Captain Herbert was elected a Member of the Committee of Papers, in the room of Colonel Hodgson, gone to Europe.

Read extract from a note from Mr. Calder, stating that the donation of 20,000 Rupees, made by the King of Oude to the Society, had been realized, and only waited for a favourable remittance.

Resolved, that on receipt of the donation, it should be suitably acknowledged by the Society.

* The Burmese lacker you sent me to examine appears to be coloured artificially.

Upon the proposal of the President, it was resolved, that Lord WILLIAM BENTINCK should be requested to become the Patron of the Society; and the President was commissioned to communicate the same to His Lordship.

The Secretary communicated to the Meeting the Prospectus of a new Botanical work, by Dr. WALLICH, entitled Plantæ Asiaticæ Rariorae, to consist of three hundred Engravings of plants, with descriptions in Latin and English, to be published in twelve numbers folio, at £2 10*s.* per number.

The following articles were presented for the Society's Museum: a Kampti hat, spear, and sword, and a Thibet sword, by Lieutenant WILCOX; a stuffed Nepal pheasant, a kitten with a double body.

Various instruments used in the Charak, by Baboo RAMCOMUL SHEN.

Read a letter from Mr. BRETON, Superintendent of the Native Medical School, presenting his various publications, in the Oriental and English languages, for the use of the students of that Institution.

Read a letter from Mr. WOOD, presenting copies of the following works lately issued from the Asiatic Lithographic Press:

A map of the country forty-six miles round Calcutta; a map of Hindoostan in the Persian character; the Book of Roads of India; the Resources of India; the Boostan, with Commentary and Dictionary; the diamond edition of the Goolistaun.

Read a letter from Mr. MACNAGHTEN, presenting his work on the Principles and Precedents of Hindoo Law.

The Prayers of NIERSES CLAJENSIS, in twenty-four languages, was presented by Mr. AVDALL.

The Transactions of the Societe Royale d'Agriculture et de Commerce de Caen, with sundry tracts, were presented by the Society.

Read a letter from the Secretary to the American Philosophical Society, announcing the despatch of several books not yet received.

A Memoir of a map of the eastern branch of the Indus, by Captain BARNES, was presented by the author.

The Meteorological Register at the Surveyor General's office, for February and March, was presented by Captain HERBERT.

A copper plate found in the district of Dacca, was presented by Mr. WALTERS.

A copper plate found at Jhoosy, was presented by Mr. BROWN.

Read a letter from Major JACKSON, forwarding Meteorological Registers kept at Prome, during September, October, and November, 1825.

Lieutenant WILCOX submitted a map of Asam, intended to illustrate his Memoir on the Progress of Geographical Discovery in that quarter.

Read a Description of the Instruments used in the Charak, and an Account of the Ceremony, by Baboo RAMCOMUL SHEN.

Read an Account of an ancient Copper Tablet, found in the district of Dacca, by Mr. WALTERS, with observations by the Secretary.

Read a note by the Secretary on the Inscription found at Jhoosy.

A Biographical Account of NIERSES CLAJENSIS, was submitted by Mr. AVDALL.

2.—AGRICULTURAL AND HORTICULTURAL SOCIETY.

An extraordinary Meeting of the Agricultural and Horticultural Society was held on the evening of the 29th April, for the purpose of nominating the Committee of Management and other purposes mentioned at the last General Meeting. Sir EDWARD RYAN, President, in the Chair.

SIR EDWARD RYAN, before taking the Chair, thanked the Society for the honour they had done him in electing him their President for the ensuing year. At the same time, he could not help feeling conscious, that the Society might have selected from their members many better qualified to occupy their Chair than himself. He could however assure the Society, that he would endeavour to assist, as far as he was able, in promoting the plans and objects of the Society, in which he took the greatest interest.

He wished to have it clearly understood, that at the time he accepted the office of President, (the choice of the Society having been communicated to him by letter from the Secretary,) he was entirely ignorant of all that had passed at the preceding meeting; and, indeed, he was ashamed to say, of the proceedings of the Society generally, never having attended at any meeting before the present. From the public newspapers, he first learnt the proceedings of the former meeting. He thought it due to himself to give this explanation. He was now fully informed of all that had taken place at the preceding meetings of this Society, and he should feel most happy to act as their President for the ensuing year.

It was resolved, that Sir EDWARD RYAN be requested to wait upon the GOVERNOR GENERAL and Lady WILLIAM BENTINCK, and solicit their becoming Patron and Patroness of the Society. The following gentlemen proposed severally by Mr. ALEXANDER, Mr. CALDER, Mr. PATRICK, Mr. HURRY, Mr. ROBISON, &c. were duly elected members of the Society; viz. WM. BLUNT, Esq., The Hon'ble COLONEL FINCH, F. J. HALLIDAY, Esq., J HAWKINS, Esq., GEORGE ALEXANDER, Esq., CAPTAIN FITZGERALD, Engineers, EDWARD TROTTER, Esq.. JOHN MOORE, Esq., J. BRUCE, Esq., of Gazeepoore, JAMES THOMPSON, Esq., of Buxar, Dr. CHEEK, of Bancoora, J. MCKILELICE, of ditto, THEODORE DICKENS, Esq., Barrister, —DESILVAR, Esq., of Backergunge, T. P. B. BISCOE, Esq., CHARLES PRINSEP, Esq., WILLIAM PRINSEP, Esq., GEORGE PRINSEP, Esq., HENRY MACKENZIE, Esq., MARTIN PETRIE, Esq., M. HAINES, Esq., of Kishnagur, WM. STORM, Esq., J. M. BRIDGEMAN, Esq., of Juanpoor, PETER ANDREW, Esq. DAVID ANDREW, Esq. Junr. THEOPHILUS LLOYD, Esq., Commodore HAYES, CHARLES TROWER, Esq., JOSHUA SAUNDERS Esq., and Baboo NUBBOKISSEN BONNERJEE.

The Secretary stated that he had, in company with RAMCOMUL SEN, proceeded, on the morning after the last meeting, to the Allipore Garden, to ascertain whether it was the wish of Mr. MITCHELL, Head Gardener, to remain with the Society, or leave it, as had been understood, with the view of bettering himself; and that as they had found it to be Mr. MITCHELL's desire to stay with the Society, they had given orders to him to stop every expensive work in progress, and to reduce the number of workmen, and every expenditure, as much as possible. They had also desired him to prepare his accounts, and make out a short report on the state of the Garden, particularly its ability to repay *any* part of the expense, which had been bestowed *upon* it.

That he had received, and now submitted the accounts and report forwarded to him by Mr. MITCHELL. That he had written to Mr. H. T. PRINSEP, Secretary of Government, expressing the regrets of the Society, at being obliged to give up the Poosah Garden, also to Mr. WILKINSON and Mr. SLOANE, of Tirhoot, the Committee of Management of the Garden, and to Serjeant WATSON, the overseer.

Read a letter from Mr. PRINSEP, in reply, stating that steps would be taken by Government for resuming possession of the Garden.

Read a letter from Mr. BLAQUIERE expressing his ready compliance with the wishes of the Society, to take grafts from his Mozambique Orange Trees, and forwarding prepared specimens of the very peculiar method by which the Bire Tree is grafted upon, and improved, called the flute method;—the same which is mentioned in the Essay of their Vice-President, RADHACANTH DEB, submitted at the last meeting.

Read a letter from Mr. BEGBIE, of Banda, a Member of the Society, requesting to be supplied with some fresh Coffee Berries, as he was desirous of introducing the coffee plants into that district.

Resolved, that as the Society does not possess any berries at present, the Secretary be requested to procure them for Mr. BEGBIE if possible.

The Secretary laid before the Meeting a letter from Messrs. ALEXANDER and Co. the Treasurers, handing the accounts of the Society, closed to 30th April, current, and which, including a very recent payment of 2,600 Rupees by Government, and two sums of Rupees 500, by RAMCOMUL SEN, their Collector, showed an apparent balance due to the Society of Rupees 3,600; but it was necessary to state, that the bill for printing the Transactions would fall to be immediately paid, and nearly twelve months' expenditure on the Poosah Garden, which would probably amount to Rupees 1,500 more, besides about 500 Rupees as arrears of wages for the Allipore Garden, and other small bills,—so that he considered the Society, although out of debt, without any immediate funds.

It was proposed by the President, seconded by Mr. CALDER, and agreed to, that in order to relieve the Secretary from the great load of business which must devolve upon him, and the better to conduct the business of the Society efficiently between one regular meeting and another, a commiteee of Management be appointed, by ballot, consisting of the office bearers of the Society, and ten ordinary members, five of whom to be Europeans, and five natives, with power to add to their number.

Upon a scrutiny, by ballot, the following appeared to be the ordinary Members chosen for the Committee.

European.	*Natives.*
Mr. HURRY,	Raja BUDDINATH ROY,
Captain JENKINS,	RADAMADUB BONERJEE,
Mr. ABBOTT,	SEEBCHUNDER DOSS,
Mr. MINCHIN,	DWARKANATH TAGORE,
Mr. KYD,	COSSINOTH MULLICK.

It was proposed by Mr. ROBISON, seconded by Mr. CALDER, and agreed to, that Sir ROBERT COLQUHOUN, Bart. and R. T. PLOWDEN, Esq. be added to the Committee.

It was proposed by Mr. ALEXANDER, seconded by Mr. MINCHIN, and agreed to, that PURSOONATH COMAR TAGORE and OBEYCHURN BONERJEE, be likewise added to the Committee.

The President proposed that the Committee, now elected, should consider the following, as the more immediate objects of their labours :

1st. To examine and report upon the Funds of the Society ;

2d. To examine the list of Members, and ascertain the arrears of subscription, and to report the names of such members as are in arrears, and who, upon application made, decline paying up their subscriptions;

3d. To examine all the papers of the Society, and to report thereon ; to select any papers for reading at the Meeting of the Society, that have not hitherto been noticed, and to which the Committee think it desirable the attention of the Society should be called ;

4th. That the Committee refer to the Prospectus and queries, drawn up and circulated by Dr. CAREY when the Society was first established, and report how far the objects of the Society, as detailed in those papers, had been effected or lost sight of.

Resolved, that these proposals of the President be considered as the subjects to which the Committee are, in the first instance, requested to turn their attention.

It was proposed by Mr. ROBISON, and in reference thereto, the bill for printing the Transactions of the Society, and the Accounts of the Treasurers, were referred to the Committee ; that as these duties would require considerable exertion on the part of some individual Member of the Committee, Captain JENKINS be now requested to undertake it, under directions from the Committee.

This proposal was seconded by the President, and Captain JENKINS kindly agreed to render every assistance in his power.

It was proposed by the President, and agreed to by the Society, that all payments and disbursements of every kind be first sanctioned by the Committee of Management, and that having been so sanctioned, the Secretary, and one Member of the Committee of Management, do sign all checks that may be drawn upon the Treasurer of the Society, for such payment or disbursement.

Mr. MINCHIN presented to the Society three Treatises by Mr. INGLEDEW, of Madras, on the Culture of the Mango, Vine, and Red Rose.

The thanks of the Society were voted to Mr. MINCHIN, for his acceptable present

Mr MITCHELL, Head Gardener, submitted to the Meeting 14 bottles of Arrow Root, prepared by him from the *Maranta Arundinacea*, (the plant furnishiog the real West India Arrow Root,) the produce of the Garden, and which had very lately been introduced into this country by Mr. LEYCESTER, from the West Indies via the Cape of Good Hope

Mr. MITCHELL informed the Meeting, that the plant was of the most easy culture and very prolific, and that a farina, the very same as the real West India Arrow Root, could, in a short time, be manufactured here, to any extent, and free from adulteration, for the supply of the Hospitals, and other purposes ; and might soon become an article of export from Calcutta, instead of being imported into it, as at present, in large quantities, and always adulterated with Potato farina.

A plant had been long known in India, called Teekhur, (the Curcuma Angustifolia of Botanists,) which furnished a farinaceous powder, resembling Arrow Root, and which Dr. WALLICH thought well of as a substitute for the West India Powder, until the introduction of the real plant by Mr. LEYCESTER in 1827, when he stated to this Society, his very great preference of the latter, and anxiety to see it widely introduced.

The Secretary proposed to submit the specimen, now furnished from the Society's garden, to the inspection of medical men and druggists, with the view of obtaining a report upon it, and allowing its intrinsic qualities to be as widely known as possible. He also, in reference to the above, submitted the plan of a machine (taken from the 4th vol. of the Mechanical Repository), which appeared admirably calculated for the purpose of reducing the bulbs to a pulp, thereby securing a complete separation of the farina; and as the Society would possess, against next season, a large stock of roots and plants, it would be very desirable to have such a machine in the garden.

Mr. KYD, on examining the drawing, was also of opinion, that it would answer the purpose desired, and offered to construct the machine and present it to the Society.

The offer of Mr. KYD was most thankfully received. A small tin machine was laid on the table, resembling the mouth or spout of a gardener's watering pan, which had been furnished by Mr. BLACQUIERE, and was made by him for the purpose of being inserted by a *bheestee* (in an instant) into the mouth of his *mussuck*, and thus enable him to water seedlings and plants of all kinds without deluging them, or disturbing the earth about their roots; as it would be found on trial with this instrument, costing only seven annas, that when the water was at any distance from the plants to be watered, one bheestee could do the work of more than two men, so great is the loss of time of pouring from a *mussuck* into a watering pan, and from thence upon the plants.

The hour of eight o'clock being considered too early in the hot season for the meetings of the Society, it was resolved, that, in future, the meetings should be held at half-past eight o'clock; that is, from the 1st of March to the 1st of November, and at eight o'clock from 1st November to 1st March; the next meeting to be at half-past eight o'clock.

It was resolved, that the first meeting of the Committee be held at nine o'clock of the forenoon of Saturday, the 9th May, at the office of Mr. KYD, in Clive Street Ghaut, who kindly offered a room there for that purpose; and the Secretary was requested to have the books and papers in readiness there, for the inspection of the Committee.

It was proposed and agreed to, that an Anniversary Dinner Meeting of the Society, be held within the hall of the Asiatic Society, on Thursday evening, the 14th May, at seven o'clock; it being understood, that it was quite optional with members to attend the Anniversary Meeting, or not, as they felt disposed, and that each member be permitted to bring a guest.

Baboo RAMCOMUL SEN was requested to supply the Committee with the list of members of the Society, and also of the names of such as were in arrear, that the Committee might report upon the same to the next meeting of the Society.

The meeting adjourned to the second Wednesday of June, being the next regular Meeting of the Society.

VI.—SCIENTIFIC INTELLIGENCE, MISCELLANEOUS NOTICES, &c.

1. *Mechanical Contrivances described in No. 2. Objections answered.*

In No. 4 of this work a correspondent has shown, what he considers, the fallacy of the mechanical arrangement described in No. 2. His remarks apply strictly to the plate given in that number. But the author requests us to state, that in repeating the experiment for the satisfaction of a friend, long before seeing A. N.'s remarks, this source of fallacy was fully recognized and obviated; the strokes being made perfectly free, and no counteraction allowed to the weights, as supposed by A. N. He intended to have brought forward these experiments for publication in the 3d number; considering them, as did three out of four of the spectators who witnessed them, perfectly decisive in favour of the beam filled with fluid. The objections of the fourth witness requiring a modification of the apparatus, the publication was deferred till the experiments could be repeated; and such is the difficulty and delay attending the execution of any thing of this kind in Calcutta, that the proposed modification has not been yet executed, or the experiment been repeated under such circumstances, as to warrant the author in coming forward after the objections that had been urged. In his own mind, however, he has no doubt of the utility of the invention, and fully confides in his own ability to demonstrate it to the satisfaction of every unprejudiced person. All he asks is a clear stage and no favour. For the present, then, we may consider the subject " sub judice*."

2. *Notice of the Newly discovered Coal Mine.*

The meritorious officer whose proceedings we noticed in our last number, has, since the date of the letter therein mentioned, visited and reported on the coal mine lately discovered in the Palamao district. There are three localities where coal is found; two of them being near the spot marked *Cole mine* in Arrowsmith's Map, and at the junctions of the Amanut Naddi with the Colga River at a place which is marked in that map *Sidra*, but its proper name is Singra. The Amanut Naddi is also laid down wrong in that map; it is made to flow into the Fulgo, which runs by Gayah, but instead of going in that direction it flows due west by Jubra and Tirhussy to Sidra. The coal is at the point of junction, and has been laid open by the Amanut Naddi. The whole of the coal which is above the water line is good for nothing; it contains no bitumen, or rather the bitumen has undergone such a change as to deteriorate the coal. Under the water line it is not much better, at least in the two places where trials have been made. A shaft was sunk 10 feet deep into the rock below the upper seam without coming at another. From the appearances, however, it is thought another seam will be met with on piercing sufficiently deep.

The route followed furnishes two interesting lines of communication and corroboration with those of the late Dr. Voysey. Between Sherghàti and Palamao, and between Palamao and Meràl, (15 miles S. E. of Untàri,) no rock is observable but granite and gneiss; the same which Dr. Voysey found in his passage of the Vindiya range in proceeding from Ràniganj to Sherghàti, and again from Sambalpùr to the place where he died. But in all the streams which fall into Cél river, and also in the bed of that river, there are seen beds of gritstone and micaceous sandstone, sometimes accompanied with beds of slate clay, which frequently include large lenticular masses of dark grey compact limestone, and very frequently seams of bad coal. These sandstone deposits are evidently superficial, lying upon primary rock. They greatly resemble the rock of the Mahadeo Hills, which appears to be the same as the millstone-grit of Connybeare, termed by Phillips old sandstone, and distinguished from primary sandstone. The coal of this formation is generally bad, and in this respect the character holds; for the coal in the Mahadeo and Palamao hills is equally bad, I think.

The collection of birds had increased to 120; not including the most common species.

3. *Formation of Artificial Diamonds.*

It is known to most of our readers that some years ago a considerable interest was excited by some experiments, made in America, in which particles of diamonds were supposed to have been artificially formed during the combustion of charcoal, graphite, and anthracite, by the intense agency of Dr. Hare's deflagrator. Professor Silliman, amongst others, appeared to have received the opinion of their possible production in this way with some indulgence; but a closer attention to the phenomena and more rigorous examination of all the circumstances of the case showed, that these pretended diamonds were merely fused particles of the several earths and alkalis, which are known to enter into the composition of common wood charcoal, the pure carbon having been entirely consumed.

Another supposed method of forming diamonds artificially has lately been brought forward in France by M. Gannal. He has submitted the details of his process to the Academie des Sciences; and he appears himself convinced that small crystals of diamond were produced in the experiment he records. It is as follows.

Carburet of sulphur being prepared and covered with a layer of water, several rolls of phosphorus were introduced, and on coming into contact with the carburet of sulphur, appeared to liquefy and sink down to the bottom of the matrass, thus forming a third stratum of fluid distinct from the other two. The matrass was then shaken; the liquid grew thick and milky; and after a little rest separated again,

* The objections alluded to referred to the necessity of making the beam oscillate before the weight would descend; having to lift a double load. The space too through which the weight fell, was too limited to allow a positive opinion as to whether the action would continue any length of time.

not into three but into two distinct strata. The upper one was water; the under one phosphuret of sulphur; and between both was observed a white powder which, when exposed to the sun's rays, exhibited all the colours of the prism, and was therefore supposed to be a multitude of little crystals.

In the repetition of the experiment, under the idea of obtaining larger crystals, the shaking was omitted. In three months the two lower liquids appeared to form but one. There was now a difficulty to separate the supposed formed diamonds from the phosphuret of sulphur, on account of the inflammability of the latter. It was however at last effected; and M. Gannal was rewarded by finding several crystals, twenty of which were large enough to be taken up on the point of a penknife, and three were as large as a grain of millet. These last having been submitted to the inspection of an experienced jeweller in Paris, they were pronounced to be REAL DIAMONDS.

Analyses of Books.

Transactions of the Geological Society of London: Second Series, Vol. ii. *Part 3d.*

The 1st and second parts of this volume have been published some time. The third part, being the conclusion of the volume, has just reached us; and as it contains some papers that will be considered in India rather interesting, we hasten to give an account of them.

XXII. *On the volcanic District of Naples. By* G. Poulett Scrope, *Esq. F. G. S. F. R. S. &c. pp.* 337 *to* 352.

Mr. Scrope is the author of a work on volcanoes, which has attracted a great deal of attention, and in which a very clear and full account of the several phenomena is followed by a view of the author's hypothesis or mode of explaining these appearances. He has published also several shorter papers on subjects connected with volcanoes or volcanic rocks in the Transactions of the learned body of which he is a member, as well as in some of the scientific journals of the day. The importance of the subject begins to be generally acknowledged. Nor can there be a stronger proof of its increasing interest, than the fact of a geologist like Mr. Scrope giving so much of his attention to it. A great revolution has taken place since the time when volcanoes were considered mere local phenomena, and were supposed to be explained by the casual inflammation of beds of coal. The discovery of the metallic bases of the earths has thrown a strong light on the causes of these phenomena; while their number, so much greater than had been supposed, gives new views of the importance of the part they play in the great scheme of nature. The identity of origin with ordinary volcanic products of the extensive class of trap rocks begins to be more than surmised; and to the admirers of that original genius that prompted the Huttonian theory, nothing can be more gratifying than the daily strength that theory is gaining as facts accumulate, and appearances are more studied. What can be more favourable to the truth of a theory than the fact, that almost every new discovery adds strength to it, and removes some ill considered objection? Thus, whether, with Sir James Hall, we view the curious modifications of the ordinary phenomena of heat under extraordinary pressure; or, with Sir Humphry Davy, decompose those substances, till his time considered simple, and view them so greedy of oxygen that they take it from water or even ice, bursting out into flame from the rapidity of the combustion; in each case we are struck with the triumphant nature of the answer, which the knowledge of these facts would have enabled the eloquent defender of the Huttonian theory to give to those who could so little weigh the value of two rival theories. But to return from this digression.

Mr. Scrope in the present paper proposes to give an account of such phenomena, observed in the volcanic district of Naples, as have been overlooked by preceding geologists, Menard de la Groye, Necker de Saussure, Brieslak, and more lately Dr. Daubeny. He considers the volcanic district of Naples to include " not only Somma, Vesuvius, the coast of Sorrento, and the immediate environs of Naples, Pozzuoli, and Cuma; but also the islands of Procida and Ischia, with which they are as closely connected in composition as in geographical situation. This linear groupe, which ranges N. E. to S. W., is terminated at either extremity by the two principal volcanic mountains of Ischia and Vesuvius. The latter, as I have mentioned in a previous memoir, seems to be in communication with the group of Albano and Rome, through the intervention of the Rocca Monfina and other minor vents of volcanic matter scattered along that remarkable longitudinal valley, which divides the limestone range of the Sabine Hills from the principal trunk of the Apennines. Another chain of volcanic products seems to take off also from Ischia towards the Ponza group, through the islands of Ventotiene and San Stefano, maintaining a parallel direction with that first mentioned, and indeed with the general range of the escarpment of the elevated Apennine strata, at the foot of which these explosions have found an issue."

"It is certainly a remarkable confirmation of the opinion elsewhere expressed, as to the general parallelism of lines of volcanic eruption to the nearest linear ranges of elevated strata, that not only is there along the western coast of Italy this correspondence between two decided trains of volcanic vents, and the two parallel ranges of the Sabine mountains and the Apennines; but also, where a massive embranchment breaks off from the latter at nearly a right angle, forming the promontory which bounds the bay of Naples to the S. E. and the almost contiguous island of Capri, a corresponding line of volcanic vents (viz. that connecting Vesuvius with Ischia) is found to take off from the principal line, at no great distance, and at the same angle, so as consequently to run parallel with the transverse elevated range."

"If we call to mind the general tendency of the rocks composing the secondary strata to yield to any disruptive force along fracture lines at right angles, or nearly so, one to another, it will appear highly credible that this parallelism, in two directions of the ranges of elevation and eruption, has been influenced here, as elsewhere, by that general, though at first sight, insignificant cause."

Mr. Scrope next enters into some detail to show, that the appearances at Monte Somma, which he considers a type of the volcanic structure, are not to be explained by the common opinion of the sides of the mountain having fallen in; nor yet by that of which Von Buch, Dr. Daubeny, and Humboldt are the supporters, viz. of the whole mountain having been produced by the elevation of flat strata on their edges round a central aperture by force of elastic gases. On the contrary he concludes, that all volcanic cones have been created by the repeated superposition of one volcanic bed on another, all flowing from a central source; a process which cannot fail to produce this peculiar form of hill. He contends in fact "that the parallel and sloping beds which with a quâquâversal dip compose every such mountain, were not deposited horizontally or otherwise than at a high angle of inclination; and that the angular elevation they have since sustained is in general comparatively trifling: in short, that the conoidal form preserved by an habitual volcanic mountain in every stage of its formation, is owing to the same obvious cause which communicates the similar figure to a simple volcanic hillock, the produce of a single eruption, viz. the accumulation of erupted matters round a central orifice.

After giving a very full description of all the vents, extinct or ignivomous, within the circle intended to be described, Mr. Scrope adopts the conclusion, that "this part at least, of the western coast of Italy has suffered an elevation of some hundred feet since the epoch of eruption of the greater number of volcanic mouths whose products we have been employed in examining, and which are certainly much more recent than the tertiary or Subapennine formation. In this elevation it is at least probable that the whole chain of the Apennines shared; and indeed there are traces of it in the limestone cliffs of the Monte Circello and the Calabrian coast, which are in some parts thickly perforated by recent lithophagi at a height of more than a hundred feet from the present level of the sea."

"Whether this elevation took place at once or by successive heavings accompanying the earthquakes which have habitually affected the western Apennines, from Rome to Palermo, is a question which it requires further inquiries to elucidate. From my own observations I should be inclined to doubt that any sensible change has taken place in the relative level of the land and water round Naples since the Roman era, (the few facts which have been brought forward to support the idea of such change, being contradictory, and to be otherwise explained); and for this reason I should prefer the conclusion, that the rise of the plains of Campania from below the sea was produced by some convulsive crisis of subterraneous expansion, occasioned perhaps by the long obstruction of the superficial vents,—those natural safety valves that by their occasional activity at present, may be supposed to obviate the recurrence of such tremendous operations.

XXIII. *Supplementary Remarks on the Strata of the Oolitic Series and the rocks associated with them, in the Counties of Sutherland and Ross, and in the Hebrides.* By Roderick Impey Murchison, *Esq. Sec. Geo. Soc. F. R. S. F. L. S.* pp. 353 to 368.

This paper is supplementary to one published in a former part of this volume, for want of which the details would not be either so intelligible or interesting. We shall confine ourselves, therefore, to such extracts as are less dependant on preceding statements, or may more easily be illustrated.

"The promontory called the Ord of Caithness, which constitutes the north eastern termination of the deposits of the Oolitic series in Sutherland, has been described as a granitic rock composed of much felspar and quartz, with a substance in a decomposed state which may have been mica. Now although such is the prevaling character near its junction with the secondary strata, an examination carried on more in the centre of the mass has detected so many examples of well crystallized mica, that this rock must be considered a true granite."

"In the previous memoir I stated, that wherever this rock comes in contact with the beds of the Oolitic series, the latter are compounded into a remarkable breccia: and recent observation has not only fully confirmed the conclusion which I drew from these phenomena, viz. 'that the granite of this coast must have been elevated at a period subsequent to the deposition of the Oolitic strata,' but has also led Professor Sedgwick and myself to the conviction, that it has been upheaved in a solid form, and that, in breaking through those submarine deposits which might not perhaps have been originally in contact, it has so fractured and dislocated their beds as to have prepared them for reconsolidation in the state of a brecciated rock."

Consistently with this opinion it is found that "where the granite disappears, a more full development of the secondary strata would take place, and such is the case in this district; for with the recession of the granitic ridge, the regular formation of the Oolitic series, from the sandstone of the calcareous grit down to the base of the inferior Oolite, are laid open, including the coal field of Brora."

In some remarks on the Sutors of Cromarty he states, that what he had supposed in his first paper to be granite he found, on a closer examination, consisted chiefly of a feldspathose and quartzose gneiss, much foliated and generally nearly vertical; but in many situations so decomposed as not to be distinguishable from some varieties of the granite of the Ord of Caithness. This gneiss is associated with subordinate slaty rocks hornblendic and talcose, and is repeatedly traversed by large and small veins of true granite."

"Many writers have contended (and it seems now to be pretty generally admitted), that the granite must have been in a fluid state at the period when these veins issued from its mass; and others have further presumed, that the gneiss must then also have been in a state of softness. But in whatever mode these ramifying and tortuous veins in primary rocks may have been produced, a very different explanation is requisite to account for the fractured and brecciated beds of the Oolitic series which accompany the elevation of the granite on the coast of Sutherland. There, it is evident, that the granite, when upheaved, could not have been in a fluid state, since it has neither penetrated nor overflowed the contiguous masses of solid breccia; in such situations, therefore, the disturbing rock was at the period of its elevation most probably in a compact and crystalline form; in which case, when forced up against the overlying strata, it must have fractured the sandstone, limestone, and shale, thus preparing the materials which, when recemented, formed the breccia above described. But we have additional evidence of the elevation of the granite, *en masse*, upon this N. E. coast of Sutherland, where it has not only brecciated the beds of the Oolitic series but has also thrown up the red conglomerate to the summits of many of the mountains whose bases consist either of granite or of gneiss charged with granitic veins. In these positions the old red conglomerate which, when undisturbed, passes beneath the Oolitic series and its coalfield of Brora, presents that anomalous appearance which without explanation might lead to the supposition of its being an overlying deposit.

Amongst some remarks on the denudation of Braambury and Hare Hills we find the following: "I remarked in my former paper that these hills probably owe their present form to denudation; which supposition is now confirmed by the exposure on their surf ce of innumerable parallel small furrows and irregular scratches, both deep and shallow, such, in short, as can scarcely have been produced by any other operation than the rush of rock fragments transported by some powerful current. Upon my first visit these markings being only imperfectly visible in one situation near the quarries, I was unwilling to enlarge upon the fact; but Mr. Barton, the director of these works, has since cleared away the turf from other parts of the surface, and these operations have uniformly exposed similar phenomena." And again "These appearances so closely resemble those in other places described by Sir J. Hall and Dr. Buckland, that further detail seems unnecessary; and the large slabs which I have had the pleasure of presenting to the Society, completely elucidate the case."

Mr. Murchison notices traces of a fresh water formation occurring in the Isle of Sky. Flattened masses of shelly limestone were found "on the north eastern shores of Loch Staffin containing five species of Cyclas, one Paludina, one Neritina? one Ostrea, one Mytilus, and some undescribed bivalves. It adds materially to the interest of these remains, that two species of the Cyclas, the Paludina and the Ostrea, prove to be identical with the fossils of one of the upper beds of the weald clay described by Dr. Fitton as occurring in Swanage Bay, Dorsetshire, and in the Isle of Wight. Here therefore we have a decided indication of a formation of fresh water, or at all events of æstuary origin, which had never before been traced north of Aylesbury in England; and this would seem to prove, that although the continuity of such deposits may have been more limited in extent than those of marine origin, still the causes which gave rise to a deposit of the former class in England may at the same epoch have been producing corresponding effects in the North of Scotland and in other widely distant localities."

Pitchstone has been observed in several of the trap veins that transverse beds of the Oolite series. This fact, it is observed, "is of importance in assisting us to limit the antiquity of those trap rocks which contain that mineral."

"The organic remains, collected on this last occasion in various parts of the Western Islands, comprise many characteristic fossils tending to confirm the comparison already instituted, and to identify these deposits with the Oolitic series and lias of English geology. A supplemental table of fossil shells gives a view of these acquisitions and concludes the paper. This table we may hereafter find room for.

XXIV. *On the Fossil Remains of two new Species of Mastodon and of other Vertebrated Animals, found on the left bank of the Irawadi. By* William Clift, *Esq. F. G. S. F. R. S. &c. Conservator of the Museum of the Royal College of Surgeons.*

XXV. *Geological Account of a Series of Animal and Vegetable Remains and of Rocks collected by* J. Crawfurd, *Esq. on a Voyage up the Irawadi to Ava in* 1826 *and* 1827. *By the Rev.* W. Buckland, *D. D. F. G. S. F. R. S. F. L. S. Professor of Mineralogy and Geology in the University of Oxford.*

These two papers contain the particulars of one of the most interesting contributions yet made in India to the stock of science in Europe, by which is established "the grand point of the occurrence of formations in the south east of India analogous to the tertiary and diluvial formations of Europe, and containing respectively the remains of animals the same which the formations of Europe contain; or very similar to them." And if, with the learned author of the second of these papers, we admit "that the result ot these discoveries, though intensely interesting, and a splendid example of what may be done by the skill and activity of one zealous individual, is rather to stimulate than gratify our curiosity;" let us also hope "that the expectation entertained of more detailed and more extensive information from the future investigations of the most intelligent among our countrymen, whose professional duties call them to the eastern world," will not be disappointed. Let us at least be assured, that if we refuse to gather the rich harvest ripening under our hands, there are others less dead to the charms and the solid advantages of true knowledge, who will not imitate our indifference. It is some consolation to those who cultivate science for her own sake to know, that if England refuse to add these to her other laurels; they will not be altogether lost to the civilized world while FRANCE is so zealous in making new acquisitions and in exploring every possible source of discovery*.

It is unnecessary, to say that these valuable contributions are the result of an examination of the collection made by J. Crawfurd, Esq. when ambassador at the court of Ava. This collection was contained in seven large chests, and consisted of fossil wood and fossil bones, as also of specimens of the strata that are found along the course of the Irawadi from its mouth near Rangoon up to Ava, a distance of nearly five hundred miles.

* Those who knew M. Diard and the late M. Duvancel will not require to be told of their labours. M. Dussoumier, of whom Baron Cuvier, in his prospectus of the "Histoire des Poissons," makes honourable mention, is known to have collected, in the short period of his stay in this country, (whither he came as supercargo of his own vessel,) 3000 specimens of fishes; a number scarcely credible when we consider the many other calls on his attention during the time, and the short period in which he accomplished his task. A new labourer in the field has recently appeared, an accomplished naturalist particularly skilled in the details of geology: and notwithstanding the increasing vitality which this part of our system has began lately to exhibit, we fear he will carry off many of the triumphs which ought to have been ours long ago.

As a public officer of high rank, as representative of the Governor General of British India, Mr. Crawfurd must have possessed facilities which no unassisted private individual can ever possess. And yet it is curious to remark how much the discovery was owing to chance, and how nearly it was missed. "An accident that delayed for some days the steam boat in which Mr Crawfurd was descending this river, allowed him to land, accompanied by Dr. Wallich, and to investigate the structure of the country for some miles on the north-east of Westmasnt. The accident arose from the shallowness of the water when the steam boat was descending, which, fortunately for geology, caused it to run aground near the wells of petroleum, where the left bank of the river presents a cliff of several miles in length, generally perpendicular and not exceeding eighty feet in height. At the bottom of the cliff the strand was dry, and on it were found specimens of petrified wood and bones, that had probably fallen from the cliff in the course of its decay; but no bone was discovered in the cliff itself by Mr. Crawfurd aud Dr. Wallich; nor were they more fortunate in several places where they dug in search of bones in the adjacent district. This district is composed of sand hills that are very sterile, and it is intersected by deep ravines: among the sand are beds of gravel often cemented to a breccia by iron or carbonate of lime; aud scattered over its surface at distant and irregular intervals were found many fragments of bone and mineralised wood, in some instances lying entirely loose upon the sand, in other cases half buried in it, with their upper portions projecting, naked, and exposed to the air; they appear to have been left in this condition, in consequence of the matrix of sand and gravel that once covered them undergoing daily removal by the agency of winds and rains, and they would speedily have fallen to pieces under this exposure to atmospheric action, had they not been protected by the mineralisation they have uudergone.

"On examining many of the ravines that intersect this part of the country, and which were at this time dry, the same silicified wood was found projecting from the sandbanks, and ready to drop into the stream; from the bottom of which the travellers took many fragments, that had so fallen during the gradual wearing of the bank, and lay rolled and exposed to friction by the passing waters. Some of these stems were from fifteen to twenty feet in length, and five feet in circumference. These circumstances show, that the ordinary effect of existing rains and torrents is only to expose and lay bare these organic remains, and wash them out from the matrix to which some other and more powerful agency must have introduced them."

"Of the total number of bones in this collection, about one-third have suffered from friction; and of the remainder, nearly all appear to have been broken more or less, before they were lodged in the places where Mr. Crawfurd found them irregularly dispersed. Many fragments also of the ivory have been rolled considerably; but no one specimen of that substance, or indeed of any bone in this collection, has been reduced to the state of a perfect pebble: from this circumstance we may infer, that the waters which produced the rolling these have undergone, were not in violent action during any very protracted period of time."

The bones of this collection have been referred by Mr. Clift to the following species.

Class MAMMALIA.

Order PACHYDERMATA	Mastodon latidens
	——— elephantoides
	Hippopotamus
	Sus
	Rhinoceros
	Tapir
RUMINANTIA	Bos

Class REPTILIA.

Order CHELONIA	Trionyx
	Emys
SAURIA	Leptorynchus
	Crocodilus vulgaris

The proportional number of bones belonging to each of these animals may be stated as follows.

Mastodon	150	Ox, deer, and antelope	20
Rhinoceros	10	Gavial, and alligator	50
Hippopotamus	2	Emys	20
Tapir	1	Trionyx	10
Hog	1		

The two species of Mastodon are new, and what is interesting to remark, they appear to fill up an interval which existed between the genera Mastodon and Elephas.

"On comparing the teeth of our Mastodon latidens," says Mr. Clift, "with those of the Mastodon of the Ohio (M. giganteum), we shall find the elevated points or ridges in the tooth of the former more numerous, less distant, and the interstices less deep than in those of the latter; in short, we shall observe that the teeth begin to assume the appearance of those of the elephant. On advancing to Mastodon elephantoides, we shall find all these features of similarity more strongly developed, the points and ridges are still more numerous, and the structure, were it not for the absence of *crusta petrosa,* becomes almost that of the tooth of the elephant. In both, though the teeth are formed upon the principle by which the tooth of the Mastodon is distinguished from that of the elephant, the crown of the tooth wears away more like the tooth of the elephant than that of the other Mastodons; and when worn, exhibits a surface not unlike that presented by the worn tooth of an Asiatic elephant."

Of the two species of Mastodon, *M. latidens* was the larger. "The tusks, judging from the alveoli, must have been of equal volume with the tusks of the largest living elephant." His habits "must have borne a close relation to those of the elephant. The proboscis must have been an organ of equal power with that of the elephant, for collecting the food to be subjected to the action of the powerful grinders; and this food (judging from the general structure of the teeth and the more compact jaw bone) probably consisted of harder vegetable matters than those which the slighter structure of the elephant's jaw usually encounters."

"The size of *M. latidens* appears to have equalled, if it did not surpass, that of the largest living elephant. A string passed round the lower jaw, over the anterior part of the grinder where it is worn, measured two feet four inches; while a string passed round the lower jaw of the largest Asiatic elephant in the museum of the college, at the same point, gave two feet three inches; and the cranium of this elephant has always been considered a very large one."

The *M. elephantoides,* though smaller than the preceding, appears to have had a closer affinity to the elephant, and this not only in the teeth, but in the contour of the jaw. It appears to have formed the transition between the two genera of Mastodon and Elephas. Mr. Clift, however, thinks it "not impossible that there may yet be a link wanting which might be supplied by an animal having a tooth composed of a greater number of denticules, increasing in depth, and having the rudiments of *crusta petrosa,* that necessary ingredient in the tooth of the elephant, and the entire absence of which distinguishes the tooth of the Mastodon."

For the satisfaction of the scientific naturalist we subjoin the specific characters.

M. latidens.—M. dentibus molaribus latissimis, denticulis rotundatis, clevatis; palato valde angusto.

M. elephantoides.—M. dentibus molaribus latis, denticulis numerosis, compressis.

Of the other remains nothing can be predicated with certainty as to the species, the fragments being too few or imperfect.

"It is worthy of remark, that most of the bones do not appear to have undergone any mineral change, with the exception of being abundantly penetrated with hydrate of iron; and that they are very brittle. This last circumstance, arising from the loss of their animal gluten, indicates that they are of great antiquity, and that they have not been imbedded in any very compact soil."

"The teeth of the Mastodon from the banks of the Ohio, which lie in a strong blue clay, have almost as much animal matter as we should expect to find in a recent tooth."

"The bones which form the subject of this memoir are almost in every instance fractured; and the fracture, from its direction and cleanness, the sharpness of its edges, and the firm texture of some of the bones, appears to have been produced by a very great power operating with sudden violence." It may be added that these remains "indicate the existence of animals which could only have found subsistence in vast forests or widely extended plains, in marshes, or deep broad rivers."

Dr. Buckland in his paper remarks: "It cannot but occur to us in this stage of our inquiry as remarkable, that not one fragment is found in all this collection, either of the elephant, tiger, or hyæna, which now abound so much in India; whilst the mastodon, whose living analogue exists not upon earth, must probably at one time have swarmed in the districts bordering on the Irawadi. The same anology which emboldened me in my first paper on the cave of Kirkdale, to anticipate the discovery which was speedily made of hyænas' bones in the diluvium of England, arguing on the fact of their existence in the diluvium of the European continent, at the present moment, encourages me also to anticipate the future discovery of the elephant, tiger, and hyæna in the diluvium of Asia. I would also argue, on the same grounds, that it is highly probable we shall hereafter find the mastodon in our own diluvium, and most recent tertiary strata."

Dr. Buckland then discusses the question of the origin and date of these remains. After observing that " it is of course impossible for any person, who has not been on the spot, to decide with certainty on a question which requires so much minute local investigation by a very experienced observer," he concludes that we must admit one of the three following suppositions.

1. " Either they were lodged in the most recent marine sediment of the tertiary formation, like the elephant in the crag of Norfolk, the rhinoceros of Placenza, and the mastodon of Dax and Asti."

2. " Or in antediluvian fresh water deposits, analogous to those which contain the rhinocéros, elephant, hippopotamus, and mastodon in the Val d'Arno."

3. " Or in diluvial accumulations more recent than either of these formations, and spread irregularly, like a mantle, over them both."

" Now as we find, on careful examination of the matrix adhering to these bones, that it contains neither fresh water nor marine shells, and is wholly different in character from all the specimens which contain such shells, and which thereby enable us to refer them respectively to fresh water or marine origin ; the most probable conclusion we can arrive at is, that the bones belong to neither of these formations, and that their matrix is of the same diluvial character with that in which the greater part of the fossil bones of Mammalia have been discovered in Europe."

Dr. Buckland also thinks the following formations may be recognised in Ava, with more or less certainty. " 1.Alluvium. 2.Diluvium. 3.Fresh water marl. 4. London clay and Calcaire grossier. 5.Plastic clay with its sands and gravel. 6.Transition limestone. 7. Grey wacke. 8. Primitive rocks, marble, mica slate. There are also indications (but less certain) of new red sandstone, and magnesian limestone."

1. To the alluvium belong the deltas occurring from Prome to the sea, and a number of islands that are continually forming and shifting at various places along the whole extent of the actual bed of the Irawadi. 2.To the diluvium are referred the sand and gravel beds containing the mineralised bones. 3. The fresh water formation occurs a little north of the petroleum wells, " and is at an elevation of 150 feet above the Irawadi. The specimens of it consist exclusively of marly blue clay containing fresh water shells of the genus Cyrena : the shells are very thick and heavy, nearly three inches in diameter, and judging from the great quantity imported, must be extremely abundant." 4. From the hills near Prome was obtained a coarse grained, yellow, shelly and sandy limestone, scarcely distinguishable from the calcaire grossier of Paris ; and from several places higher up the Irawadi, particularly at Pugan, we have a dark bituminous slaty limestone, in which Mr. Sowerby has recognised the following fossils as identical with those of the London clay : Ancillaria, Murex, Cerithium, Oliva, Astarte rugata, Nucula rugosa, Erycina, Tellina, Teredo, teeth of shark, scales of fishes, pebbles of rolled black bone, unknown radiating fossil resembling coral."

" This recognition of a stratum so nearly resembling the London clay in so distant a part of Asia receives still further interest when viewed in conjunction with the information that has been afforded to us by Mr. Colebrooke, as to the existence of a similar formation at Cooch Behar, in the north eastern border of Bengal, where the Brahmaputra emerges into the plain. Here Mr. Scott discovered strata of yellow and green sand alternating with clay, that lie horizontally at the height of about 150 feet above the level of the sea, and contain organic remains resembling those of the blue clay of the London and Hampshire basin.

" Mr. Scott has also discovered at Robagiri in this same district a stratum of white limestone containing Nummulites and vertebræ of fish, surmounted by beds of clay which contain the same Nummulites and also bones of fish with shells of Ostrea and Pecten."

" Near Silhet the Laour hills, composed of white limestone loaded with Nummulites, form another example of tertiary formations in the eastern extremity of this province. And the section near Madras, given by Mr. Babington, shows the same tertiary formations to exist also on the western shores of the Bay of Bengal."

5. In many of the specimens from near Prome, a soft green and yellow sandstone was found, resembling that of the plastic clay formation. " Mr. Crawfurd describes these as associated with reddish clay, intermixed with sand and pebbles, in words that are almost equally applicable to" the " English plastic clay pits at

Reading or Lewisham. He found them in many places where he landed along the shores of the Irawadi; and near Pugan and Westmasut they were associated with brown coal and petroleum precisely as we find them containing brown coal all over Europe, and connected with wells of petroleum near Parma, and also in Sicily, and near Baku on the west coast of the Caspian. Near the petroleum wells of Westmasut Mr. Crawfurd also found large selenites resembling those that occur at Newhaven in our plastic clay. In Ava, as in Europe, they seem to be coextensive with the clay beds of the tertiary formation."

6. "The transition limestone appears, from the few specimens" obtained, "to be of the same character with that of Europe, but in these specimens there are no organic remains." 7. "There are specimens of greywacke much charged with carbonate of lime, from so many distant points along the Irawadi, that in the absence of better information we may conjecture the fundamental strata of this region to belong to the transition series, and that they are covered more or less by the tertiary strata and diluvium which we have been considering."

8. "From the mountains of the Sakaing chain, a little above Ava, we have much pure mica slate and statuary marble in its usual connection with mica slate and hornblende rock; this marble is of the finest quality, and extensively employed by the natives in making images of Budda."

"The specimens afford no decided example of secondary rocks in this district; but a reddish sandstone, which is used for architecture in the construction of thrones to receive the images of Budda, and a limestone which resembles the magnesian limestone of England, may," it is thought, "with more probability be referred to the new red sandstone than to any other formation."

"The extent and relative position of all these strata it was impossible to ascertain from the few opportunities afforded to Mr. Crawfurd of landing from the steam boat in which he made his voyage: these may become the subject of future investigation."

In an appendix is given a translation of a passage from Ferishta, in which mention is made of numerous bones (some very large) being found in cutting through a mound or bill that lay between the Sersutti and Sulima rivers, both of which join the Setluj. The site was near the town of Pinjore, N. Lat. 30° 47′ E. Long. 76° 54′.

And in a postscript notice is taken of a statement published in Lieut. Alexander's travels in the Burman Empire, of the petrifying quality of the waters of the Irawadi, and their daily and hourly operation. Dr. Buckland shows, what every reader has doubtless perceived, the improbability of the statement; and further its opposition to Mr. Crawfurd's observations. In another part of the volume, he brings forward the positive testimony of Dr. Wallich to prove the incorrectness of the statement.

XXVI. *Description of Fossil Remains of some Animals from the North Eastern Border of Bengal. By* J. B. Pentland, *Esq. in a letter to* W. H. Fitton, *M. D. P. G. S.*

This is a short paper in which Mr. Pentland gives an account of a discovery he has made, in examining the collection of specimens from Caribari in Cooch Behar, presented by Mr. Colebrooke, of the remains of four different species of Mammalia; 1. a species of the "Anthracotherium of Cuvier; 2. a small species of Ruminant allied to the genus Moschus; 3. a small species of herbivorous animal referable to the order Pachydermata, but more diminutive than any of the fossil or living species; and 4. a carnivorous animal of the genus Viverra."

The Anthracotherium appears to Mr Pentland to form a new species, and he proposes to name it *A. Silistrense**. Mr. P. intends resuming the subject.

XXVII. *On the Cycadeoideæ, a family of Fossil Plants found in the Oolite Quarries of the Isle of Portland, by the Rev.* William Buckland, *D. D. F. G. S. F. R. S. F. L. S. Prof. Geol. Oxford. pp.* 395-404.

This is an extremely interesting paper, and we wish our limits would allow of our giving it in full. But our analysis has already extended to such a length, that we are forced to compress more than we could wish. We will at least endeavour to give the most interesting facts.

In the celebrated freestone quarries of the Isle of Portland have been found remains of a silicified plant, which from their peculiar shape have obtained from the workmen the name of petrified birds' nests. Their mineral character varies from that of coarse granular chert to imperfect chalcedony. On being submitted "to

* From one of the ancient names of the Brahmaputra.

Mr. Brown and Mr. Loddiges, they immediately recognised a resemblance to the genera Zamia and Cycas which compose the existing family of Cycadeæ, a resemblance which further investigation has tended fully to establish." Their structure will perhaps be best explained by a detailed comparison.

"In the *Zamia horrida* of the Cape of Good Hope, the transverse section exhibits one narrow circle, composed of radiating plates, placed in the cellular substance that forms the stem or body of the plant; and nearly at equal distances between its centre and circumference, and in a section near the base of *Cycas revoluta,* we have two narrow circles of radiating plates, placed also in the cellular substance that forms the stem, and both of them nearer to the centre than to the circumference of the plant; the outermost of these circles is the most narrow. Neither Zamia nor Cycas has any covering of true bark, but the stem is enclosed in a thick case made up of the persistent bases of decayed leaves: each of these terminates externally in a lozenge shaped impression or scar, the convex surface of which formed the joint from which a leaf had fallen off. A dense and continuous series of these leaf joints entirely surrounding the stem gives it more the appearance of a pine apple*, or enormous fir cone, than of a vegetable trunk. On comparing these peculiarities of structure with those displayed in our fossil specimens, we recognize a correspondence highly curious and satisfactory. Like the recent Cycadeæ, our fossil stems are inclosed in no true bark, but have a thick case made up of the flat persistent bases of decayed leaves, which at their inner extremity touch the cellular tissue of the body of the trunk, and terminate externally in an oblong gibbous joint resembling the leaf joints of *Zamia horrida*. These bases of leaves rise upwards and are most perfect near the summit of the trunk: but lower down, the oldest of them lie horizontally and at right angles to the trunk, which they entirely encircle, and are more and more compressed as they are nearer to the root. In form, position, and structure, as far as it can be recognized, these parts resemble what we find in the recent Cycadeæ; as yet no leaves have been found adherent to any of the fossil plants. At the summit of most of them is a cavity, the contents of which appear to have been removed before petrifaction took place: the relation of this cavity to the trunk and leaves may be seen by comparison with living plants of *Zamia horrida, Cycas circinalis,* and *Cycas revoluta,* referred to in the note†. Although the trunk is so tall in *C. circinalis* as sometimes to exceed 30 feet in height, it rarely attains 6 feet; in *C. revoluta,* and in other species it is much shorter.

The trunk of many Zamias is also very short. In the fossil specimens yet discovered, it varies from five inches to a foot in height, and from eight to fifteen inches in diameter. They are generally terminated downwards by a broad flat base without any adherent roots; but traces of the commencement of roots, having a cavity at their centre, are visible in Sir G. Grey's specimens. At the base of other specimens there is only a coarse irregular reticulation, apparently connected with the commencement of roots. The interior of these stems presents two varieties of structure, which we find accompanied respectively by a corresponding variation of external form, size, and character in the bases of the leaves. These differences are sufficient, in the opinion of Mr. Browne, to establish a new family, to which we may refer the two extinct species before us, the one resembling a recent Zamia, the other a recent Cycas; and to which from its near approximation to the existing family of Cycadeæ, the name of Cycadeoideæ seems appropriate."

"In the largest and most abundant species *(Cycadeoieda megalophylla)* the bases of the leaves vary from one to three inches in length, having nearly the form and size of those of *Zamia horrida,* and measuring from one-fourth of an inch in the shorter, and from one to two inches in the longer diameter of their transverse section. The trunk is short and flat, and exhibits a deep cavity at its summit resembling a bird's nest."

"The origin and use of this cavity is rendered perfectly intelligible by comparing it with that at the summit of the *Cycas revaluta* which bore fruit at Farnham, in 1799, of which a description and plate are given by Sir J. Smith‡. This cavity is surrounded with a magnificent crown of leaves, whose stalks are set round its mar-

* In the plate it has exactly this appearance.—E. G.

† "See in plate XLVI. fig. 4. a plant of Z. horrida; and in Curtis's Bot. Mag. for June 1828, pl. 2826, a male plant of C. circiualis in flower; and in Trans. Lin. Soc. Vol. VI. pl. 29, a female plant of C. revoluta in fruit."

‡ Tr. Lin. Soc. pl. 29. p. 312.

gin like the bases of the leaves in our fossil specimens. The cavity itself is occupied by a cluster of fronds producing the fruit or drupæ; and it is a striking coincidence, that Sir J. Smith, in describing this cavity, makes use of the same comparison (hollow like a bird's nest) which has been applied by the quarry men at Portland to the fossils I am now associating with the recent Cycadeæ. In the central cavity of this fossil, there are no remains of frond or fruit, but a convex mass of cellular tissue, which probably formed the support of the proliferous fronds. Where the trunk is broken below the summit, we find the same central mass of cellular tissue as in the transverse section of the stems of recent Zamiæ and Cycades."

"Near the circumference of both specimens there is a laminated circle as in the trunk of a recent Zamia, but differing in that it is much broader and placed nearer the circumference of the stem: the large and visible plates of the circle, when magnified with a lens, appear made up of smaller plates almost invisible to the naked eye, more numerous and closer to each other than in the laminated circles of a recent Zamia."

"Between this radiating circle and the other case of leaf stalks, is a narrow band or ring of minutely cellular substance, analogous to the similar but much broader band of cellular tissue that divides the radiating circles from the bases of the leaves on the recent Cycadeæ."

"In the second and smaller species (*Cycadeoidea microphylla*) the bases of the leaves are also lozenge shaped and about an inch in length, but small and numerous, much like those of the Xanthorrhæa or gum plant of New South Wales. The trunk is longer in proportion to its width, whilst its transverse section exhibits at the centre the same indistinctly cellular appearance as the species last described; but near the circumference instead of one it has two laminated circles, and interior to each of these a narrow band devoid of laminæ, analogous to the two bands of cellular substance that are placed in similar relation to the two laminated circles in a recent Cycas."

"These two circles, like the one circle of *Cycadeoidea megalophylla*, approach the circumference, whilst those in Zamia and Cycas are placed nearer the centre of the stem."

"There is also a further analogy between this fossil species, and the recent Cycas, viz. that in each case the outermost of the plated circles is the most narrow of the two, and the cellular band between, is in both cases also narrow."

"The engraving of Cycas revoluta, copied from the *Hortus Malabaricus*, exhibits many plated circles divided from one another by narrow cellular bands, and these also are placed nearer to the centre than to the circumference of the stem."

It appears from these and other details that the fossil plants approach something nearer to the structure of dicotyledonous woods than the Cycadeæ; and that they form a link between these latter and the coniferæ, from which they are otherwise so widely separated, though resembling them perfectly in the character of their organs of fructification.

From the particulars of various, hitherto considered anomalous, fossils collated by Dr. Buckland, he draws the conclusion, that one or both of the cognate families of the Cycadeæ and Cycadeoideæ belong to the beds of the Oolite series.

"M. Adolphe Brogniart has pointed out the inferences with respect to climate that may be drawn from the varying character of vegetable life in the three grand epochs of geological formation: viz. the great carboniferous period: the period of the secondary strata from lias to chalk inclusive; and the period of the tertiary strata above the chalk."

"The plants before us render it probable that the climate of these regions, at the time when the Oolites were deposited, was of the same warm temperature with that which produces a large proportion of the existing Cycadeæ.

M. Adolphe Brogniart is also of opinion, that it exceeded the temperature of our modern tropics at a still more early period when it maintained the extraordinary vegetation of the great coal formation; and that it was less than tropical, though warmer than it is at present, in the period to which we owe our tertiary strata. To this theory I see much reason to incline, and confidently look forward to its future development in the examination of the Flora of the fossil world, which he is now so actively conducting."

GLEANINGS

IN

SCIENCE.

No. 7.—July, 1829.

I.—*On Hygrometry.—No. 2.*

To the Editor of "Gleanings in Science."

SIR,

In my communication published in your second number, I promised to return to the subject of hygrometry, and show what are yet the desiderata of the question. I mentioned, that we laboured chiefly under the want of accurate and full experiments, to enable us to determine the constants of the equation, (which I there gave,) in order to apply it to practice. I have just received your third number, in which has appeared another communication on the same subject by your correspondent P. This paper supplies the very experiments I was in search of; and although not quite prepared to follow up the subject with the fulness I contemplated, I cannot deny myself the pleasure of showing the utility of my formula, by applying it to these admirable experiments. Before I conclude my letter allow me to return this gentleman my thanks for his valuable communication, which has undoubtedly finished the experimental part of the edifice commenced by Dalton. Unless I am much mistaken, there remains little now to be done in this branch of physics; and the problem of determining the moisture in the air is become as simple nearly and as level to ordinary capacities as that of the temperature or the density. It is a curious illustration of the subject to think that metereologists have so long had in their hands, without knowing it, the best hygrometer that can be devised; the most accurate and the cheapest; while the inventive spirit has sought it in vain in oat beards, deal boards, catgut, whalebone, ivory tubes, rat's bladders, and the thousand and one other substances proposed from time to time for this purpose. But I am reminded of the narrow limits of your work, and must therefore hasten to the proper subject of my communication. With every wish for the success of it,

I am, Mr. Editor,
Your obedt. servt.
D.

From the remarks I have had the advantage of hearing on my first paper, protected by my incognito, I incline to draw the conclusion that I have myself fallen into the mistake censured by me in another " of having perplexed plain men and obscured a simple subject." I am sensible, in fact, that I have given my readers credit for being equally familiar with the subject as I was myself. I have in consequence slurred over many subjects that required explanation; and have thus become obscure where I could least afford it: I shall endeavour to avoid this error on the present occasion.

The object of hygrometry is to determine the quantity of moisture existing at any moment in any gas, but principally in the atmosphere; and the probability of rain.

The moisture in the air exists in two states or conditions: 1. That of a transparent invisible gas; and 2. That of cloud. In the first case it has all the properties of the gases; such as elasticity, expansibility by heat, compressibility by pressure, &c. In the state of cloud it is otherwise: it has changed the gaseous for the fluid form; but the particles into which the gaseous atoms have condensed, are so wide apart, as to remain so, not being attracted by each other; and they are so small, that they are borne up by the resistance of the air, and thus float in a medium the specific gravity of which is less than 1-800th of their own*.

* The resistance of the air to any body increases or decreases as the square, the weight as the cube, of the diameter. We may imagine then a size of particle of the densest matter which shall float in the lightest medium.

With regard to the invisible aqueous gas which we may call vapour, we have ascertained the following facts: 1. Like air its rarity increases and consequently its speci c gravity decrea es with the temperature: the law too is the same which air follows. 2. Like air it has a certain spring or elasticity which, like that of air, is capable of be'ng easured by the column of mercury it will support. 3*. *Unlike* air its compressibility is limited; for on exceeding a certain pressure, which is different at different temperatures, the vapour changes or condenses into cloud.

SCHOL. The pre sure which aqueous vapour will bear without condensation is called the *maximum tension* or *constituent force* of the vapour; sometimes simply the *force* or *tension*. It is generally expressed in inches of mercury. Thus at temperature 212, it is 30 inches; at 192°, 20 inches; at 162½°, 10 inches; at 135°, 5 inches; and at 8.°, 1 inch. These temperatures are called the *constituent temperatures* of vapour having those tensions.

4. With air, we know, cold has the same effect as compression: so with aqueous vapour. If we cool a portion of the latter below the constituent temperature, that is to say, the tempteraure answering to its tension or elastic force; part of it will be changed or condensed into cloud.

SCHOL. Vapour may he heated above its constituent temperature, but cannot be cooled *below* it without change of state. It is the lowest temperature at which vapour of that tension can exist. It is therefore called *constituent*, inasmuch as the vapour, if cooled below it, changes its state or constitution from the gaseous to the fluid.

5. Two volumes of air of different temperatures being mixed, the resulting temperature will be the arithmetical mean: two portions of aqueous vapour of differeat constituent temperatures being mixed, the resulting temperature will be something *higher*. The constituent tension of vapour in fact increases in a higher ratio as the temperature is higher: the mean of the constituent tensions can therefore never correspond with the mean of the temperatures answering thereto; but is always something more. In order, then, to re-establish this correspondence, part of the vapour suffers condensation; by which the temperature being raised, and the tension diminished, the two powers are found at last in equilibrio. An example will make this clearer. The constituent temperature of vapour of 1 iuch force is 80; of ,5 inch ,59°.—The mean of the temperatures is 695, and of the tensions ,75 inch. But a temperature of 695, can only uphold vapour of tension ,71 inch; vapour of a greater tension as of ,75 in. on being exposed to a temperature of 69, 5 would be partially condensed; till the extrication of heat and diminution of tension together, restored the relation that must exist between the two phenomena. In fact vapour of tension, 75 inch would require a temperature of 71. The higher the temperatures, the greater will the discordance be.

SCHOL. The preceding fact furnished the grounds of Dr. Hutton's theory of rain. Two portions of vapour, each at its lowest temperature, being brought into intimate mixture, the result is partial condensation, which, if it be sufficiently copious to allow the small particles to congregate into drops of sufficient size, will terminate in rain; but if the supply of moisture fail, and the particles remain small as to be huoyed up by the resistance of the air, theu is a cloud or fog the consequence.

It appears theu that the great point in metercology is to ascertain the constituent temperature of the vapour actually existing in the air. Comparing this temperature with that likely to be establisbed by currents or radiation, we obtain a measure of the probability of rain; which ought thus to be more or less copious, as we find the temperatures more or less differ. The constituent temperature of vapour is also called the *temperature of the dew point*. It is determined directly by Dalton's experiment described in my former paper, or by Mr. Daniell's hygrometer founded on the same principle.

In India it does not appear that these methods are at all times practicable, so low is the dew point occasionally. Under the most favourable circumstances they are troublesome, and not likely to be repeated too frequently.

The moistened-bulb thermometer which indicates the temperature of an evaporating surface, affords much greater facilities; facilities equal in every respect to those available in observing the temperature. A great point is gained too by using this instrument; the power of observiog the extremes, which no hygrometer yet proposed has effected. By employing a register thermometer, and covering the bulbs with cloth kept moistened, we may obtain an idea of the maximum and mini-

* This must be understood to refer to ordinary pressures. By employing extraordinary force it is supposed that air also may be liquefied.

mum quantities of evaporation, and thence of the extreme hygrometric states of the air in any interval of time.

I shall now show how the constituent temperature of vapour or the dew point can be determined from the indications of the moistened-bulb thermometer. Water is known to evaporate at every temperature. The rapidity of evaporation, it is also known, depends amongst other things upon its temperature; the dependance being such that it is proportional to the force of vapour due to that temperature. These propositions relate to a dry atmosphere.

If the moisture in the air be at its constituent temperature, i. e. that below which it will not bear being cooled without condensation, then evaporation must be altogether suspended; because as the temperature is still the same and the vapour is at its maximum tension, it cannot receive any accession. We may be certain, then, that if evaporation be entirely suspended, the constituent temperature of the vapour in the atmosphere will be the same as the temperature of the atmosphere itself.

In the case then in which the atmosphere contains a maximum of aqueous vapour, evaporation stops altogether. In the case in which there is no aqueous vapour whatever in the air, evaporation is as the force of vapour belonging to the temperature of the evaporating surface. In the intermediate case, that is, when a less quantity than the maximum is contained in the atmosphere, how is evaporation affected? In this case it is found to be proportional to the tension of vapour due to the temperature of the evaporating surface, *minus* the tension of the vapour in the air.

But now determine the rate of evaporation? One of the most striking phenomena of evaporation is the cold produced by it; the consequence of the absorption of heat attending the conversion of water into vapour. This depression of temperature must evidently be as the evaporation; or rather the momentary depression will be in proportion to the rapidity of the evaporation. The momentary depression is equal to the momentary increment of heat which would take place, were the cooling power of evaporation suspended and the moistened bulb thermometer allowed to assume the temperature of the air. This is known by experiment to be as the 1,233 power of the total depression. The evaporation will then be as the 1,233 power of the depression. But the evaporation is as the tension of the evaporating surface *minus* the tension of the vapour in the air. Then, finally, the tension of the evaporating surface *minus* the tension of the vapour actually existing, is as the 1,233 power of the depression. As the temperature of the evaporating surface is given us by experiment, we have only to compare it with the temperature of the air to obtain the depression. From these two data the third or unknown quantity, the tension of the moisture in the air, is easily found.

Trusting that this rather lengthy detail has made the rationale of the subject a little clearer, I proceed to the consideration of the formula with which I terminated my last paper.

This was $(f)\, n\frac{w'}{s'}+f'n'\frac{w'}{s'}=\frac{L\,e\,(F'-f)\,30}{B}$ (A)

There is an oversight in this formula, for which the original of my paper I fear may be answerable. It should have been

$$(f)\,D\left(n\frac{w'}{s'}\right)+(f)\,D\left(\frac{w'}{s'}\right)=\frac{L\,e\,(F'-f)\,30}{B}* \qquad (B)$$

D being the depression.

The 2d member of the left side of the equation is so small on all ordinary occasions, that when the experiment is properly performed it may be reduced to 0. We will therefore, to simplify the consideration of the subject, reject it altogether; and putting A = the constant $n\frac{w'}{s'}$ and dividing by e, we shall have

$$\left((f)\,D\right)\frac{A}{e}=\frac{L\,(F'-f)\,30}{B} \qquad (C)$$

The value of the function (f) as given by MM. Dulong and Petit being substituted, and the constants brought to the left side, the equation will become

$$\frac{D^{1,233}\,A\,B}{L.\ e.\ 30}=F'-f \qquad (D)$$

* The use of the character f, on one side of the equation, to express a *quantity*; on the other, *a function*; is objectionable: it was occasioned by a want of the proper character. It has been attempted to guard against the obscurity by enclosing it in brackets thus (f).

To apply these to the experiments in question, we must consider, that as the a. was dry $f = 0$. The values of A and e must be determined from the observations themselves; to do this the formula will become

$$\frac{A}{e} = \frac{L\,F'\,30}{D^{1\cdot233}B} \qquad (E)$$

We will now take one of the results in page 80 to determine the numerical value of this equation. The temperature of the air was 60°, surface of evaporation 39°,9, Depression 20,1 Barometer 29.27*.

Now in the above equation

B (Barometer) = 29, 27.

D (Depression) = 20, 1.

F' (Force of vapour due to temperature of evaporating surface) for 39°, 9 = ,2635 inch.

L (proportion of mass of water to the vapour required to be evaporated to produce a fall of 1°.) for 39. 9 =895,9†

The calculation will now stand thus

Log. of F' (,2635 inch)		9,42078
Log. of 30		1,47712
Log. of L (895,9)		2,95226
Log. of $D^{1\cdot233}$ Log. of 20°,1 X 1,233)	Ar. Co.	8,40619
Log. of B. (29,27)	Ar. Co.	8,53358
Log. of $\frac{A}{e}$ = sum		0,78993

In this manner the value of the constant logarithm was found for each observation; these values are as follows. 7899, 7732, 7763, 7840, 7859, 7872, 7837, 7771 and 7505, the index being 0.

These agree sufficiently well, with the exception of the last, and there appears to be some anomaly in that observation. The mean of the first eight is 0.7822 and of the whole 0.7787. The first answers to the N. N. 6 056 and the latter 6.008. The formula (C) divided by A and multiplied by e will then become

$$D^{1\cdot233} = \frac{L.\,(F'-f)\,30}{6.056\,B}$$

And extracting the root,

$$D = \left(\frac{(L\,(F'-f)\,30}{6.056\,B}\right)^{\frac{1}{1\cdot233}}$$

Taking $f = 0$, B = 30, and substituting for F' and L the values due to each of the temperatures 40°,45,50,55,60,65,70,75, 80, I find the following series.

Temperature of air.	Temperature of evaporating surface.	Depression.
Deg. of Fah.	Deg. of Fah.	Deg. of Fah.
142,10	80	62°,10
129,11	75	54,11
117,05	70	47,05
105,86	65	40,86
95,40	60	35,40
85,62	55	30,62
76,42	50	26,42
67,57	45	22,57
59,60	40	19,60

* Mean of all the observations, page 79.

† As the publication referred to in my former paper for the value of L may not be at hand, I subjoin a few values which will be sufficient for verifying these calculations, or making new ones: 40°898, 50°919, 60°941, 70°962, 80°983, 90°1005.

From this series the following is easily found by interpolation and corrected for barometer 29,27.

Temperature of air.	Depression of moist Bulb.	Depression from Table p. 80.	Difference.
140	61,3	62,6	1,3
130	55,1	55,5	0,4
120	49,2	49,1	0,1
110	43,5	43,3	0,2
100	38,2	38,0	0,2
90	33,1	33,0	0,1
80	28,4	28,5	0,1
70	24,0	24,2	0,2
60	20,0	20,1	0,1

Excepting the first, which, as I said before, appears to include some anomaly, the differences are here within the errors of observation.

The above may be sufficient to show the value and the uses of the formula. I am now constructing tables from which the hygrometrical state of the air shall be readily found, the temperature of the air and of a moist evaporating surface being given. The table given by your correspondent page 81 founded on experiment is almost sufficient; nevertheless it will be interesting to compare the results of the formula with those of observation. I may add, for the use of those who observe with Leslie's form of the instrument, that we must multiply the constant 6.056 by $\left(\frac{50}{9}\right)^{1\cdot233}$ or increase the logarithm 0.7822 by 0.9122 = 1.6944 the natural number of which is 49.5. Adapted to Leslie's hygrometer then, the formula (D) will become

$$\frac{49{,}5\, H^{1\cdot233} B}{L\ 30} = F' - f$$

where H is the indication of Leslie's hygrometer; F′ is, as throughout this paper and the preceding, the force of vapour due to the temperature of the evaporating surface; and f, that of the dew point. As this last is the quantity sought, it may be more conveniently given

$$f = F' - \frac{49{,}5\, H^{1\cdot233} B}{L.\ 30.} \qquad (G)$$

L always corresponds to the temperature of the moist surface. This temperature in the case of observing Leslie is = temperature of air — H × 9 ÷ 50; 50° of Leslie's scale being equal to 9° of Fahrenheit.

Postscript.

The value of the co-efficient 1.233 I thought it preferable to take from Dulong and Petit, as their experiments appear to have been conducted with such care. It would however be easy to deduce them from the experiments given p. 80, as follows.

The equation (D) being divided by the constants, we shall have in the case of dry air when $f = O$, putting $n = 1.233$

$$\frac{D^n}{L} : \frac{D'^n}{L'} :: F' : F'' \text{ or } D^n : D'^n :: F\,L : F''\,L' \text{ that is } \frac{D'^n}{D} = \frac{F''\,L'}{F'\,L}$$

or in logarithms, n (Log. D′—Log. D) = Log. F″ + Log. L′—Log. F′+Log. L that is

$$\frac{\text{Log. } F'' + \text{Log. } L' - \text{Log. } F' - \text{Log. } L}{\text{Log. } D' - \text{Log. } D} = n$$

In this way I calculated the value of n for each of the observations page 80 as compared with the first, and found the following values 1.204, 1.230, 1.267, 1.266, 1.267, 1. 252, 1. 222, 1.181, mean 1.236, or, rejecting the last, 1. 244. The first is only 003, the second 011 more than Dulong and Petit's index; and this excess may be owing to our having neglected to make any estimate of the effect of radiation. On this latter account it might perhaps be preferable to use this value of the index, where the actual formula is considered to be too complicated: in this case it will be necessary to alter the value of $\frac{A}{e}$. Calculating as before the logarithm will be found to be 0,7520, 0,7486, 0,7504, 0,7578, 0,7597, 0,7617, 0,7503, 0,7451, 0,7239, mean 0.7500, or, rejecting the last 0. 7532.

This answers to the natural number $5\frac{2}{3}$. The formula (D) will be transferring F to the other side of the equation

$$f = F' - 5\tfrac{2}{3}\,\frac{D^{1\cdot244}\,B}{L.\ 30} \qquad \text{(H)}$$

With a table then of the 1-244 powers of the first 30 numbers, we may from this formula at once calculate the tension of the vapour in the air, having from observation the depression of the moist thermometer.

I am not yet prepared to say which formula will agree best with observation.

II.—*Proposal for the Establishment of an Agricultural Institution for the general Improvement of the Country.*

In what length of time natives and Europeans are likely to become more cordial than they are at present, is a question, that can only be determined by those who have the power of accelerating, or retarding that event.

The more admired writers on the subject of Indian improvement, have strongly advocated every means of conciliation: but so long as a system prevails that draws a line of demarcation between the two classes of subjects, no great approximation can ever arise.

In a condition not unlike that of the "adscripti glebæ" of Europe some centuries back, or that of the people of Russia in the present day, the great mass of the inhabitants of Hindustan pass from one set of masters to another, as often as the delinquency of the zemindar compels the State to transfer the superintendance of a district to other hands.

The dependance of the small farmer on the precarious justice of the zemindar, it is imagined, might in some degree be removed, by a gradual and cautious mixture of Europeans, and a dispersion of stock farms in the country, under wholesome regulations. The European might be bound to observe all his engagements, and would be more under the control of the State than the native. Any serious displeasure at his conduct would most likely ruin all his future prospects; whereas the native has often the greatest advantage he can reap, derived from the most notorious oppression.

It is not contemplated, that any attempt to administer the agricultural concerns of the country would ever be executed on any scale of importance to excite alarm in the native mind; but, that in those situations where stock farms may become established, the management of them should be chiefly or, perhaps, entirely in the hands of Europeans in the first instance.

It would be optional perhaps with Government to demand documentary evidence of just views and proper administration of such establishments, which might be reasonably expected perhaps on the grounds of statistical as well as of other considerations.

A Board of Agriculture might be formed for the purpose of informing Government of the state of the agriculture of the country, at which the most intelligent and respectable natives might deliberate, with or without European associates, under the protection of the State. The board might besides agriculture embrace such other statistical objects as should leave the native department to depend solely upon their own exertions; while the institution might be in the first instance considered as little more than the nucleus of a more important organ, which should patronise and support every species of improvement, and report annually the progress made in each branch of its labours, for the information of Government.

An institution of this kind would tend to bring the minds of the upper classes of natives to an exercise, by which in time their judgments would become formed and ripened.

It would have the beneficial consequence of accustoming them to formal deliberation, and would by degrees prepare their minds for the consideration of more refined subjects, by affording ample opportunities for that exercise of serious thought which would give them a considerable degree of confidence and importance in their own estimation, as in that of the people in general; and serve cautiously to develope those powers of mind, which would elicit occasion for their exertion on all correlative subjects.

It would be breaking the ice, without incurring the imputation of rash concession or of dangerous innovation:—it would be the commencement of a species of pastime that would promise in the end great moral advancement; or, to say the least, it would divert some leisure hours to the most worthy employment of their intellectual faculties; and, if other sacrifices were to be obtained, by a stronger appeal to vanity and ambition, the gain would be divided between the State and the community; while the society under an humble title would move forward in a smooth and equal course, that might defy malice even to interrupt its progress.

The great object, to induce the people to qualify themselves to THINK for themselves in minor concerns, once attained; the second step in the process of advancement would be to allow them to ACT for themselves in such affairs as their knowledge would qualify them to have the management of.

Here then it is conceived is no impracticable or dangerous method, by which the employment of native talent and wealth might be enlisted on the side of public improvement, that would create no expense to the state, while pernicious opulence in some instances would become diverted from demoralizing, to proper and legitimate objects, and puerile recreations would be converted into intellectual exertions for the general advance of public prosperity.

The plan proposes neither coercion nor concession; it simply invites the respectable and opulent to assist in forming a public institution having extensive improvement for its object. The natives themselves are more than willing to obey such a call; and if their ambition should desire some trifling favours, there would be no risk in making such concessions to people whose best exertions could be pressed into the service of the State, to accomplish the most desirable of all objects to every good Government.

The proposal partakes of nothing that is forced or unnatural. The people themselves are daily ripening into greater civilization, and nothing can be more natural and reasonable than to give a proper direction to the best intentions and to the most zealous exertion of their powers to improve their condition.

The adoption of all the objects could be made a very gradual one. The institution left to mere public feeling to originate it, would want that dignity which conflicting interests aided perhaps by other unfavourable circumstances would never ensure.

Undoubtedly, important and honourable as is the occupation of a merchant, it is commonly a very lucrative one; and if many do not realize large fortunes, the mercantile body is known to command those objects of wealth in some instances perhaps, which it would be difficult, if not impossible, to acquire in the ordinary routine of this class of society in Europe. The paucity of objects of commercial enterprise, of necessity tends to enhance the value of those speculations which are deemed most remunerating. Of these indigo is perhaps the most important, since it is almost the only medium by which private fortunes can be remitted to Europe. For the culture of the indigo plant the best soil is required, and the value of the manufactured produce is commonly so great, as to induce the cultivator to procure as much land of this description as he is able.

It is scarcely possible to conceive, under the wretched system of husbandry that prevails in all parts of the country, that frequent collisions and disputes should not arise from pursuing so obnoxious a species of cultivation, one so likely to produce misunderstanding between the two classes.

Even were the cultivator of such a crop as that of indigo protected from the absolute ruin which a bad season frequently occasions; that fever of mind which such an unsteady source of independence must commonly induce, would still have a tendency to create desires which can seldom be realized, and would eventually serve only to unfit the individual for any sober pursuit that might require a greater exertion of patient industry.

The ruin that has hitherto so frequently been entailed on the cultivator of indigo, will continue to recur, until a better method of agriculture shall point out more certain means of providing a reasonable independence.

Were coffee, cotton, and sugar on a large scale to be substituted for indigo, with the requisite attention to those necessary improvements which demand the exercise of patience and skill, the gambling speculations of the present day would be soon abandoned.

Large stock farms, employed as the means of producing good staple articles, would in the end be found more certainly profitable than that of a dye which barely yields a precarious subsistence to the actual labourers and responsible individuals.

Articles more bulky than indigo, but not less valuable to the agriculture and commerce of the country, would be produced in an abundance that would revive the shipping interest, and raise the character of the trade of the metropolis.

Cotton would become improved by a greater length of staple, which would ere long invite fixed capital to the country, and restore the manufacture of a necessary of life, that Europe in her commercial grasping had filched from an industrious people.

The native capital would then be brought to light, and the want of money would become a less frequent complaint.

The wealthy Bengal native merchant, in the variety of objects for the profitable investment of capital, would then feel no necessity to conceal his treasures ; while the extension of the market, by banishing effective monopoly, would serve to produce effectual and beneficial rivalry.

To accomplish such objects would demand a patient perseverance in a system that would perhaps scarcely quadrate with some commercial views : it is hardly to be expected that those, who hope to realize a considerable fortune in a few years, would be likely to enter heartily into such a plan.

Principals on the spot could alone succeed ; since commercial speculations which have embraced agricultural objects, have been ascertained to be any thing but advantageous to proprietors, whose necessary absence could admit the investigation of no detail, nor any certainty in their calculation of returns.

The real cause of the deplorably depressed state of commerce is to be found in the absence of many objects, or its restriction to those articles which, compared with the productive powers of the soil, are but the small superfluities of a poor country, that are extracted at an expense totally unworthy of their value.

India may be said to be one continuous mine of wealth, which can be brought to market by that amount of skill and capital, that will grow out of improved habits and new institutions.

That the people should ever attempt any considerable improvement, whilst all the skill and machinations of Europeans are arrayed against them, so as to induce the most wealthy to plead improbable poverty, is quite impossible.

If the country is ever to advance in real improvement, the Europeans must cordially join, and identify interests.

New institutions, new relations must be commenced to effect such objects.

The erection of a single institution, whose pretensions at first should be very humble, would in time serve as a basis for a noble superstructure that, in no very great distance of time, would be able to confound those invidious distinctions, that the European endeavours seek to render as lasting, as they may promise to be advantageous.

The disadvantages under which the native capitalist labours, must be apparent to every person of observation.

The opulent and well informed natives require nothing more, as a signal to make a forward movement, than a tender of that encouragement from the State, without which they must remain content and satisfied with their present humble position.

To give all the effective support they would require, would in the sequel bring into collision opposite interests ; and to hold the balance even would demand the active employment of a powerful arm, in order to counteract the machinations of a confederacy that would infallibly become actively engaged to procure, by every means and suggestions, a contravention of any plan for improvement, that must needs affect so many opposite feelings and wishes.

An approximated estimate of the whole of the lands under tillage in indigo cultivation alone, at an average of 15 begahs for every maund of manufactured produce, would for the most abundant seasons assign an extent of this single cultivation equal to a district of one thousand square miles*.

The most productive years have more than once yielded the large quantity of 143,000 maunds, while the least productive have yielded on some occasions not more than 65,000 maunds.

The natural spirit of trade justifies the supposition, that no very considerable fluctuation would occur as to the extent of speculation ; and therefore it may be fairly concluded that nearly the same quantity of land will be annually employed.

* 1620 begahs to the square mile or ½ of an acre each.

Such disastrous vicissitudes need only to be known to occur, to occasion those melancholy reflections they are well calculated to produce in a reflecting mind.

The 20 lakhs of begahs employed in indigo cultivation, probably yield to the state forty lakhs of revenue yearly ; while the official value of the gross returns perhaps fluctuate between 180 and 280 lakhs of rupees. Were cotton, coffee, and sugar to be produced on land of equal fertility, and to the same extent, it is conceived, that the gross returns would be more than double those of Indigo, which last suffers a fluctuation of value in this respect for each begah of from 9 to 14 rupees.

The cultivation of land for the production of coffee and cotton must be very negligently attended to, that would not yield 20 rupees for each begah, or 36 rupees under good management.

The amount of these last in weight would exceed the most favourable season of indigo cultivation at least fifty fold, and require, instead of the limited tonnage of four or five thousand tons of shipping, at least twenty times as many ships as indigo employs, on a supposition even that more than one-half of these more commercially valuable staples were consumed in the country.

The notorious fact that a petty factory commonly requires several patches of land for the requisite supply of the plant, is a sufficient reason to conclude, that larger establishments, excepting under some very particular circumstances of locality, must necessarily become spread over a considerable portion of an extensive district to the perpetual annoyance of many of the Ryots, and the production of those frequent litigations, the least inconvenience of which is that of rendering the native population discontented.

To these objections ring-fence estates, for the purposes of cultivating coffee and cotton, would not be liable.

Boundary pillars would properly mark their extent, and preclude unfriendly collision between the two classes.

The prosecution of steady agricultural pursuits of a better character, would produce better habits, while a liberal policy should direct proprietors to hold out a higher remuneration than what stipends would comprehend.

The versatile employments of the stock farm, and of agricultural pursuits, would bring the Europeans and natives into a state of closer intimacy, which improved habits and mutual good offices would not be tardy in strengthening; while an equitable system of general administration of such an establishment would procure the most industrious exertions, and tend to elevate alike the character of the Europeans and of their native fellow subjects.

H. D. E.

III.—*Inquiry into the Causes of Light and Colour.*

Whilst occupied in the examination of colouring matter, I could not avoid casting a glance at the source from whence this body derives its peculiar characteristic.

As my knowledge of Newton's researches on this subject was very superficial, and I had no books to which I could refer for information, so I was unwilling to introduce into the Memoir on Colouring Matter a few crude and unsatisfactory remarks. I have therefore thrown together in this paper such observations respecting Newton's theory of light and colour (as far as it is known to me) as were suggested by my inquiries into the properties of colouring matter.

Newton dissected light, and showed that it possessed so many rays, of so many colours; and that these colours depended upon the size of their particles: he said that white light consisted of all the coloured rays united together, and affirmed that if light were white only, there could be no colour.

My inquiries into colouring matter have led me to a modification of this theory. Causes and effects in nature are always uniform, and it is natural to conclude that the same cause which produces colour in light, produces it throughout nature ; and that it is every where subservient to the same laws. In point of fact colour is a modification of light only, and the person or substances are agents causing that modification.

Newton's theory is inadequate to the explanation of several facts. He concluded that the union of colours caused whiteness,—a very egregious fallacy; and blackness, he thought, depended upon particles very small, being a supposition which I believe to be opposed to fact.

Doctor Bancroft and other chemists, sometimes speak of the base of oxygen, of course implying that oxygen is a compound body. The subject requires much greater consideration than it is in my power to bestow upon it; I will, however, with a view of explaining the theory of light and colours, upon the assumption of the existence in the atmosphere of another principle besides that which combines with bodies during combustion, venture to say a few words upon this topic. Chemical phenomena often seem to be at variance with any general rules; and although there can be little doubt that a general rule obtains, yet the change in the relation towards each other of subtle substances, the existence perhaps of fluids of which we can obtain no satisfactory evidence, and their modifying influence upon which we cannot calculate, render argument imperfect and inconclusive, because it must necessarily be founded upon a very partial and clouded view of facts.

Stahl explained combustion upon the assumption, that the burned body lost phlogiston. Lavoisier overturned the hypothesis by demonstrating that by undergoing combustion bodies did not lose, but gain; and he further showed, that their gain proceeded from an accession of oxygen. I think Lavoisier took away from our knowledge as much as he added to it, and that, instead of using his discovery to subvert and destroy the old doctrine, he should have added the one to the other, and blended them both together. I cannot prosecute the investigation of this subject further than may be necessary to the exposition of the theory of light and colour; so that my remarks may only have the effect of rendering darkness visible; they may perhaps discover the nakedness of the land, but be little calculated to dress and improve it.

Phlogiston is the life of chemistry; without it, the science would be destitute of all truth and likelihood. Chemical action never takes place without effecting a change in the relation of substances towards each other; and the avidity of all bodies for phlogiston and oxygen occasions them to be the chief instruments in producing chemical action by means perhaps, generally, of antagonist powers. The effect caused by alkalis and acids on colours could not escape observation, and consequently certain colours were made the test of the presence and absence of these agents; perhaps in their action upon oxygen and phlogiston, acids may take away oxygen and communicate phlogiston, and thus promote solution; whilst alkalis may abstract phlogiston and impart oxygen, and thus favour insolubility. But though this may be true in the instance cited, and in a great many more, yet certainly it is not universally. So far from it that we frequently find a number of highly oxydated bodies extremely soluble. Is it not possible that this may be owing to another principle? Where a combustible substance for instance is converted into charcoal, the phenomena observable (setting aside the consideration of the products) are a change of colour, through all the grades of white, yellow, and brown, to black; and a correspondent change in its solubility till it ends in being one of the most insoluble things in nature. Now these effects are perhaps the result of the loss of phlogiston and the union of oxygen with carbon; more oxygen being the same as more colour and greater insolubility, and the converse. But should this charcoal be burnt with free access of air, what happens? Why it is dissolved into a gas, and has no colour at all. Oxygen we have hitherto found to cause colour and insolubility, and to expel phlogiston: does it all at once change its nature, unite with phlogiston, destroy colour, and become eminently solvent? Should oxygen not be a compound body, there might yet be another principle in the atmosphere, union with which may be the cause of its assuming the gaseous form, and producing effects seemingly so opposite; this principle may dissolve oxygen, and phlogiston may dissolve carbon, and these constituents may be in antagonist states to each other, their mutual necessities, like the wants of mankind, being the cause of their congregation. This idea of the component bodies of some fluids, and perhaps also solids, being in antagonist relation to each other, was forced upon me by what I observed with regard to colours. Indigo is insoluble; certain substances capable of depriving it of oxygen destroy its colour and dissolve it. These substances and the indigo, although dissolved together in the same fluid, cannot possibly be in chemical union, being in a state of counteraction. Perhaps one substance barters its oxygen for the other's phlogiston. Sulphur, on this supposition, may be a base with oxygen, and its conversion into an acid analogous to what has been suggested respecting carbon. Metals may be bases with phlogiston and bases with oxygen; but being for the most part incapable of uniting with phlogiston *per se* in an antagonist relation to oxygen and the unnamed principle, and forming acids, they enter into a further union with oxygen and become oxydes*. But though incapable of this combination *per se*, yet I am disposed to think that, aided by the affinity of other bases, such as nitrogen, sulphur, chlorine, &c. they may be

* There is some obscurity here which it is impossible to clear up by reference to the author. The MS. affords us no assistance.—Ed.

deprived of oxygen and combined with phlogiston, and in this manner would I account for their solution in acids. Acids are said to dissolve metals by oxydating them; this process would, I suspect, obstruct and prevent their solution. I have attempted to show that their operation on colouring matter is directly the reverse, and I very strongly suspect that the same is the case with regard to metals. Acids are in common use in the arts for cleansing metals and rendering them perfectly pure; tartar, alum, and common salt, mixed in certain proportions, have even the property of whitening brass and copper; gold and platina are dissolved by chlorine, and when dissolved by nitro-muriatic acid the same compounds are the results as when chlorine alone is the solvent.

All metals are held more permanently in solution by chlorine and muriatic acid than by other acids. Present to acids metals combined with much oxygen, the acids cannot dissolve them; perhaps the oxyde takes oxygen from the acid, and acids cannot dissolve oxydes. I consider all these circumstances as tending to show that the action of acids on metals is disoxygenating. I conceive that carbonic acid gas destroys life and combustion by taking oxygen from the animal and burning body. Should this be so, that acids disoxygenate metals in order to dissolve them, then the unnamed principle would attract the oxygen and phlogiston, and the base with which it should be united attract the metallic base. Phlogiston may be given out by either the unnamed principle, or the metal, or both; and thus, when saturation has taken place, the oxygen, for want of the necessary quantity of phlogiston to form a metal by combining with the metallic base, precipitates with it in the form of an oxyde. If, on the contrary, metals or combustible substances precipitate the metallic base, they supply the necessary portion of phlogiston, and the metal is regenerated in its metallic state.

Should what I have advanced on chemical phenomena render generally probable the existence of this principle, then combustion may be the combination of the combustible with oxygen, and the union of the unnamed principle with phlogiston; the latter, under certain circumstances, constituting light. There is no greater difficulty to be overcome for maintaining this supposition than for explaining chemical facts in general. To constitute any body a number of conditions are always absolutely necessary; so many atoms of such an alkali and such an acid may be the constituents of such a salt, and yet it is evident that they may be dissolved together in the same menstruum without the salt being formed. In like manner so much phlogiston and so much of the unnamed principle may be necessary to produce light, and yet we can suppose them to be present under circumstances where light may not be the consequence. If light be the effect of phlogiston and the principle supposed, chemically combined in certain proportions, the proportions and the combination are indispensable requisites. Is it not conceivable that the proportions and combination may be so exact and perfect and so independent of external circumstance, that no sensible heat should be given out? And on this supposition may not the comparative permanency and low temperature of phosphorescent objects be accounted for? Or may not the proportions and combination be in such a state, or so counteracted by other bodies, as to give out great heat without producing light? or under another set of circumstances give out both heat and light? Besides which, the eye must be an exceedingly uncertain test of the quantity and presence of light under a vast variety of circumstances. Light then may possibly be phlogiston and the supposed principle in a state of chemical combination: this principle would probably occupy the upper regions of the atmosphere, thus presenting itself to the phlogiston in the purest and most proper state for chemical union. And phlogiston transmitted by reflection would be likely to be diminished in quantity, and therefore to produce a paler, or in other words less light, and which would be attended therefore with but little redundant heat or uncombined phlogiston. Then what is colour? perhaps it might be considered as the decomposition and consequent diminution of light. I have observed that oxygen and phlogiston appear to be* antagonist principles, and that commonly as one is combined with any body the other is set free. I conceive that colour may be occasioned by white light exchanging a portion of phlogiston for a portion of oxygen, by which the density of the particles is increased, which determines the nature and shade of the colour, that the intensity of light is diminished, and when saturation has taken place, is extinguished, and black is the consequence.

Should these opinions be well founded, then colour is a decomposition of the coloured substance; the light by greater affinity appropriating the oxygen, and the coloured substance taking the phlogiston. In this way may be explained the fading

* Hence perhaps polarity, electricity, *&c.*

of colours; and the use of mordants in occasioning durability may partly be by affording a mechanical cement and partly by chemical compensation, i. e. by supplying oxygen in proportion as it is abstracted. The difference which sometimes exists between reflected and transmitted light, would depend on the portion of oxygen appropriated by the light: in either case colour or no colour would depend on the relative affinity of the substance and light for oxygen; and durability and fugitiveness of colour would turn on the nature of the coloured substance, whether it cooperated with light in effecting decomposition or repaired the loss by restoring oxygen.

Newton concludes that the size of the particles which regulated the nature of the reflected colour, depended on the density of the reflecting substances; he therefore thought, from their effect on colouring matter, that alkalis were incrassating, and acids attenuating; and Mr. Delaval, with the view of illustrating and establishing the theory of Newton, made an immense number of experiments, for which he was honoured by the Royal Society with the Copley gold medal. Doctor Bancroft and M. Berthollet controverted the hypothesis of Mr. Delaval; but although I do not believe that colour depends on the density of the reflecting substances, yet it is remarkable that Newton's opinion regarding the properties of alkalis and acids on colouring matter is in perfect harmony with those which I ascribed to them, and which I attribute to a general law of action in oxygen by which, when it enters into such a state of combination as qualifies it for uniting with light and forming colour, its effect whether on light or any other substance is incrassating.

In investigating the properties of colouring matter I found that, in proportion as it combined with oxygen, its colour was deepened and its solubility diminished, which would seem to accord with Newton's doctrine concerning the size of the particles; since it is to be expected that the size of the particles would correspond to the rarity or density of the substance. To take indigo for an instance: fully oxydised it is dark blue, heavy, insoluble; less oxydised lighter blue, specific gravity diminished, and still insoluble; further reduce its oxygen, it is green and soluble, and so on till the colour is destroyed.

My hypothesis merely transfers these phenomena to light: it assumes white light to be a chemical compound of phlogiston and the supposed principle; and that by passing through a person or particular media, or by reflection, or transmission by particular bodies, it parts with phlogiston and unites with oxygen in certain proportions; that these proportions determine the size of the particles, as shown in the instance of indigo, and the size of the particles determines the colour.

Agreeably to this theory the mixture of all colours would not destroy colour, but occasion exactly the effect which experience proves that it does. Black would not depend on small particles but on the contrary, taking charcoal as evidence; this is the most insoluble substance in nature, it perhaps of all colouring matter contains the largest portion of oxygen, and therefore possesses the largest particles. I conceive the difficulty and inconveniences connected with the black dye or the art of dyeing black proceed from these causes.

I do not think that bodies possessing no oxygen (even supposing their existence capable of proof), and yet affording colour (take chlorine as an example which derives its name from its green colour), would be any valid objection to the hypothesis. Suppose the whole weight of facts to be in favour of oxygen being the colouring principle, and suppose the prism to afford a proof of oxygen passing through transparent bodies, it would be easy to conceive that bodies possessing no oxygen themselves may yet occasion the chemical union of light and oxygen.

The property of the prism at its two extremities, even beyond the visible rays, of oxygenating and disoxygenating, seems to me to confirm the view I have taken of the power of different media to occasion the union of light with oxygen; and I think the connection of light and heat, their distinct nature, and all the phenomena presented by them are perfectly reconcilable with the assumed theory. It is not, however, by any means intended to contend for the soundness of all the opinions incidentally introduced. Chemistry is scarcely out of her leading strings: we see through a glass, darkly; and it is only by accurate observation, precise experiments, and new discoveries that the sciences can hope to acquire strength and solidity.

E. M.

IV.—*Remarks on Adie's Sympiesometer.*

This instrument is now in frequent employment; and although it can never be rendered equal in accuracy to the barometer, still in some cases, and particularly at sea, it has become a very useful substitute for the latter, and therefore any hint towards perfecting its construction should be acceptable to the inventor as well as to the public.

The Sympiesometer tells the changes of the atmospheric pressure, by its action upon the volume of gas contained in a simple air thermometer. There are necessarily two scales to it; the barometric scale of inches being made to slide over the thermometric scale, so as to furnish an adjustment for every change of temperature, which latter is ascertained by a very delicate mercurial thermometer enclosed in the same case.

Mr. Adie's description says, that both of these scales are graduated "by experiment," a fully sufficient cause for the irregularities which I have noticed in many of them. In one made by Dollond, the thermometric degrees alone were 25 per cent. too small; in short this is just one of those instruments, the graduation of which by the maker should not be trusted before a careful re-examination has been made; for it may be laid down as a general rule, that unless the divisions are unalterable,—as inches;—or easily limitable,—as between the fixed points of a common thermometer,—they are for the most part *imaginary*, and placed at random by the maker to save himself the trouble of delicate experiments, which in many cases it may not be in his power to perform.

In the case of the Sympiesometer, it is first necessary to verify with great nicety the thermometric degrees; the barometric scale may then be easily deduced by calculation as follows:

1. All gases expand .00208 of their volume for each degree of Fahrenheit's thermometer*:

2. The volume of gas is constantly in the inverse ratio of the barometric pressure; that is,

The bulk of gas under a pressure of	31	inches being	1,0000
The same under the pressures	30	will be	1,0333
	29		1,0688
	28		1,1072
	27		1,1485

To express these differences (.0333, .0688 &c.) in terms of thermometric degrees, it is merely necessary to divide them by .00208; thus

The expansion produced by a diminution of pressure from,

$$31 \text{ inches to } 30 = \frac{,0333}{,00208} = 16°,05$$

$$31 \quad 29 = \frac{,0688}{,00208} = 33,07$$

$$31 \quad 28 = \frac{,1072}{,00208} = 51,54$$

$$31 \quad 27 = \frac{,1485}{,00208} = 71,38$$

rise on the thermometric scale,

Two instruments tried by myself had these divisions in the following erroneous proportions:

16,6	33,4	51,1	and	69,3
15,4	31,3	48,		65,0

There can be no doubt, that the theoretic divisions are alone correct, and that there can be no occasion to verify them experimentally.

It is, however, a material defect in the Sympiesometer, that the two scales made on the foregoing principles can only be mathematically correct, for *one constant pressure*, and *one determinate temperature*: for as the expansion from either cause is directly in proportion to the total bulk of the gas; if this bulk is increased by a diminution of pressure in the atmosphere, then the thermometric degrees should be increased at the same rate; or if the bulk be expanded by an increase of heat, then the inch scale should be expanded in the like proportion.

Assuming, therefore, that the thermometer is graduated for the standard pressure of 30 inches, the degrees under other pressures will bear the following ratios.

* i. e. 00208 of *their volume at* 32°.—Ed.

At 31	inches, 100° will require a length of	96°,7
30		100,
29		103,5
28		107,1
27		111,1
26		115,4

In the same manner, if the barometric scale be supposed to be adapted to the standard temperature 60°, then above and below that degree, the length of the *whole scale* (as it is not a scale of *equal division,* it is better to use this expression) must vary in the following proportions.

At 30°— the length will be	,9376
40	,9584
50	,9792
60	1,0000
70	1,0208
80	1,0416
90	1,0624
100	1,0832
110	1,1040
120	1,1248

After this explanation, it is easy to discover the cause of discrepancy in the two instruments mentioned before :—the first appears to have been experimentally divided at a temperature of 50°, and the other at a temperature below the freezing point, or in the depth of winter !

The most convenient method of obviating these sources of error, is to describe the lines which mark the divisions of inches and degrees, as parts of rays conveying to a distant point, instead of drawing them parallel to one another in the ordinary manner : perpendicular lines may then be drawn parallel to the line of *standard* divisions, at such distances on either side as to intersect new scales of divisions adapted to given variations of the bulk of air from pressure or temperature as above described.

Such scales might be printed and attached to common sliding rules, and it would be only necessary to have on the Sympiesometer a single scale of temperature, and indeed any air thermometer might then at once be converted to the purpose of a barometer on the same principle.

Q.

V.—*On the Firs of the Cásiya Range, and the possibility of transporting them into the Brahmaputra.*

We have much pleasure in publishing the following account of the firs in the Cásiya Range, drawn up by Captain Jones of the Quarter Master General's Department.

I shall commence by stating that there are only two places, where in my opinion the fir timbers could possibly be floated. These are the Sóripáni four miles N. W. of Nancláo, and the Bar-pani, 37 miles east of the former, on the Jáintia road.

The Sóripáni is just 2000 feet below Nancláo, and 2400 feet above the level of the Brahmapútra, as measured with a barometer, and the same result nearly was obtained by using a theodolite : the point measured from, was the bridge of fir timbers and planks 60 yards long. The trees here are larger than those above or below this altitude. Many may be procured of 2 feet in diameter (bark included) ; the general breadth is between 14 and 18 inches in diameter, in the rough state, tapering very slightly to the top : the lengths vary a good deal, from 30 to 90 feet in timbers of the same thickness. The species appears to be that resembling the Scotch fir, having a long grain and containing a considerable quantity of turpentine, the knots being 2 or 3 or more feet distant from one another: the colour of some of the planks when dry is yellow, and in these I think there are fewer knots, and further apart, whilst in other timbers the wood is nearly white and much softer.

A little lower down the Sóripáni, after several considerable falls, the fir trees are very numerous, and any number from 10 to 13 inches may be found, the wood of the same description. These firs do not thrive well after leaving the immense stratum of sparkling granite which extends to 40 miles, (known) east of Nancláo, and 8 and 10 miles north and south of it. Beyond this to the eastward, in this latitude (25° 40″ 00′,) the country has not been explored, being inhabited by the tribes of Nâgos, who are not inclined to let travellers into their country; but N. and S. of Nancláo where the granite rock terminates, the fir trees get dwarfish and are soon lost altogether. With regard to the transport of the timbers I cannot speak so positively, yet I should imagine the smaller ones below the Sóripáni Bridge might be got down in the floods, and it is not impossible but what the large ones might also. The river has several falls from the bridge for 8 or 10 miles. The whole distance in a straight line to where the Sóripáni river falls into the Brahmapútra being about 36 miles; the general course of the river being nearly N. W. The lower extremity near the Brahmapútra is known to be navigable for 10 miles or more, which shortens the distance a good deal. The current in the floods is very rapid, and the general breadth in the hills is 60 yards, the depth of water varying to extremes, like all hill torrents; but in the floods the depth is 12 to 14 feet; and its velocity may be conceived when it is stated, that the fall of water is 2400 feet in 26 miles, where it enters the plains, and 10 miles further falls into the Brahmapútra.

It would be perhaps worth while to have 2 or 3 specimens brought down, and if on examination, they should be found of the proper description for the purpose required, the attempt to bring them down into the Brahmapútra might then be made; and I have no doubt, it would partially succeed if made at the proper season. The best months would be July, August, and September.

I shall now refer back to the river to the East of Naucláo, the Barpáni. The fir timbers here are, I think, larger than on the Sóripáni, by 5 or 6 inches on an average. The wood is the same in appearance in all respects. I shall therefore proceed to the nature of the river; and the probability of transporting the timbers. The river in the hills is from 80 to 90 yards broad, sounding through a gulley formed by two high ranges clothed with the firs from about the middle of the descent to the water's edge: this river has always two feet of water, and has a very navigable appearance. The natives say, there are several falls lower down the stream, and also assert that the river becomes subterraneous for upwards of a mile, and then emerges again. The distance in a straight line into the plains is about 50 miles in a direction N. E. where it enters the Copelli river, and a few miles further falls into the Callang opposite Círhôr Chóki: the river is navigable a short distance before it falls into the Capelli and downwards into the Callang, and from thence into the Brahmapútra, 12 miles above Gáohatti at all seasons of the year. Notwithstanding the tale of the inhabitants about the river running through a hill of granite rock, which I take to be an idle one, I think there would be a better chance of succeeding with the timbers in this river, than the Sóri-páni near Nancláo, but nothing for certain can be known until the experiment is tried.

It may not be unnecessary to state, that there would be little chance of getting assistance from the Jáintia people near the Barpáni, or the Nancláo inhabitants near the Sóri-páni: the wood-cutters must be sent from the Assam side, at Nancláo. Mr. Scott, the Governor General's agent, has generally found it necessary to purchase the trees before cutting them to prevent dispute, but the sum was quite trifling that they asked.

I shall conclude by mentioning, that several fir timbers are washed down the Callang in the rains.

We have also been favoured with the following extract of a letter from a gentleman in the Cásiya Hills on the above subject.

I send you a specimen of Cásiya fir. The specific gravity will probably be found to vary considerably in different specimens, according to the quantity of resin contained in the wood, which is sometimes so great as to render it very heavy. If care were taken of the trees, by putting a stop to the annual conflagration of the grass that grows under them, they would evidently attain a much greater size than those seen by Captain Jones, as they are to be found in the sacred groves, which are carefully preserved, full 90 if not 100 feet in height. I cannot yet speak positively as to the question whether it is possible to get them floated down any of the rivers. The difficulty is occasioned by the beds being blocked up by immense granite boulders, under which the water in some places flows, and all the best trees are produced where the boulders most abound. I fear, however, that if it were found practicable to get them into the Brahmapútra, the expense of transport to Calcutta would prove too heavy; for as it would be necessary to go a part of the way against the stream, they must either be loaded on boats at Jaffer-ganj, or sent by sea from Narráin ganj ; and on this account, I should doubt whether the Bareilly firs might not be brought to market cheaper, although grown further off. They were, I believe, tried, and not found to answer.

Remarks by the Editor.

By the Bareilly firs, we conclude the writer of the foregoing means the firs growing on the low sandstone range opposite Bareilly, and distant about 70 miles. From the character and relations of the Cásiya Hills, we conclude them to be a continuation of the former, and the two firs to be one and the same. That of the north-western mountain provinces is known to botanists as the P. *longifolia:* it bears three very long (sometimes 18 inches) leaves in a sheath. The fruit is a perfect cone, and the seeds are winged. With regard to the quality of the wood, it is generally considered good: noother is used at Almorah for the purposes of roofing, chowkuts, pannel doors, &c. The question, however, of transport is probably, as in the Cásiya Hills, a doubtful one. We shall be happy if any of our readers in that quarter will favour us with some information on the subject. As to the statement of its having been tried in Calcutta, and not found to answer, we rather think this refers to another pine, the *Deodar*—which is so different a wood, that though highly valuable also, it would never be used for the same purposes, and might consequently be pronounced, if tried with that view, not worth the trouble of transport. This latter too must have been in the case of the Deodar many times greater than in that of the fir.

The specific gravity of one of the specimens sent with the above letter was 595. Captain Baker had the kindness to try the strength, and found that a piece with a breadth of ,8 inch and depth ,9 inch bearing 15 inches, required 271 lbs. ; a second 255 ; a third 247 ; mean 258. But he observes, that in such small specimens no fair conclusion can be drawn.

The above, such as it is, is a *higher* result than Mr. Barlow obtained with firs at home in the proportion of nearly one-fourth.

VI. *Comparative Value of different Methods of Raising Water.*

The annexed Table exhibits a statement of the comparative performance in three different methods of raising water, and the expense of each method, or the number of hogsheads raised in different periods of half a day, or in six hours, to a whole day of twelve hours.

The labour of baling being a very hard work, the men are supposed to work no more than six hours at the rate of 20 deliveries in each minute of time ; and when it is considered, that the actual delivery of a load is $\frac{1}{3}$ of a cubic foot, or 21 pounds of water, and that the basket (wet) and rope must weigh at least 11 or12 lbs. more, and are at the extremity of a lever at least 3 feet long, the exertion of each man is considerably more than 16 lbs. it is perhaps equal to 22 or 24 lbs.; and that this exertion could not be kept up for a whole day, is a circumstance known to every one acquainted in the least with ordinary labour.

In estimating the performance of bullocks in drawing water, the greatest advantage is derived from the greater height to which the load is raised, since the difference between drawing water 11 or 12 feet, and 40 to 45 feet, has not been observed to give more than about $\frac{2}{5}$ths more delivery for the shorter draught; so that in such moderate height baling is $\frac{1}{3}$ dearer according to the table, and the men's labour applied on the walking beam, would seem to be $3\frac{1}{3}$ times as cheap as that of bullock labour with their necessary attendants, for small heights.

For heights of from 30 to 45 feet:

1st, baling gives from 40 to 50 hogsheads for each rupee expended ;
2d, bullock labour from 135 to 150 hogsheads ;
3d, and the walking beam about 500 hogsheads.

Or the 3d method is to the 1st as 10 : 1
the 3d to the 2d is as 10 : 3
and the 2d to the 1st as 3 : 1

or, the 2d method has as much advantage over the first for heights exceeding 30 feet, as the 3d method has over the 2d under such circumstances.

A Table showing the performance and its cost, in raising water from 10 to 45 feet by different methods of employing animal power. The expense of each labourer is put at two annas per diem, except in the instance of the walking beam, put half as much more, on account of wear and tear of machine.

No.	Height raised in feet.	No. of persons equivalent.	Method.	Stages.	Loads per hour.	Cubic feet per load.	Cubic feet per hour.	Hours' labour.	Total performance in the period. Cubic feet.	Total performance in the period. Hogsheads.	Hogsheads each man.	Number of hogsheads raised for one rupee.	Period of labour.
1	10	5	Baling ...	2	1200	$\frac{1}{5}$	400	6	2400	300	60	480	whole day.
2	11	3	Bullocks ...	1	44	1.75	77	6	462	$57\frac{3}{4}$	$19\frac{1}{2}$	312	half day.
3	11	1	Walking beam	1	800	.5	400	8	3200	400	400	2250	whole day.
1	45	50	Baling ...	10	1200	$\frac{1}{3}$	400	6	2400	300	6	48	whole day.
2	45	5	Bullocks ...	1	32	1.75	56	12	672	84	$16\frac{4}{5}$	135	
3	45	6	Walking beam	4	800	.5	400	12	4800	600	100	533	
4	45	100	Baling ...	10	1000	$\frac{1}{3}$	332	12	3984	498	5	40	

The relative performances are for small heights for each method.

	Hhds.	Ratios.	Hhds. per Rupee.
Baling,	60	or 3	480
Bullocks,	20	,, 1	625
Walking beam,	400	,, 20	2250

For heights 30 to 45 feet.

	Ratios.	Hhds. per Rupee.
Baling,	3	40 to 48
Bullocks,	$8\frac{1}{2}$	135 to 150
Walking beam,	25	533

In the baling method the number of deliveries was observed to be 22 $(19+25 \div 2)$, but the number is taken here at 20 per minute, or 1200 per hour; 1000 ought perhaps to be a maximum, supposing even that there were a complete relief as per second statement, No. 4.

The height assigned in the "Gleanings," No. 4. of 10 ft. seems to be an error, since the first stage did not exceed $3\frac{1}{2}$ feet, which would leave $6\frac{1}{2}$ feet for the height of delivery for the 2d stage, a height that is obviously most inconvenient for the kind of instrument employed.

H. D. E.

VII.—*Proceedings of Societies.*

I.—Asiatic Society.

Class of Natural History and Physics.

Wednesday, 2d May.

Honorable Sir E. Ryan in the Chair.

Mr. Walters of Dacca presented a box of rock specimens and minerals from the Cásiya Hills. Captain Bruce a collection made in a journey along the hills of Rotásgerh and Sasserám, with a letter descriptive of their localites.

A paper on the Geology of the Peninsula from Hydrabad to Masulipatam, with an explanatory section, by Lieut. McPherson, Madras N. I., was read.

A paper by James Kyd, Esq. on the Tides of the river Húgli, the result of observations from 1805 to 1828, with Tables, was also read. Some specimens of lithographic impressions, taken from a Jysselmir stone, were presented by Mr. Smith of the Commercial Lithographic Press. The stone was forwarded to Calcutta by Lieut. Boileau of the Engineers. The particulars of the locality are not yet ascertained. It appeared well suited for all common purposes, and in some respects is superior to the Bavarian stone; viz. in the depth of the impressions and the clearness with which it gives them out.

2.—Medical and Physical Society.

Wednesday, 4th April.

Messrs. W. Venour, W. B. Webster, G. Anderson, and H. Crocket, proposed at a former meeting, were duly elected.

Letters were read from the Madras and Bombay Medical Boards, intimating the acceptance of these bodies collectively, of the office of Patrons of the Society, and of the Chairmen individually of that of Vice-President, as resolved at a former Meeting.

From the following Members communications were read, requesting that their names be withdrawn from the list of subscribers to the Society, on the ground specified by the others whose withdrawal was intimated at the preceding Meeting,—namely, the recent reduction of their allowances, and their inability to meet contingent expenses; viz Mr. A. Wood, Surgeon of Artillery, and Messrs. G. G. Browne, J. T. Pearson, E. J. Agnew, H. Fulton, A. V. Dunlop, and G. Turnbull, Assistant Surgeons. With reference to these intimations it was resolved, that, should the gentlemen who have withdrawn their names, hereafter, from any cause, think proper again to join the Society, they shall be considered eligible, without going through the form of ballot, or paying any additional admission fee.

The 1st volume of the Transactions of the Agricultural Society was presented by that body, and a copy of the *Moonzal Quanoon*, a medical work in Arabic, by the General Committee of Public Instruction, for the Library of the Society.

The following papers were then read and discussed by the Meeting: Some Observations on particular Remedies for the Cure of Cholera, by Mr. R. M. Martin: An Account of the Progress of the experimental Medicinal Garden at Massuria, by Mr. Royle, communicated by the Medical Board.

Wednesday, 2nd May.

A letter was read from Mr. A. Gibson, of the Bombay establishment, requesting his name might be withdrawn from the Society on the same plea as was expressed by the other Members who have seceded, viz. the great reduction in his income by the recent Government orders, which are in operation at that presidency in the same manner as in Bengal. A letter was read from Mr. Bernard, proposing that the Society should address Government on the subject of the recent orders reducing the allowances of medical officers. It was resolved, that the objects of the Society being purely scientific, and many members of it not belonging either to H. M.'s or the Hon'ble Company's Service, they could not with propriety adopt Mr. B.'s proposal.

The following communications received since the last Meeting were submitted by the Secretary: An Essay on Phlebitis, or Inflammation of the Veins, by Mr. Greig: A Case of, and Observations on, Infantile Cataract, by Mr. Raleigh. Specimens of Epsom Salts, prepared at Balrámgerhi near Balésar by Mr. Ince, were presented by the Rev. Mr. Garrow. Dr. Conwell, of the Madras Establishment, acting staff surgeon Prince of Wales' Island, presented for the Library a copy of a work by himself on Indian Pulmonic Disease, and the Use of the Stethoscope, together with the instrument so named. Babu Rám Comel's Observations on the Native Employment of Madár, and Mr. Raleigh's case of Cataract in an infant, were then read and discussed by the Meeting.

3.—Agricultural Society.

Thursday, 21st May.

Honorable Sir E. Ryan in the Chair.

The following gentlemen were severally proposed as members of the Society, and duly elected; A. Dobbs, Esq., P. O' Hanlan, Esq., John Master, Esq., Lieut. Col. Frederick, B.E., S. L. Master, Esq., C. Heard, Esq., J. Allan, Esq., F. T. Fergusson, Esq., Rajah Narsinchander Rái, Babu Lali Mohen Tagor, Babu Bissunáth Móti Lal.

After the business of the election the members adjourned to the upper hall, where they held their Anniversary Dinner.

The president informed the meeting, that the Governor General and Lady William Bentinck had done them the honour to become Patron and Patroness of the Society.

The Society have established Committees of Agriculture, Horticulture, Publication, and Translation.

Wednesday, 10th June.

Honorable Sir E. Ryan in the Chair.

The following gentlemen were elected members: His Highness the Nawaub Saulat Jang Bahadur, Mr. F. Mendes, Lieutenant Colonel Conway, C. B., Madras Army, Captain Watkins, ditto. Captain Sage, Captain C. M. Wade, Captain White, Lieutenant J. F. Boileau, Mr. Pearson, Mr. R. O'Dowda, Mr. W. Smithson, Mr. J. Carey, Mr. Walters of Dacca, Babu Harrachander Lahóri.

A letter was read from Mr. Prinsep, Secretary to Government, intimating that the Púsa garden had been transferred to the Púsa Stud Establishment.

The Secretary submitted a communication from Captain Davidson, of Engineers, which accompanied a quantity of seed of the plantain tree, preserved by him at Bareilly; and informed the meeting that he had handed these to the Society's gardener, for the purpose of being sown and propagated so as to ascertain their qualities.

A letter was read from Mr. Cracroft, of Dacca, communicating his success in cultivating a very fine species of fig-tree, brought by him from the Cape of Good Hope in 1827, and offering to supply the Society's garden with grafts. In his communication, Mr. Cracroft detailed a method of destroying the insect which proves so destructive to the fig-tree.

A letter was read from Mr. D. C. Smith, of Húgli, kindly offering to procure, for the Society's garden, grafts from the best mango trees in the district.

The Secretary submitted letters to his address from His Highness the Nawáb Saulat Jang Bahadur, which accompanied figs raised by him in his garden at Chitpore, of a large size, and of the finest flavour, being 6½ and 5½ sicca weight, and 7½ and 6½ inches in circumference. The Secretary expressed his regret, that such fine specimens could not have been kept to lay before the meeting; he had, however, practically satisfied himself of their great superiority over any figs he had ever seen in Calcutta.

The Secretary stated to the meeting, that he had lately received from Mr. Piddington two quart bottles of Virginian tobacco seed, and one of Persian Tobacco, for distribution among the members of the Society.

Mr. Calder submitted a letter to his address from Capt. Stacy, 32d N. I. recommending to the Society to obtain their supplies of up-country seeds, rather from the higher stations of Secróra, Míret, &c., than from Cánpúr or Patna, and sending him for presentation to the Society, a quantity of seeds from Simla, consisting of a hill cedar cone, acorns, raspberry, juniper, sycamore, &c. &c.

Mr. Calder also submitted a paper on the Manufacture of Indigo, by Mr. Piddington, and a paper on the Cultivation of the Sugar Cane, and the Manufacture of Sugar, by M. Cheron of Corabári, in which the improved method of the West Indies is blended with the practice of this country.

A paper by Mr. Halhed, on the Cultivation of the Sugar Cane, was also presented.

The Secretary submitted a paper by himself, on the Propagation of the Mango and Peach, and other stoned Fruits in this Country, grounded on the experiments of Mr. Knight, President of the London Horticultural Society, during a period of 40 years, which demonstrated that every fruit tree should be grafted on stocks, not only of its own genus and species, but such only as were raised from seeds of the most cultivated kinds.

The Secretary also submitted the late Dr. Ingledew's account of the method used by the native mális of Mysore, for rearing such of the turnip, carrot, or radish plants, as were intended to produce seed for the following season, so as to prevent the deterioration of the vegetable, which occurs in all hot countries.

Mr. Calder presented a Report by the Agricultural Society of New South Wales, on the state of agriculture there.

Mr. Bruce presented a copy of the Charter of the London Horticultural Society—a copy of the Bye-Laws for the garden, and a list of the Society's members.

The Secretary presented a copy of a circular sent by the President of the United States of America to their foreign Consul, in September 1827, directing them to assist in procuring, so as to introduce into the States of the Union, all foreign trees and plants which promised, under proper culture, to flourish and be useful, together with such notices of their cultivation and natural history, as may be obtainable in the country where they are indigenous. To the circular are appended highly useful directions for putting up and transmitting all kinds of plants and seeds.

Mr. Mitchel, the gardener, submitted a list of the plants which were now in the Society's garden, and could be spared to such members as applied for them.

The Secretary informed the Meeting, that a copy of the Society's Transactions had been sent to each member, as well as to all foreign learned Societies.

It was resolved, upon a recommendation of the General Committee, that a certain portion of the investments of European and foreign seeds should be distributed to all the members of the Society gratuitously, and that the shares of up-country members shall be delivered to any friends in Calcutta, who may apply for the same in their behalf.

It was further resolved, that a Secretary of Foreign Correspondence be appointed, having reference to the introduction from other tropical countries, especially America and the West Indies, of all useful and important agricultural and horticultural productions. Mr. Piddington being requested to undertake the office, expressed his acquiescence ; and he was declared Foreign Secretary accordingly.

VIII.—*Scientific Intelligence, Miscellaneous Notices, &c.*

1.—*The Dugong.*

The following is an extract from a letter addressed by his correspondent in Edinburgh to a gentleman in Calcutta.

" I cannot allow the present opportunity to pass without mentioning the interest which has been excited by the head of the Dugong sent by you to the Society. On examining this head it was discovered, that it differed in some points from that in the college museum. An examination of the latter then followed ; and it was found that the artist who had prepared it, had filled its jaws with the teeth of cows, horses, and even with ivory teeth. As it has been drawn in this state, and so presented in Baron Cuvier's works (from this authority), it became important to set the matter right ; and the Royal Society therefore have resolved to have a plaster cast taken from the head you sent, and to send it, with a copy of Dr. Knox's notice regarding it, to M. LeBaron Cuvier for his information.

" I was absent in France when your last donation arrived, and I fear some mistake has taken place regarding it, as I see by some memoranda that you appear to have sent home the skeleton of the same animal of which we possess the head ; but I cannot ascertain, that any skeleton has been delivered at the Royal Society, while I see by Dr. Knox's paper, that *shortly after* the head was put into his hands by our curator, he received from professor Jameson the bones of the same animal, entirely disarticulated by maceration, and sent from India in a separate and dry state. I have not yet been able to ascertain where this mistake lies, as Dr. Knox is not in a situation to be applied to on the subject. I subjoin an extract from the paper (which has not yet been read at the Society), and which will show you the importance which is attached to the subject.

" ' As a complete skeleton of any animal is by far the most valuable part of its anatomy, I felt anxious to compare the bones of the Dugong placed in my hands by professor Jameson with the writing and drawings to be found in the works of Sir E. Home, and Baron Cuvier. The conclusion I have come to is, that neither of these gentlemen has ever seen the complete skeleton examined previously to the disarticulation of the bones, and *that the whole subject will require to be carefully re-examined.* I freely admit, therefore, that it is almost solely with a view of inducing the gentleman, who was so attentive to the Society as to transmit the head of this animal, to lay the Society and zoologists in general under still deeper obligations by transmitting to it an entire animal.' "

2.—*Radiation of Caloric.*

In the Quarterly Journal of Science, July to October, is a short review of Addison's Remarks on the Influence of the terrestial Radiation of Caloric upon local Salubrity. The reviewers remark : " The work contains many clear statements, with some accurate reasoning, which we can with confidence recommend to our readers. The last section treats upon a subject altogether new in medical science, though the facts to which Mr. Addison refers have been long known to the cultivator of chemistry. That the radiation of caloric from the earth will have a very great influence in the production of various diseases, we are certainly much induced to admit ; and we feel induced also to believe, with our author, that the activity of MALARIA may very much depend upon this process. The remarks and observations which Mr. Addison has made upon diseases as they appear in tropical climates, certainly furnish a powerful statement in favor of the view he has taken. We earnestly recommend this subject to the profession of which Mr. Addison is a member; the conclusion he has drawn are, that *all those places where the radiation of caloric goes on with rapidity, will be found subject to great vicissitudes of temperature, to fogs, heavy dews, and other noxious precipitations from the air, whereby they are rendered cold, damp, and oftentimes extremely unhealthy; while* ceteris paribus *those situations where the terrestial radiation is diminished will be proportionally warm, drier, of a more agreeable temperature, and more healthy.*"

3.—*Brittle Pens.*

To the Editor of Gleanings in Science.

SIR,

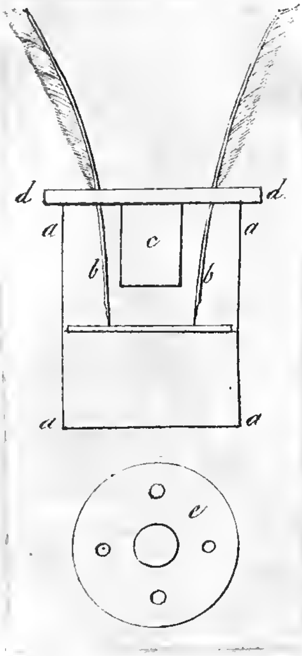
If you have ever experienced the inconvenience of having your pens opened by the hot winds; you will readily grant the utility of some plan to prevent such an annoyance. Should you consider the accompanying very rough sketch of an inkstand as deserving of notice as I have found the original useful, you will probably when convenient give it a place in your "Gleanings."

The invention is not my own, but suggested by plans adopted by two friends. In one the floating wood was wanting, and the pens became too wet; in the other the ink holder was separate, which produced occasional inconvenience: by combining both I think the idea is improved.

I remain,

Your obedient servant,

22*d May* 1829. X. Y. Z.



For want of something better I have been obliged to use a common tumbler; but the whole might be greatly improved in appearance.

a. a. a. a. The tumbler filled half full with water.

b. b. A piece of wood a little less than the circumference of the tumbler, floating in the water to support the pens.

c. The inkholder.

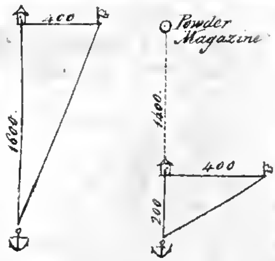
d. d. The cover with holes to admit the pens and inkholder.

e. A bird's eye view of the cover.

4.—*Royal Clarence Sextant.*

The advantage to be gained by the royal Clarence sextant will never counterbalance the expense of such an instrument, when it is in the power of every man who knows any thing of navigation always to calculate the angle which the objects must subtend at certain distances when he is approaching one of them on a line perpendicular to their distance. It is only in such cases that the royal instrument can be useful, and then not more so than the common quadrant. How easy is it for a commander, knowing that a light-house and flag-staff are N. by E. and S.

by W. from each other, distant 400 yards, (*see figure,*) to say; As the 1600 yards at which I wish to anchor from the light-house is to radius, so is 400 yards to the tangent of the angle the objects must subtend where I am to anchor (14°. 2'. 10") ; then by keeping the index of his quadrant fixed at that angle steer down W. by N. upon the light-house, until by the quadrant the two objects cover the one the other. If the distance from the light-house is to be less than the distance between the two objects, the Clarence sextant could not be used. As for instance, suppose that I am acquainted with a deposit of gunpowder being within the fort 1400 yards from the light-house, and by placing the bomb 200 yards from the light-house, a bearing of W. by N., I could throw my shells directly over the light-house to the distance of 1600 yards, in that case the objects must subtend 63°. 26'. 6", and could not be measured by the Clarence sextant (*see second figure*).



If the triangle formed by the ship and two objects becomes obtuse, by being obliged to steer down upon the light-house in a course not perpendicular to the base, the utility of the Clarence sextant also vanishes. In surveying, it is never likely to be of any use beyond the common sextant. And any table of corrections that can be made to adopt the sextant to an acute angled triangle or to an obtuse one must be voluminous and involve expense ; and I am sure that any man who knows not how to obtain what he wants in that way by using a common quadrant is not fit to have charge of a bomb.

NAVIS.

5.—*Nepal Paper.*

Extract of a letter dated Edinburgh.

" I can only add briefly, that there is reason to think that if the Nepal pulp for making paper could be sent to this country at a cheap rate, in large quantities, it would form a valuable addition to the resources for making such papers as require great power of endurance. The banks feel much interest in it ; and I should have liked to have had it in my power to have the experiment fairly tried here of making bank note paper of it ; but unluckily the quantity of *bricks* or *pulp* sent was not adapted to the size of the macerating engines at the paper works, which require a charge of 112 lbs. and the whole weight at my disposal was only 17½ lbs. I have after much difficulty succeeded in getting an intelligent paper maker to try it, and he has just concluded his experiment, the result of which he has been prevented from bringing to me by indisposition. I look for him daily, and shall give you on some future occasion an account of his observations and opinion, with specimens of what he may have produced. You will find along with this a little bit of an unfinished sheet*."

" It appears to me, as far as I can judge at present, that it is desirable that a trial of 112 lbs. weight should be made, and also that a quantity of the paper, as made from it in India of different presses, should be sent for examination and comparison. You will observe that in the state in which it is in the specimen sent, its great susceptibility of dilatation by damp is likely to make it useful as an hygrometer. I have given a piece to Mr. Adie for trial."

In a second letter it is observed : " The engraver reports that even in the rude state in which the material appears in these specimens, it affords finer impressions than any English-made paper, and nearly as good as the fine Chinese paper which is employed for what are called *Indian paper proofs*.†"

Another trial was about to be made with some fresh stuff that had recently arrived.

* We have seen this specimen. It is extremely tough, with something of the feel and appearance of oil skin.

† We have seen a specimen of a bank note printed on the Nepal paper sent home. No impression can be finer, the most delicate lines in the complicated patterns, resembling the ornamental work called engine turning with which part of the note is covered, being tranferred to the paper with the most perfect truth and distinctness. And yet this paper had not a tolerably even surface.

IX. Analyses of Books.

1.—*Asiatic Researches; or, Transactions of the Society instituted in Bengal for inquiring into the History and Antiquities, the Arts, Sciences, and Literature of Asia, Vol.* xvii. *Part* 1. [*Physical Science.*]

Considering the large field of inquiry presented to us by India in particular, and Asia generally, it must be confessed, that little has been done by us for the cultivation of natural history or physical science. With the exception of one department of the former (which we allow has had justice done it,) and the few papers of Col. Lambton on the latter, little, if any thing, has been published in India on these subjects. Nor has this been for want of an appropriate vehicle of publication. Sixteen volumes of the Transactions of the Asiatic Society have been given to the public, but this work has continued to be essentially literary in its character; the scientific communications being too few in amount and too slender in pretension (with one or two exceptions) to require much consideration.

This leaning of the Society towards literature, to the almost exclusion of science, has been the effect of various causes into which we do not feel ourselves called upon here to inquire. The founder's views, deeply imbued as he was with an enthusiastic love of eastern lore, necessarily had their influence, and this was increased by our position in the country. The views of a government situated as ours has been naturally led to the encouragement of the study of the languages of the country, which thence became the road to preferment, so that the strong stimuli of fame and profit were made to increase the tendency originally given to the inquiries of the Society by the genius of the founder.

But though the circumstances in which the Society originated, and for some time grew and prospered, were, if not adverse, yet far from favourable to the cultivation of science, the subject was not altogether lost sight of; a very early byelaw of the Society having provided for the meeting of a class or committee especially applying itself to the cultivation of science. This provision had not, it is true, produced much fruit; for, though repeatedly revived, these meetings have always after a time fallen off, from what reason it is difficult to say. The last attempt made to revive them about this time last year promises to be more successful. They have had regularly meetings monthly since February last year, and many papers of interest have been presented and read at them. From these and some papers which had been previously in the possession of the Society a selection has been made, and the result, forming the first part of the seventeenth volume, is now before the public. We shall endeavour to give our readers an idea of the contents of the volume.

The papers are sixteen in number, of which all except three are more or less connected with geology. This department of natural history has latterly engrossed a large share of the public attention in Europe. It is still in its infancy, yet teeming every day with discoveries of the greatest interest. In India the subject has been altogether untouched, and the first breaking up therefore of so rich a field promises an abundant harvest. It is not then to be wondered at, that the number of geological papers should so greatly exceed the others. For a long time this subject must continue to afford the most promising field to the Indian observer; and we may therefore expect that the transactions of this branch of the Society will continue to have a leaning towards geology.

I. *General Observations on the Geology of India. By* James Calder, *Esq. pp.* 1 *to* 22.

Mr. Calder begins his paper by paying a just tribute to the memory of the late W. H. Voysey, Esq.*, to whose unpublished notes he acknowledges himself indebted for much of the materials of his essay, and whom he justly describes as having fallen a victim to that unwearied zeal and ardour in the pursuit of science, which no toil or even danger could daunt. There is little doubt, that had the life of this indefatigable observer been spared, his labours alone would have served to raise the standard of Indian research, and to wipe out the reproach of indolence and indifference sometimes preferred against us by our enemies.

After noticing Mr. Voysey's great merits, and deploring his untimely fate, Mr. Calder proceeds to take a general but rapid survey of the great mountain tract, extending from Cashmír to Assam, the highest ridges of which are covered with per-

* We should be glad to see some of the friends of this gentleman give some account of his life and labours. We think they owe it to his memory.

petual snow, even on the border of the Torrid Zone. "On the north we have the stupendous chain of the Himmalaya, extending from the confines of China to Cashmír, and the basin of the Oxus. That vast accumulation of sublime peaks, the pinnacles of our globe, is so extensive, that a plane resting on elevations of 21000 feet, may be stretched in one direction, as far as the Hindu Coh for upwards of 1000 miles, above which rise loftier summits, increasing in height to nearly 6000 feet more. Primitive (primary) rocks alone have been found to compose all that has yet been explored of the elevated portion of that chain. Towards the base we find sandstone, composing the southern steps of the chain, and forming the north-eastern barrier of the valley of the Jumna and Ganges, by which and the diluvial plains of upper Hindustan, this great zone is separated from the mountain ranges of the peninsula. The opposite or southern boundary of this valley is of the same rock. Advancing to the south we come to three inferior mountain ranges, on which the peninsular table land of India may be said to rest, or more properly to which it owes its peculiar form and outline. We may consider these ranges separately, as the western or Malabar, the eastern or Coromandel, and the central or Vindhya."

"The principal in elevation, and most remarkable in continuity of extent, is the western range, which commences in Candesh and runs along the Malabar coast, within a short distance of the sea, in an unbroken chain to Cape Comorin; excepting where it is interrupted near its southern extremity by the chasm which opens into the valley of Coimbetúr. The direction of this range deviates but little from north and south, bending a little eastward towards its southern extremity; its elevation increases as it advances southward, the highest points being probably between latitudes 10° and 15°. N. where peaks of granite rise to 6000 feet and upwards. The northern extremity of this range is entirely covered by part of the extensive overlying trap formation, to be more particularly described hereafter, extending in this quarter from the sea shore of the northern Concan, to a considerable distance eastward, above and beyond the ghats, as far perhaps as the river Tumbúdra and Nagpúr." The usual features of basaltic countries are described as often observable. "The hills rising abruptly in perpendicular masses of a tabular form or in mural terraces piled on each other, like great flights of steps leading to some giant's throne, are frequently separated by immense ravines: the whole clothed with luxuriant forests of teak and other trees, forming some of the most beautiful and romantic scenery of India. The elevation of this part of the range seldom exceeds 3000 feet, but to the south its height gradually increases, and granitic rocks begin to reappear rising above the surface between 17° and 18° N. latitude, and from thence probably continuing to form the summits of the chain with little interruption all the way to Cape Comorin."

The curious formation of iron clay or *laterite* is represented as forming a succession of low hills towards the sea-coast, and evidently resting on primary rocks; the latter (granite) coming to the surface at Malwan, Calicut, and some other points. This laterite appears to pass over into Ceylon.

On the eastern side the arrangement is something different. "The plains of the Coromandel coast form rather a broad though unequal belt of land between the mountains and the sea, exhibiting the alluvial deposits of all the rivers and streams that descend from the southern portion of the table land. The mountain chain that forms the eastern boundary of the peninsula, begins to diverge eastward where its continuity is interrupted by the valley of Coimbetúr. From thence it breaks into a succession of parallel ranges, inferior in elevation and in unbroken continuity to the western chain; and in the further progress northward, after branching off into subordinate hilly ranges, occupying a wide tract of unexplored country, and affording valleys for the passage of the great rivers that drain nearly all the waters of the Peninsula into the Bay of Bengal: this eastern range may be said to terminate at the same latitude as that of the commencement of the western. Granitic rocks (principally syenite) seem to form the basis of the whole of these eastern ranges, appearing at most of the accessible summits from Cape Comorin to Hydrabad." On these rest the usual series of primary rocks.

In the vallies of the Pennar, the Krishna, and Godaveri, various subordinate rocks are found, of which perhaps the most remarkable is what Dr. Voysey called the diamond breccia, in the Nella Mala range of mountains. Towards Vizagapatam and Ganjam granitic rocks (syenite and gneiss) predominate. They are occasionally covered by laterite. Granite is again found in the province of Cuttack. Traces of coal too have been observed there, and gold is extracted from the sands of the Mahanaddi. Laterite is traced through Mednipúr by Bishenpúr, and Bancora to Birbhúm, reposing sometimes on sandstone, more frequently on granite and gneiss. "At

Bancora the calcareous concretion called *cancar* begins to cover the surface of the granitic and syenitic rocks, which rise above the surface to considerable elevations in that district."

The great coal field occupying both sides of the river Dummnoda succeeds next. The extent of this field is not precisely known, but it appears to be connected with that of Sylhet and Cachar. The shale covering this coal has sometimes vegetable impressions and even animal remains: amongst these "Dr. Voysey distinguished a *phytolithus*, a *calamite*, a *lycopodium*, and one specimen of a gigantic species of *patella*."

The new road to Benares "passes over granitic rocks, of which the ranges of hills on the left and the whole country as far as the Són and round by Shérgháti and Gaya, is probably composed. On approaching the Són river and crossing the hills behind Sasseram, sandstone begins to appear, and continues to be the surface rock, with probably only one considerable interval, all the way to Agra; forming, as before noticed, the southern barrier of the valley of the Ganges and Jumma: that interval occurs in the low lands of Bundelkhand, where the remarkable isolated hills are all granitic."

The Vindhya range running from east to west may be said to unite the two longitudinal ranges. It is composed of granitic rocks (including as usual gneiss and syenite), on which lie sandstones, and over all an extent of trap rocks not perhaps equalled in any country: according to Captain Franklin the sandstone is synonimous with the newer red or saliferous sandstone. It is found "flanking the great primary branch which runs to Udayapúr on the side of Guzerat, and to the north it sweeps into the desert to an unknown extent."

"With regard to the rocks of more recent formation, India is peculiarly barren." The lias limestone has been detected in Bundelkhand by Captain Franklin, but no trace of the oolites or chalkbeds, unless the *cancar* be their equivalent—an opinion to which Captain F. appears to incline.

This general sketch of the geological structure of the country is intermixed with a great deal of valuable detail, which could not be rendered intelligible in any summary such as the limits of our work would admit. In a note an account is given of a vein of hornblende in syenitic granite broken through by a basaltic dyke. Fragments of the syenite are found in the dyke; and in some of these fragments the vein of hornblende is seen.

II. *On the Geology of a Portion of Bundélkhand, Boghelkhand, and the Districts of Ságar and Jebelpúr. By Captain* James Franklin, 1 *B. N. C. pp.* 23 *to* 46.

Captain Franklin in the first part of his paper details the particulars of the routes travelled, with the several observations made at each place. He then gives what he considers the result of his observations in a general view of the geological structure of the districts in question. Our business is of course chiefly with the latter.

Dr. Voysey it appears considered, that the basis of the whole peninsula of India is granite; "he had traced it along the coast of Coromandil lying under iron clay; also in the bed of the Godavéri river from Rajamahendri to Nandáir; he had specimens from the base of the Sítabaldi hills of Nagpúr, from Travankúr, Tinnevelli, Salem and Bellári. To this may be added Mr. Stirling's account in his Memoir on Orissa where he says, "the granite appears to burst through an immense bed of laterite; rising abruptly at a considerable angle." These localities are all on the southern side of the central or Vindhya chain. Captain Franklin considers that to the north the proofs of granite being the basis rock are equally numerous and convincing. Whether there is a series of the primary strata between this granite and the secondary rocks must often be left to conjecture; but in the bed of the Nermada between Lamarta and Beragerh may be seen a series of beds from gneiss upwards, each in its place graduating into one another imperceptibly, and preserving the same dip and direction.

But the principal feature of Captain Franklin's paper is his having identified the sandstone so extensively developed in the tract under review with the new red or saliferous sandstone of European geologists, and the limestone resting on it with the lias limestone of England. These are interesting results, and are deserving of attention from our Indian observers, inasmuch as the author of this paper had the great advantage of examining these rocks recently at home.

Captain Franklin founds his opinion of this being the newer red sandstone on the general parallelism of its stratification to the horizon, and on its saliferous nature. He describes it as consisting of fine grains of quartz, cemented by clay and coloured more or less by the red oxyd of iron. It is occasionally compact, but its general character is rather friable than compact, and it sometimes contains balls of clay. Its thickness is of course variable. At the Bouti cataract it is 420 feet; at the Chachai and Tons cascades thicker, and at the Bandair hills must be still more so. This formation was traced from the pass of Tára near Mirzapúr by Kattra, the cataract of the Tons river, Simeriah, Birsinhpúr to Hathi, where it was exchanged for the limestone which Captain Franklin thinks synonymous with lias. In an eastern direction the sandstone extends through the Bandair hills to Lohargong and probably farther, being in this direction more exactly correspondent with the "Bunter Sandstein" of Werner, and the stratification horizontal.

"At Hathi as before observed the limestone" first appears. The general aspect of this limestone is dull and earthy; its stratification horizontal or nearly so, and always conformable to the marle or sandstone on which it reposes; its lower beds are thin and separated by argillaceous partings; and some portions of it, particularly the white variety, are sufficiently compact for lithographic purposes: the middle beds are usually of a dark smoky grey colour, always exhaling a strong argillaceous odour, when breathed upon, and sometimes containing fragments of petrified wood, and of the stems of ferns, (Nagound); and it is this variety which burns into strong lime and has the property of hardening under water. The yellow kind is generally compact, usually dendritic, and if polished like the Cottam marble, might be used for ornamental architecture; its external surface frequently presents branches and prominences resembling (as Mr. Greenough expresses it) the interlacings of ivy; and in this state it might be used for rustic architecture. This limestone appears to be the same as the lias limestone of England. "It was traced from Hathi via Lohargong as far as Pattarya."

The next great feature in the geology of this tract is the extraordinary development of the overlying rocks which are usually designated by the general term trap. The upper part of this formation is usually globular, the nuclei of the decaying masses varying in size from an egg to a large bombshell. Under the stratum of globular trap, is a bed of indurated wacken or amorphous trap of a rusty brown colour, sometimes scoriform or of a small cavernous structure, and sometimes columnar: under the amorphous trap is a stratum of limestone (Ságar and Pattariya). This limestone contains alumine and silex passing into chert, and occasionally resembling indurated clay and, though rarely, clinkstone. Generally speaking it appears as if partially calcined; and when the trap with which it is associated reposes on sandstone (Ságar) it contains nodules of that rock imbedded in it.

Below the limestone (Ságar) is a stratum of amygdaloid containing calcareous spar and a few zeolites. It resembles the toadstone of England and reposes on sandstone.

This trap formation, which appears to Captain F. to be the floetz trap of Werner, was traced from Pattariya through Ságar, Jyasinhnagur to Tendukaira. In the latter part of this route chalcedony, semiopal, mealy zeolite, cacholong, agate, jasper, and heliotrope are seen in abundance scattered on the surface. In the bed of the Barana east of Tendukaira, the trap may be seen resting on red marle or sandstone. In the valley of the Nermada the latter formation is wanting, and the older strata, highly inclined and even perpendicular, make their appearance. The stratification is always unconformable to the sandstone. Captain F. points out the valley of the Nermada as an interesting field for geological inquiry. He considers it as the effect of denudation.

The southern boundary of the Nermada valley is also composed of trap rocks, which extend as far south as Chapra or Leoni, and thence eastward towards Mandela, Amerakantak, and Sohajpúr. It appears to be the same rock as that at Ságar, though something harder. It, however, here rests on granite or gneiss.

"A very curious calcareous conglomerate is noticed as occurring in the beds of rivers, the sources of which are in trap countries. "It is composed of rounded fragments of wacken, basalt, sandstone, quartz, and occasionally of other rocks varying from the size of a pea to that of an ordinary grain of sand, cemented by calcareous matter. When the particles are fine, it in some respects resembles calcareous sandstone, and has sufficient cohesion for architectural purposes. Its stratification is always horizontal, the coarse parts being lowest; and it reposes indifferently on every rock."

The overlying rocks of the Ságar district, Captain F. observes at the conclusion of his paper, are not only the most extensive, but, considered as geological phenomena, they are the most important in that district and perhaps in India. Their extent, he adds, is truly astonishing; and he thinks that when their correct outline shall have been ascertained the representation alone will be correspondently striking. They repose upon every rock from granite to sandstone, and its inferior limit in the latter case is 1350 feet (Ságar). The great extent of these rocks and the absence of all regular formations posterior to lias (should future research confirm this position) he thinks are the remarkable features of Indian geology.

The paper concludes with a list of latitudes, longitudes, and elevations, which we shall here reprint.

Place.	Latitude North.			Longitude East.			Elevation.
Lalganj,	25	1	25	82	18	25	504 feet
Barounda,	24	57	20	82	14	00	
Kattra,	24	53	20	82	6	30	520 ,,
Hanmanna,	24	46	15	82	4	15	1219 ,,
Bilohi fall,	24	48	45	81	57	15	1128 ,,
Bouti fall,	24	46	40	81	49	20	1000 ,,
Gerh,	24	49	15	81	39	50	1036 ,,
Keuti fall,	24	49	15	81	27	00	923 ,,
Chachai fall,	24	48	0	81	17	20	990 ,,
Tons fall, deduced from the same,	24	47	15	81	15	05	890 ,,
Simeriah,	24	48	34	81	8	55	1009 ,,
Birsinhpur,	24	48	30	80	57	20	1064 ,,
Hatni,	24	43	18	80	49	15	1070 ,,
Sohawel,	24	34	27	80	46	40	1059 ,,
Nagound,	24	34	21	80	35	25	1099 ,,
Girwar,	24	33	0	80	25	30	1216 ,,
Lahargaon,	24	31	15	80	19	00	1251 ,,
Do above Calcutta,							1231 ,,
Mehewa,	24	24	20	80	7	25	1181 ,,
Tigra,	24	18	20	79	59	05	1093 ,,
Garreho,	24	15	38	79	49	35	1131 ,,
Hatta,	24	7	45	79	35	10	1183 ,,
Narsinhgerh,	24	1	08	79	24	20	1314 ,,
Bikain,	23	53	02	79	13	10	1263 ,,
Saipur,	23	53	15	79	2	30	1442 ,,
Pareneah,	23	51	30	78	56	10	1544 ,,
Sagar (tent),							1953 ,,
Do. above Calcutta,							1926 ,,
Do. town,	23	50	30	78	44	00	1940 ,,
Do. Cantonments,							1989 ,,
Do. Residency,							2050 ,,
Jysinhnagar,	23	37	12	78	33	30	1943 ,,
Garreah,	23	30	17	78	39	00	2094 ,,
Tandah,	23	23	37	78	41	40	1851 ,,
Ghano,	23	22	55	78	49	45	1724 ,,
Sehajpur,	23	17	50	78	53	30	1515 ,,
Tendukaira,	23	10	40	78	58	30	1338 ,,
Dobi,	23	17	12	79	04	55	1704 ,,
Gt. Deori,	23	24	18	79	04	50	1705 ,,
Chandpur,	23	34	21	79	06	15	1575 ,,
Gara Kota,	23	47	25	79	07	30	1345 ,,
Reilli,	23	38	50	79	03	45	1350 ,,
Rangir,	23	37	45	78	55	20	1522 ,,
Chandpur, vide above.							
Gt. Deori, vide above.							
Kusiari,	23	23	48	79	11	55	1584 ,,
Bynsa,	23	24	38	79	20	00	1394 ,,
Samnapur,	23	19	45	79	25	00	1546 ,,
Patteriah,	23	15	25	79	34	45	1395 ,,
Natwara,	23	10	30	79	40	20	1426 ,,
Teor,	23	09	20	79	51	45	1396 ,,
Jebelpur,	23	10	40	79	58	15	1458 ,,
Do. Cantonments,							1470 ,,
Do. Residency,							1500 ,,
Pannagerh,	23	19	15	80	02	30	1477 ,,
Pouri,	23	23	00	80	08	50	1423 ,,
Majgowa,	23	24	15	80	14	30	1550 ,,

III. *The Trap Formation of the Ságar District and of those Districts westward of it as far as Bhopalpúr on the banks of the River Newas, in Omatwara. By Captain* T. Coulthard, *B. A. pp.* 47 *to* 81.

This paper comprises chiefly very full topographical and orological details of the tract indicated. As it is impossible to do justice to these without in fact reprinting the substance of the paper (and even then they would be unintelligible without maps and sections as well as specimens), we must confine our analysis of the article to a brief notice of the general results.

The author of the preceding paper has noticed the great extent of the trap formation in central India, his researches having come upon the eastern limits of the tract described in the present paper. The details given by Captain Coulthard fully bear out Captain Franklin's opinion. Undoubtedly, as observed by Captain F., it is one of the most striking features of Indian geology, not less remarkable than the absence of all the newer formations from lias upwards. A correlative circumstance of great interest, is, that it is precisely in these formations, so extensively developed in Europe, that the remains of *tropical* vegetables are found. Now when we consider the invariable connection hitherto observed to exist between certain formations and certain classes of the organic kingdom, we can scarcely expect to find these remains, though so closely allied to, if not identical with, the present productions of these countries, while the rocks with which they are always associated are absent. Should then the above conclusions be found to stand a further examination, they would at least militate against the belief of the universality of the newest of the secondary strata, and would prove that comparatively with the primary they are local and of small extent. What other particular consequences may be deduced from these views, we leave to abler pens than ours to point out; but they do appear to us to be full of interest, and as supplying ample food for speculation.

The trap district described in this paper in thus limited. A line drawn between Shappur and Pattaria to the red sandstone hill which overhangs Tendakaira will form the eastern limit. On the south the line will pass thence to Hasanabád. On the west a line drawn from a point between Sultanpúr and Dewas to Bhopalpur marks the junction of Capt. Dangerfield's newest floetz trap* with the subject of the present paper, which may therefore be said to extend to Udayapúr. On the north the limits are not so well defined, but a line from Bhopalpúr to Seronj will be included, while at no great distance from it sandstone is found at the Maltoun pass and Granite at Tírí. To the Eastward it is bounded by a granitic range which crosses the Nermada at Jebelpúr. To the southward beyond the alluvium of the Nermada a primary range is also found, as is also in the latitude of Hirapúr, while this basin, as it were, elongated E. & W. is occupied by a sandstone deposit, which occasionally breaks through the otherwise continuous covering of trap that occupies the surface. It has an extent, Captain Coulthard thinks, of perhaps more than fifty-four thousand square miles.

This tract is throughout hilly; " but it may be better understood if it be said that at Ságar, in its neighbourhood, for eight or ten miles around, and also in every part south of Ságar, within the prescribed limits, and as far west as Hasanabad, may be seen ranges of low hills extremely clustered though always detached, bending about in their short course towards all points of the compass, and thus forming valleys of every conceivable form, though not commonly of any extent, and never difficult of access. But if the view be extended beyond the neighbourhood of Ságar, towards the east, or the west, or the north, expanded valleys will gradually meet the eye, while the hills recede from it sinking in the horizon as they surround valleys farther removed from Ságar, until these valleys are enlarged into extensive undulating plains, studded over with isolated trap hills, occasionally of a conical, commonly of no determinate form; and ever and anon a short range of the same, deviating little from a straight line, will have its beginning and its ending within view."

With regard to the level of this land above the sea, it may be observed that Ságar is generally speaking the highest point in this tract, though there is a peak to the eastward of Raisen, which has an elevation of more than 2500 feet. The centre of the cantonment at Ságar is 1983 feet above the level of the sea, as determined by

* By this barbarous term, as we will venture to call it, though sanctioned by many high authorities, is meant, we conclude, that the trap in question lies on new red sandstone. But it lies also on granite (see preceding article.) Is it still floetz trap, or does it suddenly change its nature and become primitive?

barometrical measurement, and the hill at the mint of Ságar is something more than 2300 feet (trigonometrical result.) From Ságar round in every direction, the general level diminishes, but westward towards Bhópalpúr very gradually, as the supposed level is in that quarter somewhere between 16 and 1700 feet above the sea. Eastward towards Hatta and Garakóta also the diminution is gradual, the elevations being in general 1500 feet. It is towards the north the most rapid; the Maltaun pass, thirty-six miles north of Ságar, being elevated only 1000 feet; Serong W. of N. 800; and Hírapúr E. of N. between 1000 and 1100. The primary range skirting the alluvium of the Nermada (the southern boundary) is about equal in height to the trap and sandstone hills in the neighbourhood of Ságar, while that of Bundelkhand (to the north) is 1000 feet lower.

The valleys and extended plains are every where composed of a trappean or basaltic mould, blackish in colour, which reposes on a bed of either basalt or of compact wacken. This wacken is disposed either in ovoidal or angular pieces. "Under it lies an amygdaloid, decomposing and decomposed—which as a retentive clay keeps up the water near the surface throughout this tract."

The hills are either trap or sandstone. The former are sloping and rounded, having seldom any thing of an escarpment to mark them. "Their surfaces are thickly strewed with masses of basalt or wacken imbedded in a basaltic or wacke clay." "From one hundred and twenty to one hundred and fifty feet, may be said to be the general height of those that rise above the rank of swells and knolls; whilst a hummock, a cone, or something of a truncated cone, occurring in their otherwise even outline, and which serve to characterize them from their sandstone companions, partly increases the elevation."

"The sandstone rock is very prevalent as a mere mound or rise constantly having a village upon it, and situated often on the plain, oftener on the edge of the plain, with a trap hill partly resting on it. In particular parts of the country, however, ranges of sandstone hills occur, equalling, though never exceeding in height and extent of range, those of the trap."

Red marle or clay is frequently interstratified and galls of clay imbedded. The colour varies from a dark chocolate, through various shades of red, to white. Two colours are often found intermixed, a deep chocolate, and a white either in alternate streaks, or in a ground with spots. These are generally amorphous. The schistose varieties are clouded, streaked, (transverse to the structure,) zoned, the colours being green, brown, red, and yellow. These varieties are highly micaceous. The strata are always horizontal, and on a full view of all the phenomena, Captain C. considers himself justified in pronouncing it to be the new red sandstone of Macculloch, and to belong to the lowermost strata of that formation. But he adds, what also struck us on a view of the specimens, that it is not always a freestone, but on the contrary "a hard glassy splintery substance:" in fact, a true quartz rock, or not to be distinguished from such in hand specimens. Some varieties are so schistose in their structure as to be quarried for flags, slates, &c.

There is a great sameness amongst the trap rocks of this formation. They are in general of an earthy homogeneous aspect, with little if any resemblance to the crystalline members of this family, whether syenite, or greenstone, nor is there even a clink-stone or a clay-stone. They may be described rather as a series of basalts of a fine grain; of wackens and amygdaloids. They appear to be all composed of an intimate mixture of felspar and hornblende in an earthy state, the latter mineral characterizing the harder varieties. In general, no appearance of structure can be detected, but when otherwise, it is of the prismatic character; and it may be seen sometimes even in hand specimens, which break with a cleavage into four sided prisms*.

* The author of this paper thinks it "much to be wished, that the term basalt could be extended so as to include all those rocks named wackens; for although there is some slight diversity of fracture and frangibility, and some little variation in colour, yet a difference in name seems quite uncalled for in regard to them, and only calculated to mislead." In his further remarks, however, he admits, that the basalts are all compact without any foreign mineral imbedded, whereas the wackens are not so, and generally contain imbedded portions of the mineral, which belongs to their associated amygdaloids. If this be the case, and the wacken be not one of a numerous series of fine gradations between basalt and amygdaloid, then the distinction appears well-founded, and the difference of names worth preserving. In all questions of the identity of compound rocks, where the grain is too fine to admit of perfect certainty as to the mineral nature of the component parts; we

A very full and technical description is given of the principal and most marked varieties, which we regret we have not room for. The most generally diffused of them is thus characterised. "It is what is called a compact indurated wacken, in colour black, with a very distinct brownish tinge. When first fractured, its surface has a much more glimmering appearance than the basalt; but, unlike the basalt, exposure to the atmosphere soon changes its surface into an earthy, dirty whitish colour. It is often very tough, very refractory under the hammer; but its fracture is flat and dull, not sharp and splintery, or approaching to the conchoidal. It occurs in pieces, in length, breadth, and depth, pretty nearly the same, a foot in measurement, and which are set closely together, so as to form something like a stratum in the hills or in the valleys as the base of the basaltic mould; and it is also the predominating variety in those hills, which are of such constant and general occurrence, consisting of large rounded and angular masses thrown up together in the utmost confusion, with very little clayey matter intermixed; and lastly, it may often be seen abstracted and alone, in something like large uniformly ovate masses, having a brownish and wrinkled exterior, and imbedded in a sombre reddish brown clay."

The amygdaloids contain the following minerals: calcareous spar, green earth, chalcedony, and quartz, common and amethystine. Zeolites are scarce, olivine is abundant as an imbedded mineral; but neither hornblende or augite have ever been met with as such.

The same curious white earthy limestone mentioned in the preceding paper is here noticed as belonging to the trap formation. It is described as harsh and gritty to the feel on the fresh fracture, "small rounded (round?) particles of calcareous spar of a yellow colour" are sparingly imbedded. Though generally of a light colour approaching to white, it is occasionally reddish, brick red, chocolate brown, or brownish black. It appears to pass into amygdaloid, into jasper, and into hornstone or chert. It is never found in the vallies, always on the hills and low swells, forming the basement stratum, but ascending above the level of the contiguous vallies. A suggestion is thrown out, that this limestone may be the lower lias, half calcined, and otherwise disguised by the trap.

The water is always near the surface, whether that be sandstone or trap. In the former case, the rock itself is sufficiently retentive: in the latter it is the toadstone that keeps it up. In the valleys, in the dry season, it may generally be found at from three to twenty-five feet below the surface.

At Sanwa, distant from Ságar forty-five miles, the trap is succeeded by lias, which extends to Panchamnagar only nine or ten miles from Hírapúr, where, as before noticed, granite is found. The trap where it ceases, ceases suddenly, having a vertical thickness of sixty feet, and underneath it may be seen the sandstone. Beyond this point are seen three hills, consisting "of sandstone masses rather sparingly and loosely set together in much red clay and quartzose matter." They are of no great height, but are steep, and separated by ravines or watercourses. From the summit of the last of these, the view into the valley of Hírapúr looks over an intermediate conglomerate range; and on descending from the summit, the sandstone is seen resting on a stratum of brownish black ferruginous clay and earthy iron ore. The conglomerate, or breccia rather, consists of angular pieces of white felspar or grey limpid quartz, imbedded in a dark, highly indurated red clay, or a quartzose basis. The granite of Hírapúr is conterminous with this rock; and it is thought, that a section through the line noticed, would be extremely interesting, as showing the junctions and relative positions of these several rocks. At Hírapúr, the granite is surmounted by this conglomerate, which is connected again with a stratum of iron ore, on which the new red sandstone rests. No fossil remains have been noticed in this tract.

IV. *Remarks on the Geology of the Country, on the Route from Baroda to Udayapúr, via Bírpúr, and Salumbhur. By* James Hardie, *Esq. Asst. Sur. B. N. I. pp.* 82 *to* 99.

This paper carries us still farther west; so that taken with the two preceding, it nearly completes a narrow strip reaching from Lohargáon to Udayapúr, the former in longitude 80° 15, the latter in 74°.

think the system of mechanical analysis would be found useful. If it could be clearly established, that basalt and wacken, and amygdaloid, or the rocks so named by us, are all composed of the same ingredients, and nearly in the same proportion; the conclusion would be inevitable, that they should have but one generic name, specific terms being added to designate any accidental quality.

Mr. Hardie proceeded from Baroda to Udayapúr; and such remarks as he could make on the nature of the rocks in a hurried march, are here thrown together. He does not pretend, he says, "to give a correct geological description of the country." To effect such a task time was wanting.

From Baroda to Balasinur, a distance of 56 miles, nothing is seen but a uniform expanse of alluvial soil. At Balasinur we first observe rock formations occasioning a diversity of surface. The only rock is a "conglomerate principally composed of agates and other quartzose minerals." Some of the agates are of considerable size. At Pandua, ten miles farther, in a northerly direction, is seen "a close grey granite, composed of greyish felspar, translucent quartz, and dark coloured mica, with hornblende occasionally disseminated through it:" it passes into a larger grained rock "composed of large masses of reddish grey felspar, nearly transparent quartz, and silver coloured mica." "Some of the masses of quartz" "were upwards of a foot square, and many of them nearly transparent, through the substance of which prismatic crystals of schorl were seen to shoot. These rocks were not stratified."

Five miles beyond Bírpúr, a more hilly country occurs, though the hills are low, and their summits form a kind of table land which is, like the plains, covered with an alluvial soil of great depth. The stone used for building is a compact quartzose sandstone, or rather a ferruginous quartz; and it appears to be distinctly stratified. Mr. Hardie thinks that the alluvial deposite which covers the plains of Guzerat, and ascends even the lower hills, as above noticed, can hardly be attributed to the operation of causes still in action. He appears in fact to consider it as diluvial. Whatever may be the case with regard to the deposite in question, we have ourselves not the smallest doubt as to the reality of this distinction, first insisted on by Dr. Buckland, and supported with so many happy illustrations by Professor Sedgewick. It is quite apparent in some of the deposites in India, and perhaps even more so than in those of Europe.

Guzerat he compares to Bengal, and describes it as having a rich soil, cultivated by a comparatively speaking civilised people; covered with numerous tanks and wells and rivers.

From Bírpúr to Dewari, six miles N. E., the hilly country continues presenting a more ridgy appearance, and having at a distance a uniform and even outline. Their slopes are covered with *debris* of the local rocks, which consist of, 1. a greyish quartz rock, inclining to slaty structure, 2. a ferruginous quartz, and 3. a pure white quartz; the three occurring in frequent alternations, and the latter always appearing to be amorphous.

After leaving Dewari, various modifications of quartz rock and clay slate alternating with, and occasionally passing into each other appear, and in highly inclined strata. These would perhaps by some be termed transition, but Mr. H. cannot see the use of this term, as he considers that there is an uninterrupted series between these and the oldest rocks.

Five miles from Dewari a plain covered with a thick soil commences, which continues for thirteen miles to Hartúna. Occasionally small hills or rising grounds make their appearance, composed of the above strata, still highly inclined, clay slate being more predominant. At Hartúna the superficial rock in the plain was found to be a sandstone with a clayey basis, and of a soft friable nature. It was a variegated rock, with spots of a reddish colour dispersed over a whitish ground. Below this was found a grey calcareous sandstone, both arranged in strata very slightly inclined. Nothing like organic remains was found. A vein of quartz was seen to traverse it about 6 feet in breadth below, diminishing to $1\frac{1}{2}$ above. The quartz was of a pure white colour and crystalline structure. It presented the appearance of a number of rounded masses, which appeared to be derived from the neighbouring hills, cemented together by calcareous matter. Mr. Hardie considers this sandstone to be synonymous with the new red sandstone of Jameson.

In Captain Dangerfield's geological map of this part of the country, a coarse granular limestone is laid down as extending in this direction; no such rock was however observed. His hornstone, Mr. Hardie observes, may be the same as the stratified quartz here described. A hornstone so called by Captain D., the author of this paper has often examined at Udayasagar, and he has no hesitation in pronouncing it to be quartz rock.

In making these remarks Mr. Hardie would not be understood as wishing to detract from the well earned merits of Captain Dangerfield. He thinks that we are indebted to him for much very valuable information, although to make a correct map of the country was more than could be effected by an unassisted individual without devoting years to the investigation.

From Hartúna by Pít to Ghátah, distance eighteen miles, the country is much broken (latterly more so), and numerous small rounded hills were observed, exhibiting at their surface vertical strata, chiefly of clay slate, but occasionally of quartz rock. The clay slate seemed to lean sometimes towards quartz rock, sometimes towards chlorite slate, which itself is found in small quantity. The beds of white quartz, which alternate with the slates, have a fissured appearance, as if consisting of a congeries of detached masses, varying in size from 2 or 3 inches to a foot in diameter, and closely packed together. About five miles before reaching Ghátah, a hill of serpentine was observed. It was not stratified. It was of a greenish colour with a tinge of brown; containing grains of magnetic iron ore disseminated.

In the next 12 miles to Sagwára mica slate began to prevail, its approach having been announced in the preceding stage by scales of mica disseminated in the clay slate. This rock, with alternations of quartz rock, continues for 22 miles, by Jariana to Jaitana. The quartz rock is often of a pure white, and at a distance when on the surface might be mistaken for snow. It is either compact or coarse granular, the concretions being about the size of a bean. It is certainly stratified and conformably to the slates. Detached fragments of it are seen covering the face of the country, and frequently immense isolated blocks of several yards in diameter were seen topping the hills, and piled on each other, often in a very fantastic manner. Latterly mica and clay slates prevailed.

Between Jaitana and Salumbhur, the mica slate passes into gneiss, in which beds of granite occur. The hills are generally ridge-shaped and sometimes peaked. In the beds of nullahs strata of *cancar** were observed. These appear to be of some extent, occupying equally the highest and the lowest situations. It is sometimes soft and friable, sometimes more crystalline. It appears sometimes to convert the inferior slates into a calcareous rock by the infiltration of carbonate of lime in solution, and the formation of calcareous spar. The gneiss continues over a very rough and uneven country for 24 miles beyond Salumbhur by Gingla, alternating with granite, and containing beds of hornblende slate, which rock prevails towards Gingla. In a bed of it was observed a vein of quartz and felspar. Between Gingla and Kutawar several varieties of granite and gneiss occur, into the minute description of which our limits forbid us to enter.

Similar modifications continue to Thanna, seven miles further, with others of hornblende rock†, and in particular a gneiss in which hornblende replaced mica. Chlorite slate was also observed.

The observations made in the vicinity of Udayapúr are deferred for the present, being meant to be included in a separate communication, the subject of which will be a description of the valley of Udayapúr.

V. *On the Diamond Mines of Panna in Bundelkhund, by Captain* James Franklin, 1*st Reg. B. C. pp.* 100 *to* 122.

The diamond mines of Panna are confined entirely to the range called Bindáchal which, like the neighbouring range, the Bandair hills, is composed of the new red sandstone, the subject of the preceding paper of this author. The diamond is found in a conglomerate, which is either *pacca*, i. e. a gritstone with a siliceous cement containing pebbles of ancient rocks, or it is *cacha*, and contains pebbles of more recent rocks in an argillaceous cement. It is also found in a superficial bed of *lalcacra* or red ironstone gravel, mixed with ferruginous sand or clay. This gravel is water worn, and sometimes rounded like swan shot. It is sometimes covered by vegetable soil, sometimes by a bed of yellow clay containing particles of common *cancar*. In this latter case, being found to contain calcareous matter, it is termed *padda*.

"The *pacca* or rocky matrix is very limited, stretching generally from Camariya to Brijpúr along the course of the Bagin river. It is excavated at Camariya, Bijipúr, Bargari, Myra, and Etwa; there is also a small deposit of it near the town of Panna," and at Brijpúr. Of these the mines of Camariya and Panna are the most noted. At the former place they are fifteen feet deep, the strata of the new red sandstone lying on the rocky matrix of the diamond. At Panna they vary from 20 to 50 feet in depth, the matrix being not more than a foot and a half. The Sakeriya mines are situated in the *cacha* or immature matrix. A shaft examined here

* This rock has perhaps some analogy with the cornstone of some of the English counties. See Geol. Trans. vol. II. N. S.

† Hornblende rock is stated to pass into greenstone, but is not greenstone essentially different by containing compact felspar (a mineral, by the bye, for which a name is wanted), which is seldom if ever found amongst any but overlying rocks?

gave the following beds: 1. 8 feet of vegetable soil: 2. 8 feet of *piri matti* or common *cancar*, imbedded in yellow clay; 3. 4 feet of *laleacra* or red ironstone gravel, in ferruginous clay; 4. 2 feet of *detla* or white quartz gravel; next followed sandstone; and then the *cacha* matrix. The thickness of the *detla* stratum is considered a matter of augury: it ought not to exceed two feet. At the Udesna mines the same matrix underlies laterite*, there called *macha*.

The preceding are all termed *gahira* or deep: "the *chila* or superficial mines are to be found in every part of the diamond tract; excepting only a circuit of about five miles from the cascade of the Bágin river, where it appears that denuding causes have swept them away, and all their contents into the glen of that river. Their matrix is always red ironstone gravel in ferruginous sand or ferruginous clay." "These mines rarely exceed five or six feet in depth, and are often much less. With regard to their produce I am inclined to think they are very precarious, notwithstanding some of the largest diamonds have been found in them. It is common to hear complaints of having found nothing for many months." "They have an advantage in requiring little or no outlay, and are consequently wrought by all classes." "The diamond is occasionally, though very rarely, found on the surface."

Besides the above localities where this mineral is supposed to be *in situ*, there are some where it is found in deposites, with which it appears to have been swept away from its native beds, as at Majgoha and in the glen of the Bagin river." In the latter the diamond is found under rocky *debris* both on the banks and in the bed of the river, and also in the basin which receives the cascade. This latter, Captain Franklin thinks, has never been properly excavated. The Majgoha mines, which are on the western boundary of the diamond tract, are situated in a hollow, resembling an inverted cone, the diameter of which is about 100 yards, and the depth not less than 100 feet. On its periphery superficial mines are wrought in sandstone; but the most valuable are in the green mud with which this hollow is filled, and which is covered with a thick crust of calcareous spar. Fifty feet is the greatest depth to which they have carried them, as the water at that depth overflows them, and compels them to desist. It is possible that here, as in the cascade of the Bagin river, "superior means might be applied with effect and perhaps with profit."

In all the mines the method of search is the same. The matrix (which if rocky requires to be broken) is thrown into a trench and well washed, to clear it of the clay: to ensure this object more effectually it is sifted with fine baskets. The fragments are then spread in a thin layer on a smooth floor plastered with clay or cow-dung, and when dry the whole is passed under the hand, and searched three several times. Notwithstanding all their care, some smaller diamonds sometimes escape their scrutiny; and it has happened that the refuse fragments being again examined, they have been detected, a circumstance which appears to have given rise to the opinion of their reproduction. This explanation applies particularly to the Majgoha mines, where the matrix being in some measure calcareous, no washing is sufficient to free the diamond completely.

The mineral is classed according to the native denominations, as follows, 1. *Lilwaja*, transparent and colourless, very scarce. 2. *Banspati motichar, ghirya* or *maska:* the first has a greenish tinge, the second is also greenish but with a pearly cast, the third is yellowish and of a greasy or resinous lustre: these kinds are common, and their price is thirty *Srinagari*† rupees for one *rattee* weight: thirty-five for two; forty for three; forty-five for four; and fifty for those of five *ratti* weight. 3. *Sambara* and *charchara*. These appear to be laminæ of large diamonds that have been broken. They are of a good water in general, and being cheaper, (twenty rupees the *ratti* increasing as above) are found the most profitable by the jewellers. 4. *Bengula pashmi, pira* and *matta:* these are yellowish green, yellow, and clove brown; their price is fifteen rupees the *ratti*. 5. *Rekatberar*: this is the rose colored variety; its price is twelve rupees the *ratti*. 6. *Kala, garas* or *jalidar:* the first is black or very dark brown, and the second, as its name implies, includes all diamonds that are flawed: the worst may be purchased for eight, and the best for ten rupees the *ratti*.

The revenue of these mines is divided amongst the Rajas of *Panna, Banda, Chircari*, and *Jaitpúr*. The first of these, who has by far the largest share, receives about 30,000 Rs. annually. This revenue is the produce of a tax originally fixed at one-fourth of the value of all diamonds below eight *rattis* weight, but the tax levied is said to exceed this; and for all higher weights there is no stipulation. The whole

* "The laterite is an aggregate of ironstone gravel cemented by an argillo-ferruginous cement; it therefore something resembles pisiform iron ore."

† The *Srinagari* Rupee=0,86 of a Sicca Rupee.

produce of the mines may be about 120,000 Rs. The natives appear to think they are diminishing in value, and Captain Franklin concludes (from the limited extent of the matrix) with reason.

The Bangapilli mines in the south of India appear to Captain Franklin, from Drs. Heyne and Voysey's descriptions, to correspond exactly in geological features with these; and he considers the including rock to be the new red sandstone. Dr. Voysey, however, he states, terms the rock "the sandstone breccia of the clay slate formation." Captain F. thinks the term breccia objectionable (as it doubtless is), the fragments being rounded; and with respect to the "clay slate formation," he objects to it, as Dr. Voysey himself allows that he does not mean the *thonschiefer* of Werner, to which rock alone the term clay slate is applicable, in Captain F.'s opinion*.

It has been supposed, that diamonds are always found at the same level above the sea. To enable observers to judge whether this be universally true, a list is given of the elevation of the several mines, which range from 1250 to 1500 feet above the sea. In the conclusion of his paper, Captain Franklin enters into some speculations on the origin of the diamond, which he supposes may be owing to the action, under extraordinary pressure, of subterraneous heat on vegetable matter. We think that the facts are yet wanting, on which to build even a plausible hypothesis of the origin of this mineral.

VI. *On the Geological and Mineralogical Structure of the Hills of Sitabaldi, Nagpúr, and its immediate vicinity. By the late* H. W. Voysey, *Esq. H. M. 67th Foot, pp.* 123 *to* 127.

This paper is a short one, and we shall therefore give it at full length in a future number.

VII. *Observations on the Geological Appearances and General Features of portions of the Malayan Peninsula and of the Countries lying betwixt it and* 18° *North Latitude. By Captain* James Low, *M. N. I. pp.* 128 *to* 162.

"The general features of the Indo-Chinese regions seem to be alternate ranges of hills, stretching nearly north and south, and conforming occasionally to the general direction of peninsular tracts, and of valleys of various breadth, through which flow large rivers."

"The principal ranges are, that which divides Assam from Ava; the Siamese and Ava range; the Siamese and Cambojan; and lastly the Cambojan and Anam range." "The broadest valley seems to be that of Ava, and the narrowest the Cambojan."

As in all tropical countries, the luxuriant vegetation is opposed to the accurate researches of the geologist; add to which political jealousy here increases the difficulty. Captain Low concludes then that his observations, limited though they are, will not be unacceptable.

His account commences with the friendly state Perak in the Malayan Peninsula, situated in Lat. 4°. It appears to consist of a plain of 15 miles, to which eastward succeeds the tract of mountains that runs N. and S. and seems to form the backbone as it were of Malacca. Of the plains nothing has been learned except that they are alluvial, but the Bountin Islands off the coast are granite. Of the mountains, the only particulars collected are their produce in gold, tin, and antimony. Limestone is also found; and from the accounts of natives it is supposed coal may be. There are several hot springs.

Proceeding northward, we arrive at Penang, composed of a grey desquamating granite. On the shores of several of the small islands lying near it, there is found a conglomerate tinged with oxyde of iron. But no particular account of its character or relations is given. The peninsular range is in the latitude of Penang, about 4000 feet high. It is also rich in tin, and it is reported that there is an extensive table land in the bosom of the range situated N. E. from Penang.

That part of the Cedda coast opposite Penang, is remarkable for mounds of sea shells found several miles inland. Tin appears to be found in several detached hills. The Cedda peak, termed by the natives *Gúnong Cherai*, is the principal object of interest. It is about 3000 feet above the sea, which washes part of its base. Its summit has not been yet explored by European foot, owing to the jealousy of the Sia-

* Werner's classification of rocks being founded on the examination of a very limited tract of country, it is not to be wondered, that his views should be found occasionally to fall short of the boundless variety of nature. Macculloch, if we mistake not, has in particular extended the application of the term clay slate much beyond the boundaries of Werner's *thonschiefer*.

mese. From specimens brought to Captain Low he finds that it consists of granite. Gold is said to be found, and tin was formerly procured from it. Iron ores abound; and its vegetable riches are scarcely less than its mineral, as all the valuable woods of this coast are found on it.

Northward of Gúnong Cherai, after passing the Cedda river, which rises in the central range and waters a tract of rich soil, is to be seen the elephant rock. It is a dark mass of granite, and shoots abruptly to the height of 400 feet. North of this point the coast continues low, but turning to the Laueeang islands we find granite still prevailing. The southern limit of the limestone formation, which Captain Low has traced northward as far as the province of Martaban, is here to be seen.

Several miles beyond, the Trang rocks begin; one of these, which Captain Low visited, he considered to rest on a granite base, the superincumbent mass being heterogeneous. Limestone, veins of quartz, ores of iron are the principal ingredients. At the south end, about half way up the cliff, there are magnificent natural arches. A cavern has been formed quite through the north end of the rock by the action of the sea below, and the gradual decay of the structure above. This cavern, which was visited, abounds in stalactites. The roof is about fifty feet high, and dome shaped though rugged. " Here were observed flimsy ladders of flexible cane, stretched betwixt projections of the rock, and on emerging from the cavern, similar ladders were observed to have been arranged up the face of the cliff, in a zigzag manner, here fastened to a jutting point of rock, there reeved through a perforated angle. These had been thus placed by adventurous Malays in quest of the edible birds nests. Their trade is more dangerous than that of the samphire gatherer, or the Hebridian birder; but it is more profitable than either. Several of the birds' nests islands in this line have been so tortuously hollowed out by the slow operation of ages, that previous to going in, the nester fastens to the entrance the end of the clew he takes with him, that he may not lose his way. On these occasions they use dammer torches. The eye of the swallow which builds these nests, must be peculiarly formed to enable it to work and nestle in such a labyrinth where total darkness prevails."

Near and to the north of this rock is a very rocky island, called *Ka Pesa* by the Siamese: the general structure corresponds with the preceding. Granular magnetic iron ore, imbedded in a calcareous and micaceous gangue, were found at the north end, where large masses have fallen from the cliffs.

At the north side of the narrow entrance to Trang harbour, in N. Lat. 7° 20′ is a remarkable calcareous rock, with several caverns in it. Pulo Tilibon, which forms the north side, exhibits granite and ironstone, with veins of quartz in it. The Trang river is broad, with a high ridge running at right angles to it, on the west side of the entrance. Granite rocks here protrude through the soil, which is red and ferruginous. The shore is overspread with lumps of micaceous iron glance, in small rounded particles, black but yielding a reddish streak, and when reduced to powder adhering tothe magnet. Most of the small islands lying betwixt Trang and Junk-ceylon, seem for the greatest part composed of granite. It also prevads in the latter island, and again tin is associated with it. A range of hills, the highest of which, perhaps, does not exceed one thousand feet, stretches longitudinally through the island, with the exception of a large break in the middle. The Pafra strait, which separates it from the main land, is narrow and rocky. In 1821, this island had a population of 6,000 (Siamese).

" The tin formation seems to run in a continuous line from the southern extremity of the Peninsula, up to about 15° N. latitude. Beyond this point, neither Burmese or Siamese have discovered any mines." It shows itself again in Thampè, one of the provinces of the Shan, in about 20° N. Lat. and Long. 99 to 100° in the form of stream ore. They have also lead mines. Junk-ceylon was supposed formerly to yield 500 tons of tin, but has now dwindled to little more than 20. It is produced at about one-half the market rate. The mines are pits of from twelve to twenty feet deep. " The ore is generally in round or oblong masses, with well defined crystals, and in a matrix of quartz, or bedded in masses resembling half decomposed granite, yet of considerable hardness." The furnace in which the ore is smelted is oblong in shape, and about three feet high. Alternate layers of ore and charcoal are put into it; and the usual horizontal tube bellows of the Chinese, is kept incessantly at work during four complete days of 24 hours and one night, when the furnace is cleansed. The tin begins to flow after it has been lighted a few hours, and is run into moulds while fresh ore and fuel are applied.

The bay of Phunga which stretches north east from Junk-ceylon, is remarkable for the magnificent rocks with which it is studded. They occupied a line of about ten miles; the height does not exceed five hundred feet, and seldom falls short of two hundred. Their northern extremity lies behind the town and valley of Phunga; the southern rests in the sea: their direction is nearly that of the Trang rocks. At a distance they have a pyramidal outline which, on a nearer approach, is changed to columnar, yet they are not basaltic but calcareous. There are grottoes here also lined with calcareous spar, with stalactites depending from the roof, and a curious natural tunnel, the height of the arch being twenty feet.

The valley of Phunga is about three miles long by one, on an average, in breadth. It is hemmed in by rocks and hills to west and east. Those on the west, less abrupt, seem mostly granitic; those on the east, with perpendicular precipices of four and five hundred feet, and highly picturesque, are calcareous. At a short distance, they look like chalk, being covered with the agaric mineral. Tin abounds in the granitic range. The population here is about 8,000 souls, including 600 Chinese and 100 Siamese priests.

Northward of Junk-ceylon are several calcareous rocks off the coast frequented by birds' nest gatherers.

The Peninsula now narrows extremely, and with it the central range; but Capt. Low is of opinion, that there is no break in it, and that it must always present obstacles to any attempt at forming a communication between the Gulf of Siam and Bay of Bengal. He admits, however, that a traveller may, by running up the Kra, or other stream, in a boat, get within three days' journey of a navigable river on the other side.

· Tin is said to abound between Junk-ceylon and Mergui.

The Coast of Tenasserim, from 10° to 12° 30′ N., is shut out from the ocean by high, and generally rocky islands. Those which form the west side of Forrest's straits are composed chiefly of granite.

Beyond the principal of these, named Domel, there is a considerable opening, where a distinct archipelago of bleak and rocky islands begins, stretching north and south. The belt consists of four or five parallel rows of islands, and may be about twenty miles in breadth. They are not laid down in the charts. The channels between them are for the most part deep, and frequently there is no anchorage at half a cable's length from them.

"Their formation is primitive (primary); the granite is occasionally associated with black schistose strata or sandy slate." Limestone was not observed, though, as some of the islands are frequented by the nest gatherers, it is to be inferred they are calcareous.

We are now arrived at the boundary of the British and Siamese teritories. Of Siam few particulars are known. The gold mines at Bantaphannae, in the latitude of Mergui, appear to be of no consideration; their produce yearly is not more than 15,000 Rs. The gold is found in an alluvial deposit.

Returning to Tenasserim, we find the islands fronting Mergui of granite, as also the hill on which the town stands. Argillaceous petrifactions are said to be found here; some petrified crabs were obtained. The province of Mergui abounds with tin ore, especially to the southward.

Tavoy is a hilly province. The foot of the hills is about 10 miles east of the town; they run chiefly north and south: the streams which are found between, after escaping from them, turn to the westward, and flow through the level plains to the sea. Grey granite is the prevailing rock, with occasional patches of slate. The tin mines are merely stream works. Antimony has been obtained in small quantity. About 15 miles north east from Tavoy is a hot spring, having a temperature of 144°. It has no taste, and the incrustation derived from it is pronounced to be calcareous. The rock from which it issues is a transition slate, effervescing slightly with acids.

There is a road leading across the hilly tract into Siam, but exceedingly difficult, and only passable by foot travellers. It was visited by Captain Low. The height of the pass, which by the road is about sixty miles, is 3000 feet. Four very distinct and higher ranges of hills were seen within the Siamese frontier. About 40 miles may be allowed as the width of the hilly tract here.

In his route to Yè, Captain L. found only granitic rocks. At Enbieu near Kalingaung, a curious hot well was met with, about forty feet in diameter, having a temperature of 104°. The low hill on which the town of Yè is situated consists of decomposing granite.

Martaban offers a promising field for the labours of the geologist. It is bounded to the north by a branch of the great central range dividing it from Siam. On the south the Balami in a narrow stream divides it from Yè. On the east is the Siamese range showing at intervals peaks, the highest of which may be about 5000 feet. Through this range there is but one good pass, termed Pra-song-chú by the Burmese, and Phra-chedu-sam-ong by the Siamese, i. e. the pass of the three pagodas. It is in Latitude 15° 18′ 00″ N. Longitude 98° 22′ 15″ E. according to Captain Grant's observations. On the west the sea is the boundary partly, and partly the provinces of Chetang and Tham Pagú. It may be computed to contain about twelve thousand square miles.

The principal river is the Krung Mautama (of the Peguese) or Santien (of the Burmese), which rises in a range of mountains to the northwest of Cheang Mai in Laos, passes within two or three days' journey of that capital, and after a turbulent course enters the plain country in 18° 20′ N. Lat. It is joined at the Kakayet stockade by the Yien Zalen river, which flows from the Haphún hills lying in a north west direction. Eight or ten miles beyond the stockade, it has a bar of granite rocks across, and is not navigable to the smallest canoes. Hence it has a more tranquil course, and it joins the sea at the Khyet Khamí Pagoda. Opposite to Martaban, it may be about a mile in width. " The other rivers are the Dang, Darni, Kyung, which joins it at Mahi Phra Pagoda; the Gyen Kyung, which falls into it at Phra Pyú or the white pagoda; the Attaram or Attirjan, which enters it nearly opposite to the town of Martaban; the Wakrú Kyang, which disembogues near the Kyet Khami pagoda; and the Dang-wein-kyáng, which pours itself into the gulf of Martaban. These are all navigable far inland by large boats."

The chief hills within the province are, part of the Tavoy range; next, a short range running across one of the upper branches of the Attaram river; the Jeu-kyet-phra-táng; a high-peaked hill, fifteen or twenty miles to the westward of the town; the Joga-ben-táng to the northward, and the two insulated hills called Dang-dami and Magin. Granite is the prevailing rock. The detached, abrupt rocks and hills that shoot up in the plain are composed of limestone. The potter's earth, of which the Pegu jars are made, is found in abundance near Martaban. Schistose strata occur in the neighbourhood. About fifty miles up the Attaram river, and within a mile of its eastern bank there is a hot fountain, the temperature of which is 136°, called *Yé-bú* (hot-water) by the Burmese. The diameter is thirty feet, and the depth considerable, the discharge about 20 gallons in a minute. A strong bubbling appears in the middle. The deposit from it appears to be chiefly calcareous; it is also slightly chalybeate.

On the Sanlún river in a rich alluvial country, dwell the tribes of Khyens or Carians. They are a fine race of people, of much fairer complexions than the Peguese or Burmese, with whose deportment theirs favourably contrasts.

Various details of limestone rocky hills are given, but at Kakayet stockade close to the hills granite again begins. Several specimens of regularly crystallized quartz were picked up.

" The Khyen-Ni or Red Karians, who inhabit the jungly and hilly tract, stretching from this place in a northerly direction, are of a very savage and warlike disposition. They use thick buffalo-hide for armour, and fight with spears and poisoned arrows."

The climate of Martaban appears temperate. The following averages will give an idea of it.

	7 A. M.	4 P. M.
Fifteen days in May,	78	82
Twenty-five days in June,	72	78*
Forty-two days, 1st July to 14th Aug.	77	80

In conclusion, Captain Low observes: " From all that has been here stated, it would seem, that granite forms the basis of all the continuous ranges of hills on the coasts I have described; that a bold and marked lime (-stone) formation runs parallel to these ranges, but that this is occasionally interrupted, as far as can be judged of from an examination merely of the surface; that schist is of very frequent occurrence; and that tin in shape of an oxyd, and invariably associated with the granitic hills or formed (found?) in their vicinity," and iron, are the principal metals throughout this wide range.

VIII. Description of the North-western Coal District, stretching along the River Damoda, from the Neighbourhood of Jeria or Jeriagerh to below Sanampúr, in the Pergana of Sheargerh. By the late Mr. Jones *of Calcutta, pp.* 163 *to* 170.

IX. Examination and Analysis of some Specimens of Iron Ore from Burdwan. By H. Piddington, *Esq. pp.* 171 *to* 177.

* In the text 73°; we suppose a mistake for 78°.

X. *On a new Species of Buceros. By* H. Hodgson, *Esq. B. C. S. pp.* 178 *to* 186.

XI. On some Petrified Shells, found in the Gawilgerh Range of Hills, in April 1823. *By the late* H. W. Voysey, *Esq. H. M. 67th Foot, pp.* 187 *to* 194.

These four papers being all of them short, and on subjects possessing both variety and interest, we shall venture to reprint them in full in a future number.

XII. An Account of some Minerals collected at Nagpúr and its Vicinity, with Remarks on the Geology of that part of the Country. By Captain F. Jenkins, *pp.* 195 *to* 215.

Nagpúr, the capital of the Berar state, is in Lat. 21° 10′, Long. 79° 14′. It is situated on the bank, and nearly at the source of the insignificant stream, the Nág Nadi. The elevation is about one thousand feet above the sea. Its site is interesting in a geological point of view, being the point of junction of the great western trap, with a great granitic formation, the extent of which is yet but partially determined. Captain Jenkins considers it to be part of that found on the confines of the plains of Bengal, reaching from the Ganges below Patna to the sea at Ganjam, and a continuation of the great ranges of the coast traceable probably above ground through this vast extent.

"The hill of Sitabaldi, the extreme eastern point of the trap formation, would appear to be insulated from the range of hills to the west of it; or its connection is by a narrow neck; for the sinking of wells round the base of the hill has shown it to be nearly, if not entirely surrounded by gneiss. This gneiss is at its base much decomposed, and of a greater elevation than the next adjonning uncovered gneiss in the city; which is, however, extremely shattered, and the whole bears the appearance of having been up heaved and disturbed by the basalt." *Cancar* is abundant in the neighbouring soil.

"To the west, north west, and south west, trap entirely prevails; to the north east, east, and south east, black soil in the immediate neighbourhood prevents the observation of the underlying stratum; but there is little doubt of its being gneiss, as this is the nearest rock displayed in those directions." To the north decayed gneiss is met with, but only for a short distance; after which the same deep soil covers all vestiges of rocks till we reach the granite of Waragaon and Suradé.

"This granite of Waragnon is remarkable for the great quantity of felspar in it, from its having no mica, or a very small quantity, and the quartz being chiefly disposed in masses, and exhibiting frequently large cavities lined with fine crystals." Dolomite is found at Khorari, west of Waragaon, and about six miles north of Sitabaldi. To the west of this point is the trap range.

"Near the cantonments at Kamti is found sandstone with an argillaceous cement and disseminated spots of mica. The dip appeared to be south. The rock is much broken, and the fissures, horizontal and vertical, are filled with seams of lime (-stone) about half an inch to an inch in thickness. It is covered near the village by earthy red iron ore. Granite and gneiss have been found in digging several wells; but under such circumstances as to leave it doubtful whether they were huge boulders belonging to a bed of conglomerate known to exist, or real beds or veins of those rocks. There is a ridge of gneiss at the extreme left of the cantonment, so that the latter supposition has this circumstance to favour it.

At Silwara, north of Korari about two miles, are extensive quarries of argillaceous sandstone; the strata are very regular, though of unequal thickness, and dip to the south, at an inclination of about 30° to 35°. Between Silwara and Patan Sangi, the sandstone rises into a low hill, upon the surface of which lies conglomerate. It is also found near Sánér in the bed of the stream, and appears to cover an extensive tract beyond the pass of Kelode on the road to Sindwara.

North of Kamti on the high road to Ramtek no rock is found at the surface, but in a well about half way, and at the depth of 30 feet, a decomposed granite is seen, and boulders of quartz with schorl disseminated are found in a neighbouring nullah. The swell at Satak and Nagardan is also quartz. Close to Ramtek again gneiss begins to appear. North of Ramtek, the first range of hills is quartz rock: further on gneiss occurs, and at Dongertal, eleven miles from Ramtek, the hills are of granite. On the south and west are beds of marle affording tolerable lime.

On entering the jungle towards Kumári white disintegrated micaceous schist appears, and further on red limestone in considerable mass, having an east and west direction. Granite veins occur in it, which by the wear of the limestone are left standing up two or three inches high like protuberances. This rock passes into one, consisting almost entirely of manganese.

Gneiss, micaceous schist, and quartz rock are also found, and *cancar* in large blocks below the limestone.

Proceeding from Ramtek to the west a low range of broken hills occurs, a confused mass apparently of quartz boulders; further on a granite country is entered apparently of a crumbly nature or decomposing. Below the village of Nayakund, a dike of gneiss perfectly vertical crosses the Pésh river. As it interrupted the navigation, Captain J. was employed to cut a channel through it, so that the whole of its interior was laid open. The rock varied in its character from granite to micaceous schist, the former being apparently included masses. Round these, as round similar masses of quartz, the grain was found to be disturbed and bent from its otherwise straight direction. The adjacent rock was a grey granite, occasionally traversed by veins of a different granite producing disturbance.

Gokula is three or four miles up the Pésh from Nayakund; the river is here again dammed up by a very extensive dyke of crystallized limestone. The left bank of the river is of gneiss, the right of clay with a conglomerate of pebbles; the limestone being confined to the bed of the river, and apparently unconnected with either side. As at Kumárí, it passes into quartz rock coloured by manganese. Towards the left bank granite and gneiss were found passing into, and intermixed with, the limestone.

Tumuli occur in this neighbourhood marked by rings of stone, which were in some instances as much as 50 or 60 paces in diameter. Though examined in different places nothing was found.

From the specimens collected between Nayakund and Gokula it is inferred, that the Pesh crosses a trap country in some part of its course, or at least meets with beds or veins of those rocks. In going to Sindwara it was observed that the ascents and tops of the passes were of trap. Galena was found in small quantity in quartz rock at Parsúni.

Returning to Nagpúr by a route intermediate to the granite at Waragaon and the range of hills on the north, quartz rock arranged in vertical strata was found near Bishwamber. On the Kanhán at Matní Muhoda, a similar dike of contorted gneiss to that at Nayakund occurs. It is accompanied by cellular iron clay. The gneiss proceeds beyond Bandera, and is probably part of a great granitic formation meeting by the way of the Sanji hills and Retenpúr at Ramgerh; the great granitic range, which sweeps round by Balishwur and Cattak to the Coromandil coast. Red ochre of good quality is found near Komta under the Sánji mountains, and gold dust was procured from one of the nullahs, that fall in that direction into the Wyne-Ganga. At Leoní occurs basalt apparently connected with that at Chapara; and bordering on it at Chaori is iron clay resting on gneiss, but of limited extent. Granite then succeeds, which continues to Nagpúr. A few miles from Leoní, there is limestone.

At Sindwara about 60 or 70 miles from Nagpúr is found granite.

The gneiss of the valley of Nagpúr extends by Kelode to Lokedera, overlayed in many parts by extensive but shallow masses of puddingstone similar to that at Putansinhí and Śánér. "After ascending the last ghat, which was covered with trap,the rock met with is granite; and this I traced nearly to Baitúl; the ascending ghat to the valley of Baitúl, and last few miles only being trap. The top of the valley of Baitúl is granite; and this formation extends north nearly to Hasanabad, with some small interruptions of sandstones and trap, the bottom of the valley is trappean; part of the great trap of the west, with which it is connected by the valley of the Tapti and the Gawilgerh and Assírgerh ranges of mountains, and it is united by Multai and Pandurna with the hills of trap, whose extreme promontory in this direction east is the hill of Sítabaldi."

XII. *Notice of the Occurrence of Gypsum in the Indo-Gangetic tract of Mountains, by Captain* J. D. Herbert, *Sup. Min. Sur. pp.* 216 *to* 223.

This paper records the localities of small masses of this mineral found in the mountains which bound the Dehra Dún to the north. These are 1. The bed of a stream which leaves the hills immediately below the village of Nágal, and in the bed of which is situated the rock called Sansar Dhára, " a perpendicular bank of fifty feet in height, which for a breadth of sixty or seventy feet is faced with pendent stalactites from which descends a continual shower of drops." The gypsum occurs about two miles beyond this spot, at the confluence of another stream which comes from the left. 2. In the bed of the stream which joins the Sansar Dhára Nallah, just opposite that rock a small deposite was found. 3. On the ascent from the village of Rajpúr to the hamlet of Jari Páuí in veins in a blue limestone: and 4. On the northern face of the same range, a little above the hamlet of Ránon.

A locality is quoted from the Geological Transactions, vol. i. N. S. of gypsum found by Lieut. (now Captain) Gerard, in the bed of the Spíti, as also of anhydrite, and a specimen of the latter (a rolled piece) is mentioned in a note to have been received from the neighbourhood of the snowy peaks.

Excepting that which occurs as a vein in blue limestone, which is fibrous, the gypsum which is the subject of this paper "is of the variety called foliated granular; it is of a snow white colour, the lustre is equal or a little superior, perhaps, to that of white marble. It is scarcely translucent." The specific gravity is about 2, 24, the hardness 2,0.

With regard to its geological position and relations, it is found in superficial masses, which can hardly be called either beds or veins, apparently independent and limited in extent. It frequently contains fragments of the associated rock, occasionally it is associated with a clay slate, the crevices of which when opening on the surface had been filled up by the mineral, apparently from infiltration. More commonly the rock is of an anomalous character. It has all the aspect of a limestone, but refuses to effervesce with acids, or at least does so very feebly. It is frequently of a deep black colour, and has a fetid odour, particularly when struck or fractured. The odour is that of sulphuretted hydrogen. As it passes into well characterised limestone, it must be considered geologically as one of the numerous types of that rock, though, as it is so highly charged with argillaceous and probably siliceous matter, its claim to the title of a limestone, would not be so obvious in a hand specimen." These rocks the author of the paper inclines to consider as belonging to the transition class, but the gypsum he believes to be of comparatively modern formation, and similar in its origin to those masses of stalagmitic limestone which are found in every rock, from the oldest gneiss to the newest floetz rocks." This opinion he founds "on its limited extent, its being associated with a sulphuretted limestone, and lastly from its containing fragments of the neighbouring rock."

From the particulars given in a paper of M. Brochant's on the gypsum of the Alps, which is noticed in a postscript, it is thought additional confirmation is given to this opinion.

XIV. On the Fertilizing Principle of the Inundations of the Húgli. By H. Piddington, *Esq. pp.* 224 *to* 226.

This is an extremely interesting, and, we will add, valuable paper. We shall reserve it for republication.

XV. On the Mineral Productions of that part of the Himmalaya Mountains lying between the Satluj and the Káli (Gàgrah) Rivers, considered in an economical point of view: including an Account of the Mines and Methods of working them, with Suggestions for their Improvement. By Captain J. D. Herbert, *9th Reg. B. N. I. late Sup. Min. Survey, and Assistant to the Surveyor General of India. pp.* 227 *to* 258.

The minerals found in these mountains may be divided into those which do not, and those which do yield metal. The non-metallic minerals are as yet unproductive, and, with the exception of bitumen, are not articles of commerce. They are as follows:

1. Sulphur; 2. Green sulphate of iron; 3. Alum; 4. Bitumen; 5. Graphite; 6. Gypsum; 7. Limestone; and 8. Potstone, or indurated talc. To these of rocks, have been added granite and porphyry, and subjoined, are given a few particulars on Borax, a mineral brought from the adjoining country of Thibet.

Sulphur is found in the deposites of hot springs mechanically mixed with carbonate of lime, and in the galleries of lead mines. In the former state it occurs in the bed of the Rámgunga and Garjía rivers in Kamáun, and in the latter at Mywár, on the Tóns in Jaunsár. Green sulphate of iron is found in connection with the first noticed deposites of sulphur. It might be obtained in any quantity from the iron pyrites of the copper mines. Alum is found as an efflorescence on micaceous schist in the bed of the Cosillah below Almórah; and it is supposed, in many other places. Bitumen exudes from a limestone on the road between the cantonments of Almórah, and Lóhughát. Graphite was found in round and kidney shaped masses lying on the surface of a highly carburetted micaceous schist. The specific gravity was determined at 2,21 to 2,26. It was not of the best or finest quality, though far from bad; and the author appears to think that it may be yet found in considerable quantity. Of gypsum, the details have been anticipated in a previous paper. The limestones most worthy of attention are described as 1. A white dolomite of a fine grain approaching to compact. 2. A variety exactly answering to the description of the Iona marble. 3 Another a flesh coloured dolomite, with purple clouded delineations. These latter two are compact. 4. A marble of a more crystalline grain found on the road to Bhadrináth above the Bishunganga. Potstone is found, as in Ireland, in the formation enclosing the copper mines. Serpentine occurs within the Gurkháli territory; but none has yet been discovered on the British side of the boundary. A porphyritic grey granite is to be had in sufficient quantity close to the cantonment of Almórah; and fragments of a greenish grey porphyry, the crystals being white, have been found, but the site of the original mass is yet to be discovered.

Borax is imported from beyond the frontier, where it is extracted from a lake in which it is said to be continually reproduced. It is sold in two states, raw and picked. "The latter consists entirely of crystals varying in length from one to one-eight of an inch. These crystals are flat, hexagonal prisms with trihedral summits. They are of an oil green colour, and nearly, if not quite opaque." In the other state it is mixed either with borax dust or impurities. In the picked state it is far from pure; but the author found no difficulty in purifying it, and obtained from one solution borax equal to every purpose of the arts. The price at Bagésur, the mart where it is sold, is 5 Rs. per maund.

The metallic productions are the principal of the mineral resources, and even these appear to be scanty in amount and inferior in quality—though the author of the paper seems to think capital alone wanting to render the amount larger, and the quality superior. The metals are copper, lead, and iron; for the gold dust obtained from the sands of rivers seems scarcely worthy of notice. It is stated, that though no tax is levied, yet a man may barely earn his daily hire as a labourer by washing the sands on his own account. The total annual revenue of the copper mines is short of 3000 Rs.; and supposing the produce to be double, this is still but a small sum. It is stated, however, that this small produce is owing to the miserably contracted scale on which the mines are worked; the want of any means or system of ventilation, of drainage, of removing the ore from the mines, of pounding, washing, roasting, smelting, refining it. Of these operations many are not performed at all; and of those that are, none effectually. The want of any means of drainage must often occasion the loss of much valuable ore. The galleries are described as so narrow and low as to resemble more "the burrow of an animal than the path of a human being." The ore is removed from the mine on skins drawn along the floor of the gallery by boys in a creeping posture with a string tied round their waist. The ore is pounded by hand. It is roasted in an open forgehearth urged by air bags, and is smelted in the same. That a proper system only is wanting greatly to increase the value of these mines, is said to be apparent from the very small average value of each mine compared with those in Chili. Were only the three principal mines in these provinces to attain each of them, the average of the 500 mines in South America, i. e. 6 tons each, the amount would be ten fold what it is at present. And that there really is copper in the country, is evident; not only from the numerous indications of this metal which must attract the notice of every traveller, but from the occasional high profits realized by the lessees under extraordinary circumstances. Thus the mines of Pókri, which are now leased at 600 Rs., are said to have produced one year in the time of the Rajahs of Kumaoon 50,000 Rs. And in consequence of the occurrence of an earthquake, the lessee of the Dhanpúr mine is said to have made large profits some years ago.

The copper mines are worked at Dhanpúr, Dholrí, Gangóli, Síra, Pókri, Khari, and Shór Gurang. The first is situated in a red compact dolomite of such consistence and stability, that the galleries are much more roomy than in any of the others. Being situated at a high level, the interior is comparatively dry, so that the miners follow the ore in every direction, and, what is also a great advantage, the working season continues all the year round. The ore is of that species called grey copper (the *fahlerz* of the Germans), and contains iron, besides sulphur and copper. The latter is in the proportion of fifty per cent.

The Pókri mine is situated in a tender talcose schist, passing on one side into talcose gneiss, on the other into chloritic schist. The ore, judging from the refuse of the mine, is deemed to be *vitreous* and *purple copper*, the two most valuable of the sulphurets. The galleries had all fallen in, so that no very precise judgment could be formed. The Sira and Gangóli mines are situated in beds of indurated talc or potstone, contained in dolomite. The latter rock, and the potstone occasionally, form durable works; but the potstone is most commonly in a state much resembling reunited *debris*, and it is then little better than so much mud. The ore of each of these mines is *copper pyrites*: it is accompanied by *iron pyrites*. The other mines are similarly situated; but their produce is altogether trifling and unworthy of notice. The author recommends several improvements in the management of the mines; but for these we must refer to the paper.

The iron mines are numerous; and considerable quantities of the metal are manufactured, but by such a rude process, that the article is very inferior, and indeed scarcely fit for the construction of those tools and implements in the preparation of which may be found the chief source of the demand. The ore is attempted to be reduced in an open forge hearth, worked by a pair of air bags; the reduction is so imperfect that a great deal of labour is required by repeated heatings and hammerings to bring it into workable order. The ore is commonly the *red oxyde*, sometimes hæmatitic (Dhuneeakot on the Cosillah), sometimes in masses composed of loosely cohering glimmering scales, soiling strongly, and feeling unctuous to the touch (Rámgár); sometimes in the form of a breccia, the fragments being reunited by a calcareous incrustation, (Katsáu on the Ramgangá.) This latter variety is said to produce a good iron, probably from the adjustment by nature of the calcareous flux; for the miners do not appear to be aware of the value of such an addition, and never use it. The *yellow* or *hydrated* oxyde is found at Cháogerka both ochry and compact. The former sometimes contains crystals of *octahedral, magnetic iron ore;* and in the neighbourhood were found pieces of conglomerate consisting of grains of this mineral and of quartz, each of which was a natural magnet, and had two or more poles. At Síl in Bissahir is a mine of this ore. It is found disseminated in mica slate in such quantities as in favourable specimens to equal half the weight or one-third the bulk. The specific gravity of this ore is 4,8. The iron obtained from it is highly valued. Similar suggestions are offered for the improvement of the iron works as of the copper, but we must hasten to conclude this already too lengthy abstract.

The lead mines are numerous, but those most worked at present are situated on the Tóns river at no great distance from the Déhra Dún. There are three places where works are carried on, Aiyár, Maiyár, Boréla. The first named place is on the right bank of the river below the village of Bhutnór and within the limits of Sirmúr. The other two are on the left bank, and in Jaúnsár, a pergunnah the superintendence of which is vested in the officer commanding at Déhra. The latter two mines produced formerly 6000 Rs. they have dwindled down to 1650.

The mine at Bhutnór or Aiyár is situated in clay slate. The rock is tender and fragmentary, and the galleries though supported by timbers frequently fall in. The ore is found sometimes in quartz veins, sometimes in the slate itself. At Maiyár the containing rock is the same, but harder and more firm, so that propping is not required. The labour of excavation is however greater. At Boréla the containing rock is limestone supposed to be a bed in the clay slate. This also affords durable galleries, without further expense than that of the excavation.

The ore at all three places is the same, a steel gray *fine granular galena*, having a specific gravity of 7,2. At Maiyár it is accompanied by iron pyrites and in one gallery by sulphur. The mode of reducing the ore is the same as that practised in the case of the copper and iron ores; but it answers better in the case of this metal than of those. A curious fact is, that the ore and reduced metal sell by weight for the same price at Kálsi, the nearest mart. Perhaps the sulphur which may be collected pays the expense of reducing the ore.

XVI. Tables exhibiting a daily Register of the Tides in the River Húgli at Calcutta from 1805 *to* 1828*; with Observations on the Results thus obtained. By* James Kyd, *Esq. pp.* 259 *to* 267.

This article we shall re-print.

The volume contains maps and sections fully illustrating the geological papers. Of these the map of Bundelkhand and Bágilkhand, by Captain Franklin, is very well executed, and affords a favourable idea of the state of the arts in this country. There is no name to it, so that we cannot give credit to whom credit is due.

The two coloured plates of *Buceros Nepalensis*, executed at Mr. Smith's Press, are also very good. The last paper has five very full linear tables showing all the most interesting phenomena of the tides. Upon the whole, we think we may congratulate the Asiatic Society as well as the public, on the result of this attempt to extend the sphere of their inquiries.

GLEANINGS

IN

SCIENCE.

No. 8.—*August*, 1829.

I.—*Results of a Second Series of Experiments on the Elasticity and Transverse Strength of the Principal Indian Woods. By Captain* H. C. Baker, *Superintendent Iron Suspension Bridges.*

The speeimens ou which the following experiments were made, eonsisted, besides many of the varieties which formed the subjeet of the first series, of some novelties, of whieh the following enumeration may be given.

Lance Wood, or Menaba'n, of the provinee of Tavoy, a very tough, elastic wood, of box yellow colour; used for knife handles, spear shafts, &c.; would probably answer well for bows, buggy shafts, and poles of carriages, and turnery.

Kussum—a elose but eross grained wood of tough fibre; would probably answer well for naves and felloes of wheels, perhaps spokes; the lateral eohesion of the fibres eonsiderable; used in the hills for sledges for extricating the timbers; is produced in abundanee, and yields timbers of 26 to 32 feet average length, five or six feet mean girt; said to be unassailable by white ants and other inseets.

Kassu'mah—not very abundant, 26 to 40 feet average length, five to seven feet girt; stated to be in the hills used for "boats and beams;" this appears, like the Pussooh, a very brittle but close wood; yields suddenly; of a light red colour, would probably answer better for furuitnre than engineering purposes, in which I should say, it was decidedly unfit to be trusted; said to be proof against worms and white ants.

Pandar, of straight coarse fibre; abundant, 26 to 40 feet length, five to seven girt; a dingy yellow eolour, almost resembling the Gumbhar; used in the hills for boxes, doors and pannellings; would probably do extremely well for scaling ladders, plankiug of ammunition boxes, ship masts and spars; eombines lightuess and strength, and is said to be proof against worms.

Dho'ri, abundant, of large size, but warps much, and is said to be perishable; of a dark brown eolour.

Assa'n, abundant, 26 to 40 feet, five to seven girt; used in the hills for planks, and spokes of hackeries—in structure like oak; is regular in its defleetions, and tolerably elastic; may probably prove a very valuable wood in the gun-carriage, and powder barrel manufaetories, for framing and wheel-making, cheeks and transoms; said to be proof against inseets.

Pa'njar, tolerably abundant, attains eousiderable growth; said to be employed for boxes, tables, planking, and ploughs; a very dense, tough, and elastie wood, but rather knotty; would answer admirably for either struts or ties where gravity is not of mueh consequence. For poles of carriages, buggy shafts, naves, felloes and spokes of wheels, rather than for planking or pannelling, I should think it suitable; its eolour, a dingy yellow like the Paundur.

Jammuah, very abundant, and attains large size; lightish red colour; used in boat building, and said to be very durable in fresh water; tough in fibre, but eross grained; not subjeet to the ravages of worms or white ants.

Seriss, very abuudant, 20 to 29 feet long, seven to eight in girt; a good deal resembles Sissooh, both in structure and colour; used only in making wheels in the hills; not subject to the ravages of worms or white ants.

Karmai'n, very abundant and of large growth; a bright opaque yellow colour; used in the hills, in making furniture of all kinds, boxes and light pannelling or picture frames, modelling blinds and venetians, organ pipes and wainscotting; it will probably prove very valuable, but does not appear calculated for heavy stress of any kind; unassailable, it is said, by white ants, &c.

Dha'bah, abundant, yields timbers of 25 to 30 feet, five to six feet girt; a very strong and elastic but heavy wood, a good deal like the Pánjar, and may be applied to the same uses.

Bha'nji, a dirty brownish colour; abundant; little used and not much known; may answer for felloes of light wheels.

Teko'li, abundant; yields timber 25 to 30 feet long, five to six feet girt; opaque reddish yellow, used for light furniture, boats, and doors; has little strength; may probably answer well for picture frames, and wainscotting, and panelling.

Thengan or Sengan, produced in the new eastern provinces, and yields timber from 30 to 45 feet, five to six feet girt; used for the same purposes as Teak, than which it is considered more durable; used for boats, by the Chinese for junks; colour, a reddish saul; a strong and elastic wood, admirably adopted for the gun-carriage department.

Thaba'n, dull, straw colour, used in house building and boats; produces timbers of 35 sometimes as long as 70 feet, a good wood for planking and pannelling.

Pyeng-ma or kha-mung-ni—of pale cedar colour; furnishes good crooked timbers for ship building, for which purpose it is exported from Rangún to Bengal, supposed by some to be of similar species to red Jarrúl, and well calculated for guncarriage purposes.

Kad-wot, pale saul colour, yields very large timber for masts and keel pieces 46 to 73 feet, of light structure.

Kanza karro, a heavy but durable wood, something like the Pyeng-ma both in colour and structure; used for house building purposes, and grows to a large size.

Ana', a heavy but certainly not a tough or elastic wood; said to be much prized, for its durability, amongst the Burmese, and used in constructing royal buildings, religious edifices &c.; of the Jack wood genus.

Tha-gat-ni or za-gat-ni, is a heavy durable wood, cedar red, chiefly applied to house building purposes; yields timber of about 50 feet maximum length.

Saul Wood, from the Baggri jungle, north of Mednipúr, dwarf tree, of a close even structure, finer in appearance even than the Morung timber, and nearly as strong.

Peer Saul, a light kind of Sissúh, produced in the Baggri jungle, north of Mednipúr.

The specimens were seven feet long, two inches square, and the distance betweeu the supports six feet.

The numerical values of C. (cohesion) in the second column of the Table, denote the direct cohesive strength in pounds avoirdupois of one square inch sectional area of the several woods. They are the mean of four to six results.

TABLE III.

Number of the experiments.	Names of the woods.	Specific gravity.	Elasticity perfect.		Breaking weight in lbs.	Ultimate deflection in inches.	Value of U $=\frac{l^2}{d\Delta}$	Value of E $=\frac{l^3 W'}{ad\ \delta}$	Value of S $=\frac{lW}{4ad^2}$
			Weight in lbs.	Deflection in inches.					
361	Old Saul Beam. Specimens 2 inches square, 6 feet bearing distance.	1058	450	1.05	1161	3.9			
362		1000	450	1.15	993	3.6			
363		1000	450	1.1	969	5.0			
365		888	450	1.35	911	3.65			
366		944	450	1.25	809	3.9			
367		894	450	1.575	861	4.1			
368		1055	450	1.35	913	4.0			
369		1000	450	1.175	1076	4.3			
370		1000	450	1.45	913	3.7			
	Mean results	982	450	1.27	956	4.0	648	8398080	2151

Number of the experiments.	Names of the woods.	Specific gravity.	Elasticity perfect. Weight in lbs.	Elasticity perfect. Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U, or $\frac{l^2}{d\Delta}$	Value of E or $\frac{l^3W'}{ad^3\delta}$	Value of S, or $\frac{lW}{4ad^2}$
371	Philibeat SAUL	1000	450	1.2	902	4.7			
372	Chowker.	1000	450	1.1	975	5.0			
373	C=lbs. 20,231.	1000	450	1.2	954	4.6			
374		1000	450	1.2	904	4.3			
375		1000	450	1.2	852	3.8			
376		100	450	1.125	95[illegible]	4.4			
	Mean results	1000	450	1.2	924	4.4	589	8748000	2075
377	Seasoned SOONDREE.	1000	500	1.1	1159	5.3			
378		1105	500	1.1	978	3.8			
379	C=lbs. 29,076.	1117	500	.9	1263	4.0			
380		111[illegible]	500	1.2	1298	6.5			
381		105[illegible]	500	1.3	1107	4.6			
382		1105	500	1.1	1264	4.9			
	Mean results	1099	500	1.1	1178	4.8	540	10600000	2650
390	TEAK beam.	684	300	.825	967	3.4			
391	Very old, from	684	300	1.125	79[illegible]	3.6			
392	Dutch house at	647	300	.9	917	4.2			
393	Chinsurah.	631	300	1.025	704	2.5			
394		666	300	.85	913	3.2			
395		666	300	1.1	756	2.9			
	Mean results	663	300	.80	841	3.3	7[illegible]5	8748000	1892
396	Seasoned TEAK.	764	300	.95	966	4.25			
397	From Cossipore,	777	300	1.1	940	4.5			
398	half wrought, C=	722	300	1.0	757	5.6			
399	lbs 15,861.	789	300	·7	1228	5.0			
400		684	300	.95	987	4.3			
401		722	300	1.1	966	25.			
	Mean results	743	300	.96	974	4.8	540	7290000	2191
402	SISSOO.	777	300	1.275	908	5.5			
403	Seasoned, Cossi-	833	300	.85	1088	4.3			
404	pore, C=lbs.	789	300	1.0	1018	4.5			
405	12,385.	833	300	1.075	1035	4.8			
406		833	300	.9	1041	3.6			
407		833	300	.9	966	4.35			
	Mean results	816	300	1.0	1010	4.5	576	6373400	2272
408	GOMAR OR GAMBHAR.	805	150	.925	454	8.0			
409		813	150	1.0	406	6.0			
410	Morung fresh from	793	150	1.075	448	7.0			
411	Jynughur Timber Agency, 1828, C= lbs. 7008.	796	150	1.025	418	6.0			
	Mean results	802	150	1.0	432	6.7	386	3186700	972

Number of the experiments.	Names of the woods.	Specific gravity.	Elasticity perfect.		Breaking weight in lbs.	Ultimate deflection in inches.	Value of U, or $\frac{l^2}{d\Delta}$	Value of E, or $\frac{l^3 W'}{ad^3 \delta}$	Value of S, or $\frac{lW}{4ad^2}$
			Weight in lbs.	Deflection in inches.					
412	LANCE WOOD OR	863	200	.6	520	1.25			
413	MAINA-BAUN.	900	250	.55	978	3.85			
414	Specimen	873	250	.575	884	4.0			
415	66 inches long,	869	250	.55	1039	4.2			
	60 ,, bearing,								
	2×1½ inches.								
	C=lbs. 28,737.								
	Mean results	876	250	.568	855	3.325	540	7922500	2137
416	KOOSSUM.	1098	400	1.1	756	2.35			
417	Fresh from Jynu-	1102	400	1.1	652	6.8			
418	ghur Timber Agen-	1073	450	1.1	1123	4.5			
419	cy, September 1828.	1064	450	1.275	768	2.75			
	C=lbs. 18,121.								
	Mean results	1084	240	1.14	825	4.1	632	9208000	1856
420	KUSSOOMAH.	1108	250	.95	861	4.2			
421	Fresh from Jynu-	1075	250	.975	809	3.8			
422	ghur Timber Agen-	1084	250	1.075	809	4.6			
423	cy, September 1828	1098	250	1.1	756	4.1			
	C=lbs. 14,790.								
	Mean results	1091	250	1.0	809	4.2	617	5832000	1820
425	PAUNDUR.	764	250	.95	835	4.0			
426	Fresh from Jynu-	838	250	.85	756	2.9			
427	ghur Timber Agen-	739	250	.85	809	4.5			
428	cy, September 1828	742	250	.95	704	4.8			
	C=lbs. 15,564.								
	Mean results	771	250	.9	776	4.0	648	6480000	1746
383	DHOREE.	716	200	.725	704	6.0			
384	Fresh from Jynu-	706	200	.625	650	3.6			
385	ghur Timber Agen-	735	200	.425	661	3.2			
386	cy, September 1828	904	200	.95	585	4.2			
387	C=lbs. 13,694.	700	200	.875	672	4.6			
388		700	200	.775	695	4.2			
	Mean results	744	200	.73	661	4.3	602	6391000	1487
429	ASSAUN.	946	300	.875	844	5.4			
430	Fresh from Jynu-	930	300	.875	844	5.4			
431	ghur Timber Agen-	1000	300	.875	913	7.0			
432	cy, September 1828	1069	300	.875	1012	6.1			
	C=lbs. 17,690.								
	Mean results	986	300	.875	903	6.0	432	7997000	2032

Number of the experiments.	Names of the woods.	Specific gravity.	Elasticity perfect. Weight in lbs.	Elasticity perfect. Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in inches.	Value of U, or $\frac{l^2}{d\Delta}$	Value of E, or $\frac{l^3 W'}{ad^3\delta}$	Value of S, or $\frac{l W}{4ad^2}$
433	PANJUR.	1069	450	1.1	1216	7.0			
434	Fresh from Jynu-	1102	450	1.1	1159	4.1			
435	ghur Timber Agen-	1069	450	1.125	1097	6.0			
436	cy, September 1828	1081	450	1.175	1056	5.4			
	C=lbs. 29,864.								
	Mean results	1080	450	1.1	1132	5.6	462	9543000	2547
437	JUMMOOAH.	1136	250	.975	809	4.2			
438	Fresh from Jynu-	1015	250	.95	861	4.4			
439	ghur Timber Agen-	1129	250	.85	913	6.5			
440	cy, September 1828	1029	250	.9	756	5.0			
	C=lbs. 13,947.								
	Mean results	1077	250	.92	835	5.0	518	6326000	1878
	SERISS.								
441	Fresh from Jynu-	828	250	.95	675	3.3			
442	ghur Timber Agen-	932	250	.9	705	4.3			
443	cy, September 1828	849	250	1.075	811	4.3			
444	C=lbs. 19,227.	932	250	.9	648	3.8			
	Mean results	885	250	.956	709	3.9	664	6103000	1570
445	KURMINE.	656	300	1.1	855	4.5			
446	Fresh from Jynu-	706	300	1.175	901	4.7			
447	ghur Timber Agen-	743	300	1.075	906	4.2			
448	cy, September 1828	695	300	1.175	813	3.8			
	C=lbs. 14,115								
	Mean results	700	300	1.13	869	4.3	602	6220800	1955
	DHAUBAH.								
449	Fresh from Jynu-	1060	450	1.0	1227	5.0			
450	ghur Timber Agen-	1000	450	1.2	1018	3.8			
451	cy, September 1828	1031	450	1.125	1141	4.1			
452	C=lbs. 20,689.	1047	454	1.175	1124	5.5			
	Mean results	1009	450	1.1	1130	4.6	563	8994200	2542
453	BHAUNJEE.	928	250	1.075	720	4.3			
454	Fresh from Jynu-	803	250	.95	709	5.0			
455	ghur Timber Agen-	921	250	1.125	540	3.6			
456	cy, September 1828	910	250	1.0	761	4.2			
	C=lb. 13,372.								
	Mean results	890	250	1.0	682	4.3	602	5832000	1534
457	TEKOLEE.	736	200	1.1	604	4.6			
458	Fresh from Jynu-	762	200	.95	706	5.1			
459	ghur Timber Agen-	719	200	1.0	648	3.5			
460	cy, September 1828	800	200	1.075	602	4.8			
	C=lbs. 10,081.								
	Mean results	754	200	1.03	640	4.5	576	4665600	1440

Number of the experiments.	Names of the woods.	Specific gravity.	Elasticity perfect. Weight in lbs.	Elasticity perfect. Deflection in inches.	Breaking weight in lbs.	Ultimate deflection in lbs.	Value of U, or $\frac{l^2}{d\Delta}$	Value of E, or $\frac{l^3W'}{ad^3\delta}$	Value of S, or $\frac{lW}{4ad^2}$
424	Morung Saul		450	1.225	873	3.2			
461	Chowker.		450	1.225	1263	4.8			
462	C=21,346.		450	1.125	1239	5.3			
463			450	1.3	1065	3.3			
464			450	1.175	1082	3.5			
465			450	1.25	1111	4.3			
	Mean results	1000	450	1.2	1105	4.1	632	8748000	2486
494	Saul Wood.	12. 11	400	1.15	1166	5.0			
495	Buggree Jungle,	12. 9	400	1.15	1090	5.0			
496	fresh, received No-	12. 1[illegible]	400	1.25	1090	4.2			
497	vember 1828.	12. 6	400	1.15	1039	4.2			
498	C=22,320. lbs.	12. 1[illegible]	400	1.25	1132	5.6			
499		13. [illegible]	400	1.2	106[illegible]	6.0			
500		12. 2	400	1.1	1068	5.1			
	Mean results		400	1.1	1084	5.0	518	8482910	2439
501	Peer Saul.	10. 10	350	1.3	908	4.3			
502	Buggree Jungle (a	10. 2	350	1.7	1006	4.2			
503	species of Sissoo.)	10. 14	350	1.5	925	5.1			
504	C=lbs. 15,681.	10.	350	1.35	826	5.4			
	Mean results		350	1.2	916	4.75	545	6804000	2161
470	Thengan.	9. 11	450	1.15	1146	5.8			
471	Fresh from Tovoy,	9.	450	1.2	1065	5.4			
472	1828, per Ernaad.	9. 4	450	1.25	1144	6.0			
473	C=lbs. 19,720.	11. 8	450	1.3	1120	3.7			
	Mean results		450	1.225	1118	5.2	498	8569460	2515
474	Thabaun.	8. 6	200	.625	745	3.0			
475	Fresh from Tavoy,	8. 14	250	.75	948	5.2			
476	1828, per Ernaad.	8. 6	200	.625	750	2.7			
477	C=lbs. 12,671.	8. 6	250	.75	1039	5.8			
	Mean results		225	.68	870	4.2	617	7718890	1957
478	Pyeng-ma.	9. 2	300	.925	978	5.5			
479	Fresh imported	9. 0	300	1.075	890	3.5			
480	from Tavoy coast,	9. 2	300	1.1	913	4.725			
481	1828, per Ernaad. C=lbs. 15,132.	9. 2	300	1.225	815	5.5			
	Mean results		300	1.8	899	4.8	540	6794000	2023
482	Kud wor.	10. 6	300	.8	908	4.2			
483	Fresh from Tavoy,	10. 4	300	.775	972	3.5			
484	1828, per Ernaad.	10. 2	300	.775	931	3.3			
485	C=lbs. 21,920.	10. 2	300	.725	1001	3.3			
	Mean results		300	.75	953	3.6	720	9031000	2144

Number of the experiments.	Names of the woods	Specific gravity.	Elasticity perfect.		Breaking weight in lbs.	Ultimate deflection in inches.	Value of U, or $\frac{l^2}{d\Delta}$	Value of E, or $\frac{l^3 W'}{ad^3 \delta}$	Value of S, or $\frac{lW}{4ad^2}$
			Weight in lbs.	Deflection in inches.					
486	KANZA-KURRO.	9. 6	300	.875	937	3.4			
487	Fresh imported	8 14	250	.725	786	3.0			
488	1828, per Ernaad.	9. 2	350	.8	1105	4.4			
489	C=lbs. 18,111.	9.. 2	350	.9	1175	3.2			
	Mean results		312	.825	1000	3.5	740	8806300	2250
490	ANAN.	13. 6	150	.4	745	2.0			
491	Fresh imported	13 12	150	.475	600	1.8			
492	from Tavoy coast,	13. 12	150	.4	820	2.0			
493	1828, per Ernaad. C=lbs. 19,763.	13 12	150	.375	786	3.3			
	Mean results		150	.41	740	2.3	1120	8534000	1665
509	THAGAT-NI.	10. 2	450	.95	1181	4.7			
510	Fresh imported	10. 0	450	.875	1106	4.8			
511	from Tavoy coast,	10. 2	450	.9	1016	4.4			
512	1828, per Ernaad. C=lbs. 23,506.	10. 0	450	.9	1027	3.3			
	Mean results		450	.9	1082	4.3	602	11664000	2434
505	English OAK.	8. 12	200	.9	652	3.8			
506	Old.	8. 8	200	.95	585	3.3			
507	C=lbs. 12,315.	9. 4	200	.925	579	3.3			
508		8. 14	200	.925	602	3.3			
	Mean results		200	.925	604	3.3	785	5043890	1539

II.—*On the Measure of Labour in India.*

To the Editor of Gleanings in Science.

SIR,

A correspondent, Ω, in the 4th Number of your Journal, has described a method of baling water very much practised in India, and so simple as to be well entitled to the name of primitive. He does not however allow it to be an effectual or profitable mode of raising water, nor to bear any competition with the more complicated means of the Europeans.

The result of his estimate is indeed very disadvantageous, when he compares "six average Bengallees with one feeble old Englishman:" but I hope to convince him from the data furnished by himself that this comparison is fallacious.

The amount of any force exerted, or of any work performed, is most conveniently measured and expressed by the weight equivalent to the resistance overcome, multiplied by the velocity, and by the time during which the action continues; or, assuming the time to be a day, the work may be designated by the weight raised to a given height; in which form as only one term is variable, the comparative results of different kinds of labour, can be readily classed and appreciated.

There is a marked difference, however, necessary in calculating the working effect of any self-acting machinery, and of animal or human labour: which is, that in the latter, the element of *fatigue* must be taken into consideration, and such intervals of rest must be allowed, as shall enable the same exertion to be renewed from day to day. It is thus that from experiments of short duration very erroneous measures of labour have frequently been made. Even the celebrated Daniel Bernouilli estimated the daily work of a man at nearly double what is found by Coulomb from practical inquiries amidst the ordinary kind of labour. Bernouilli's value is equivalent to 2,41,433 troy pounds, raised 10 feet high per diem*.

The last mentioned philosopher also imagines that human labour always produces the same quantity of action, in whatever way it may be employed; or in other words that a certain quantity of work induces a certain degree of fatigue, without regard to the force, the velocity, or the time. But in reality there are a thousand circumstances capable of modifying this general axiom—the weight of the body, the position and relative force of the different muscles, and the rate of motion best adapted to each; so that although the daily fatigue of various operations may be the same, the effect prodnced will hardly in any two cases be found alike; and further, in each case there must be a particular adaptation of the three elements of force, velocity, and duration to produce a maximum effect.

Thus from the constitution of man, it might naturally be inferred, as has been proved by experiment, that he can convey the weight of his own body to a far greater height or distance in proportion, than he can the weight of his body *plus* any burden:—but as the quantum of burden only will be the useful effect produced, there must be a certain ratio of the burden to the weight of the man which will give a maximum. Coulomb makes the largest burden equal to 4-5ths of the man, and with this, the work done (including his own weight) is only one-half what it is when he is unloaded; and the *useful* effect is only one quarter as much. I am now quoting from a small table of human labour, which I have deduced from Coulomb's treatise, " Sur le Force des Hommes ;" and as this table will assist us in judging of the comparative labour of Indians, which is the object of our inquiry, I shall here introduce it.

Coulomb, it must be premised, speaks of the strong healthy Parisian labourer, whose average weight is stated to be 187,6 lbs. troy, or about 11 stone.

I have added a column of maunds, each maund being equal to 100lbs. troy:—which would be the precise value, supposing the seer to consist of 80 Calcutta sicca weight or Furukhabad rupee weight.

Table of Human Labour.

Nature of work.	Troy pounds raised 10 feet high per diem.	Maunds raised 10 feet high per diem.
Mounting stairs, bearing only the weight of the body, (lbs. 187. 8)	180103	1800
Ditto, with a burden of 150lbs,	95815	960
Ditto, work done, deducting man's weight,	49226	490
Ringing, or hoisting a pile driver with a jerk string,	66092	660
The same motion applied to the stampers at the Paris mint,	34538	345
Hoisting water from a well in buckets,	62877	630
Turning a wheel and pinion or winch,	101968	1000
Digging the ground,	85017	850

Another species of labour consists in carrying a load along a plain surface, of which Coulomb gives the following valuation.

	Troy lbs. carried 100 feet.	Maunds carried 100 feet.
Walking on plain ground without load,	307664	3080
Ditto with *maximum* load 150 lbs,	175808	1760
Quantity of work done exclusive of man's own weight,	60827	610
Wheeling in barrows (work done,)	89898	900

* The elementary horse power of a steam engine is equal to 15,84,000 lbs. averdupois raised 10 feet per diem, or 550 lbs. raised 1 feet per second.—The horse is suppo.ed to equal about 8 men, Smeaton makes the power of a man = 225,000 lbs. averdupois per diem.

Coulomb remarks at the conclusion of his essay, that at Martinique, with the same labourers, the work done was not equal to one-half, from the heat of the climate; he does not however give any data of the labour of negroes or others habituated to the heat. In this country the labour of natives must, I should think, far exceed what could be supported by Europeans, although, from size and the nature of their food, they may be inferior in physical strength.

But this is the point we are about to examine; and first let us commence with the example presented by your correspondent Ω.

It must be remembered, that baling is only adapted to the raising of water 3 or 3½ feet high: every additional height weakens the power of the arms, causes spillage, diminishes the number of strokes, and in short cuts off from the established maximum of work. Let us, therefore, limit the calculation to the first step of his example, viz. two men lifting 10 seers of water, 3 feet high, 25 times per minute, for a continuance of 8 hours.

These data yield a produce of 900 maunds, raised 10 feet high per diem, or 450 maunds for each man; which is equal to two-thirds of the water raised by a Parisian labourer; an amount of which no Bengallee need be ashamed! and sufficient to prove the advantage of their simple and cheap mode of raising water where there is ample space at command.

Ω must no doubt have been misled by the data of Professor Robison's pump; for if they are correctly stated, the feeble old man raised 3000 maunds a day, and far exceeded all that the strongest *Frenchmen* have ever effected. To this experiment, a remark I have before made will apply; in fact no inference can be drawn from such data.

Having lately myself been employed in clearing water under circumstances which required constant exertion, I took the trouble of estimating the work done by the several engines employed, which consisted of one square wooden pump, 8¾ inch bore, hastily constructed, two circular wooden pumps of 7 inch bore, seven *mots* or water bags drawn by men;—and *cauriána*, or earthen pots filled and carried away by women and children at so much a pot; besides several *dúris*, or baling baskets.

Sitting for hours on the spot with a watch in my hand, I could form a correct estimate of the work of the pumps from the length and number of strokes; and I further checked it by measuring the water raised in a given time: the same was done for the *mots*: and the *dúris* were estimated by the pace they kept in clearing away the water from the reservoir at the pump heads.

1. The square pump raising 11½ feet, required two day sets of eight men each, and two night sets of the same number. The water raised per minute was

by weight	1659 lbs.
by measure of pump	1500
say	1600

which is equal to 13000 maunds, raised 10 feet in a day of 12 hours, by 16 men, or 810 maunds per man.

2. The two round pumps raising 15 feet, with the same number of men each, delivered 8532 maunds, or for a height of 10 feet 12800 maunds, = 800 maunds per man.

3. The *mots* were furnished with 8 men, each relieved every 12 hours. With a height of 14 feet they made but two deliveries of 150 lbs. in a minute; and much water was occasionally lost:—when urged, they could draw thrice in a minute, but the average was less than twice. The work done therefore per diem was 2983 maunds 10 feet high, or 373 maunds per man.

4. Two *dúris* of 2 men each, with a relief, or 8 men in all per diem, could barely keep pace with the square pump, raising the water 3½ feet high, which is equivalent to 4000 maunds, raised 10 feet per diem, or 500 maunds per man.

A result nearly equal to that of your correspondent; but perhaps all of the above values are too high, for the men were stimulated, by the necessity of keeping the water down, as well as by my presence, to make their best exertions.

That the labour at the pumps was more severe than at the others, was proved by the preference given to the *poorwut* by the labourers:—a large portion of labour is thrown away in the latter by the delay in filling the bag:—but even in giving three reliefs to the pump there would be still a great excess of work done in proportion, and in the present instance, until they were made, the water could not be kept down at all. I did not myself anticipate such a saving of labour from the adoption of the pump: men are, however, not well calculated for pulling horizontally as in a *poor-*

wut, and it must be more advantageous to employ cattle.—It would be useful to compare the application of the latter to different modes of raising water from our wells; but I have no data whereon to argue.

5. In building it is common in India for women and children to carry the materials to the roof for the bricklayers, an average of such labour gives me the following data :—400 bricks raised 18 feet, by stairs, in 60 journeys—10 bricks to a maund, women receiving 1 anna per diem : their labour therefore is = 72 maunds raised 10 feet.

6. Having fitted up a windlass for the same purpose, I obtained, on an average of 15 days' work, 60 measures of three maunds each, raised 40 feet by two men per diem = 360 maunds, raised 10 feet.

With regard to labour in carrying burthens on a plain surface, it is not difficult to procure a few examples in this country.

7. The runners and walkers of India are notorious for the journeys which they can make. Grooms have been known to travel as far as 80 miles without resting; eight bearers will frequently carry a loaded *palkee* 30 miles : but such work could not be continued. Letter carriers and especially dâk bearers will continue to carry a *palkee* weighing 400 lbs. 10 miles a day with 8 men. This is equal to 264 maunds, carried 100 feet per man, without reckoning their own weight, or the return journey: and it is nearly half the work of a stout porter in Paris.

8. There is in many of the towns in upper India a class of porters called *palladars*, who carry grain from the river side to the different *ganjes*. They are esteemed the strongest men, and obtain high rates of hire. Their load is from 1¾ to 2 maunds, which they carry three times a day, to the distance of 1¾ miles: this is = 554 maunds carried 100 feet, and as nearly as much as a European would convey to the same distance.

9. The common daily practice of a set of 8 *pesrajes* is to convey 7 stones each, weighing about 16 maunds, to the distance of 800 yards, including bringing them up the bank of the river 80 feet high : This labour may be divided into two portions

336 mds. carried 100 feet } per man,
+ 112 mds. raised 10 feet }

and it is about equal to the last example.

We see, then, that the work of such native labourers as can earn two or three annas a day, bears a very fair comparison with that of Europeans whose daily wages may be twelve times as great. It is commonly the custom in this country to make use of a host of women and children as coolies, but expt. 7 shows this to be disadvantageous, even although their wages are less than half;—and this result was confirmed by the *caúriana* experiment of raising water, which was twice as costly as the *poorwut* method.

I should like much to be able to add to the foregoing catalogue some measurements of the daily work of that laborious and active class, the boatmen of the Ganges, who toil for 14 hours without intermission under a grilling sun, along an incommodious trackway, through long grass, or wading in currents and quicksands, dragging a heavy boat 10 or 15 miles a day; and sometimes with light craft averaging more than 20 miles. But it would be impossible to come to any accurate determination without the employment of a dynamometer. I must rest satisfied therefore with pointing out to those travellers on the Ganges who interest themselves in scientific inquiries, how easily they may measure the force exerted in tracking a boat or the resistance of the current, by passing the *gun*, or tracking rope over a pulley at the mast head, and attaching a counterbalancing weight to the lower end of it:—where the draught is very variable, a succession of weights may be applied, one under the other, so as to be lifted from the deck according to the strain. It may be rougbly estimated that about 10 seers per man will be required.

I am, Sir,

Yours obediently,

P.

Postscript.

In No. 5. of the Gleanings, which has just reached me, I perceive an article "On the Expediency of introducing Machinery into India." The writer holds out the most prosperous and encouraging hopes to all who would engage in the establishment of manufactories after the English fashion in this country. I know not why the human mind seems to be particularly given to be led astray by any delusion which is vailed in the seducing garb of beautiful and complex machinery, and to attribute to mechanism such *ad-libitum* power as to surpass all fears of competition or failure. I am not about to stigmatize the golden prospects of H. D. E. as illusory; but when he promises a replacement of capital in three years, together with a current income of 8 or 10 per cent. thereon, I cannot but think him more sanguine than safe. Neither am I by any means an enemy to the projects he advocates; for general good must be effected in the country by every improvement of machinery or of manufacturing processes, even though the individual speculators may themselves suffer.

It is the economy only which I venture to doubt, and this essential point can only be discovered by a relative comparison of the wages of labour at home and in India,—of the price of coals,—of the additional cost and keep of machinery here,—and of the rate of the interest of money. Now without entering into any detailed statement, (for which, in fact, I am not prepared,) it is evident, that the reply to all these points will be unfavourable to the application of steam power in India: and added to this, we must bear in mind what the estimated advantage of steam as a motive power is in England.

"The prime cost of an engine is about equal to that of the number of horses whose place it will supply;—the price of its feed is nearly one half."

All other things then being equal, if the feed of animals or men, or, which is nearly the same, if wages here are less than half what they may be in England, the advantage of the steam engine ceases, as far as economy is concerned; there are still advantages of concentration of force and equability of action to tell in its favour, which are no doubt of vast importance in some operations, and in such the use of steam should have prior introduction: there are also circumstances in which no other power can be applied, as in towing vessels through a dangerous navigation; here the competition of human labour does not exist, and as long as the navigation and the dangers last, the project of steamers will be profitable. But if we turn our view to the operations on land which we have seen, tried, and some of which have continued to brave the hazards of a losing speculation, I doubt whether we shall find much to contradict the deductions above made. I will appeal to the lottery committee whether they could not raise the same quantity of water more cheaply (I do not say so conveniently or so elegantly) by manual labour than by their engine? I will ask the ship-builders why we no longer see in their dock-yards the elegant steam block machinery of 1819? I will ask the merchants and speculators why it was never thought worth while to set up the splendid ice apparatus which came to India a few years since? I will inquire whether the application of steam to the irrigation of indigo lands has rapidly increased? And as to the magnificent Government manufactory to which H. D. E. alludes in terms of just praise, I will venture to prophesy that, however the fabrication of the article may be improved and perfected thereby, the cost of its manufacture will be more than doubled. I have excluded the steam boats employed among the shipping from the list of unfavourable speculations, but I cannot even extend this privilege to steamboats intended for inland navigation; here again occurs the competition of manual labour under circumstances of simple action most favourable to the latter, and further the prevalence of regular winds affords an additional element in its favour. Say that the velocity of a journey up the river may be doubled or tripled by the use of steam, still the difference of expense is so great, that pinnaces and budgerows may double their complement of *dandees*, may establish relays of towmen on the river side, so as to increase their speed nearly to a par with the steamer; and they will still eclipse the latter as much in cheapness, as in the very essential points of comfort and accommodation.

In fine the advantage or disadvantage of introducing steam power must entirely depend upon the price of labour, and it should never be held out of sight that in England and North America, where steam is so successfully employed, labour is more costly than in any other country, and coal and iron are in the opposite degree cheapest and almost inexhaustible.

Should these observations appear more than justly discouraging to those who have embarked in new manufacturing speculations in India, I beg in conclusion to assure them, that all I have advanced is founded merely on general considerations, without the practical knowledge of any experiment on the subject; and I shall be most happy to be set right by the faithful *exposè* of any concern which has been long enough at work to have formed a just estimate of its capabilities.

III.—*On Cooling Wines.*

Cool wine in a climate like that of India being so great a luxury, we cannot doubt that the re-publication of the following letters by Dr. Walker of Oxford, on the artificial production of cold, as taken from the June and July numbers of the Philosophical Magazine, for 1828, will be generally acceptable.

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

It is now forty-one years since my discoveries on the "artificial production of cold," were first made public by their appearance in the Philosophical Transactions for the year 1787, and several succeeding volumes. Passing over what has already been published respecting them, I shall proceed to a detail of a few other circumstances as a kind of appendix, which I have for several seasons intended to offer for publication, had not other matters, as professional avocations and professional communications, too much engaged my attention to allow of it.

Immediately on the announcement of the discoveries, as above stated, I received various proposals from respectable persons respecting their practical utility in this country. I answered these by a declaration that wherever natural ice could be obtained and preserved, this must ever supersede the use of the artificial means alluded to. It is true, that I had an eye to their application in hot climates, as between the tropics; and so soon as my experiments became public, a treatise on the diseases of tropical climates appeared from the pen of Dr. Mosely, who fixed upon one, which he considered the most appropriate, and strongly recommended its adoption as a very valuable acquisition, as well in a medicinal point of view, as a luxury.

Relinquishing, from various causes, the design of applying them myself to any such purpose, I took care, however, to point out, in my original communications, the complete efficiency of them for such intention to their utmost extent, and the best mode, as it appeared to me, of applying them in hot climates.

Understanding a few summers ago, that a manufactory had been established for preparing ice creams, as well without the use of ice, as with it, and likewise for making for sale an apparatus for the purpose,—I was induced to visit it. I examined the apparatus,—a very appropriate one for the purpose, and likewise the freezing powder, which I instantly recognised to be the weakest in power of my various compositions for the purpose, but possessing the advantage of being readily recovered repeatedly for the same purpose with undiminished effect. This powder, by its taste and appearance, I found to be a mixture of sal ammoniac and nitre, which I was informed was repeatedly recoverable in a fit state for refrigeration. I originally exerted every effort, in vain, to increase its power by the addition of a third ingredient, possessing likewise the advantage, merely by evaporation to dryness, of being repeatedly recovered for the same use. This powder, as related in my original communications, consists of equal parts by weight of sal ammoniac and nitre. By way of test, I recovered it by evaporation twelve times, without any abatement of its efficacy, as originally stated.

It is unnecessary to enter into a description of the apparatus just mentioned, or the principle and mode of its application, especially as the whole is embraced in the following statement. A circumstance occurred here (at Oxford) which occasioned the method to be put to the test of useful application. A confectioner, happening in a scarce season to be unprovided with natural ice, applied to me for assistance. I assured him that in the large way (as I have stated in my original communications) the best method was to freeze water first, and then to use the ice in the usual way for freezing creams. Accordingly an apparatus of large dimensions, of rather an oblong form, was made of tin, (fitter for the purpose if cased with wood,) consisting of channels so constructed that the water to be frozen should be subjected to the freezing mixture on both sides. This, properly prepared, was placed in a cool cellar during the night, and early in the morning (the temperature in the open air in the shade in the day time being above 80°) the ice was collected, which amounted to several pounds in weight. This ice, which was as limpid as the finest flint glass, was applied in the usual way, and with the apparatus ordinarily used by confectioners for the purpose of freezing creams; and the mixed powder, of which he had procured an adequate quantity, repeatedly recovered by evaporation over his hot iron plates, for fresh use.

I shall now present the immediate object of my present communication; viz. what I consider to be the best mode and fittest apparatus for cooling wine in summer, for freezing creams in the small way for private use, and likewise for freezing a small portion of water, merely as an experiment for public or private exhibition.

The drawing annexed* is designed to represent on a small scale the coustruction and exact proportions of each freezing apparatus, and likewise the construction and form of the apparatus for cooling wine.

Fig. 1. is an apparatus for freezing water on the smallest scale, as above mentioned, in the hottest weather. The vessel for containing the freezing mixture is three inches and a half in width, and its height equal in measure to its width; and the tube for containing the water to be frozen five-eighths of an inch in width, and reaching, as represented, very near to the bottom of the vessel: there is likewise a rim or continuation of the vessel, without a bottom, to insulate it from the table or stand it rests upon. The apparatus itself consists of two parts; viz. the vessel for containing the freezing mixture, and its cover, in one piece with the tube, fitting close over it (represented together in the drawing.) When the water is frozen, upon taking off the cover and wiping the tube, the solid ice will have become detached by the heat, and on inverting drop out.

The process may be known to be completed by the going off or melting of the hoar-frost, which exhibits a curious appearance outside the apparatus.

Fig. 2. Consists of an apparatus in one piece; viz. the vessel for containing the cooling mixture, and the cup or can (if I may so call it) for receiving the decanter, its top rising somewhat above the height of the vessel for an obvious reason, with a cover that will admit of easy removal (in the drawing represented together.) This apparatus likewise has an appendage or rim like the former, to insulate it from the table:—it may be convenient to be possessed of a couple of these.

Fig. 3. The apparatus for freezing creams, in which the freezing mixture is to act on both surfaces of the part containing it, as being more economical and expeditious, is not so simple. This however consists only of two parts; viz. the vessel for containing the freezing mixture; and a cover, to which is attached, in the same piece (instead of a tube or cup as in fig. 1.), a concentric annular cavity or chamber, in which the prepared cream is to be frozen: this cavity, forming a circle within the vessel itself, is open at the top, as represented, and of course closed at the bottom, and reaching very nearly (as the tube in fig. 1.) to the bottom of the vessel; this secondary part, as likewise represented, fits close as in fig. 1. over the vessel containing the freezing mixture. The proportions of the apparatus when together are thus: The outer space in width, two parts all round; the middle space, or that which contains the cream, one part all round; and the inner space three parts in width,—this serving as a general scale of proportions for an apparatus of any size. The proportions for an efficient apparatus, as my own, may be: for the first space ten-eighths of an inch (one inch and one-fourth); for the second, five-eighths of an inch; and for the third space, fifteen-eighths, or rather two inches, making the width of the apparatus itself somewhat above five inches and a half; its height being equal to its width, or projecting rim at the bottom likewise to insulate it from the table. It will be perceived that in the figure there are seven very small holes or apertures in the central part of this cover (one in the centre and six round at due distances,) just sufficient for the escape of the air, to admit of the ascent of the freezing mixture in the middle part of the vessel. This apparatus is somewhat elevated at the top, or slightly convex, and the part in which the apertures are placed guarded by a shallow rim to prevent an accidental running-

* We have omitted the plate exhibiting the several vessels employed by Dr. Walker; for the apparatus is so simple, that any one may construct it without the assistance of drawings. The best plan is to make the vessel of pewter which may be cased with wood. The one is made to fit into the other, and is removeable at pleasure. The wooden case may be made by a cooper, of staves bound with iron hoops. The pewter vessel should be cylindrical in shape, 14 inches high, and $7\frac{1}{2}$ inches in diameter, with a rim which rests on the edge of the wooden case. The lid may be of wood, with an iron hoop, which will form a rim to fit the head of the wooden case.

Where pewter may not be procurable, a wooden case lined with sheet lead will answer equally well.

At the usual temperature in this climate during the hot weather, say 85 of Fahrenheit, water may be cooled forty or forty-five degrees by a mixture of dry and well pounded saltpetre and sal ammoniac in equal proportions, allowing one measure of the cooling powder to a little more than one measure of water. A quart measure of water holds about a seer weight of saltpetre and sal ammoniac, and three quarts of water, with about $2\frac{1}{2}$ seers of the salts, will cool champaigne or hock in a very superior degree. Claret requires considerably less cooling.

over of the mixture into the part containing the cream. This apparatus should be furnished (as expressed in the figure) with an outer cover similar, but less elevated, to the one at fig. 2. Previously to use, it will be proper to ascertain the quantity of liquid the apparatus will contain when together, and mark its height; likewise the proportion of the ingredients for furnishing a given quantity in measure should be known. Thus if the three salts are used (which I would recommend to a private individual, always doing so myself, although these cannot be recovered for future use, but being more efficacious than the two only,) for each pint, small or old measure, will be required of sal ammoniac and nitre each, (equal parts by weight, reduced together into fine powder) six ounces; and of Glauber's salt, in clear crystals and dry, four ounces and a half, freshly reduced to fine powder, or kept from the access of air, in a separate parcel from the former; and water ten ounces, or enough to make up one pint in measure when added to the former ingredients:—of course, the whole must be well stirred together, and expeditiously, before introducing that part of the apparatus which contains the article to be frozen, and occasionally afterwards, till the object is completed, avoiding as much as possible any accidental accession of heat. A freezing mixture composed of sal ammoniac and nitre with water, all at the temperature of 50°, or nearly so, (they may all be reduced by water from a pump by drawing off a sufficient quantity first,) will from 50° produce a cold of 22° below the freezing point, and with the addition of Glauber's salt to 28°. The confectioners find a degree of cold at 12° or 15° below the freezing point sufficient for their purpose; but it must be recollected that the cold produced by salts dissolved in water is not so durable as with ice and salt; the duration of the refrigerating power in the above mixtures will of course be in proportion to the quantity and thickness of the apparatus. In the way the confectioner managed, the mixture in the apparatus retained its freezing property till the morning; my usual way is, in extreme hot weather, to place the vessel containing the powdered salts in the coldest water drawn from the pump previously; but in the ordinary way it will suffice to add the cold water without the above precaution: it may be advisable to be provided with a second quantity of the ingredients to preserve the cold by a renewal of the mixture. The drawings are taken from an apparatus of each kind of my own,—they are made of tin, for want here of a fitter material, and are painted outside of a grass-green colour. The confectioner abovementioned laid in a stock of a hundred weight of each of the articles; viz. sal ammoniac and nitre; the former at the rate of one shilling per pound, and the nitre at fourpence, which of course when mixed, was at the moderate price of only eightpence per pound. Glauber's salts may be procured in the large way at the rate of about twopence per pound, and by the single pound at fourpence. The apparatus abovementioned may be only half or three parts filled for use; care must be taken in every instance that the surface of the subject to be acted upon be rather below the surface of the freezing mixture.

For cooling wine, the coldest water drawn from a pump will be quite sufficient; however, if required, a small portion of the cooling powder may be added to the water.

The addition of Glauber's salt, it may be observed, increases the density of the mixture, which then becomes a better conductor of the cold, if I may so express myself, and moreover retains the same temperature longer: of course it will be bet-

It must always be kept in mind, that the cooling fluid should reach up to the neck of the bottle to be cooled.

Those who will take the trouble of using a thermometer to ascertain the degree of cold produced will soon learn the best proportions of all the ingredients, and may then vary them accordingly to produce any degree of cold required, to the extent of nearly 50 degrees.

As sal ammoniac is expensive, compared with saltpetre, it may be observed that Glauber's salt produces a considerable degree of cold when employed with saltpetre. For example, a seer of saltpetre will cool three pints of water at 85, 20 degrees. To this add a seer of Glauber's salt, and 10 degrees more cold will be obtained. These salts cannot, however, be recovered by evaporation as the mixture of saltpetre and sal ammoniac can, without injury to their powers of cooling. Glauber's salt may be procured at Patna in small crystals for about three rupees per maund. In its common state of *chara-nimak*, it produces heat by solution in water, instead of cold. But if the solution be partially evaporated, crystals will form on cooling, and these crystals are the pure sulphate of soda, which when re-dissolved produce cold. The crystallization of the *chara-nimak*, can be effected in the cold weather only.

ter of the two to overcharge than undercharge the proportion of the salts to the water. It will be apparent, for obvious reasons, that the part containing the subject to be cooled should be as thin as may be, and the whole of the external part in every apparatus, thick.

This detail may probably appear prolix to any person induced by curiosity only to look it over; but to any one who means to put it in practice, the whole will be found essential, and with a little attention and experience become familiar and easy, and which I have endeavoured to combine every advantage the subject will admit of; and as coming from the "fountain head," it may not prove uninteresting to some at least of your numerous readers.

I am, Gentlemen,
Your most obedt. servt.

Oxford, April 28th, 1828. RICHARD WALKER.

The following remarks are contained in a second letter, published in the July number.

The drier and finer the mixed powder of sal ammoniac and nitre is, the better; and the pulverization is best effected, in the first instance, with a heated pestle. Glauber's salt in an efflorescent state, or which by long keeping or from access of air has changed to powder, is unfit for the purpose; in this state producing heat by solution in water. The best way of preparing the frigorific mixture is by previously placing the powdered Glauber's salt, and giving it a level surface, at the bottom of the vessel, and upon that the mixed powder of sal ammoniac and nitre; adding first about half the quantity of water, and immediately after the remaining portion, stirring the whole together each time. The vessel containing the powdered salts, as above stated, may remain thus any convenient time before adding the water. (Care must be taken to stir the evaporating mixture towards the end of the process, and not to urge it too far.) Nitre being a much cheaper article than sal ammoniac, more easily reduced to powder, and producing about 16 degrees of cold by solution in water, may supersede the use of the mixed powder for cooling the water in which wine is placed. This powder, moreover, is useful, occasionally, as an addition to mixtures of ice and salt, to increase the power and accelerate the process.

The proportions of the articles given in my former paper are adapted to the temperature of 50°; at a higher temperature, of course, the water will dissolve a somewhat larger portion of the salts, and the effect will be proportionably greater. Thus the most powerful mixture, given in my table of frigorific mixtures, consisting of phosphate of soda, nitrate of ammonia, and diluted nitric acid, will, when mixed at the temperature of 50°, produce a cold of 21° below 0°; and if mixed in due proportions at 100°, it will produce, in an instant, a cold of 20°; viz. a reduction of eighty degrees. By means of this mixture, as I have been informed, water has been frozen solid "under the line."

IV.—*Proceedings of Societies.*

1. ASIATIC SOCIETY.

A Meeting of this Society took place on Wednesday, the 1st July, the President in the chair. The Count de Noè was elected an honorary member, and Captain MacDougal, Mr. Baillie, and Baboo Kasinath Mullick, were elected members of the Society.

The President informed the Society, that Lord William Bentinck had consented to become the Patron of the Society.

Letters were read from Dr. Tytler, Mr. Montgomerie, and Mr. Allan, requesting their names to be withdrawn from the list of Members of the Society.

The circumstance of Dr. Tytler's secession led to a resolution, that a letter should be addressed to that gentleman expressive of the Society's regret at his secession, and of the sense entertained by the Society of the zeal he had always manifested for its interests.

A letter was read from Mr. Calder, announcing the realization of his Majesty the King of Oude's splendid donation to the Society of twenty thousand rupees, and also of a donation of five thousand rupees from his Majesty's Minister, Yatimad-ud-Dowla. The full amount of both these donations has been remitted by Mr. Ricketts, the Resident at Lucknow, who had handsomely made up a considerable loss by the exchange, which would otherwise have fallen on the Society.

It was resolved that the acknowledgments of the Society, and a copy of the Researches be presented to his Majesty the King of Oude; and the thanks of the Society be given to Yatimad-ud-Dowla and Mr. Ricketts, and that a Committee be formed to carry the resolution into effect.

Extracts were read from a letter addressed to Mr. Fraser by Dr. Gerard, relative to Mr. Csomo De-Körös*. It was resolved to settle a certain monthly allowance upon Mr. Csomo, and that a supply of books likely to be serviceable to his researches, as far as procurable in Calcutta, be sent him.

The coat, cap, and staff used by Byragees, were presented by Baboo Ramkomol Shen.

A letter was read from Mr. Martin, presenting a cast of the face of a native of New Holland, and a specimen of the gum of the Acacia Mimosa of that country.

A specimen of the stone† used for Lithographic printing in Tibet, and of the printing, were presented, through Sir C. T. Metcalfe, by Dr. Gerard.

A letter was read from Mr. Grant, presenting copy of an inscription taken from the walls of an old temple at Nhiatrang, in Cochin China, by Captain Ross.

The Meteorological Registers for April and May were presented by Captain Herbert.

The following books were received from the American Philosophical Society: Transactions of the Society, Vol 3—Articles 2 to 9. Elliot's Botany—Tracts. From M. Klaproth: Etude Comparative des Langues. From M. Marcoz: Astronomie Solaire d'Hipparque. From Chevalier de Goulianoff: Essai sur les Hieroglyphes d'Horapollion. From A. Von Hammer; Origines Russes: and Reponse a Tutundjou Aglou. From the Asiatic Society of Paris: Journal Asiatique, Nos. 7 to 10. From Maulavi Abdul Hakeem: a Grammar of the Arabic language. Baboo Sibchandra Das presented a Map of the Geography of the Puranas.

Class of Natural History and Physics.

Wednesday, 15th July.

Honourable Sir E. Ryan in the Chair.

A Geological Sketch of Assam and Sylhet, and three boxes of minerals, were presented on the part of Mr. Scott.

A letter was read from Mr. Duff, accompanying some specimens of minerals collected by the late Dr. Carr, in an expedition across the Goomadong Hills, which separate Ava from Assam; among these specimens, (which were chiefly collected in the mountain streams,) is the tooth of a Mastodon in good preservation.

A letter was read from Captain Franklin, noticing his near approach to Jubbulpore, and the connection of his present with his former survey.

A paper by Mr. J. Tytler was read, giving an account of the preparation of *Olecere*, or a kind of wax for candles, &c. from castor oil, hardened by nitric acid.

A paper by Captain J. D. Herbert was read, noticing geological specimens from the Cossyah Hills in Assam.

A paper was presented by Captain Herbert from Colonel Hodgson and Monsr. De Blosville, on the inclination and declination of the magnetic needle.

A paper was presented from Lieutenant J. Finnis, giving a summary description of the geology of the country between Hoshungabad and Nagpore, with specimens of the rocks.

V.—Miscellaneous Notices.

1. *On an Acid Secretion in a Species of Beetle.*

On the morning of the 19th ultimo, I perceived two apterous beetles upon the ground, and wishing to procure one, I alighted for that purpose. On stooping down, however, I felt a smarting on my face, similar to that occasioned by the action of some strong acid upon the skin; and a pungent acid odour, like that of aromatic vinegar, was diffused around. At first I imagined, that the acid odour might arise from some accidental cause; but on proceeding a little farther on, I found another insect of the same species on the ground, and a mile onward a fourth, on the trunk of a mangoe tree. This last, on being taken, emitted from the mouth two or three drops of a chocolate coloured acid fluid, with the same acid

* The substance of this communication will be found in our 4th Number.

† It appeared to be a fine-grained chlorito-argillaceous schist.

odour as the first. I believe this acid to differ from the formic; but I have no means of determining the point.

This insect is a common one here; I have also seen it in the Jungle Mehals and Ramghur districts; but I do not recollect having before perceived the above properties: I should observe that the morning was foggy with a heavy dew falling.

In a species of Phrynidea, I have also found an acid, having a similar odour to that of the abovementioned insect; but it is emitted from the posterior part of the animal (perhaps from the jointed tail) with such force as to be immediately divided, and to put on the appearance of steam or smoke: may not the stories of insects spitting fire, have their origin from something of this kind? It happened once that this insect ejected his acid upon my hand, and I *think* the stain was permanent, like to that caused by nitric acid.

In order that others may determine the species of the above apterous beetle, and the acid it emits, I subjoin a few of its characteristics.

Colour. Black, with six white spots on the elytra and thorax.

Size. Length two inches; breadth of the thorax $\frac{1}{2}$ inch, of the abdomen $\frac{3}{4}$ inch.

Antennæ. Setaceous; placed before the eyes; the scapus and pedicellus white above, and black below.

Mouth. Perfect.

Mandibles. Forficular; toothed; tapering to a point.

Maxillæ. Hard; lobe, fixed, hooked, and internally hairy.

Palpi. Six, two labial, four maxillary.

Labrum. Square, entire, surface uneven, naked, and horizontal.

Labium. Hard, long, narrow, externally convex, internally concave.

The male is distinguished from the female, by his having the prothorax lengthened backward, into two processes; and he is rather larger than the female in the unimpregnated state.

Midnapore, 2*d June* 1829. T. J. P.

2. *On the Production of Cancar.*

A substance has been lately noticed as being common in all the shallow outlets of the salt water lakes or marshes, which seems to bear some affinity to the description of lime called "red nodular *cancar*."

The physical history of *cancar* has always remained a puzzle to inquirers. It is difficult to conceive, that a substance of such general occurrence and uniform appearance can be the detritus of any stratum of limestone of prior existence to it; and yet few would be hardy enough to advance even a supposition that *cancar* could have been created in the form it presents to our eye.

I find it equally difficult to persuade myself as to convince the few whom I have consulted, that the substance now noticed perfectly and satisfactorily solves the problem; still as there is more than mere room for hypothesis, perhaps some of your more scientific correspondents will be induced by this notice to bestow a somewhat more patient and profitable investigation upon the subject, and thereby confer a benefit upon the science of geology.

The substance when first taken from the bed of these shallow creeks is nodular, hard to fracture, from one to three inches in diameter. and irregular. Its colour reddish while fresh and wet. The whole surface appears alive with myriads of shell fish adhering to the surface.

It is found in greatest abundance and perfection in the months succeeding the rains, when the water is fresh in the lakes, and the discharge of water greatest. It almost disappears (or perhaps decomposes), in the dry months, when the waters of the lake become salt. It does not, however, entirely disappear; the red nodules are still found, but not so hard and almost without the shelly coating.

When allowed to dry in the atmosphere the colour changes to a whitish grey; and when fractured and examined, there appears to be a perfect coating of minute cellular shell-work upon a nucleus of iron clay, with an admixture of sandy particles. To this cellular coating the shell fish adhere closely.

The substance when burnt does not slake; but, which is curious, on exposure to the air after burning, although as hard as stone, in a few days it loses its hardness, begins to crack, and crumbles to the touch. When in this state, ready to crumble to pieces, immersion in water appears to restore its hardness instantly and permanently.

The mass is evidently not entitled to be classed among the *cancars* as found; but perhaps it is worthy speculation to inquire what would follow, if a stratum of such nodules were buried in soil and gradually or suddenly compressed and incorporated with the other alluvial strata of clays and sands.

Its fate has been watched, when thrown up from the beds of those *nullas* on dry land, and exposed to the sun and atmosphere. It undergoes an evident decomposition, first losing its shells and shelly coating. The red hard nucleus then gradually softens, and the mass itself crumbles, and is in time lost in the soil, to which it imparts some portion of its colour.

If in the decay of the shelly exterior, the lime, instead of disengaging from the rest, could be supposed to incorporate with the nucleus iron clay, we should evidently have the elements of red nodular *cancar;* although perhaps in the present specimen, there is too small a proportion of lime (50 per cent.) to compose any thing but a *cancar* of very inferior quality.

3. *Analytical Examination of different Waters.*

From where taken up.	Quantity of the water. oz.	dr.	The quantity of salt obtained. dr.	scr.	gr.	The degree of heat applied.	The time it took to evaporate h.	m.	Remarks.
Megnah Doudcundah, ..	4	0	0	0	1½	150°	3	36	Very dirty.
Diamond Harbour, ...	4	0	0	2	7	150°	3	13	
Chittagong Water,......	4	0	0	1	6	158°	3	30½	
Luckipore,............	4	0	0	1	3	150°	3	20	Very dirty.
Saugor Sea Water, ...	4	0	0	1	½	150°	3	40	
Culna Water,..........	4	0	0	0	0	152°	3	26	Very dirty.
Patanhant Dacalty River Water,	4	0	0	0	5	150°	3	22	

The above examination was conducted by Dr. Young, Surgeon to the Suburbs of Calcutta. Dr. Young also examined the tank waters brought from Ságar Island, and from Kijree, and found in the Kijree 3 grains of salt to a pint; in the Ságar, 6 grains to a pint, above 1811.

4. *Atmospheric Tides.*

In No. 5. of the Gleanings, p. 158, is an account, taken from the Philosophical Transactions, of a theory proposed by Mr. James Prinsep of Benares, to account for the daily and annual tides of the atmosphere as indicated by the barometer. He refers these changes to the action of *temperature*, and not of gravity, and from the tables he has given, he has left little doubt as to the real connection of the two phenomena, i. e. rise of the barometer and fall of the thermometer. In the 59th number of the Foreign Quarterly Review is an article, giving an account of Laplace's fifth or supplementary volume of the *Mecanique Celeste*, from which the following is extracted.

" In the last chapter of this book he discusses the influence of the sun and moon in causing periodic oscillations of the atmosphere. From the comparison of a great number of observations of the barometer and thermometer made in the Royal Observatory of Paris with the formulæ which express the action of the two planets, the influence of these bodies seems distinguishable in causing an atmospherical flux and reflux, but its quantity is extremely small, and by the application of the calculus of probabilities Laplace found that the existence of the phenomenon, in our latitudes at least, is very doubtful. He is rather disposed to ascribe the lunar influence indicated by the observations to the elevation and depression of the waters of the ocean, which form a large portion of the base of the atmosphere, and not to the direct effect of the attractive forces of the moon. *The solar action he attributes to the expansion caused by the heat of the sun.* An equally numerous and accurate set of observations made at the equator, where the actions of the sun and moon are exerted with the greatest force, would probably have the effect of determining this curious but as yet doubtful point."

GLEANINGS

IN

SCIENCE.

No. 9.—September, 1829.

I.—*On a New Species of Buceros.—By* H. Hodgson, *Esq. B. C. S.*

[From the 17th vol. As. Res. Pt. 1.]

Order Insessores; Tribe Conirostres; Family Bucerid; Genus Buceros, Species new; Buceros Nipálensis, *Dhanésa*, Ind.

This remarkable and very large species, which I have the advantage of contemplating at leisure in a live specimen, measures, from the point of one wing to that of the other, four feet five inches; and from the tip of the beak to the extremity of the tail, three feet six inches, whereof the beak is eight inches, and the tail, one foot five inches. Its body, in size, exceeds that of the largest raven, and is lank and uncompact, having a rather long and very flexible neck slightly ruffed, a bill and tail of extreme length; high-shouldered powerful wings, and short strong legs. The colour may in general terms be said to be black, with a white-pointed tail, and white patch on the wings: the figure, upon the whole, and in the bird's most accustomed attitudes, is clumsy and heavy.

Let me now attempt a more particular description, beginning with the specific dimensions, which are as follows:

	ft.	*in.*
Wing to wing, ...	4	5
Beak to tail, ...	3	6
Tail, ...	1	5
Bill, length of, ...	0	8
Ditto, depth or height of, ..	0	$3\frac{1}{8}$
Legs, ...	0	10
Whereof, thighs to the knee,..	0	5
Tarsi, to ball of foot, ..	0	$2\frac{1}{2}$
Central toe and claw, ..	0	$2\frac{3}{4}$

The skinned carcase measures, from first to last joint of neck, eight inches; from last joint of the neck to end of rump, nine inches.

The *bill,* which is large even for this genus, is nearly straight from the gape to the tip, but still having, upon the whole, a slight incurvation, which is most sensible along the ridge of the upper mandible, and especially towards the base of it, where the arch is conspicuous, but without any abruptness. The substance of the bill is perfectly hard and apparently solid, not "cellular," or "hollow*," unless in a manner traceable only by dissection, which I do not pretend to affirm or deny. The latter compression is great, so great as to render the edges above and below somewhat sharp, to destroy almost the convexity of the sides, and to leave hardly any breadth to the bill, except at the base, where it is a little thickened, but still much less broad than high. The upper mandible is strengthened by six large prominent ribs, running obliquely down nearly the whole breadth of it, and extending lengthwise from the base beyond the centre. These ribs present their prominence edgewise to the surface of the bill, giving it there an undulatory form: elsewhere, the surface is perfectly smooth. The inner margins of the bill are, by nature, united and entire, but with their edges cut out, and interlocked towards the base, and so they continue to be in the oldest birds. Towards the tip, the inner margins are, in old birds, much and irregularly broken, and separated by hard use; and the ridge also is broken by similar means.

* The words thus indicated as quotations, refer to the generic character.

That the inner margins of the bill are not naturally "serrated," in this species at least, I am enabled confidently to say, from having a well grown young bird, with a perfect bill before me.

The upper mandible of this species is not furnished with an accessory member, in this respect agreeing with the Senegal Gingala and crimson hornbills. Both mandibles are nearly equal, and tend to a point, which is obtuse, especially in old birds.

The base of the culmen, as far down on either side as the nostrils, is feathered; the remainder of the base of the bill entirely naked.

The *tongue* is very small, triangular, and flat. The nostrils are small, rounded, basal, placed high on the sides of the bill, and covered with recumbent feathers.

The *region of the eyes* is naked, except over the brows, as far forward as the nostrils, where the skin is feathered. The *eyelashes* are strong, flattish, and tend outwards, with their tips incurved. The *legs* are short, very stout, and unfit for walking: the *tarsi* very short; in front, a little feathered at top, elsewhere shielded by a succession of single, strong, transverse scales: the *toes* disposed three before and one behind, of moderate length, dilated, flat, strong, scaly, very imperfectly separated; the anterior outer toe being united to the central one, beyond the second joint, and the interior inner toe, beyond the first joint. This imperfect fissure of the toes, joined to their extreme flatness beneath, gives to the soles of the feet a singular character: and the legs are so placed in the body, that the bird, in perching, grasps somewhat obliquely: *claws* arched, compressed, truncated.

The *tail* is greatly elongated, cuneiform; erigible: consisting of ten unequal feathers. The *wings* are high-shouldered; powerful; of moderate length; inclining to round; the first and second quills not being so long as those that follow, and these again, not much longer than the succeeding ones. The naked skin round the eyes and base of the bill is of velvety softness, and runs connectedly from the eyes to the edges of the bill next the throat; and where it terminates below, or at the junction of the lower mandible and of the throat, is a large angular space void of horn, from the edges of which depends a bag, as large as a domestic fowl's egg, of smooth naked skin. This bag the bird fills and empties at will; but never changes its colour, as the Abyssinian hornbill (which is also provided with a similar appendage) is said to do.

The feathers of the head, neck, and body beneath are of a remarkable texture and substance. These plumes (if plumes they can be called) are somewhat elongated, and have long discomposed webs, and both shafts and webs are of a wiry or hairy substance. Those of the head and neck, which are rather longer than the rest, form a sort of pendant ruff, that is capable of partial erection at the bird's pleasure. This ruff has the advantageous effect of taking off from the monstrous disproportion between the huge bill and comparatively small head and neck; but on the other hand, its erection, from the scanty set, and separated web, of the feathers, exposes the coarse nasty skin of the neck. The rump is, I think, considerably more hardened and flattened beneath than that of other birds; and the reason of this peculiarity, and of the shortened tarsi, would seem to be to allow the bird to rest its weight upon the rump and tarsi; for the vast size of the bill probably disturbs the equilibrium, and will not permit this bird to perch at ease, as other birds do, with legs straightened and resting on the feet.

The colour of the plumage has already been stated in a summary way. It is fitting, however, to be more particular upon that head. The discomposed, wiry feathers of the head, neck, and body beneath, are perfect black: the remainder of the plumage, or that of the entire back, wings, and tail, (with the exceptions to be immediately noted,) also black, but reflecting with the aid of a strong light, a deep blue gloss, and sometimes, but seldom, a deep green one. The third, fourth, fifth, sixth, and seventh quills of the wings, for about three inches from their points upwards, are pure white; and so likewise is the terminal third, and more of the tail. The naked skin round the eyes and base of the bill is a rich light blue: the bag depending from the throat, bright scarlet. Of the eyes, the sides are red; the pupils black. The bill is white, with a greenish yellow tinge, and the ribs of the upper mandible black. The feet are dark brown, approaching to black. The figure of the bird is infinitely various in various attitudes. The familiar posture is a squat, with the neck feathers ruffled out, the neck retracted within the high shoulders of the wings, and the tail frequently erected like a magpie's, at other times dropped; and in this attitude the bird has a very stupid and clumsy appearance. When it raises itself on its feet, puts its neck partially forth, closes its neck-plumes, and drops its tail, the outline of the body is long, narrow, and not unpleasing. But to see this bird to advantage, mark him when dressing his plumage with the fine shoulders of the wings projected, the strong, nervous legs exposed to view, and the flexible neck extended and arched backwards. His figure has then some of the graces, and even terrors of the nobler birds of prey. Its disposition is placid and tranquil; but it is not therefore deficient in spirit, and when a captive and caged, though it hates, it fears not the approach of dogs, and to man's approach is quite indifferent. It is easily tamed, both from its confidence and quiet habits. Its habits are sedentary: it dislikes strong light and heat, and tenants the deep woods, covering the hills which overhang the great Saul forest. Its more peculiar haunts are the largest trees, especially such as are decaying, the trunks of which it perforates from the side, making its abode within upon the solid wood, and having its mansion further secreted by an ingeniously contrived door: so that it is difficultly found, and more difficultly taken. That which is now before me was extracted from the tree by cutting down to its nest with axes. I am told it pairs, and is not gregarious. It cannot walk, but advances on foot forwards and sideways, by hops, like a crow, or magpie. Its flight is horizontal and heavy, with neck retracted and tail dropped. The voice of the mature bird is usually a short, hoarse croak; but when angry or alarmed, it utters a cry not unlike a dog's bark. If left alone, it seldom speaks, but when once excited to utterance, is most pertinaciously noisy.

To ascertain the habits, in respect to food, of a very rare and shy bird, is extremely difficult. After much inquiry, I gather that this species of Buceros feeds chiefly upon fruits, but, when urged by hunger, does not refrain from various kinds of reptiles: judging by the structure of its bill, legs, and claws, (the bill is far less formidable than it seems to be, and the claws are very obtuse,) one should conclude that it is not raptorial, even in the meanest sense: and its perfect freedom from all offensive odour, as well as the excellency of its flesh, (which is much esteemed by the mountaineers for the table,) seem to go far towards proving that it is almost exclusively frugivorous. Nevertheless, it cannot be denied that, in the tame state, this species will eat meat (either raw or dressed) with as much apparent relish as fruits; and its natural habits in regard to food must, therefore, for the present remain doubtful. That which I am describing, is fed principally with boiled rice, mixed with ghee, and made up into large balls. Water it never touches. The throat is very wide, and the swallowing powers prodigious. Whatever is offered to the bird as food, is gulped entire, after being rubbed, more or less, according to the exigency, between the huge mandibles of the bill: and if not capable of being thus disposed of, it is rejected. As a consequence of this mode of feeding, the bird is apt to be incommoded by its food, after it has reached the upper stomach; in which case the substance swallowed is immediately and easily regorged into the bill, rubbed a little more, and swallowed again.

Its odious voice, awkward gait, frequently erected tail, and sombre pied plumage, proclaim its relationship to the *Corvidæ* of the *Stirps Corvina*: whilst its superior size, huge bill, gressorial feet, and tiny, triangular flat tongue, are family features that cannot be mistaken.

M. le Vaillant complains of the unnecessary multiplication of species in this genus. Yet I venture to anticipate, that the bird now described, will be allowed to be a new species. I am not sure whether it be male or female, nor can I satisfactorily learn if the sexes are distinguished by any diversity of appearance. But so far as my information can be trusted, it may be presumed that the bird, above described, is a male, and that the female bears a general resemblance to the young bird; which I now proceed to describe.

With the parent bird, a young one was likewise taken. When brought to the Residency, in the beginning of August last, it answered to the following description, and was then tolerably well grown, and well fledged. Wing feathers of the head, neck, and body beneath, dingy red: tale entirely white, save at either extremity, where there was a margin of black: iris of the eye, greenish white: bill unribbed on the upper mandible, and with the green tinge stronger than in the old bird's bill: inner edges of the bill quite smooth and united: naked skin round the eyes and base of the bill, and bag beneath the chin wanting the fine colours of maturity: voice like the clacking of a brood hen, falling now and then into the shriller, but homophonous note of the Guinea-fowl; in other respects, like the mature bird. Now, in the middle of November, the changes noted below have taken place: the bill less green and more like the mature bird's; the first rib of the upper mandible developed: the naked skin at the base of the bill, and the bag beneath the chin, taking rapidly the fine hues of maturity: the basal third and more of the tail, black; and the tip no longer black: the dingy red of the body beneath, darkened a good deal on the thighs and vent: the voice hoarser and like the mature bird's: the inner margins of the bill, still perfectly entire.

The above particulars, how tedious soever, are yet worthy of record in regard to a new and a very rare species of bird. The old bird has recently died; and the young one will probably not long survive him. Should it do so, we shall perhaps be thus enabled to settle the question of male and female, and at all events, may note the changes which the species undergoes in the progress to maturity.

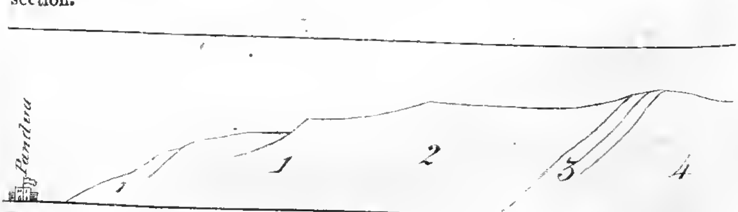
It is proper for me to conclude with remarking, that having no extensive or scientific knowledge of ornithology, I have been obliged to rely for the materials of the above description upon untutored eyes and ears, sedulously employed and assisted by careful reference to Shaw's Zoology.

II.—*Some Account of the Cásiah Hills.*

The country to which the following observations apply, and which has now passed under the sway of the British Government, is comprized under a long irregular figure about 50 miles in length, between the parallels of 25° 8′ and 25° 52′ north lat. and the meridians 0° 0′ and 0° 20′ west from Sylhet. About 15 miles on the northern, and seven miles on the southern side, of this tract are occupied by wooded hills, forming the descent from an open table land, which fills all the central parts; which though traversed occasionally by ravines, and broken into steep knolls of considerable altitude, may yet be described as remarkable for the ease with which it may be traversed by pedestrians and laden cattle in almost any direction. The difficulty of access to this pleasant region (which is marked by the coolness and salubrity of its climate) exists (on the side of Sylhet) in the ascent from Pondua, which, notwithstanding the improvement made in the road, will ever appear considerable, though only so by comparison with the facilities of the central parts, as even this part of the road as is not in fact at all so steep or impracticable as mountain paths in India usually are.

This portion of the route commences about three miles north of Pondua, and terminates in seven more miles at Músmai, from which village there is a gradual ascent to the summit of the ridge above Chera, a distance of about five miles, at which the table land may be said fairly to commence, as there is afterwards no considerable and abrupt inequality throughout the road to Nanklaú, except the valley of the Bóga-páni, which however may be avoided, as will be hereafter shown. The vegetable productions of the country between Pondua and Chera are comprized in the dense jungle usually found at the foot of a mountain range in tropical countries, and which diminishes in proportion as the altitude increases, until about Chera and Sararím, the country becomes remarkable for its bareness and the great scarcity of trees. The principal among those near the foot of the hills are oranges, limes, bamboos, jack, mangoe, cotton, supari, and chumul trees, and nearer the summit (at Chera) wild raspberries and strawberries present themselves.

The rocks consist of limestone and sandstone found in the order of the annexed section.



1. Limestone; 2. and 4. Sandstone; 3. Limestone.

The sandstone is very soft and easily acted upon by the atmosphere: all the precipices are composed of it, and the noble waterfalls in this neighbourhood all pass over rocks of this stone. Many of these have a perpendicular fall of one thousand feet unbroken descent, and would, (were their breadth at all commensurate) be considered among the most considerable falls in the world. They are, when full and viewed from near points, most stupendous and magnificent objects, calculated to excite mingled sensations of pleasure and awe in the beholder. The limestone contains numerous fossils, and is exported in large quantities for making chunam.

The quarries from which it is procured are all situated near the foot of the hills and contiguous to navigable streams, by means of which the stone, which has been broken from the rock into fragments, during the dry season, is carried away in the rains to Chattac or Sonamganj, where it is burnt.

The orange trees are planted only at the foot of the hills, and yield large crops of a very superior fruit, which is exported in great quantities to Lower Bengal. They ripen in November, and are brought to market in baskets by the Cásias until the end of March, selling usually at the rate of one thousand per rupee. The supari (areca nut) grown on the hills forms also a very considerable article of trade, and, being of superior quality, is sold at a high price in the plains. These, witb pine-apples and limes, constitute the principal vegetable productions cultivated in the lower parts of the mountains. It has been already observed, that the table land commences at the crest northward of Chera, and continues, without auy marked alteration, to Nanklaú. This tract may be conveniently divided into two portions: that north of the Bóga-páni, distinguished by its bareness, the absence of all cultivation, and the character of its rocks, which are all of sandstone; the other, lying to the southward of the above river, has a superior soil, capable of cultivation, and produces considerable crops of various grain and pulse, besides large crops of paddy grown only iu numerous small valleys, by which this part of the country is intersected. The rice fields are all kept under water by the practice of irrigation, which is here necessarily resorted to.

The other characteristics of this division are its pine woods, which begin to appear at Moflang and Molím, in clumps of small trees; and the granite boulders, which are found of various dimensions, containing from one hundred to five thousand cubic feet. The practice of the cultivator in the growth of paddy appears to differ from that pursued in the plains, in the artificial irrigation of the field by ditches communicating with rivulets from which they are kept full of water, and in the performance of tbe sowing by dibble. The labouring season includes the months of February and March; but the crop ripens early, and is cut in September.

Judging from the variety and perfection of the grains and pulse grown on the slopes of hills, and the depth and richness of the soil, it should seem that wheat, barley, oats, and other European grains and grasses might be advantageously cultivated here, though a series of experiments would be necessary for ascertaining their proper seasons with reference to the frosts of winter and the heavy rains of summer. The granite boulders, which are generally disseminated over this part of the hills, appear to be chiefly if not all of artificial formation, being produced by washings for the iron which the rock contains. The process by which the iron is obtained is simple, though tedious and laborious. A soft part of the rock is pitched on, in which a gutter is formed: water is then conducted from a neighbouring spring above the spot, and made to pour through this channel, from which it carries particles the heavy parts of which (being the iron) it deposits, while the lighter ones are carried off.

The powder collected is carried to the smelting house, where it is fused with charcoal, and formed into blue lumps, each about the size and shape of a horse shoe. When this material is worked up, it is found commonly to yield not more than about half its weigbt of serviceable iron.

The remaining natural productions which appear of any value are, the slate, found in the valley between Lyjirni and Moprém; and the coal, situated between Sararím and Chera; neither of which being worked, notice of their localities can alone be furnished here, accompanied by reference to specimens of each, and a remark that each might be made available for domestic purposes in auy establishments formed in the mountains.

There has already been occasion to remark upon the excellence of the roads, which were found throughout the parts recently traversed to be all practicable for laden cattle. It should however be observed, that a considerable advantage, as regards the facility of communication between Sylhet and Assam, may be acquired by deviating from the ordinary route at Sararím, aud pursuing that by Nogandi to the banks of the Bógapáni, which may be here crossed with much greater facility than in the lower parts of its course, and (as its source cannot be very distant) might perhaps be avoided altogether by a circuit round its head. It may further be observed, that on this line of road the Kálapáni is wholly avoided. After crossing the Bógapáni, the route might be continued either by Syung and Myrung to Nanklaú; or along the western side of the U'myang through Nosingyah into Assam; by which latter course all the considerable rivers between Pondua and Assam would be avoided, and a route procured which would probably be open during the whole of the year, in so far at least as practicability of the road is considered with reference to its condition and not to the inclemency of the weather at certain seasons. By this road, which would require but little improvement to render it practicable for laden cattle, Gawahatti might be supported from Sylhet in ten days, and indeed the communication between the former place and Dacca would be shorter and easier by this than any other route. Supplies, now that the country is under the British Government, will soon probably be more abundant than they have hitherto been; but the difficulty on this head might to a certain extent be obviated by the establishment at convenient distances of depôts containing paddy which might be beaten out as required, and would be in no danger of spoiling; other articles of food might be easily carried on bullocks or elephants, though the latter description of cattle should not be extensively employed, as there is often difficulty in finding suitable food for them.

There are not throughout the hills any places deserving the names of towns; but there are a few villages distinguished either by their size or wealth: of these Chera, belonging to Dewan Sinh, Molim, Nungklaú, Lambrí, and Nángún, are the principal. In the choice of a situation for a village, attention seems first to be directed to the contiguity of water and wood; and, with the view of sheltering the houses from the violence of the south and east winds, the northern face of a hill is generally preferred. The houses of a village are always separate, each standing in its particular enclosure, round which are a few sheds for cattle, &c. They are all built of deals, are thatched with grass, and provided with a floor of substantial planks: each house has commonly two small doors and a fire-place, but no windows.

The climate appears to be influenced, not merely by the altitude, but also by proximity to the central parts of the range; those parts which lie near the edge of the table land, as Sararim, Chera, Mamalú, being subject during part of the year to mists and heavy falls of rain, which appear to be of less frequent occurrence about Molim, and the parts between Lambrí and Moflang, for which reason, as well as on account of the superiority of the soil and evenness of the surface, these parts would seem best suited for the establishment of a Sanitarium. Some idea of the coolness which prevails here in the summer may be formed from the circumstance of cloth dresses having been worn by choice throughout the month of May by all the Europeans engaged in the late operations, although that month is considered by the natives, as in other parts of India, the warmest in the year. During the winter, frosts are of frequent occurrence, and ice is commonly found in all the rivulets at day-break, but melts in the course of the day No snow ever falls.

Of the character of the people little perhaps has been learnt lately in addition to former information; except perhaps that the reputation for bravery which had been assigned to them by some observers has suffered a considerable abatement; though it is not improbable that, with the advantage of superior arms, and under the direction of intelligent leaders, they might be found capable of supporting their pretensions to a military name. Certain it is that, while the majority of the Cásias, during the late contest, showed a want of resolution, some few individuals among them behaved with great coolness and determination.

In their ordinary habits they appear, like most savage tribes, to be idle and inactive, throwing all labour upon the women, who not only manage the house affairs, but work in the fields and perform the heavier duty of porters between the different markets. It will be useless here to note peculiarities in the customs and institutions of the people; nothing new having been recently learnt on those points, which were ably and accurately described by Mr. Scott in his account of the march across the Jyntea mountains made by him in 1824.

In no particular do the Cásias of these parts appear to differ from their brethren in that country; but as they seem to have no very strong prejudices and are free from the influence of any very powerful superstition, great changes in their character and habits will probable result from a more extended intercourse with Europeans and natives of the plains.

The power and equity of the British Government will ensure them the possession of their rights, while its moderation will encourage them (under the guidance of qualified settlers) to develope the resources of their country, in the quiet enjoyment of which, it may be expected, these people will speedily become an orderly, happy, and contented race. F.

Catalogue of Rock Specimens.

No. 1. A purplish brown, rather compact, argillaceous sandstone, perhaps, more correctly, a variety of grey wacké.—From a precipitous face of the neck joining the ridge of Mamaloo to the mountain on which Leykanchoo is situated, near the centre of the valley, and head of the Purapoongee Nulla, about 1000 feet above the plains.

No. 2. Coal. Bituminous; of a subschistose structure, but fissured in every direction, so as scarcely to bear handling.—From the ridge above Chera Puoji, and about two miles north of that place (!) the seam is about 3 feet wide, and shows itself on the surface among the sandstone rocks of which the strata around are composed: altitude 4,600 feet above the plains.

No. 3. A white sandstone, consisting of irregularly shaped grains of quartz, semi-transparent, united by a white gritty basis: soft and crumbly.—At Lowsung in strata, specimen taken from the crest of the hill, altitude 5,600 feet.

No. 4. The same, coarser grained; may be termed a gritstone.—Apparently the same with No. 3, but the locality was different, it was found on the crest of the hill imbedded in clay.

No. 5. The same finer grained with a yellow basis.—At Lowsung, strata under No. 3.

No. 6. The same, the basis more developed, apparently decomposed felspar. The grains of quartz, which is glassy, more sparingly interspersed.—Same place, in small blocks on the surface.

No. 7. Hornbleude schist, consists chiefly of hornbleude and felspar, though there is also some quartz. It might be called hornblendic or syenitic gneiss.—From Lowsung. The specimen is simply a fragment, of which the locality could not be traced for want of time and opportunity, but as it appears to contain some metalliferons particles, it may be possibly worth while hereafter to search for it.

No. 8. Fine grey wacke slate. (Talco-argillaceous schist.)—Slate. From the valley of the Oominarg between Lyjirmee and Moprem, found on the banks of the nulla in large lamellated blocks protruding from the surface.

No. 9. Fine granular quartz rock, contains schorl.—From Nogandee: in blocks.

No. 10. Ditto almost compact.—Ditto: in strata.

No. 11. Syenite: in boulders at Nogandee.

No. 12. Small grained gneiss.—In loose fragments at Nogandee, estimated altitude 6000 feet.

No. 13. Stalagmitic concretions from the cave at Músmai. They are of a spherical shape and of two kinds, one with a tolerably even surface, composed internally of spherical coats alternating in colour, something like the structure of an agate; the others have a cauliflower-like surface, and are internally more irregular and less compact, though the concentric structure be still visible.

No. 14. Pieces of bituminous coal from the bed of the Jadukátta near Laour. These specimens resemble the mineral of the true coal formation most closely.

III.—*Thoughts on Mineralogical Systems.*

If we define geography to comprise a description of the earth as it is; it may be conveniently divided into two sections. The one may consider the *arrangement* of the surface; the other the *constitution* of it. The first is properly called GEOGRAPHY; the 2d, GEOLOGY. The latter then, we see, considers the materials of the globe, and their arrangement beneath the surface, as it is at present discoverable; and it professes to give a correct description and account of them. To do this, however, it must call in the assistance of other sciences.

On a cursory examination of the surface of the earth, it appears to be of a very irregular and heterogeneous composition, nor does it seem easy to reduce the anomalous appearances that present themselves to any thing like system. A more particular examination, however, will establish to our satisfaction some points not devoid of interest.

The most indifferent observer can distinguish between those soft masses of loose aggregation which form the surface in cultivated districts, and the hard persistent masses that occasionally are seen to break through the surface, and frequently rise into lofty mountains. The former may be distinguished as *debris;* the latter, it is unnecessary to say, are rocks.

Debris, as the word signifies, is formed of the fragments of what was once solid, and had form. When very much comminuted and mixed together, it is difficult, without the aid of chemistry, to detect the different substances of which it consists. But in general, and without any very nice chemical examination, we may distinguish

1 Peat,
2 Vegetable mould,
3 Clay,
4 Sand,
5 Loam,
6 Diluvium or gravel.

Rocks are divisible into two grand classes; 1st, those having a simple or homogeneous aspect; and 2d, those which appear to be compound.

Observation teaches us, that the simple rocks are very uniform in character in whatever quarter of the globe they are found, and the chemist finds their composition equally so. Observation also teaches us, that of the compound rocks the different parts, where they can be clearly separated, agree also in the uniformity of their characters, though derived from the most distant localities.

These homogeneous substances seem then to constitute the first objects of study. An analytical investigation of rocks naturally fixes our attention on them as the last term of the division. Of these every terraqueous substance, not immediately derived from organic beings, is composed; and to understand and recognise the former, an intimate acquaintance with the latter is indispensable. These homogeneous substances are called minerals, and the knowledge of their properties constitutes what is called mineralogy, which we may therefore see is but a branch, the alphabet in fact, of geology.

1. It is not every substance which to the eye appears homogeneous, that is to be considered a mineral. To constitute a mineral species we must have the same simple substances united in the same proportions. It will not invalidate this definition to say, that chemistry has often presented us with the same constituents, united in the same proportions, for minerals evidently different; and again has differed in its analyses of different specimens of what was evidently the same mineral. This is in fact but to say, that chemistry as a science is not yet perfect, nor chemists infallible. It still remains incontrovertible, that the composition of a mineral is its essence, or that which determines it to be what it is and nothing else. If you conceive that modified, you must allow its identity gone. It is no longer the same thing, and consequently, not the same mineral species.

2. Since minerals of different species are found to differ in external characters, it is evident, that the latter may be used with due caution as a key to the former. In attempting, however, this investigation of the mineral species, we are to recollect that our results must be confirmed by the chemist when doubtful, for we can never pretend to put the methods in competition. We must not exalt the handmaid before the mistress. In some instances, however, it must be confessed, that the doubts founded on a comparison and consideration of external characters have been confirmed by a more careful analysis. In fact, to attempt to learn any branch of science by a rigid adherence to any one method or principle to the exclusion of every other, is to sacrifice knowledge to pedantry, the substance to the shadow. If our object be to really know the thing, we must not quarrel with the means. Hauy has justly observed " L'on peut repondre a ceux qui penseut que la mineralogie doit se suffire a elle meme sans avoir besoin de se meler avec des sciences etrangeres, que dans des operations si elementaires et que n'exigent q'un si petite depense de moyens et d'effets; on ne voit proprement ni le chemiste ni le physicien on n'y voit que le mineralogiste qui interroge la nature d'une maniere plus pressante et plus heureuse."

3. A mineral species being once fixed, and its more obvious properties correctly described, these will be in general sufficient to enable us to recognise a specimen from a new locality. All those minerals, then, the investigation of which has been completed, may be thus recognised; and to effect this successfully we may now proceed to consider what are the particulars best fitted for this purpose.

4. I am here merely speaking of the art of recognising minerals, which may thus be seen to depend on the analytical method. The synthetical method arranges them in groups of various orders, so as to give a clear and methodical idea of all the facts known of their properties, their nature, value, &c. in fact, their history. This is of course the more comprehensive part of the subject, but cannot be called the most valuable, inasmuch as without the other, it is of no use whatever. It signifies little that we know every particular in the history of the diamond, if we are not qualified to distinguish it amongst a hundred other substances. It is in fact the founda-

tion stone of the building, and to erect a system without attending to this essential part of the fabric is to build upon sand; yet, with the exception of Mohs, this has been the mistake of every mineralogist. Hauy, however, had the good sense to perceive that there are two distinct objects to be attained, and the merit of indicating clearly the nature and objects of each; nor did he claim for his own more merit than was due; for he expressly calls it a method of classification and not of discrimination (Disc. Prel. page xv.)

5. We are now then to inquire, what are those characters or properties which may be advantageously applied to the discrimination and recognition of minerals to this end. It is sufficient to consider what is the object to be attained. This is the certain recognition of the species, and by signs the value of which is easily and promptly determinable. It results, then, that the more definite a character be, the more valuable it will be. In particular, if any character can be found which is expressible in figures, it will bring to the solution of the question that precision which is always obtainable as soon as a subject of inquiry is brought under their dominion.

6. We have two characters of this description, SPECIFIC GRAVITY and HARDNESS. The first expresses the number of volumes of water equal in weight to a given volume of the mineral, and we may suppose *a priori* that this number would be pretty uniformly the same when determined for the same mineral species; and in fact, such is the case, or at least the variation is within such small limits as to leave the character all necessary precision. The hardness is expressed in figures which indicate the place of a mineral in the whole series, arranged according to their relative hardness. This character, though equally susceptible of precision as the former, has not had that attention paid to it as yet to render its employment sufficiently satisfactory; nevertheless, even with the limited scale as yet determined, it will be found a character of very great value.

7. We may, in considering what has just been stated, judge how great was Werner's error in rejecting the aid of numerical determination in the case of the only two characters, generally applicable, which are susceptible of it. By employing such vague terms as *heavy, rather heavy, rather light, light,* &c. *hard, very hard, not particularly hard,* he broke down the only barriers erected by nature to separate the species; and thus rendered the science of mineralogy a confused mass of gradations or transitions, in which nothing appeared certain except the resemblances. He in fact shut the door of knowledge to his scholars, who were thus restricted to the acquisition of an empirical tact, by which, if they had sufficient practice and a good memory, they could recognise any mineral which happened to be like those they had before seen, but if otherwise they could determine nothing. Nor did their determination, even in the case of those they had seen and known, ever approach the certainty required in scientific determinations. It was at best a mere guess, and might or might not be right or wrong as their memory served or failed them.

8. There is another character which is equally susceptible of the application of geometrical considerations, and which therefore, with the addition of the other two, would introduce great precision into the determination of the species, were it universally applicable. This is crystalline form, which however, unfortunately, is only found in a limited number of each species, and these the most rarely met with. It is therefore useless for those (the greater number) which possess no regular form; useless, I mean, as a principle on which to found an analytical arrangement, though, as forming a very interesting part of the history of every mineral, it is a branch of the science not to be undervalued. Indeed it is in the study of the crystalline relations of minerals that the student will meet with some of the brightest generalisations in the whole compass of the science.

9. Similar remarks may apply to one other, and the only other character which admits of equally precise determination; I mean the refractive power of the substance, which, were it universally determinable, would form a character of great value. But to enable the observer to determine the refractive power of a mineral, it must be in some degree transparent, a circumstance not true of one mineral in three. We are compelled, then, equally to reject this as a fundamental character in our attempt at an analytical arrangement of minerals, although, like the preceding, it may be often used for the illustration of the species.

10. Connected with this character, and in some measure susceptible of the precision which figures give, is the property of polarisation. The number and position of the axes of a crystal is known to depend, or rather be connected with the system of crystallisation to which it belongs; and if we could, in the case of crystalline figure being indefinite or effaced altogether, determine the number and position of

these axes, we should be able to refer it to its proper system, and consequently, by combining other characters, to discriminate it and fix its name. Now the phenomena of polarisation, it appears, afford a means of effecting this; and many happy applications of the principle have been made by Dr. Brewster, who in this manner has detected the crystalline forms of several substances till then doubtful or misunderstood, which determinations have been afterwards confirmed by the crystallographer, on obtaining more perfect specimens or a fuller series. But I believe that to examine the phenomena of polarisation in this way requires that the mineral should be more or less translucent; and that in the case of opaque minerals the optical mineralogist will be at a loss. Dr. Brewster is, however, we understand, preparing a system founded chiefly on these characters; and till the work appears and the full practical application of the method is explained, it would be rash to decide on its value. That the method will afford valuable aids to the analytical method, however, cannot admit of a doubt.

11. There are other characters which, though not susceptible of numerical determination, may yet be found of some use at least in the formation of the larger divisions. Colour is vague, and has a great range even in the same species; but it is found, that some minerals which possess colour retain it when reduced to powder—while others in the state of powder are white, however bright their colours in the solid state. This property is sufficiently remarkable and definite to be applied in separating minerals into at least two grand classes. The same may be said of lustre, which in most of its modifications is sufficiently vague, and also variable in the same species: under one aspect alone it appears to be an essential character, and therefore such as may be relied on, and in this relation it is also definite: this is the kind of lustre called metallic, a term which requires no explanation. It is susceptible of one modification, which enables us to proceed one subdivision lower: it may be retained or lost in the powder.

12. There still remain a few characters which are in like manner applicable to the separation of minerals into two classes; such as solubility in water, nearly synonymous with taste; solubility in acids, in which it is to be distinguished whether they effervesce, gelatinise, or are quietly dissolved; fusibility before the blow pipe, or the contrary, each of which is susceptible of many sub-divisions. In general these characters though they are sufficiently definite and certain, yet belonging as they do to many minerals, they are better fitted for the establishment of the larger divisions; while the specific gravity, hardness, and crystalline form, where it occurs, are the true tests of the species. The particulars relating to these characters, and the modifications of which they are susceptible, will be considered further on. At present it will be necessary to take a view of the remaining characters.

13. These as they have been enumerated by mineralogical writers are; fracture; electric phenomena by friction or heat; phosphorescence; magnetism. The first character is utterly worthless; and has long occupied a place in our systems to the exclusion of more valuable matter. Writers have rung their wearisome changes on their conchoidal, even, and uneven fractures; frequently making one mineral susceptible of every fracture, while hundreds are described in the very same terms. In reality the substance of many of these miscalled systems has consisted of little beyond a "darkening of counsel by much words." And the disappointed student finds himself as far from the knowledge or discrimination of minerals, or farther even, than when he first began. I may speak thus of them without imputation of presumption or unbecoming harshness; for, I have had occasion to lament my dependence on them, and to pronounce them by sad experience, "the persecutors of students and the thieves of time."

14. Electric phenomena are doubtless very useful in distinguishing species where other methods fail, but the application of this test is more obvious in the smaller divisions than in the larger. The same may be said of magnetic phenomena, which are indeed only to be observed in two or three species at most.

15. We may then return to the consideration of those characters which depend on the application of chemical tests,—characters the more valuable, inasmuch as they afford every promise of uniting the advantages of the synthetical and analytical methods, or natural and artificial arrangements; but that they may be useful in the latter point of view, they must be such as are easily applicable at all times and places. Solubility in water, or taste, is eminently a character of this kind; and it is found to belong to a small class of bodies which are in every system distinguished from the others. Solubility in acids is in some respects less convenient, on account of the necessity of carrying about the acid; yet it is so definite, and so well separates bodies which it is perceived, from other considerations, should be kept separate, that

it would be a pity to relinquish it. These two characters are, however, after all of but limited application; and a very large number is left which are neither soluble in water, or acid, and to which no character derived from the colour of the powder either is applicable: to enable us to distinguish these into sections or subdivisions, more or less extensive, the blowpipe comes to our assistance.

16. The right management and skilful application of this instrument of chemical research is the true test of an accomplished mineralogist. It is with its accompaniments a complete laboratory in miniature, and scarcely an experiment that can be performed in the one, but may also be resolved with the other. If the answer obtained be not equally exact as to the particulars, yet it is always sufficiently so to satisfy all the demands of the mineralogist. What this little instrument is capable of, may be seen by the feats of Gahn, the founder of this microscopic chemistry. He was in the habit of exhibiting by its means the iron in a piece of paper, or the alkali and silex in a straw. Professor Berzelius' work on the use and application of this instrument is a treasure to the mineralogical student; and he will find in that work knowledge that cannot be valued too highly. It is, however, to be regretted, that this distinguished philosopher was so biassed by his theoretical notions, regarding chemical combination, as to adopt an arrangement which to the student must be worse than useless. The singular contradiction too of founding a synthetical arrangement on observation with an instrument, the character of which is eminently analytical, seems to have completely escaped him.

17. When we submit a mineral to the action of the blowpipe, either an effect is produced or not. In the former case it may be of various character. Thus the mineral may fuse quietly and without difficulty; or it may fuse with intumescence; or with difficulty; or it may be infusible; although a certain effect is produced. This latter may either consist in decrepitation; exfoliation; disintegration; or there may be change of properties without sensible phenomena. In the case of fusing, the result may be a colourless or coloured glass; an enamel coloured or uncoloured; a scoria; or a bead of metal. With regard to the minerals which remain still unchanged when exposed to its action, we are not entirely without resources. We can add to them certain reagents, and by the effect of these distinguish one from another. Carbonate of soda, borax, and phosphate of soda are those most commonly employed; and the result with each of these, being examined, will scarcely permit of doubt with regard to the name of the substance under examination: the discrimination even of species may be thus brought under its dominion.

18. But our resources do not stop with the mere use of the blowpipe, for a complete system of microscopic chemistry, has been gradually established to aid and assist in doubtful cases. In this department, the late celebrated Doctor Wollaston shines conspicuous, for the ingenuity and precision of many of his methods. Mr. Smithson has added largely to the subject, and to us appears to have left little to desire. His paper on the detection of the several acids, inserted in the Annals of Philosophy, is extremely valuable to the mineralogical student, whom it enables to make in a few minutes as unexceptionable a qualitative, if not quantitative analysis, as the chemist could in several hours. In fact we consider with this aid, added to all those we have detailed, the tyro need never be at a loss for the name of a mineral, if it have one; or for the knowledge that it is a new species, if it be not as yet really named. To collect and digest into a system all the facts as yet known, and determine a few new ones when there is doubt or discrepancy, is all that is wanting. And he who will execute such a task will do more for the progress of mineralogy than has been yet effected by any single work.

19. To illustrate all that I have said, and show the application and use of very heterogeneous characters in discriminating minerals, I shall conclude this rather long paper with a sketch of a synoptical arrangement drawn up for my own use. The skilful mineralogist will perceive many errors and many deficiencies, and I am sensible of many myself; but it will serve to show, how valuable such an arrangement would be if correctly executed. It is my intention hereafter to improve it as much as possible, and I shall therefore be obliged to any of the readers of the Gleanings for any hints to this effect.

20. In this table I have not used any of the usual terms of classes, orders, or genera. We know that with the exception of the species none of these determinations are to be found in nature, and in fact logicians will tell us that even to the use of the latter term in mineralogy, there are well grounded objections. My synopsis being founded on the dichotomous method, has only the latter terms co-ordinate, i. e. the species; but this is an additional reason for urging its utility. In reality it would be easy to apply such distinctions to my table, and thus conceal its dichotomous nature; but, in my opinion, this would be to circumscribe its utility, and take from its clear-

ness. For, as the observing reader knows, every true analytical arrangement must be formed in this way, however disguised. Thus, if we know the specific gravity of a mineral, we ask ourselves is it below or above 2-5; if the former, is its hardness below or above 3-5; if the former, is it combustible or incombustible; if the latter, is it sapid or no; and so on. Perhaps there is an exception in the case of an arrangement founded like that of Linnæus, on numbers increasing by unity and with no intermediary determinations.

21. To show how little there is of certainty or reality in these fancied classes and orders, I would only refer the reader to the work of Mohs, the best certainly that has yet appeared. His orders are completely indefinite; so many conditions are attached to each, notwithstanding the small number of species which it may contain; and so arbitrary and unconnected are these conditions, that it is perfectly impossible to conceive any thing like a clear idea of the character of the order. In reality they are rather lists of species than real abstractions. The use of a classification, at least of an analytical one is, if it have any use at all, to assemble species together, that have the characterising property or properties in common But to congregate heterogeneous bodies, each of which requires a condition or modification of the character to admit it; this is any thing but generalisation. We see, for instance, in the order Haloïde, that to admit Gypsum two conditions are necessary.

22. Though I do not recognise the existence in nature of any such distinctions as classes or orders, yet I find in following out my table that certain groupes of species present themselves, the members of which seem in some measure allied to each other. These may be called sections, and I have for reference distinguished them by names. It must not, however, be supposed that I consider these groupes as coordinate or even natural; they are merely intended to form a step in the analysis on which we may rest, and thus avoid the confusion of so extensive a table as must be required to include all the known species. Each of these groupes or sections will become the subject of a separate table, which I may communicate on a future occasion. At present, having swelled this paper beyond its intended limits, I shall content myself with giving the fundamental table. Note, it has been found convenient to combine the two characters of specific gravity and hardness into a joint one, by which is meant that a mineral should have a certain hardness with a certain specific gravity. In this way, very natural as well as analytical groupes are formed. The shortest and most intelligible way of expressing this condition is by giving in numbers the value of the ratio between them: thus $G \div H =$ less than $6 \div 10$ or ,6 means that if the specific gravity be, say, 4 then the hardness must be *above* 6,6 because $4,0 \div 6,6$—,6 such a character excludes any mineral having a lower hardness, whatever its specific gravity. The figures in the last column show the number of species in each groupe.

EVERY MINERAL HAS

1. A specific gravity below 2,5			Hardness, below 3,5	Combustible,		1	Combustible	11
				Incombustible	Soluble in water,	2	Salt,	17
					Insoluble,	3	Koupholite*,	11
			Hardness above 3,5			4	Zeolite,	22
or 2. A specific gravity above 2,5,	Streak uncoloured,	Specific gravity less than 4,4.	Hardness above 4,5 insoluble in acids,	$\frac{G}{H}$ less than ,6,		5	Gem,	32
				$\frac{G}{H}$ more than,6	Sp. Gr. less than 3,3	6	Schorl†,	24
					Sp. Gr. more than, 3,3	7	Spar,	13
			Hardness below 4,5 soluble in acids,			8	Haloïde‡,	20
		Specific gravity more than 4,4				9	Baryte,	19
	Streak coloured,	Dull	Without metallic lustre in substance,	One of the prismatic colours,		10	Chromate§,	20
				Brown or black or dirty red.		11	Ore,	12
			With metallic lustre,			12	Glance,	30
		Metallic,				13	Metal,	14

* From κουφος *light*, and λιθος a *stone*. M.

† There is an objection to the use of this term, which is the name of a mineral.

‡ From ἁλς *salt*, and ειδος *appearance*.

§ From χρωμη colour. This term [illegible] objectionable, having a different sense in chemistry.

IV.—*Description of the North West Coal District, stretching along the Damoda from the neighbourhood of Jeria or Jeriagerh to below Sanampúr, in the Pergunna of Sheargerh, forming a line of about sixty five miles. By the late* Mr. Jones *of Calcutta.*

[From the 17th vol. As. Res. Pt. I.]

The face of this country is regularly undulated by short broken swells, resembling a chopping sea: the perpendicular height of many of the hills, which I have levelled, averages about sixty feet. The soil is not more than six feet deep, slightly calcareous, resting on grey sandstone that effervesces with acids, and in many places, where it is bare, an efflorescence of soda may be scraped off. The sandstone is not more than seven feet thick on the table hills, but generally thicker and coarser grained in the valleys. The coal and coal metal bassets out in many places, but are delusive guides to the miner, as the greater part of them are the saddle strata, that cover the hills like a cap, and seldom reach down to their bases: others again lie like shields or patches on the side of the hills, and extend a considerable depth below the bases: beneath all these the proper coal beds will be found. The formation appears to me, from the result of many experiments, to be wavy, and wheeling in a slight degree, carrying its line of bearing to an amazing extent with little variation; its breadth on the south west side in the direction of Bancora is not more than eleven or twelve miles from the river. Further on in that direction the protruding rocks are syenite, hornblende, quartz, and masses of mica or talc, cemented with a small portion of sand. At Bajcol, seven miles above the confluence of the Damoda and Baracan rivers, the attendants on coal are lost, and the river is blocked up with gneiss, felspar, and granite. The coal district then turns off crossing Jeria in the direction of Bahar: coal bassets appear a considerable way beyond. The attendants on coal also appear in the Baracan river, regularly downwards on the Birbhúm side, without any interruption, except a large whyn dyke that appears seven miles above Madgeah, running in the direction of Bisheupúr. The whole of the district affords rich and valuable iron ore of various kinds; but no limestone has yet been discovered, except the calcareous concretions that are found on the surface of the ground, and such as are in general use as a substitute for limestone all over Bengal.

1. *Description of the works at Raniganj, with the different occurrences that took place during the sinking of the three shafts.*

Previous to opening the works at the above place, I made small sinkings down to the clay slate or coal metal in the valleys, in every direction within three or four miles. I found them invariably dip E. by S. which threw the line of bearing to the back of the Rajmahal hills considerably inland. There being little chance of finding coal long on the line of dip, within reach, (and it soon bassets or crops out on the line of rise,) I thought it proper to begin at this place, as I never saw the coal dip any other way but with the regular strata that cover it. The Rajmahal hills are composed of mountain whyn or basalt of an amazing thickness; at one place at Móti Jharna, a section or slip may be seen of sixty or seventy feet in height, and quite perpendicular: these hills rest on red streaked ferruginous sandstone, of a very hard nature, such as is often the floor of coal, but I believe very seldom the roof: this circumstance favoured my opinion, that the line of bearing crossed Birbhúm in that direction, and on the 1st December 1815, I began the first shaft. Having made my arrangements with the workmen, the sinking went on regularly; but I was much astonished to find as I went down, that the strata gradually wheeled from E. by S. towards the N. W. and when the coal was found in shaft No. 1, it dipped N. W. which continued regular in every season and bed downwards; the dip of the upper strata forming a spiral line on the side of the shaft. The rainy monsoon having now commenced, and the workmen not attending regularly, I began sinking the shaft, No. 3, and cutting platforms round both shafts to the level of high water in the river, with open adits to make the approach easy. When the coal was found in No. 3, it was within two inches of the same level as No. 1, and dipping due south. I thought this might be caused by a sudden wave or ridge in the strata, or I might be working on the edge of a very small basin: this created much perplexity. I again tried the country round with the former result; and was then in hopes, that I had got on the pivot or point where the strata wheeled, which would throw the line of bearing towards Katwa. To get more information, I opened the shaft No. 2, and although this was four hundred and eighty feet from No. 3, on the line of dip, coal was found in the same level, dipping N. N. W., which gives, in the three shafts, a difference of level of only two inches, and a line of dip and of bearing in each different, but the strata the same. Every appearance indicated lower beds of coal, than any yet cut through; and I continued sinking in shaft No. 1, in hopes of finding the low main, with some difficulty in keeping the water under, from not being able to keep the men at work by night on account of the bears and tigers, until I found the last stone bank suddenly change its declination from half an inch to the foot, to an angle of 45°. This great dip would make it appear a primary formation, although, I am inclined to think, it is merely, what is termed amongst miners, a trouble, occasioned by the wheeling of the strata; I, therefore, did not sink farther, as the coal is always fouled by these occurrences. I am now preparing to work the nine feet bed, from the six inch band, that covers the nine inch seam of coal, up to about six feet with an arched roof, leaving three feet of coal above the arch: the three inches of clay slate that intervene will prevent the water of the eight feet bed from dripping down, and the feeders, of the seam or bed in work, will descend below the springing of the arch, and leave the roof tolerably dry. When the mine has been worked in this manner to a certain extent, the nine inch seam and three feet bed, can be readily wrought, leaving the six inches and two inches bands on the floor, as waste or dead; but if the mine is continued in work for any length of time it would be prudent to carry the waste up, and leave the floor clean. The coal of all these three beds, is of an excellent quality; its cleanliness renders it peculiarly adapted for culinary purposes; it resembles the Sunderland coal in every respect, but leaves more cinders and ashes.

2. *An account of the strata met with in sinking the colliery at Raniganj, Dec.* 1815.

	ft.	*in.*
Yellowish clay, mixed, in some places, with soft black concrete pebbles, ..	6	1
Grey sandstone slightly calcareous, ...	5	0
Yellow soft clay slate, ...	3	0
Clay slate, rather sandy, with a mixture of mineral charcoal, ..	1	0
Very hard, bluish, streaked, and brittle slate, ..	7	0
Tesselated band of grey basalt, dipping to the S. E. two inches to the foot,	1	2
Coarse grained, very hard, and gritty slate, bluish grey colour, ..	3	6
Very hard stone band, grey tesselated basalt dipping S. E. one inch to the foot,	1	10
Very hard, bluish, streaked slate as before, ...	7	0
Blackish clay slate, with faint impressions of vegetables and small bits of pure coal in many parts, ...	6	8
Black clay slate without impression of vegetables, ...	2	9
Black soft muddy clay, ...	0	4
Coal No. 1, slaty and dirty, ...	1	3
Clay slate, ...	0	2
Coal No. 2, better than No. 1, ...	0	4
Coal metal, or hard shale, ...	0	2
Coal No. 3, pretty good, ...	1	3
Coal metal, or hard black shale, ...	0	7
Coal No. 4, pretty good, ...	8	0
Coal metal, or shale, ...	0	3
Coal No. 5, very good, ...	9	0
Argillaceous stone band, with impressions of flowers, ...	0	6
Coal No. 6, better than any of the above, ...	0	9
Argillaceous stone, with impressions of flowers, ...	0	2
Coal No. 7, better than the last, ...	3	0
Black hard shale, ...	2	1
Sandstone band, ...	0	5
Hard black shale, with impressions of vegetables, ..	1	3
Coal No. 8, bad, and full of gold coloured pyrites, ..	1	1
Tesselated claystone, with impression of vegetables, ..	2	4
Grey sandstone band, ...	0	2
Shale, with impressions of vegetables, ...	0	5
Grey sandstone band, ...	0	4
Shale, with impressions of vegetables, ...	0	3
Grey sandstone, ...	2	0
Sandstone, clay slate, and other matter mixed in a confused manner, ..	3	7
Hard, sharp, gritty, grey stone, with cutters, ..	3	6
Total feet	88	

3. *Retrospect of occurrences, and opinions formed thereon, while searching for coal in Bengal.*

The N. W. coal district exhibits a considerable degree of confusion, increasing as you proceed upwards, and is admirably adapted for the use of an indolent race of people; as coal, sufficiently good for common purpose, is within the reach of every body. Knowing that dislocation of strata, always occasions the coal to be foul and dirty, I opened the works in a situation where I expected to be most free from it; but the plan of the works will show, that I was not quite successful, although I have ascertained a most valuable point, viz. the wheeling of the strata in the most desirable direction that could be wished, crossing the great line of navigation somewhere about Kutwa, where I have not the least reason to doubt, that coal will be found, and the advantages that will result must be abundant. Taking into consideration the various occurrences in the N. W. and N. E. quarters of Bengal, I am induced to think that the coal formation of both countries joins under the delta of Bengal, and that the alluvial deposit is of no great thickness; the dip of all the coal seems on the N. E. frontier, favours this opinion, and it is not improbable that this great line of coal enters China. From the Garrow hills into Cachár, I am satisfied of its continuation, as I discovered coal and its attendants the whole way, and found a piece of coal imbedded in a slate rock in Cachár. The best informed people of Manipúr assured me of their having traced it into the Barma country, but they do not use it in Manipúr for any purpose; it is called by them, "amubalang." I am inclined to think this coal district marks the easiest and best road into China. The Surma river is navigable for small boats into Manipúr; but the people on this frontier are averse to travellers proceeding into their country, and, when they have power, resist it.

One of the principal advantages which I anticipate from the introduction of a cheap and plentiful supply of coals into Calcutta, is the being able to burn lime with it, at a moderate expense. At Sylhet, the whole of the lime is burned with wood, an article that has of late become both scarce and dear, so that they are now obliged to depend on a foreign country, Cachár, for their fuel; and for which, large sums are annually sent out of our country. But in the event of the limestone being brought to Calcutta, and there burned with coal, that article could be had fresh and much superior to the lime as now brought, which has been burnt at least, perhaps, a year before. Besides the saving in quality, from the freshness of the lime, the deterioration sustained by the lime getting wet in crossing the great rivers, and the boats taking in salt-water in the Sunderbans, will be obviated; and the expense of carriage would be less, from the boats requiring no roof, and from the insurance being less, the goods being of little value, and subject to no detriment from being wet.

The Shergerh district abounds in iron ores; and I find that immense quantities can be procured there at very little expense, and from the experiment I have made, I have no doubt but extensive forges might be wrought in that district advantageously. Of other ores, there is lead in the neighbourhood of Lakshmipúr, in the Bhagalpúr Zillah; and I have reason to suppose, copper may be found in Dholbhúm near Rajwáha, in a stream called Gura Nadi, that empties itself into the Subanríc,ha.

V.—*Notice of Shells found in the Gangetic Provinces.*

In the notice in page 144 of the Gleanings, enumerating the shells procured on the banks of the Ganges by Capt. Franklin, in his progress to execute his scientific survey, it is mentioned that several genera of shells, designated as "crag shells," were found: some of these shells appear to be fluviatile; others terrestrial; and one of them, "*Scalaria*," unless there is some mistake, is a marine shell. The two former, it is probable, have been deposited by the river, which carries down, during the season of the rains, upon its surface, multitudes of both land and light fresh water shells, both univalve and bivalve, accumulating them with other refuse in creeks, where they are deserted by the retiring waters, and in the next season, are either buried under beds of clay or sand, or are carried still farther down the stream. The *Scalaria* on the other hand is probably a diluvian fossil; although in the course of my researches in the Gangetic tract, I have never yet met with any fossil shells, but those which are still to be found in the rivers or feeding on the shrubs of their banks. These I have sometimes found incrusted, or filled with calc tuff, which is forming every day in the streams.

Helix globosus, of Capt. Franklin, I cannot find enumerated among the species of Lamarck, Wood, or Wodarch, but there is a species named *H. globulus*. Can it be an *Ampullaria* which is thus designated? *Vivipara fluviatilis* is *Paludina* of Lamarck and other authors. The term *vivipara* is too exclusive for a genus, as the property to which it alludes is common to other fresh-water shells. I have taken between 40 and 50 minute shells from the body of the large Gumti *Melania*, as well as from *Paludina*.

Of *Iridina* I have not yet met with a specimen in this country. When Swainson gave his monograph of the genus in vol. 61. of the Philosophical Magazine, it was supposed to belong to the southern American continent. Since that period, M. Calliand has discovered it in the Nile, and Major Denham in the Gammaroo, in central Africa. M. de Ferussac, when enumerating the known fluviatile bivalves, in his notice on the Actheriæ collected by M. Calliaud in the Nile, states that *Cyrena* and *Iridina* live only in the rivers and lakes of the East and of India. This is well known to be the case with regard to *Cyrena*, but I am not aware upon what authority he makes the assertion concerning *Iridina*.

The following list will serve to show the number of species of each genus of land and fresh-water shells, which I have collected in the Ganges and its branches, and on the banks. The references allude to figures of some of the species which I have lately had lithographed in Calcutta, from drawings made several years ago. I have not time to draw up accurate descriptions of each species, and even were I to do so, I should hesitate to name any of them as new. This is the less to be regretted, as, at the close of last year, I forwarded specimens of most of them to Mr. G. B. Sowerby, who will probably describe those which are unknown either in the Zoological Journal or in his forthcoming work, Species Conchyliarum.

LANO SHELLS.

Colimaceæ—Lam.

1. HELIX.
   A. Brought down by the Betwa river.
   B. Bhitúra, Hamirpúr, and Banda, in uncultivated ground.
2. PUPA.
   A. Hamirpúr, Banda, &c. in uncultivated ground, and in moss among rocks, sometimes under garden pots.
   B. Hamirpúr, Banda, and Mirzapúr, on shady banks and under garden pots.
   C. Minute. Hamirpúr.
3. BULIMUS.
   A. At Hamirpúr, Mirzapúr, and Banda, under garden pots.
   B. At Hamirpúr and Banda, on shrubs in gardens and uncultivated grounds; also torpid in the dry season in chinks of pales, &c.
4. ACHATINA.
   A. Minute. Hamirpúr.
5. SUCCINEA.
   A. Bank of the Ganges at Bhitúra, and of the Jumna at Hamirpúr.
6. CYCLOSTOMA.
   A. Bhitúra. Hamirpúr and Banda in uncultivated ground.

FRESHWATER-SHELLS.

Fam. Les Lymnéens—Lam.

1. PLANORBIS.
   A. P. Corneus. Tanks and Jheels, and in all rivers.
   B. The exuviæ found in the Ganges, Betwa, and Jumna.
2. LYMNÆA.
   A. Found alive in the Berna Nullah at Phúlpúr.
   B. Do. in the Jumna and Betwa. The exuviæ common in the Gumti.
   C. Jheels near the Sind. Bundélkhand.
   D. Alive in the Bhagein Nallah, near Callinger.

Fam. Les Melaniens—Lam.

3. MELANIA.
   A. Alive in the G,humti at Juanpore; in the Gogra, attains 3 inches in length.
   B. Alive in the Jumna, Betwa, Cén, and Gumti; also the exuviæ in the Ganges.
   C. Alive in the Betwa, Gumti, and Cén. In the latter river is a variety, in which the tubercles of the shoulders shoot into spines.
   D. Gumti.

Fam. Les Peristomiens—Lam.

4. Paludina.
 A. P. vivipara. Most rivers and jheels.
 B. River Gumti, (exuviæ).
 C. Jheels in the Doâb and Bundelkhand.
4. Jheels in the Doâb, near Hamirpúr.
5. Ampullaria.
 A. Tanks and jheels, Allahabad, Mirzapúr, and Juanpúr.
 B. Jheels Bundelkhand, Juanpúr, Mirzapúr, &c.

Bivalves.

Conchæ Fluviatiles.

1. Cyclas, a minute species brought down by the Jumna in the rains.
2. Cyrena.
 A. Ganges, Jumna, Gumti, Betwa, and Cén. Found alive in the latter.
 B. Alive in the Cén and Betwa.
 C. Jumna.
 D. Jumna and Betwa.

Fam. Arcaceæ.

3. Arca. Of this genus of which no fresh-water species is, I believe, certainly known, I have a small species which I discovered at Hamirpúr in the Jumna. It is extremely rare. I possess a perfect specimen, and a single valve of a larger one. Sowerby conjectures that A. senilis may be a fresh-water species, and Schroter mentions a species A. fluviatilis as inhabiting southern India. The distance of Hamirpúr from the sea (1000 miles) sets its fresh-water origin beyond a doubt. My specimens were brought down with other small shells by the stream of the Jumna in the rains.

Fam. Naiadæ.

4. Unio.
 A. Gúmti, alive, Gogra, and U'r in Bundelkhand.
 B. Ganges, Jumna, Betwa, and Cén, alive.
 C. Alive in the Ganges, Betwa, Jumna, Cén, and Gumti.
 D. Alive in the Jumna.
5. Anodonta.
 A. Minute, in a tank at Hamirpúr, Bundelkhand.
 B? Alive in the Jumna. Exuviæ common in the Gumti, not frequent in the Ganges. The animal of this shell appears to differ much in structure as well as habits from the rest of the Naiadæ. In its habits I am led to believe that it resembles the Solenaceæ, to which family, in the disproportionate breadth of the shell, it bears some analogy; but the hinge differs widely from these shells. Its widely gaping extremities indicate a great difference in habit from the Naiadæ. I am inclined to think that it will form a new genus. Unlike the Naiadæ its interior is satiny, not pearly.

I do not think that many more species or genera will be found in the places which I have marked as the habitats of the species in the preceding list. The streams which flow into the Ganges from the N. E., the rivers of Aracan, the Nerbudda and the Satlej may, however, be expected to afford an addition to our list, when those who have opportunities of visiting them shall take an interest in natural history.

W. H. B.

V.—*On the Analysis of Dolomite.*

In No. 3. of the Gleanings, notice was taken of a method, mentioned in the course of my experiments on limestones, of determining the presence of magnesia by its incapability of forming a hydrate along with the lime when the stone is slaked after calcination.

I find that the following experiments were subsequently made in prosecution of this inquiry; and should they be deemed to possess any interest, I shall be gratified by their being inserted in an early number.

To ascertain the extent of accuracy to which the dry analysis of the compound carbonates of magnesia and lime might be carried, I made artificial admixtures of rhombohedral calcareous spar and carbonate of magnesia in different proportions, and submitted them first to calcination under a muffle, and afterwards to the operation of slaking.

As the atomic weight of magnesia and lime differ considerably, the quantity of carbonic acid extricated in their calcination, provided there be no third substance present, is a sufficient datum whence to calculate the proportions of the two earths in a compound; but the process requires great accuracy of manipulation, as every error will be magnified tenfold in the result; and it also depends upon the correctness of the equivalent numbers of magnesia and lime, which are given variously by different authorities:—having found those of Berzelius most consistent with my own experiments upon crystallized dolomite, I have adopted his determinations, which are as follows:

Lime,	28.47	50.62
Carbonic acid,	22.15	
Magnesia,	20.70	42.85
Water,	9.01	

In any compound then of the two, calling the carbonate of lime l, and that of magnesia m, and the acid in each c and c'; also making C the weight of gas lost by calcination on 100 grains, we shall have

$$l + m = 100 \qquad c + c' = C$$
$$c = 0.4376\, l \qquad \text{and } c' = 0.5169\, m$$

Therefore,
$$1.1813\, m = \frac{C}{0.4376} - l$$

and deducting
$$m = 100 - l$$

we obtain
$$0.1813\, m = 2.285\ C - 100$$
$$m = \frac{2.285\ C - 100}{0.1813}$$
$$l = 100 - m$$

I have made use of this formula in the second column of the accompanying table, excepting in the case of dolomite, which being supposed *à priori* to be a definite compound, should have its acid divided equally between the two earths:—Thus $\frac{47.2}{2}$ or 23.6 Carb. A is equivalent to Carb. Lime, 54. 0 and Carb. Mag. 45. 7 } 99. 7 which differs so little from 100, that the error may be safely attributed to the process or the tables. The formula above would only give 43,4 magnesia. The error would disappear, were the loss of carbonic acid 0.2 gr. more, which was the case in former experiments.

The magnesia which I employed was taken from a medicine chest and dried, but it seems to contain 10½ per cent. of water existing hygrometrically, as I ascertained by direct experiment. It is necessary therefore to modify the value of c' in the formula, which becomes $m = 2.285\ c - 100 \div 0.311$.

I shall now proceed to the experiments.

TABLE 1.

No.	Composition.	Loss of Carb. A. by heat.	Calculated C. L.	Calculated C. M.	Water absorbed.	Carb. Lime deduced.
1. } 2. }	Rhombohedral calcareous spar.	43.9 43.8	100	0	18.06 18.00	100
3. } 4. }	The same with 20 per cent. magnesia.	46.64 46.70	79	21	15.04 16.00	83 89
5. } 6. }	The same with 45 per cent. magnesia.	49.70 49.60	56	44	10.4	57.5
7.	Ditto, 80 per cent. magnesia.	54.44	22	78	5.9	32.5
8.	Carb. mag. (another day)	57. 5	0	100	0.	0
9.	Nerbudda crys.	47.20	54.2	45.8	10.4	57.5
10.	Dolomite.	47.20				
11.	Do. with 20 per cent. of	46.56	64.6	35.4	12.8	72
12.	rhomb. spar.	46 56				
13.	Polyhedral gr. dolomite.	46.16	69.8	30.2	10.1	56
14.	Do. with 80 per ct. lime.	44.22	94	6.0	16.3	92
15.	White marble.	44.08	100.7	0	18.4	102.5

The specimen marked 13 was a green crystal taken from a mineralogical cabinet; it appears to be composed of two atoms carb. lime. one atom carb. mag. perhaps it is the same as the rhombspar of Klaproth*.

		C. A.
two atoms carb. lime.	70. 8	= 30. 8
one atom carb. mag.	29. 7	= 15. 4
	100. 5	= 46. 2

Unfortunately, after water had been added to the above specimens they were incautiously dried over a furnace, whence it is evident that the hydrate of lime had commenced absorbing carbonic acid; in fact this was afterwards proved by dropping a little acid on some of them. Thus there was an excess of weight of one or more per cent. beyond what the lime should have taken up to become hydrated; nevertheless the result, as far as regards the total incapability of magnesia to combine with water, was satisfactory enough, and entirely bore out the observation I had made on a former occasion.

By way of removing, however, any doubt which might arise from the imperfection of the last experiments, I repeated the proof with additional precautions.

Three mixtures were made precisely alike, of 50 grains, rhombohedral spar and 50 of the magnesian carbonate; and at the same time 50 grains of Henry's calcined magnesia were heated alone to examine its action on water. The first specimen was slaked with an excess of water, and dried in a vessel carefully closed, *under* a furnace. The second was slaked in the same way, and the third by exposure to steam; and these two, and the magnesian specimen were dried in a receiver over sulphuric acid.

The results were in every way coincident and satisfactory, and leave no doubt in my mind, that the dry analysis of mixtures of lime and magnesia is capable of much greater precision than the humid analysis, in which, according to Daubeny, five and even ten per cent. of difference will result from the use of different precipitants.

With care in the process of calcination, and the check operation of slaking, I do not imagine that one per cent. of error should find its way into the result. And the method is applicable even when there is silex or any of the inalterable earths united to the carbonates; provided that the proportion of these be first ascertained by solution in an acid.

TABLE II.

No.	*Composition.*	*Carbonic acid expelled.*	*Water absorbed.*	*Carb. Lime deduced.*
1.	Carb. Lime 50, Carb. Magnesia 50,	50.54	9.00	50.
2.	Ditto.	50.55	9.03	50.2
3.	Ditto.	50.54	8.90	49.6†
4.	Pure Magnesia 50,	0.9	0.2	none

Employing the formula $m = 2.285\ c - 100 \div 0{,}311$ the composition calculated from the extrication of 50. 543 will be exactly $\begin{cases} m = 50 \\ l = 50 \end{cases}$, and the mean of the three hydrate differs only one five hundredth from the truth.

The loss sustained by the magnesia is trivial, and certainly due to a partial absorption of hygrometric moisture in the corked bottle, in which it had been kept: the access of weight in slaking must be attributed to imperfect desiccation; for magnesia appears to be exceedingly slow in absorbing carbonic acid, scarcely taking up an appreciable quantity even in a very long interval of time.

Postscript.

Dr. Thomson has given the analysis of a native carbonate of magnesia "forming whole rocks in Hindustan." He states, that "it contains much less carbonic acid than it ought, but that it will be curious to find whether the interior portions of the

* Of the five species of lime haloïde described by Mohs, not one agrees with this: we wish our correspondent had given its specific gravity and the inclinaton of its cleavage planes. We doubt not of the existence of such a species, from the agreement of the present with three analyses of Klaproth's. His specimens were from the Apennines, Hall in the Tyrol, and Laberg in Wérmeland.—Ed.

† A little of this specimen was absorbed by the moistened paper.

mountain have not retained their full proportion." His analysis gives carbonic acid 51,627, magnesia 47,566. But this is the very composition of carbonate of magnesia, according to Berzelius, whose numbers are 51,69 and 48,31, and of the accuracy of these my own experiments do not permit me to doubt. The views generally received on the subject of definite proportions do not warrant such a supposition as Dr. Thomson's.

VI. Ω's *Answer to* P.

To the Editor of Gleanings in Science.

Sir,

I have read with considerable interest the paper of P. in your 8th number "On the Measure of Labour in India." The writer well supports the cause he has undertaken, and I fully subscribe to all his deductions, which are indeed undeniable. One only remark have I to offer, that he mistakes the purport of my letter altogether, in deeming me a blind advocate of machinery *versus* labour. It was a suspicion that the method in question did not do justice to the labour employed on it,—that it gave a less result than the same quantity of labour employed in another manner which induced me to notice the subject; and it will be observed, that I stated the question hypothetically, as not advancing it on my own knowledge. My words are: "The difference here is enormous, and, if real, is certainly worth pointing out." And again, "*An arrangement* that will not permit six average Bengalees to do the work," &c. Here it is evident that it is the arrangement which is supposed objectionable, and it is by no means inferred that six natives cannot do the work of one European. On the coutrary, the arrangement is pronounced vicious that occasions so preposterous a result.

The difference however P. asserts is not real; the quantity raised by Professor Robison's pump being in his opinion purely imaginary. To this I can only answer that *prima facie* it does appear exceedingly unlikely, that Professor Robison should have advanced a statement of this kind without sufficient grounds. To gain what light I could on the subject I referred to the work quoted in your second number; and as it will better enable your readers to judge for themselves, I shall here transcribe the passage at full length.

"One great excellence of this pump is, that it is perfectly free from all the deficiencies which in common pumps result from want of being airtight. Another is, that the quantity of the water raised is precisely equal to the power expended; for any want of accuracy in the work, while it occasions a diminution of the quantity of water discharged, makes an equal diminution in the weight which is necessary for pushing down the plunger. We have seen a machine consisting of two such pumps suspended from the arms of a long beam, the upper side of which was formed into a walk with a rail on each side. A man stood on one end till it got to the bottom, and then walked soberly up to the other end, the inclination being about twenty-five degrees at first, but gradually diminished as he went along, and changed the load of the beam. By this means he made the other end go to the bottom, and so on alternately, with the easiest of all exertions, and what we are most fitted for by our structure. With this machine, a very feeble old man, weighing 110 pounds, raised 7 cubic feet of water $11\frac{1}{2}$ feet high in a minute, and continued working 8 or 10 hours every day. A stout young man, weighing nearly 135 pounds, raised $8\frac{1}{2}$ to the same height; and when he carried 30 pounds conveniently slung about him, he raised $9\frac{1}{4}$ feet to this height, working 10 hours a-day without fatiguing himself. This exceeds Desagulier's maximum of a hogshead 10 feet high in a minute, in the proportion of 9 to 7 nearly."

Now this passage is quite decisive of the question, if we allow Professor Robison to be a competent witness. And why his authority should be inferior to Coulomb's, I confess I do not see. "Your correspondent," says Ω, "must no doubt have been misled by the data of Professor Robison's pump; for, if they are correctly stated, the feeble old man raised 3000 maunds a day, and far exceeded all that the strongest Frenchmen have ever effected." What will he say then to the performance of the "stout young man" loaded with 30lb. which is equal to nearly 5000 maunds? As for me I can only repeat what I said before, "that the difference, if real, is worth pointing out." It appears that Desagulier's determination (1 hogshead of water raised 10 feet in one minute) is equivalent to about 4000 maunds.

"Who shall decide when doctors disagree?" We have here Coulomb *versus* Robison and Desaguliers; and how, except by trial, we are to determine which is right, I find it difficult to say. I may observe, however, that it does not appear to me quite clear whether Coulomb's estimate of the exertion in mounting stairs comprehends merely the height through which the man rises, or whether a sufficient allowance is made for the labour expended in the horizontal progress. Thus, supposing the steps or stairs to be 18 inches broad and 6 high, it is evident that if the exertion of rising through the perpendicular height be represented by 1800 maunds, raised 10 ft. high, the horizontal effort would be equivalent to 1800 maunds carried 30 ft. or one-fifth of a man's whole power nearly; consequently, 1800 would require to be increased to 2130 to represent the total effort per diem, in mounting such stairs,—a number, however, which still falls short of Professor Robison's estimate. Whether some considerations, which I shall notice towards the end of this letter, be sufficient to account for the difference still remaining, I must leave to the judgment of your readers. I should however further remark, that Coulomb, or rather your correspondent P., does not mention the acclivity of the stairs, which is also an important consideration; for it has been determined that if a certain height is to be ascended, it is effected with the least effort when the stairs are of the dimensions mentioned above, and consequently, supposing the same effort exerted, a greater height will be ascended on these stairs than on such as have different dimensions.

But though it is difficult to decide between these conflicting authorities, I do not see that there is any difficulty in forming an opinion of the value of this pump. That it is an ingeniously contrived one, and fully deserving of a fair trial, was and is my firm opinion; and a very few considerations will be sufficient, I think, to show that this is neither a hasty nor a prejudiced judgment. First, I would observe then, that from the table given by your correspondent it appears, that the *maximum* effect produced by the exertion of a man's force, (the fatigue being the same,) is when he mounts stairs unencumbered by any load, and using no other of his muscles, but such as are sufficient for his locomotion. Secondly, and in like manner, in moving along a horizontal plane, the maximum force exerted is when the man is unencumbered by a load, and has no other exertion to make than that of walking. The conclusion then is, I think, inevitable, that if we could so contrive matters as to make the whole of the effect produced in each of these cases USEFUL EFFECT, we should obtain more work than by any other mode of exertion.

Professor Robison's pump secures this desideratum, I may say, completely, and has the further advantage of uniting both methods. The man on the beam which works the pump rods is employed to walk backwards and forwards; sometimes up an inclined plane, sometimes level, sometimes down an inclined plane. He does nothing else. By these exertions, the most effectual a man can employ, is the water raised. The whole of the force is productive of useful effect. I think then that I am entitled to draw the conclusion, that this is the most advantageous method yet devised of raising water, and that with this pump a *maximum* of water will be raised at the expense of the same degree of fatigue to the man.

To determine the value of this maximum, however, is not easy for the reasons beforementioned, add to which we have no data to fix the expenditure of force required to move *down* an inclined plane. I should be disposed to say, that the fatigue is less than is incurred in walking along a horizontal plane. But not to assume any thing too favourable to our estimate till fairly established, let us merely suppose that the two operations require an equal expenditure of power. Let us also assume, that Coulomb's valuation of the effect in mounting stairs is the full value, and includes the horizontal progress, so that in reality on such stairs a man could only raise 1500 maunds 10 feet high in one day, the extra 300 being the equivalent to 1500 moved horizontally 30 feet.

We may easily see by considering the structure of this pump that on the descending arm the surplus weight is that of the man, and that the plunger will descend till the loss of weight, by it's immersion in the water, is equal to the weight of the man's body. This is equal to the mass of water displaced by the plunger, in other words, *to the water raised*. The man now walks up an inclined plane (say of 24°) which gradually assumes the horizontal position by the return to equilibrium consequent on his change of position. So that in reality the acclivity he has to ascend may be taken as less than 12°. Arrived at the centre of motion he is now prepared to cause the descent of the other end to an equal depth simply by walking down a plane which is varying from horizontal to an inclination of 24°, or, as before, say having an average inclination of 12°. Thus, then, the labour of the man consists *in walking up and walking down an inclined plane the length of the beam, and having an average inclination of* 12°. The USEFUL EFFECT consists in *twice the man's weight of water being raised through the sine of* 24° *to half the beam as radius*, or in other words, (as we do not affect extreme precision,) through the sine of 12°, the length of beam being made radius. In his ascent the man raises himself through the same height; this therefore balances half the water. The other half of the water may be considered as the useful effect of the force expended in descending the inclined plane, and in making the progressive motion in the ascent. The proportion which the force necessary to make horizontal, bears to that engaged in perpendicular motion is, by Coulomb's table, 1 : 17. Therefore the force required to move along the beam horizontally is the same as would raise the man's weight through $\frac{1}{17}$ of its length. The force required to descend the inclined plane being assumed to be the same, the two will be equivalent to raising the man's weight through $\frac{1}{8}$ of its length But the sine of 12° is (roughly) $\frac{1}{5}$. Wherefore the man's weight of water is raised through $\frac{1}{5}$ of the whole length of the beam, by an exertion which we have just found is only equivalent to raising him through $\frac{1}{8}$. Here then, if our data are correct, is an actual gain of effect over power.

This is a paradox, however, which I am not prepared to maintain. The reader will in fact see that it turns on our supposing an equal force exerted, when weights are transferred by a man's daily labour horizontally or vertically. To this, which is evidently a paralogism, and not to my under-rating the force required to walk down an inclined plane as compared with that exerted in horizontal progression, are we to attribute the above conclusion. But to waive every thing that might appear like begging the question, we will assume that the force required to move down the declivity of the beam, inclined at an angle of 12°, and along it in a horizontal position, is in reality equal to the force required to ascend through one-fifth of the beam's length. In this case it is evident, that *the whole force exerted* is productive of USEFUL EFFECT. This force being by the table in your preceding number, represented by 1800 maunds, raised 10 ft. high per diem, gives a result equal to the work of FOUR Bengallees with their baling ladles,—a result which, if less striking than my former one, is yet sufficiently so to deserve consideration, from all those who have any interest in the question. When it is considered too, that I have obtained it by adopting P.'s own statements, your readers will perhaps have more confidence in it. The comparison will be still more favourable, if we take the work of the six Bengallees as in my first letter, 2500 maunds; giving only 410 instead of 450 as the performance of each. So that 40 Bengallees with their scoops could only raise as much water as ONE Parisian labourer working with this pump. I may add, that the proportion it bears to the ordinary pump is that of 2 : 1. and to the method of *poorwut*, the ordinary one in Upper Hindustan 5 : 1. I shall be happy if my statements should induce any of our indigo planters to give it a fair trial. I am convinced they would find their account in it. It is particularly applicable in works situated on a steep bank of the river, where there is deep water: for it requires, and this is the only objection against its general adoption, that the water from which the supply is drawn should have a depth equal to the height to which the water is to be raised*.

Though in this attempt to estimate the performance of this pump, I have been contented to assume Coulomb's valuation of the man's exertions in mounting stairs, yet I must remark, that there is a peculiarity in the application of the man's labour to the working of this pump which is not unimportant. This is the *alternation* of going up and down an inclined plane having so easy a declivity as 12°. This is one of the considerations to which I alluded at the commencement of my letter, as perhaps sufficient to account for the great discrepancy between the two estimates. To which I may add, that the declivity being less than that which gives a maximum ascent, assimilates the work more to that of walking along a level plane. Now, as before remarked, we do not know what is the proportion between these two kinds of forces; nor would it be easy to determine it, except indeed, from the results of this very experiment; so that we must consent to resolve the question directly by trial, there being no means of arriving at an indirect solution. And when that is done, I shall not, for one, be surprised to find this kind of exertion give a higher value than that derived from experiments on mounting stairs. At all events I have, I think, made it pretty clear, that the performance of this pump, however exaggerated by Professor Robison, is superior to every other method of raising water, giving a result which is

* This circumstance would have prevented its application to the particular case, I brought forward in my first letter, even had it been otherwise recommended by convenience. Where the quantity of water to be raised is not considerable, the method of baling is evidently preferable for its simplicity, and its independence of any heavy and complicated apparatus, the frequent removal of which would of course be very troublesome. My comparison was not so much intended to apply to the question of the *applicability* of the two methods to any particular case, as to their *performance supposing them applied*.

to that by the common sucking pump in a proportion of TWO to one, to that of baling as FOUR to one.

I shall conclude by re assembling here in a table the several particulars contained in your correspondent's letter and in mine, for future comparison and discussion.

TABLE

Showing the force exerted in different kinds of human labour, as valued by the number of maunds raised 10 feet per diem.

	Country.	*Kind of Labour.*		*Authority.*
		1. *Raising Weight vertically.*		
1	England.	A stout young man, loaded with 30 lbs. working the Plunger Pump, ..	5000	Robison.
2	———	Maximum of man's force, ..	4000	Desagulier.
3	———	A feeble old man, ..	3000	Robison.
4	France.	Mounting stairs without load, ..	1800	Coulomb.
5	———	Hoisting a pile-driver, ..	660	.. ..
6	———	Stamping coin, ..	345	.. ..
7	———	Hoisting water, from a well, in buckets, ..	630	.. ..
8	———	Turning a wheel and pinion or winch, ..	1000	.. ..
9	———	Digging the ground, ..	850	.. ..
10	Bengal.	Baling, most favourable estimate, ..	450	Ω
11	———	Ditto, average of 6, ..	410	.. ..
12	Hindustan.	Pumping (*square pumps,*)	810	P.
		Do. (round pumps,)	800	.. ..
		Raising water in leather bags, ...	373	.. ..
		Baling, ...	500	.. ..
		Carrying bricks up a ladder, ...	144	.. ..
		Hoisting the same by a windlass, ...	360	.. ..
		2. *Horizontal Conveyance.*	*Mds.c.* 100 *ft*	.. ..
		Walking without a load, ...	3080	.. ..
		Ditto with a maximum load, (150) ..	1760	.. ..
		Quantity of work done exclusive of man's own weight,	610	.. ..
		Wheeling in barrows, ...	900	.. ..
		Palki bearers (exclusive of their own weight), ..	264	.. ..
		Palladars employed in carrying grain, ...	554	.. ..
		Pesraj carrying stones, ...	554	.. ..

VIII.—*On the Scale of Temperature.*

It is a curious fact, and full of interesting considerations, the greater specific heat of water at low temperatures. Is it an anomaly which holds only with regard to water, or is it true of every substance, as is the opinion of a writer in the Annals of Philosophy? On the 2d vol. No. 5 of that work, p. 100, there is an account of some experiments which appear conclusive as to the fact with regard to mercury. Yet M. Dulong and Petit's experiments have been thought to establish the contrary conclusion. Whether the mistakes they have fallen into, and which have been pointed out by Mr. Crichton of Glasgow, will account for this difference of conclusion, I find it impossible to determine, not having the particulars of their experiments to refer to. It is however difficult to understand how the writer in question could have been deceived in the particulars he records: he mixed two portions of mercury, having the temperatures 348.9 and 31, and he found the temperature of the mixture 179.3 or 100.7 below the arithmetical mean.

If it be then true, as this writer asserts, that not only mercury, but every other liquid resembles water in this respect, the fact can no longer be viewed as an anomaly. In this case it is the indications of the thermometer that are in fault, and consequently our scale of temperature must be erroneous. This was Mr. Dalton's opinion, who was one of the first to observe the fact with regard to water. His opinion was that the squares of the true temperatures would correspond with the expansions reckoned from the point of greatest density; and that whatever the fluid of which the thermometer should be constructed, if its degrees were formed on this principle, they would agree. In Rees' Cyclopædia, under the article Heat, it is further asserted that the volumes of a gaseous body, submitted to different temperatures, as measured by such a thermometer, form a geometric progression, when the temperatures vary by a common difference. Further, that the cooling of a body heated above the temperature of the medium in which it is placed, proceeds according to the law of Newton, i. e. is proportional to the excess of temperature. Lastly, that the elasticities of steam, when referred to the indications of such a thermometer, would form a geometrical series, the temperatures being in arithmetical progression.

Were this a true statement of facts, Mr. Dalton's scale of temperature would be clearly, if not the true one, infinitely the more convenient, and as such doubtless entitled to general adoption. With regard in fact to the question of which is the true scale, it is a dispute about words, for till we can settle what we understand by temperature, it is vain to inquire which is the true and which the false method of estimating its amount. But if it can be shown; that one scale more than another simplifies our consideration of questions connected with the propagation or communication of heat; if it occasion anomalies to disappear, and enable us to express the known facts after a more concise, intelligible, and universal algorithm; there are good practical grounds for adopting such a scale, even though, from our ignorance of the nature of heat and temperature, we fail to prove that it is the true one. If therefore it could be shown, that the adoption of Mr. Dalton's scale would be attended with such a simplification of the phenomena of heat, there would be little question as to the value of the scale, whatever epithet we might attach to it.

It is needless to say, that such is not the case. When the volumes of a gas are taken in geometrical progression, and the temperatures, as an arithmetical series corresponding; the elasticities of steam have an increasing ratio; as have also the decrements of temperature of a heated body, estimated as occurring in equal intervals of time. This scale, therefore, notwithstanding its promised advantages, has neve been adopted; nor is it likely it ever will, although it has still one plausible argument in its favour, —the equal ratios of expansion in gases answering to equal differences of temperature. Thus it supposes that 10° or 1°, in whatever part of the scale, occasions such an increase in the volume of a gas, as must always bear the same proportion to the volume before expansion. This is a simplicity which the common scale wants, in which the same change of temperature may either triple the volume, or only occasion an expansion of $\frac{1}{3}$.

In our attempts to form an idea of a scale of equal differences of temperature, we labour under insuperable difficulties, owing to our ignorance of the nature of heat. The common method of avoiding this difficulty is, to consider the subject in the following point of view. If we take two equal portions of a substance having different temperatures and mix them, it seems a reasonable inference that one portion will lose as much heat as the other gains; and consequently that the temperature of the mixture will be the arithmetical mean of the two original temperatures. If we apply this theorem to the mixture of different portions of water, the result does not confirm the opinion, that Fahrenheit's scale measures equal increments of heat: the temperature of the mixture is always below the mean. The fact has been attempted to be accounted for by those who consider Fahrenheit's scale to be the correct one, by saying, that water has a greater heating, and consequently a greater cooling power as the temperature falls. This is an example, amongst many that could be produced, of a mere change of enunciation in a proposition being mistaken for an explanation. For in reality, the supposition of an increasing specific heat in the water as its temperature falls, is but another manner of stating the above curious result. No reason has ever been assigned why this should be the case,—no attempt to connect it with other facts, or to refer it to a more general law. And if it be the fact, that all other liquids resemble water in this respect, we shall be more convinced of the absurdity of the supposed explanation. If however it prove, that water is the exception and not the rule, it must be allowed, that, however inexplicable, the fact is not conclusive against the truth of our thermometric scale.

This is what the advocates for that scale assert. And they add, that all liquids are more or less irregular in this respect; mercury being the least so. They therefore reject liquids altogether, as forming, by their expansion, a just and true measure of temperature. This they seek in the expansion of gaseous bodies—a class, the members of which have one property in common that seems to give them a claim to be singled out. It is this : every known gas, has the same rate of expansion for the same difference of temperature. Now it cannot be denied, that this fact does appear to promise a greater simplicity in the laws of heat when deduced from the affections of these bodies, than if they were referred to solids or liquids, the expansion of which are so extremely discordant, that probably no two bodies in either of those states have the same rate of expansion.

The theorem already referred to, which gives the arithmetical mean, as the true temperature of a mixture, is then adopted as the basis of the system. Equal weights of gas, at different temperatures, being supposed to be mixed, the temperature of the mixture is considered the middle point. Thus air at 32° has a volume = to 1, at 392 it has a volume = 1,75. These two being mixed, the volume is evidently 2,75, and that of each portion 2,75÷2 = 1,375. The temperature is 212°, as measured by the common thermometer. It is on this principle that the graduation of the air thermometer is founded. Equal differences of temperature are not supposed to be accompanied by *proportional* changes of volume, but the increments of temperature and volume, it is concluded, both form an arithmetical series.

It appears to me to be a question to which there is no very decisive reply, which is the more just method of estimating the temperature ; by proportional changes of volume, or by equal differences. The latter leads to an absurd result, when we inquire, what is the volume at 448°; for it would appear to be 0, and at any temperature under that the volume ought to be *less than* 0. By the method of proportional volumes, we avoid this absurdity ; for, however low the temperature be taken, there would still be volume. There is another objection to this method of estimating the temperatures, which as I have never seen noticed, I will say a few words about. It is assumed, that the temperature of a mixture is the mean of the original temperatures. Now, however this be with regard to liquids, I think it quite clear that it cannot hold with regard to gases : and the following considerations will probably be sufficient to establish the truth of the opinion.

I suppose we have mixed two equal portions of gases, having temperatures of 32° and 392°, their volumes being 1, and 1,75, and that their resulting volume and temperature are 1,375 and 212°, the arithmetical means. Now in this case, I say, that the gas at 32° has received more heat than the one at 392° has lost ; and consequently, that 180° between 32° and 212° indicates more heating power than 180° between 212° and 392°.

The gas which originally had a volume of 1, has expanded to 1,375. Its density has then been diminished in the ratio of 1,375 to 1. But we know, that when the density of air is diminished in the proportion of 30 to 29,7, heat is absorbed, which would have raised the temperature 1°. In other words, two portions of a gas having these densities will have their temperatures in equilibrium, though differing by 1°. The gas originally at 32° had, then, in expanding to 2,375, absorbed 32°. But that which was at 392° has only given out 24°. So that heat which would have raised the temperature of one of the gases 8° has disappeared ; and the arithmetical mean is evidently half this quantity lower than the true mean temperature, the latter being on Fahrenheit's scale 216°. This unequal partition of the latent heat of expansion, as it has been called, renders elastic fluids much less proper for the application of this theorem than liquids. The latter being incompressible, or nearly so, must have always the same temperature when in communication.

But it has been said, that their specific heat varies. This, as I before noticed, is merely to say, that in mixtures the arithmetical mean of Fahrenheit's temperatures is not the resulting one. If they all agree in this respect, as the writer I have referred to asserts, it must be admitted, that it is the thermometer and not the fluid which departs from the general law. His experiments certainly bear him out in his opinion, as far as mercury is concerned. With regard to other fluids, we have none to refer to. Nothing is more extraordinary than the fact, that though the thermometer has been invented so many years, it should be still a dispute whether two portions of mercury when mixed together give the arithmetical mean of their temperatures. In other words, whether equal expansions are indicative of equal increments of heat. The usual argument that it does so, because it corresponds *pari passu* with an air thermometer, will not hold, as I have already shown. For air in doubling it volume from heat absorbs at the same time 50°. So that in being cooled down to it original temperature it would give out, not 448° as usually supposed, but 498°.

H.

IX.—*An easy method of Predicting Occultations and Eclipses.*

Put M = AR of meridian at any proposed time.
P = Moon's horary angle = moon's AR—M.
L = Latitude (reduced) of the place.
☽ = Moon's true AR.
Δ' = Do. true north polar distance.
p = Do. parallax in altitude.
$*$ = Star's AR.
Δ = Do. NPD.
V = Angle of the vertical with declination circle.
A = Moon's true altitude.

Calculate

$$\text{Tang. } a = \text{Cot. L. Cos. P.}$$

$$\text{Sin A} = \frac{\text{sin L. cos. }(\Delta'-a)}{\text{cos. } a}$$

$$\text{Tang. V} = \frac{\text{Tang. P. sin } a}{\text{Sin }(\Delta'-a}$$

From the moon's true altitude and horizontal parallax, find the parallax in altitude* $= p$, then

$$x = * - (☽ + p.\ \text{sin. V})$$
$$y = \Delta - (\Delta' + p.\ \text{cos. V})$$

We thus get the difference of apparent AR of the moon and star (x), and of N. P. D. (y) for one instant of time, and if this has been judiciously chosen, these differences will, in general, without proceeding farther, show whether or not the occultation will take place. Now perform a similar computation for an instant (say an hour) earlier or later than the preceding, as the first result will indicate, and you will have wherewith to construct a figure showing the time of beginning and end of the occultation, together with the place where the star will reappear from behind the moon's disc; which is indispensably necessary to enable an observer to seize the exact moment of this phenomenon.

I subjoin an example, and shall select one in which no assistance (great as that undoubtedly is) is derived from the elements for the calculation of the principal occultations given in the Greenwich Ephemeris. It will appear, that without taking account of the second differences in calculating the moon's AR and PD or correcting the place of the star for aberration and nutation, still the result will not generally err from the truth more than two or three minutes of time, and will always be sufficiently correct to prepare for observation, which is the sole object in view.

The following is the calculation I made, in order to ascertain whether there would be an occultation of the star No. 651, of the Catalogue of the Astronomical Society of London, on the 8th April, 1829, at a place whose latitude reduced was about 28°. 00′ N. and estimated longitude = $5^h\ 08^m$ E. It appeared, that the conjunction in AR would be at about 4 P. M. Greenwich time, and I made the first calculation for $3^h\ 30^m$ Greenwich apparent time.

☉'s AR at $3^h\ 30^m$ Greenwich time =	1^h	08^m	36^s
Apparent time at the place of observation =	8	38	00
M =	9	46	36
☽ =	5	18	21
P = —67° 03′ 45″ = —	4	28	15

* The most simple formula for this perhaps is the following by DeLambre, from which I have calculated a table for my own use: but the common tables, having for argument the moon's apparent altitude, may be made to answer the purpose.

$$p = \frac{\text{sin. } \pi \text{ sin. N}}{\text{sin. } 1''} + \frac{\text{sin. }^2\pi \text{ sin. 2 N}}{\text{sin. } 2''} + \frac{\text{sin. }^3\pi \text{ sin. 3 N}}{\text{sin. } 3''} + \text{\&c. in}$$

Cot. L=0,27433 sin. L = 9,67161
Cos. P=9,59077 tang. P = 0,37347—

$a = 36°14' 30''$ tang.=9,86510 sin a = 9,77172 cos. ar. co. a = 0,09338
$\Delta' = 71\ 58\ 12$

$\Delta' - a = 35\ 43\ 42$ —— sin. ar. co. $(\Delta' - a)$ = 0,23363— cos. $(\Delta' - a)$ = 9,90945

V =—67° 19′ tang. = 0,37882—A=28° 12′ sin. = 9,67444

For the altitude 28° 12′ and horizontal parallax 56′ 50″, p = 50′ 28″.

log. p = 3,48116 3,48116
sin. V = 9,96504— cos. V = 9,58618

p sin. V=— 0°46′ 34″=3,44620— p cos. V= 0°19′ 28″= 3,06734
☽= 79 35 17 Δ'=71 58 12 ———

☽ + p sin. V= 78 48 43 $\Delta + p$ cos. V=72 17 40
✳= 79 18 00 Δ=72 11 32

x= + 29 17 y=— 6 08

The difference of Polar distance (y) being small, I conclude that there will be an occultation; and proceed to repeat the calculation for an instant an hour later than the preceding, the moon's apparent AR. being less than that of the star.

☉'s AR at $4^h\ 3^m$ G. T. .... = $1^h\ 08^m\ 54^s$
App. time at place of observation = 9 38 00

M =10 46 54
☽ = 5 20 38

P=—81° 34′ =— 5 26 16

Col L = 0·27433 sin. L=9,67161
Cos. P = 9·16631 tang. P=0,82897—

a=15°25′ 20″ tang. = 9·44064 sin. a=9,42476 cos. co. a=0,01592
Δ'=71 57 00

$\Delta' - a$=56 31 40 sin. co.=0,07876 cos. $(\Delta' - a)$=9,74157

V=— 65° 03′ tang=0,33249—A=15° 35′ sin.=9,42910

∴ p = 54′ 58″

log. p=3,51825 3,51825
sin. V =9,95745— cos. V=9,62513

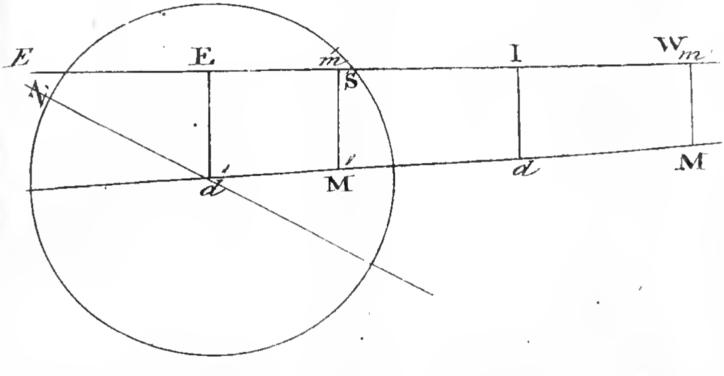
p sin. V=— 0°49′ 50″=3,47570— p. cos. V= 0°23′ 11″=3,14338
☽= 80 09 25 ——— Δ'= 71 57 00 ———

☽ + p. sin. V= 79 19 35 $\Delta + p$. cos. V =72 20 11
✳= 79 18 00 Δ =72 11 32

x'= — 1 35 y'=— 8 39

which π = horizontal parallax and N = true zenith distance. Two terms of the series are amply sufficient for the present purpose.

Draw E W for the parallel of the star: lay off $Sm = x$ and Mm (perpendicular to E W) $= y$: also $Sm' = x'$, and $Mm' = y'$.



Join and produce MM′: make Sd, $Sd' =$ moon's semidiameter, and draw dI, d'E perpendicular to E. W. I, shows the time of immersion, and E, that of emersion. Then, as the increase of the moon's apparent AR. during the interval between the 1st and 2d instants for which the calculations have been made, or as $mm' : 1^h :: m\,I$: the interval of time between the 1st instant and the immersion; and similarly for the time of emersion. In the present case, I found for the apparent time of immersion $9^h\ 09^m$, which answered to $11^h\ 01^m$ by my chronometer: I observed it at $11^h\ 03^m\ 11^s$ differing only a little more than two minutes from the calculation.

In order to find the place where the star will reappear from behind the moon; On d' as centre and radius equal to the moon's semidiameter describe a circle: make the angle E d'Z = V, then will d'Z be the vertical, which will serve to direct the observer to that part of the moon's disc, where the star (S) will reappear. But as it will seldom happen that either of the calculations are for the exact time of emersion, the angle V will require to be estimated, which may always be done with sufficient accuracy.

With regard to the signs: the distances x,x', are laid off to the right or to the left of S according as they are positive or negative: y,y', are laid off above or below the line E W. according as they are positive or negative: and Z lies to the right or to the left of E, according as V is positive or negative. In strictness when the star has any considerable declination, the quantities x,x' ought to be reduced by multiplying them by the cosine of the star's declination; but this is quite unnecessary.

T.

X.—*On different Methods of raising Water for Irrigation.*

[To the Editor of the Gleanings in Science.]

Sir,

In connection with the VIth Art. of your 7th No. should the following memoranda be acceptable, I shall be happy in having communicated them; in the hope that any suggestion, however trifling, may induce others more conversant with mechanics to produce some machine in which utility, facility, and cheapness, may all combine to produce the most desirable results from irrigation; which in many parts of this country is indispensably necessary to assist vegetation.

The irrigation of fields from tanks, by means of baskets, at different ascending levels, was described in one of your first numbers. In Behar the common and simple plan of the lever is adopted; composed of the forked branch of a tree for the fulcrum; a bamboo for the lever; and a lump of earth for the power. With this one man is able to perform the work required, but which is, in general limited to kitchen gardens. This plan is followed also in the immediate neighbourhood of Lacnaú, which indicates a level much lower than the northern and north western parts of Awadh, where it does not I believe obtain.

In the Benares district the wells are deep, and there the general mode of drawing water, is with a leather bucket, *(mót)* drawn by a pair of bullocks, and requiring the labour of two men, one to empty the bucket, the other to drive the cattle. This I should imagine to be the most dilatory, laborious, and expensive mode of any. In Bengal, at indigo factories, the Persian wheel is used, which must be considered a novelty, rather than an improvement upon any old system, when no artificial means of watering the cultivation are requisite; but the introduction of which to the westward would, I conceive, be a great improvement.

In the Dehli district, I have seen a machine which may be, and I believe is, called the Persian wheel, and of which the following is, I fear, but an inaccurate description. It is composed of one horizontal, and two vertical wheels, the former turning upon a pivot, the spokes of which catch those of one of the vertical projecting through the fellies, and turn it round; from the nave of this last runs a beam about 8 feet long, attaching itself to the second vertical wheel, which rests upon two beams placed across the diameter of the well, and working between them; to this second vertical wheel thick ropes are attached, upon which are tightly fastened a number of small earthen pots, like steps of a ladder, reaching in each side from the top of the well to the water: these successively dipping in are brought up full; each emptying its contents into a trough, in reaching the apex of the wheel, produces a *continued* stream which is conveyed to the neighbouring fields. Two bullocks driven by a boy, passing under the beam which supports the machine, turn the horizontal wheel by a shaft to which the cattle are yoked. This method is perhaps the best to accomplish celerity, and will water four *bígahs* in a day.

Another mode of drawing water, I observed near a village close to the cantonment of Nassirabad in Rájputána, which I had never seen before, nor have I met with it since. A leather bucket, similar to that used in the Benares district, in fact an inverted cone, but open at the bottom, with a leather valve to overlap and prevent the water falling: to this valve was fastened a strong piece of twine, the tenth of an inch thick, the other end being tied to the yoke, and of a sufficient length and accuracy to keep the valve shut while the bucket was rising, and in its reaching a certain height, the advance of the bullocks with the yoke pulled open the valve, which disengaging the water, it was received into a trough, and thence carried to the fields. A small pulley to the edge of the well for the twine to revolve upon prevented any friction. One man was sufficient to fill the bucket, drive the bullocks, and attend to the discharge of the water.

The utility of these different modes depends of course upon the proximity or otherwise of the water; but I should imagine this last, might with ease and advantages be introduced in those parts of the country where the *mót* is used.

I remain,

Your obedient servant,

X. Y. Z.

The lever described in the preceding paper is not a bad method by any means of raising water, and it deserves attention for its great simplicity as well as ingenuity. To obtain an idea of its performance we may put $m =$ the force exerted by the man; $x =$ the weight attached to the short end of the lever; $W =$ weight of water raised; $w =$ weight of apparatus, i. e. bucket and ropes. Put also $1 : r$ the ratio of the two ends of the levers, l being the short one. We shall now, supposing equilibrium, have the following equations.

1. $r(m + w) = x$.
2. $r(W + w) = x$.

Whence W is easily found equal to m; i. e. the quantity of water raised is equal to the force exerted by the man. In a working state it is evident that it will be a little less, because in the first step a portion of m is required to overcome friction and inertia, and in the second step a portion of x. The quantity of water raised will therefore be *minus* the man's force by twice this quantity, which, as the machine is so simple, must be very little.

From our correspondent P.'s table given in the preceding number, we gather, that hoisting water from a well in buckets, a man's force is equal to 630 maunds raised 10 feet high per diem. It would be curious to observe whether the actual produce of this contrivance answers to this estimate.

XI.—*Proceedings of Societies.*

1.—Medical and Physical Society.

Saturday, 6th June, 1829.

H. H. Wilson, Esq. in the Chair. Mr. Ogilvy was duly elected President of the Society, in the room of the late Mr. Gibb.

A letter was read from Earl Stanhope, Chairman of the Committee of Correspondence of the London Medico-Botanical Soctety, suggesting, that a correspondence should be opened between the two Societies.

On the part of the Author a copy of Mr. Annesley's Work on Indian Diseases, was presented to the Society.

A Treatise on Inflammation of the Veins, presented by Mr. J. Greig, was read and discussed.

An account of the Sulphate of the bitter Principle of the *Rohena* Bark, with specimens of the salt, was presented on the part of Mr. Piddington.

A copy of Lamarck's work, entitled "Les Animaux sans Vertebres," was presented by Mr. Swinton.

Specimens of opium and of extract of rhubarb, prepared at Saharunpore, by Mr. Royle, were submitted to the meeting.

2.—Agricultural and Horticultural Society.

Thursday, 13th August, 1829.

Honorable Sir E. Ryan in the Chair. The following gentlemen, proposed by the Secretary, and seconded by Mr. Minchin, were elected Members: A. D. L. C. Maingy, Esq., Rajah Kalee Kissen, C. R. Barwell, Esq., Francis Harris, Esq., James Dewar, Esq.

The following Gentlemen, proposed by Captain Jenkins, and seconded by Mr. C. K. Robinson, were also elected Members of the Society: Colonel Sir Thomas Anbury, C. B., Charles Fraser, Esq., Captain Penny, Captain Herbert.

As were the two succeeding gentlemen, proposed by Mr. Minchin, and seconded by Mr. Dickens: H. Compton, Esq., L. Clarke, Esq.

Captain Johnstone, proposed by Mr. Kyd, and seconded by Mr. Bruce, was also elected a member; and also Colonel Alldin, proposed by Sir Robert Colquhoun, and seconded by Mr. Robison.

The Reverend Dr. Carey, at his own request, was re-admitted a member of the Society.

The Secretary informed the Society, that since the last general meeting a boat had reached Calcutta from the Poosah Garden, bringing a large quantity of garden seeds, and some grafts prepared by the Society's superintendent there.

A letter was read from Mr. Sheppard, superintendent of Botanical Gardens, Liverpool, inclosing lists of garden seeds sent out for the Society on the *Bland*, and informing the Association that the letter of the late Secretary, Mr. Barnett, had not reached him in time to execute the commission respecting grafted trees, which however would soon be carefully attended to. The Secretary stated, that the boxes of seed alluded to as sent from Liverpool, had been landed in good order.

He further stated that, after a trial of Enrope seeds by himself, during the last nine years, he had experienced constant disappointment, whenever he attempted to raise early crops of either cabbage, cauliflower, peas, or indeed any other early crop, from the English seeds. He therefore strongly recommended that the Poosah seeds only should be used for the first sowings; delaying the sowing of English cabbage, or cauliflower seed, till the 15th of September, and of pease, till the 1st of November. He had uniformly found the best crops produced from the English seed which had been acclimated by being first sown in the upper provinces, and the produce when ripened, sent down the following year to Calcutta. It was therefore, he thought advisable, that a large supply of the present investment of seeds be sent to the gentleman now in charge of the Poosah Garden, and any other horticulturists in the upper provinces who would make the Society a return in acclimating seeds, for distribution here during the following season.

Mr. Robison's proposal was approved of, and it was resolved to place the seeds at the disposal of the Garden Committee, with power to distribute them as widely as they deemed proper.

An extract of a letter from Simlah was read, recommending the Society to form an establishment in that mountainous region for the purpose of acclimating all Europe trees and seeds, and afterwards distributing them throughout Hindoostan and Bengal. The proposal appeared a very feasible one to the meeting, but want of funds for the present stands in the way of practical effect being given to it.

A letter was read from Mr. Patrick, forwarding to the Society, in the name of Mr. Hutton, a *Nom Nom* tree, a native of Malacca, and which bears a fruit much valued to the eastward.

A letter was read from Mr. Leycester, sending some Cape plants and also Spanish walnut trees for the Society's garden.

Mr. Robison stated, that he had received from Canton (which he then presented to the Society,) three Leechee trees and a China vine, together with several other fruit trees; also a nutmeg tree from Malacca, variegated pine apples from Penang, and other plants. He also informed the meeting that Sir Robert Colquhoun had contributed several Chinese plants to the garden.

A letter was read from Mr. Prinsep, Secretary to Government, relative to an application on the subject of postage.

A letter was read from Mr. H. Wood, dated from St. Helena, stating the great advantage which would attend the cultivation of the plant which produces the Manilla hemp (Musa texilis); and recommending its extensive introduction into Bengal; also offering his services in England, in any way which would be useful to the Society.

A letter was read from Mr. Scott (Assam), forwarding the model of an apparatus, and describing a method used by him for rearing early cauliflower plants during the rains, so as to have them ready for planting out so soon as the monsoon ceases.

A paper was then ready by Mr. Bruce, describing the mode of cultivating Cotton throughout Persia, where the cotton is of a remarkably fine staple and quality.

A letter was read from Baboo Cossinauth Mullick, accompanying a catalogue made by him of the various fruits and vegetables, which are indigenous in this country, or which have been long cultivated in it, although originally brought from foreign countries, together with a list of fruits and vegetables, which have not yet been introduced into Bengal, from the western portion of Hindoostan, but which he considers would thrive, if introduced.

The catalogues contain the names of each fruit and vegetable in Bengalee, also in the English character, the country in which each is indigenous, with a description of its qualities, and the use made of it by the natives of India.

The Secretary submitted a paper by Captain Davidson, of Engineers, on the Horticulture of Upper India generally, in which the author endeavours to show that until the arrival of Europeans, horticulture as an art must have been utterly unknown, and inculcating the necessity of the Society using every means in their power to disseminate instruction by short treatises—and the distribution of good seeds.

The Secretary read the proceedings of the General Committee on 17th of July last, relative to the funds of the Society.

The President stated, that as several valuable papers had lately been sent to the Society, it was very desirable to ascertain, whether the funds would admit their being printed. He accordingly suggested that an enquiry into the subject, including an estimate of expenses, should be instituted, to be submitted to the next general meeting;—which was approved of accordingly; the Secretary kindly consenting to make the necessary inquiries.

With reference to the progress of the Agricultural Committee, the President observed, that some time since, at a meeting of this Committee, it was determined that each of the members should transmit to the President his views of the Agricultural objects, to which he considers the attention of the Society should be chiefly directed; and that such communication, duly condensed, be submitted to the Society for approval. The President further stated, that he had not received all the papers he had reason to expect until the day previous to that general meeting, when a paper containing important valuable suggestions had been forwarded to him by Mr. Piddington, their Foreign Secretary. On the whole, the President expressed his hope, that the views of the Agricultural Committee on the subject alluded to would be in proper shape to be presented to the Society at the next meeting.

A letter was read from Mr. Breton, presenting to the Society a copy, in Persian and English, of Rajah Gunsham Singh's Treatise on Agriculture, and recommending that extracts from that work, as well as Mr. DaCosta's, be lithographed, and disseminated among the Native Zemindars.

The Secretary submitted a paper from Mr. Thomas F. Henley, an experienced planter from Jamaica, on the Cultivation of Sugar Cane, and the Manufacture of Sugar, after the West Indian Plan, as far as may be applicable to the circumstances of this country.

A paper by Mr. Nathaniel Alexander on Indigo, as an Agricultural Speculation, was presented by that Gentleman.

Thanks were voted respectively to the different donors, and writers of communications mentioned above.

XII. Miscellaneous Notices, &c.

1.—*Wild Dog found in India.*

It is not perhaps generally known to our readers, that the dog is found in a wild state in different parts of India. Captain Williamson gives in his "Wild Sports of the East," some scanty notices of the animal, which he describes as then being common in the Rájmahal Hills. There is a strong probability, that the animal will be found in every uncleared mountain tract of that average elevation, and particularly that along the whole line of lower mountains, that stretch from our north western to our north eastern frontier they abound. The following notices sent us by friends give some few particulars, that are perhaps worth recording, if only to direct attention to the subject, and procure for us a more full and correct account of the animal, his habits and character. They relate, the two first to our north western mountain provinces, the last to our Assam frontier.

"The *Bowsa* or *Bhoonsa* is in size something between the jackall and the fox. He rather approaches the colour of the former, perhaps a little redder. His muzzle is long and sharp. His ears erect and short. The eyes remarkably oblique, forming in this particular a strong contrast to the domestic animal. The feet are longer than in the domestic dog, and the hind legs fully as much bent as in the cat or tiger. His tail is long and bushy, and is carried in a similar style to that of the fox. It is of a dark colour as is his muzzle. Altogether he has something the look of a fox, and yet there is a difference. They are said to hunt in packs, living chiefly on deer, and it is the opinion of the mountaineers, that they are capable in a pack of coping with the tiger. They are remarkably wild and particularly alarmed at the sight of a man, and have been known to desert their haunts when subject to be disturbed by him. They do not give tongue till they seize their prey. They are said to abound in the lower hills." D.

"The *Bhowsah* are found in most parts of the hills: there are two kinds, one denominated the *Shikári*, the other the *Lágh;* the latter is much stouter than the former, and its hair longer and darker, it is not near so fleet as the *Shikári*, but possesses a much finer nose: it quickly regains the scent when lost by the *Shikári*. It takes its name of *Lágh* from eating the offal of its prey, which the *Shikári* does not. In Kamaun these animals are numerous; and in packs of from 5 to 20, and upwards, hunt (giving tongue during the chace) every description of animals from the timid deer to the ferocious tiger; the latter they have been seen to attack and kill on more than one occasion. So convinced are the natives of the dread the tiger has of these animals, that they do not hesitate passing through the densest jungle, where they are known to be: human beings they never attack; on the contrary, immediately fly their approach." S. S.

"I have lately got a rare animal, which I propose sending to the menagerie*. It is the Wild Dog mentioned by Williamson in his 'Wild Sports of the East.' It resembles a jackall, or rather an English fox, (as far as I recollect the animal,) rather than a dog, but has smooth hair. The brush is as large almost as that of a fox. I will send it down after next month, as I want to get a breed between it and the tame dog. It is quiet enough with the keeper, but exceedingly timid, which is the character of the animal in its wild state; it being rarely seen by the inhabitants of the country in which they are found." D. S.

* At Barrackpore: the animal has arrived in Calcutta, and is we believe in the menagerie of Rajah Buddinath Roy.

GLEANINGS

IN

SCIENCE.

No. 10.—*October*, 1829.

I.—*Some Particulars regarding the Mineral Productions of Bengal. By the late* Mr. Jones *of Calcutta.*

The Rájmahl hills are not stratified, but appear to be primitive mountains, composed of black whinstone*, in large masses. Their ascent is difficult, but table land and good spring water is found on their summits. The inhabitants are of an industrious and inoffensive disposition. The hills at the foot of the mountains produce flint, nodular iron ore and beautiful agates of various descriptions, quartz crystallisations, and hard boulder-stones fit for paving.

A person by digging in the low hills near Udinallah, may be convinced that stones grow out of common earth and sand: he would there find them in every stage of formation.

The agent which hardens them appears to be ferruginous water, which gathers and concretes the earth in laminæ to a form like an egg: the ferruginous matter, is at first of a blackish purple colour, soft and soapy, and appears distinct about a line thick between the laminæ of clay, which are about three lines or more in thickness. In this state it may be broken with the fingers, but as the stone advances in growth, the cementing matter disappears and mixes with the mass; which becomes a close grained blueish boulder-stone, not easily to be broken by a heavy hammer. I dwell upon this circumstance from a conviction that soft stone may be hardened, by iron liquor† slowly dropping upon it, until it becomes durable, and may be of great use in the arts.

These hills produce wax, honey, and stick lac; but in small quantities. A few elephants, but not enough to make it worth while to catch them. No large valuable timber, but charcoal may be procured at the river side at twenty-five rupees per hundred maunds.

About Sieri-gali and Painti, very large iron mines have been worked, in former times; the ore is nodular, and would perhaps, by carefully rejecting the heads or unproductive stones, yield from twenty to twenty-five per cent. iron.

In this neighbourhood every chink and fissure within the rocks and earth is filled with a semicalcareous substance called *cancar*, of which the natives and others make an impure lime. It appears to be formed by water, carrying in solution with it, calcareous, ferruginous, and siliceous matter, which concretes in every space where it rests; in the concrete mass, the compound parts seem separate like granite. This substance shews that either there is a great quantity of limestone in the neighbourhood, or formerly has been there, and is now decomposed and carried off; however, I could find no proper limestone near that place.

In the environs of Pat,har-g,háti, great variety of iron ore is to be found, and of a richer quality than that found at Sieri-gali, and large mines of it are now open, some of them with an area of sixty feet between the pillars. This place also affords potter's clay and other refractory clay, which would answer extremely well for the purpose of lining furnaces, and I am of opinion it would be a proper place for erect-

* Probably gneiss or hornblende schist. The proper whinstone is, I believe, a greenstone or basalt.—Ed.

† Possibly solution of carbonate of iron is meant.—Ed.

ing a foundery for shot and shells or other cast iron work, but would by no means admit of working iron into bars, as the expense of procuring a power of any kind requisite to drive a mill would absorb the profit. The hills are composed of massy sandstone: granite, iron ore, potter's clay, and veins of quartz and mica, are to be found imbedded in granite.

Bedér hill, near Calgáon, is composed of enormous masses of granite and whin, with a great variety of iron ore. The bed of the river about this place is on loose and open rock. I imagine an attempt to sink a mine here would be attended with great expense and trouble; as it would require a considerable power to clear it of water.

Within forty or fifty miles of Bhágilpúr, in the Carakpúr district or zillah, a rich vein of the lead ore, named galena, is to be found; it produces sixty per cent. of lead, and perhaps silver worth extracting. The mountain is disputed property between Rája Rúp Naráen Dia and Rája Cádir Alí.

As Doctor Buchanan had visited the place and brought away samples of this ore, I judged it improper to lose time in visiting it, and therefore, sent for samples of the ore, which is the best I ever saw.

The rocks about Munghér are quartz, except a few which are composed of a slaty stone of a bluish colour, resembling in its texture mangoe wood, which is in a state of decomposition to a considerable depth, and is intersected throughout by quartz veins, which always break into small rhombohedra and cubes, and another substance appearing as if vitrified, resembling a cinder. The earth of the valleys is coarse and red to the depth of from forty to fifty feet, resting in some places on quartz, and in others on schist, the schist inclining 45°, dipping to the east. Water issues plentifully, the stone being full of rifts, and no doubt opening into the Ganges.

On the road towards Carakpúr, between Munghér and a flat well called Rísi-kund now in the jungles, there is to be found large masses of muddy white quartz, beautifully clouded with brownish red. The stone is very hard and durable, but difficult to work.

The Carakpúr hills are mostly composed of quartz, from which issues many hot springs, which constantly retain their heat in all seasons of the year. The water also is very pure. In the centre of this cluster of hills, at a place called Bímband, there is a large hot spring and a limestone rock. The spring issues from quartz. The limestone is of a pink colour, and some of it white, the fracture like gypsum: the lime made from it is very white, as the stone is pretty free from iron.

The road into Bímband is either from Málipúr or Milki, about twelve or fourteen miles from either place. Saul timbers are procurable at Bímband of a small and inferior kind: they are procured from a few inhabitants, who dwell in a valley at that place. The hills are uninhabited, the valleys fertile, but the population yet thin.

The jungles are mostly *Baír*, and the ‡ and a tree§ from which the *Cat,h* or *Terra Japonica* is produced. If the former were infected with the Lac insect, the produce would be great. I did not see a single insect of that description in any of the trees. The natives make small quantities of *Cat,h*, and dispose of it at the rate of two rupees per maund, very pure: also at the above place a black wood, resembling ebony, is procurable; it is called *Cén*, and might be made an article of commerce.

The quartz at Milki is of so pure a nature, that it might profitably be manufactured into glass. The hills in Ghedór near Málipúr produce good limestone, but the streams not being navigable, it would not pay the expense of conveyance.

At a place called Gariacól, a days journey from Munghér, is found a kind of slaty basalt‖, much used for many purposes. It dips to the west three inches in the foot, which is contrary to all the surrounding rocks; and ten feet below the base of the rock, it may be had sound and in large slabs; but in the air it will decompose in the course of a few years. On the summit of the same hill, a black, durable stone is got, which will not decompose, but it is very hard to work; and near it is a rock of grey millstone grit, much used for hand millstones.

At Masamganj the fine black basalt is got: this stone is very much in use; but it is both difficult and expensive to bring the stones out of any of the hills, the roads being bad and dangerous. There is also a white stratified substance, with small reddish streaks, at from one to three inches distance; it is softer and feels more greasy than pipe clay; it appears to be decomposed stone, and is very like white lead, or the Spanish earth used in England for the purpose of adulterating white lead. I

‡ Name illegible.—ED.

§ Possibly the *Khaír*.—ED.

‖ Quære greenstone slate?—ED.

have not as yet had time for the perfect examination of it, but I have samples of it by me.

Silhet, with the exception of a few clay hills near the station, is invariably low; and when it is not cultivated, covered with grass and reed jungles. The earth, which is considerably impregnated with iron, generally rests on a submerged forest, from six to thirty-three feet below the surface. This stratum of decomposed wood and grass is now mostly formed into turf or peat of a tolerable good quality: when it is laid bare by the rivers cutting into it, it appears in strata from sixteen inches to four feet thick, and in some places in double strata, separated by six feet of ferruginous clay and sand, but may be found much thicker near large *j,hils*, where the incumbent earth is thin.

When the population increases and the cultivation becomes more general, fuel will then be scarce; and this turf will become a valuable article to the inhabitants of Silhet, although at present they have no idea that it is combustible. In the vicinity of a village called T,hálihar near Daúl, a good strata of turf may be found.

Two of the rivers, the Surmah and Raúli, are navigable for large boats to within two or three miles of the frontier mountains, and are under the influence of the tides nearly as far. My boats swung to the ebb and flood during the springs of January.

A good distance above Maímansinh, the Surmah is navigable for large boats, of one thousand maunds as far as Chattac during the whole of the year, and for small ones from thence through Cachár, and three days journey into Manipúr.

The Baúli river is navigable for large boats all the year round, as far as Seripúr or Cheripúr, and from thence to the Patlij it is nearly dry, except in the rainy months, which commence in April: near Leaúr there are two rivers communicating with the hill rivers, and the Baúli one called Jaducála, the other Dumliah; both partly filled with sand and pebbles, but very wide.

Although lime burning is the principal business carried on in the Silhet districts, there is neither limestone or fuel to be had there; it is all imported during the rains from the mountains belonging to the neighbouring independent rajas: the stones and most of the wood from the Cásia mountains, near Laúr and Pandua; and some wood from Jaíntia and Cachár; and the whole of the timber for building boats from the latter place, with the exception of a small quantity from the southern hills towards Tipara.

The hills near Silhet produce iron ore in thin concretions, like cast iron plates*, yellow ochre, sand, and different kinds of clay; but they all rest on blue clay to a great depth, bearing an alluvial character. At the foot of the hills, to the depth of upwards of seventy feet, soft blue clay appearing as if deposited by frequent inundations.

Opposite Síltéc, in the Cachár Raja's country, where a *chóci* is kept to prevent strangers going to Cáspur, his capital, there is a nullah which runs into the Tipara hills; it is narrow, and has fall sufficient to turn water-mills. The stream is constant, and may be navigated by small boats. At about two miles from the mouth of it, is a cluster of high round hills composed of sandstone and whitish clay, and a little below the base issues a very fine petroleum oil from a stone similar to that termed by the colliers *grey post*: this stone is inclined to blue, and strongly impregnated with oil. I dug four feet through it, and found it turning slaty, which strongly indicates coal to be near. The oil is only to be got supernatant on water, which renders it tedious to collect; however, it might be procured by pumping water into a high vessel, with a number of holes, the water would be drawn off, at the lower ones, and the oil from the top. These hills can be easily found, and the experiment pushed further if necessary.

There is a Musalmán Durga on the summit of one called Arping or Arpín, to which many people resort. The oil will be found in two rocks adjoining to it, and also in another near the plain opening into the Honorable Company's dominions, the road to which is from Badapúr Thanna; I could not carry on my researches at the above place to any great extent without making arrangements, which would occasion expense and great loss of time; and as the season was advancing, I proceeded round the frontier mountains towards Susang: but if desirable, I can give instructions to labourers, under the direction of the magistrate, to examine this place further. The mountains in front, round Cachár and Jaíntia, are in general sandstone, below which, at a great depth, it is probable coal would be found. I have reason to believe the

* Quære? No assistance from MS.—Ed.

mountains in the rear produce limestone. In the small hills, at the foot of the mountains, the ferruginous water running into the sand, concretes into several fanciful forms, and also converts the sand into nodular stones; the outside hard, but very soft within.

At Pandua there is the best situation in India for an iron mill, or mills, for the manufacture of bar, bolt, hoop, or sheet iron. The mill or mills might work at the least nine months in the year, and could be constructed at a very moderate expense. The Cásias smelt the ore, and make it malleable by one process. They take fine granular iron ore, like sand, pound and wash it clean, then mix it up with water into a soft mass, and dip bits of reeds, sticks, or leaves into it, until they take up as much as they will hold, and when become pretty dry, are thrown into the top of a small clay cupola furnace, and melted down. By observing the effects of this process, it will plainly appear that the iron must be tolerably pure. They carry it down to Pandua from the furnaces, and dispose of it at one rupee and five annas per maund, which is much cheaper than it could be procured on the present European method of manufacturing it.

This iron might be collected in large quantities, and forged by mill work into the usual shapes; however I could not advise it to be attempted, unless iron should become scarce. In the hill streams about Pandua, are found fragments that indicate coal to be near their source. In the second range, limestone of a fine quality is found and carried out for sale.

The Beráci mountains contain limestone also, and the small hills at their feet, yield the greatest variety of beautiful plants to be found in one spot, perhaps in the whole world. In the mountains nearly opposite Sólager, there is a petroleum spring similar to that at Arpín; it may be found by inquiring at Bangaú Thanna, as the natives use the oil for embrocations, and in cutaneous disorders. The oil is called *atr*.

Almost all the streams which issue from the Cásia mountains near Laúr furnish abundance of fragments, which shew that the waters must have passed through beds of coal; particularly the kind called cannel. The making of coal tar, beads, cups, and amulets, from the cannel coal, I imagine has been a trade carried on by itinerants and the inhabitants about Laúr for many centuries. Facírs resort to this place, and carry away articles for sale. This coal is called by the natives *pír pathari*, and supposed by them to have originated in Musalmán saints having set fire to a mountain by throwing his sandal upon it, to convince a Hindoo Raja of his power. The source of the cannel coal is not at present known, but the rains bring down a fresh supply every year. I traced it about twelve miles down the mountains. The pieces appeared more nodular there, than some which I saw in the plains: I am of opinion it is brought down by some of the streams which enter the Potta, from the Garo hills; but it has more than one source, as I have found fragments which appeared to be detached much nearer.

Towards Susing, beyond a place called Seripúr or Cheripúr, is a small rivulet coming down from the mountains, in which is traced common coal. The strata which is only nine inches thick, is half a mile within a ravine, dipping seven inches in the foot, which is an unusual circumstance. The roof is of rotten grey sandstone slate, full of mica, incumbent on which, is massy sandstone not stratified. The pavement or floor, hard ferruginous sandstone, and the bed of the stream full of massy stone and limestone, and bituminous iron ore, with various other fragments. I tried to discover this vein in the valley, where it no doubt thickens; but found at twenty-four feet down, marks of the place having been once cultivated. At that level meeting with much water, and considering the great angle of the seam, I judged it fruitless labour, and abandoned it.

This vein was shewn to a Mr. Stark by some natives whom he had employed for the purpose of discovering it. Mr. Stark attended with me for five weeks, being anxious to learn the method of searching for coal, as his business is in that neighbourhood: I also employed four natives who have much local knowledge, and understand the Cásia language, and instructed them during several days how to search for coal. These people found another vein of coal, on the top of the second range of mountains, to which place I immediately repaired, and found the vein to be two feet thick and the coal very pure; but burns rather rapidly, although it cakes. I cleared the face of this vein, and gave Mr. Stark instruments and instructions how to drive a gallery into it, and load a few boats to send down as soon as possible, by which the quality of the coal will be known, and the expense of bringing it out nearly ascertained; but owing to many difficulties to be overcome, I fear it will come too high. This seam appears dislocated in the strata; the roof is tesselated, but in other respects like most coal roofs ; the pavement or floor is very soft, so that it may be cut to let the workmen enter ; but if the vein should not thicken, which however, there is every reason to expect, it will not pay the expense of working.

This vein dips two and a half inches to the foot to the south or nearly so. The ascent and road out is difficult and dangerous, but several other veins will, I trust, be shortly found ; as the people now know what is wanted.

In the Susing river and streams, fragments of common coal are also found, but less in quantity than about Laúr ; fifteen people could only pick up one maund in seven days, and could not find its source.

The Susing Rája advised me not to go into those hills, without a strong force ; and even in that case not to remain long, as the people were very unsettled. Although I saw no necessity for a strong force, I did not conceive it worth while, from the nature of the country altogether, to lose more time, or to incur expense by carrying my researches further in that quarter; as the difficulties (already too many) for the complete success of the business, would constantly increase.

The hills and mountains extending all along the north eastern frontier, as well as those which I saw in the Ganges, are deeply buried by earth and sand ; which I imagine has been furnished from time to time, by the decomposition of other mountains ; and in the rainy season, the country round about them being always inundated, this circumstance is much against a deep mine of any kind being successful. It is remarkable, that in hills on the north eastern frontier, the strata in general run upon one side of the mountain, and dip down the other, keeping up in some degree the shape of the surface ; and even in the plains, where a deep excavation is made, the earth is flat, in other places wavy, but often presenting an arched surface, as if small hills had been buried ; but they are certainly of recent formation.

I have no doubt, that both the coal seams on the hills near Silhet will yet be of great value to that district, particularly as fuel is becoming scarce : coal will shortly be used for the purpose of burning limestone, as well as for other purposes. But for the use of Calcutta, I am of opinion, the Jungle Meháls must still be resorted to for a supply, as the article is there to be met with under the most favourable circumstances, and there is no obstacle whatever, excepting the difficulty of the navigation of the Damúda. Whereas there are many difficulties in the way of importiog coal from the Cásia mountains in great quantities : the natives (females excepted) will work but little, the men being almost constantly at war with each other.

Their war is a war of extermination, without any object in view, except that of decapitating their enemies; the heads of those whom they catch, mostly by stratagem, they stick upon bamboos, and dance with them in the Bazars, begging for what they can get. The Béngali population along the foot of the bills is very thin, and they have a great aversion to go into the mountains ; while labourers brought from distant parts are very liable to sickness during the rainy season, of which the major part of them die.

The uncertainty yet of getting a seam thick enough to work profitably, and the distance from Calcutta being so great, that a boat at the utmost could not make more than three trips in a year, makes the business still more doubtful: the expense of carriage from that quarter would be great, as it is at present sixteen rupees per hundred maunds, and were more boats employed, this would considerably raise the price of lime (already too high.)

A considerable sum of money is at present sent out of the British territories for wood, iron, and other articles for building boats used in the transportation of lime, and in the event of the coal trade being carried on, this would also be considerably increased.

The hills on the north-east frontier furnish the following articles of trade, viz. red *jarul* wood for boat building, limestone, fire-wood, iron, wax, honey, pepper, cotton, betel nut, pawn, lemons and oranges, rattans, bamboos, ivory ; *tézpát*, an aromatic leaf like cinnamon, used in curries ; *agger*, a substance used for frankincense : it is deposited by a worm which bores its way into a forest tree, and the tree is cut up into chips, and the part collected through which the worm has passed. The natives take in return, money, rice, fish, fowl, and eggs.

Having to the utmost of my knowledge, stated the foregoing observations, I shall here add a few words regarding a plan by which the coal trade in the Jungle Meháls may succeed and become profitable, notwithstanding the objections which I made to it last year.

I propose that a few boats, some of three and others of four hundred maunds, be built on the banks of the Damúda, of Ramgerh *Sál* and iron, and that a few of the Burdwan boats (whose people are in the habit of navigating that river), be hired for the transportation of the coal. That a mine shall be opened on a cheap and simple plan, where the best coal is to be found, and carried to the bank of the river, during the dry season. That immediately upon the opening of the river, the new boats shall drop down, and the hired ones go up and load with coal as quick as possible, and proceed down with it to Amptha, and unload there; return empty, or take grain or other articles up, which would pay freight; load again, and continue in the same manner during the period which the river should continue navigable: and when the Damúda become not so, the whole of the boats should then load at Amptha, and be employed during the dry season in transporting the coal to Calcutta. By this method, the boats would be employed the whole year, and I believe they might make six at least, if not seven trips annually.

I would dispose of the new boats at Calcutta whenever a good price offered, and replace them from the building establishment.

The business ought by all means to commence upon a simple and cheap plan, and if found profitable, many useful contrivances may be introduced to facilitate the business of loading and unloading; and also in that of land carriage, which after a little time, may, in a great measure, be obviated.

Note by the Editor.

Mr. Jones appears to have been deficient chiefly in technical knowledge, the want of which may possibly occasion the scientific reader to undervalue the preceding paper, although it contains many hints which by the practical man will be duly appreciated. The result of the proposal, with which the paper concludes, was the opening of the coal mine at Raniganj, described in our preceding number, from which, and anothe mine in the vicinity since opened, Calcutta derives her whole supply of coals.

II.—*Experiments on Evaporation, made in the Vicinity of Calcutta.*

The amount of aqueous evaporation from the surface of the earth in different climates and situations has long been a subject of interest and experiment: but it has been fairly doubted whether much confidence can be reposed in experiments on such a minute scale as they have usually been confined to. The sun has more power upon a vessel of a few inches diameter and an inch or two (or less) in depth placed on a dry surface of wood, masonry, or sand, than upon a large surface of water, whether deep or shallow; the wind less, perhaps, except in so far as the larger body of water may affect it hygrometrically.

The enclosure of about 100 bigahs of dilute sea water cut off from the Lake at Ballyaghat a few months since for the manufacture of salt by solar evaporation, seemed to give opportunity for obtaining results more satisfactory. Having for another object registered almost daily the specific gravity of the water there and in several subdivisions of the ground, with the depth by estimate or fixed measure, I have thought it worth while to make them available for ascertaining the evaporation which occurred at Ballyaghat during the period they embrace.

Water marks were not fixed till the first week in April. The best and easiest method (measure of height,) is therefore only applicable during eight weeks of April and May. Comparing the specific gravities of the same spaces at different periods when the water in them remained undisturbed, I find an average addition of In. 1, 6 to the depth by mark requisite to make the increase of density correspond with the diminution of volume during those two months. This might be partly owing to errors of measure, but is sufficiently accounted for by the water contained in the very loose soil below, which must, to a certain degree, intermix with and affect that above it. Considering however that on digging three feet below the surface in November, I found water of the specific gravity of 1028 in the middle of the ground then laid dry, and that few of the flooded spaces were raised to that density before April, it is probable, the ground influence was minus in January, null or balanced in February, and at most one inch in March. For the last month I make this allowance accordingly in the following table, which gives the evaporation for the first five months of the year with more or less accuracy, as the spaces and days of trial were many or few.

Month.	Sum of days.	Number.	Spaces of trial, extent in square, feet.	Mean depth.	Mean specific gravity.	Method.	Evaporation per diem.	Remarks.
				inch.			inches.	Towards the end
January,	7	1	150,000	4.	1007.5	Sp. Gr.	.18	of the months and
February,	42	5	500,000	3.	1015.5		.1625	not satisfactory.
March,	190	11	850,000	4.	1024.		.232	Added 25 P. Ct.
April,	75	8	540,000	3.4	1052.	Wr. Mk.	.23	for ground influ-
May,	79	7	443,000	4.5	1066.		.304	ence to .1859.
Mean of,	393	7	497,000	3.8	1037.		.2226	
excluding Jan.	386	7	583,000	3.7	1039.4		.232	

In forming this table, I have rejected all observations which gave manifestly false results. The double test of depth and specific gravity was a good means of detection, and it did sometimes happen that leakage occurred when there was considerable difference of level between contiguous enclosures. By considerable, I mean three inches and upwards. Errors of small amount may have arisen from inaccurate estimate of the rain, which, falling in showers of partial influence, could not be correctly estimated by a rain-guage, stationed in Calcutta, there being none at Ballyaghat. In most cases however, and those the most important, the showers were computed next morning by the water marks of the factory. One enclosure (No. 5 E. of 36000 square feet) so situated as to lose nothing by soakage, remained shut from the 10th to the 28th May, during which time its depth of water was reduced from inches 6. 9 to 2. 4, notwithstanding 4 showers, computed at 1. 2. The daily evaporation therefore in that interval, appears to have exceeded 3. (three tenths of an inch) from sea water of average condensation 1072, and ranging between 1056 and 1120. This far exceeds the fresh water evaporation from Mr. Kyd's wooden cistern of 100 square feet and nine inches mean depth, which served for his experiments at Kidderpore in 1821 and 1822. Indeed, all the above results are much in excess of his, except as to the month of April; and I must remark, that the season was not a particularly hot or dry one, certainly not in May, which had 11 days of rain this year. I have often suspected that the locality of Kidderpore, and the circumstance of the cistern not having been kept full (the sides averaged about three inches above the water) might have been unfavourable to evaporation to the extent of even 20 per cent. or more. I believe also it would have been somewhat greater if the depth of water had been less.

I expected to find a higher ratio of evaporation on the two large tiled terraces constructed for condensing the salt at Ballyaghat, each 35000 feet area, as the depth of water upon them in most parts did not exceed an inch to an inch and a half, its temperature, under the action of the sun, ranged from 100 to 110 of Fahrenheit, and sometimes higher. In this I was disappointed, which I attribute to their low level, having traced lateral and underground pressure in two or three places. But the passage of water from outside, and from one part of the terraces to another, was so frequent, that little faith can be placed in calculations founded on my registered notes, as far as they are concerned. Rejecting a large proportion of them manifestly unfit for our present purpose, I find the following results.

Month.	Days.	Spaces.	Depth.	Sp. Gravity.	Method.	Daily evaporation.	Remarks.
March,	24	2	2.5	1047	Specific Gravity.	.144	The platforms (or terrace
April,	19	2	1.5	1085		.150	divisions) were usually cover-
May,	33	10	1.	1144		.189	ed to the width of 10 or 11 feet each, leaving 3 or 4 feet dry between them.
Mean of day,	76	5	1.7	1092		.161	

which compared with the previous table in the corresponding months (.255) shews a difference not to be accounted for, by an excess of .045 in the saltness of the water. Several observations of May give .2 in ⅔ds. brine, and one of them .22 in water, at 1110. A better criterion of the evaporation which really takes place under such circumstances is the fact, that at the salt works at Ganga Ságar on the 13th May, 19 maunds of salt were obtained from three platforms of similar construction covered with brine of an inch to an inch and a half deep on a surface of 1900 feet each; and on the 25th, 50 maunds were collected from 7½ platforms flooded to the depth of ⅝th of an inch over a space of 1800 feet each. In the former case the evaporation seems to have been about .17, and in the latter above .18 from actual brine, in a situation more humid and of lower temperature than Ballyaghat.

It occurred to me that I might obtain a proximate ratio of the aggregate evaporation from these brine fields by comparing the volume of water in them at different periods. Excluding the two larger outer divisions, liable to be affected by pressure through the bunds and under ground as the tides rose or fell, even when the gates were shut, I find that in 12 days from the 8th to the 20th May the mass of water on a space of 860000 square feet, was reduced from 377500 to 189750 cubic feet, to which adding 1500 for 65 maunds of salt, reduced into brine of S. G. 1035, we have a perpendicular loss of inches 2.64
Add rain on 12,th 14th and 17th, .9

$$3.54 \div 12 = .295 \textit{ per diem},$$

Again, rejecting the largest of the divisions above included (No. 2 W) the mean depth of which it was most difficult to compute, we have on a surface of 660000 square feet, a reduction of volume from 277500 to 156417 cubic feet, which with 1500 for the salt as before, gives a perpendicular decrease of in. 2.23
Add rain, ...9

3.13	In.
and 3.13÷12 gives *per diem*,	.261
To this however, must be added a correction for pressure from No. 2, W. through the bunds of the contiguous enclosure No. 3, 4, and 5 W. which in the whole term was certainly not less than one inch, say $1 \times \frac{149000}{660000} \div 12 =$	.019
the minimum result being	.28
A comparison of specific gravities applied to the whole space of 860000 square feet during the same period gives *per diem*, ...	.117
Add rain, ...	.075
	.192
and ground influence, inches 1.6 = ⅓	.064
	.256

But here the causes of error are more complicated, as the experiment includes large shallow spaces of unequal specific gravity. An addition of .002 to the mean difference, would raise the apparent evaporation to .3 *per diem*.

This last method of comparison I have applied to the deeper masses accumulated when the tide gates were opened at the close of the season, to let in water in reserve for the next.

On the 31st May, I found on the 2 terraces a volume of 64000 cubic feet at 1067 surrounded by water, then every where reduced below 1050. In condensation from 1050 it must have lost about 22000 feet, besides about 2000 in the production of 150 maunds of salt, since the dates to which we have back to ascertain when water exceeding 1050 began to appear in other divisions. The evaporation of these 24000 cubic feet of water appears to have occurred in 11 enclosures of the aggregate extent of 530000 feet, during a mean term of rather more than 20 days (the sum of days × the spaces being 10847000,)

which gives *per diem*, ...	.0265
Add rain computed for each division, ...	.1129
	1394

Brought forward, .1394

It is obvious, however, that the water of one field would not pass bodily ioto the next, without mixture with that, which by its rapid entry at the same time, pushed it forward. The error on this account must be very great, for we have sought in two divisions only (the terraces) the previous contents of 8, which were simultaneously open to the passage of water at 1040 and under, which 8 divisions contained about 70,000 cubic feet of brine, at about 1070 the previous day; so that less than a third had passed into the terraces, and that partially diluted; add therefore, $\frac{.0265 \times 70\,000}{22\,000}$ .0843

aod for reduction from 1070 to 1067 $.1108 \times \frac{3}{67}$ .0050

.2287

Correction for filtration from Nullah into No. 3	.004	
2 days of evaporatinn lost in do.	.002	
influence of grouud—perhaps,	.0053	
		.0113
		.240

By traciog back to a low degree of specific gravity, it was evident the large correction for mixture of waters would be so reduced, as to become of small moment, and perhaps might be disregarded. A similar method of calculation was, therefore, tried with the registers of the days undermentioned, comprehending all above 1021, the greatest range I could take, without including the 2 outer fields: it gives the following table of evaporation from 15 divisions of 86,1000 feet surface, averaging about 2 inches deep.

Date.	Surface in square feet.	Volume in cubic feet.	No. of div.	Mean specific gravity.	Volume lost by evaporation.	Mean time.	Daily loss in perp. inches.	Corrections for Rain.	Corrections for Leakage.	Corrections for Ground.	Corrected daily evaporation.	
					cubic ft	ds						
1st Ju.	905 000	484 000	14	1041.5	388 000	60	.091	.080	.011	.011	.193	
2d Do.	760 000	772 000	14	1031.4	319 000	..	.075	084	.011	.007	.177	
4th Do.	464 000	740 000	11	1029.	283 000	..	.066	.120	.010	.004	.200	
7th Do	834 000	1276 000	15	1028.	421 000	64	.093	.109	.010	.005	.217	217
13th Do.	974 000	1348 000	16	1028.	586 000	70	.117	.100	.010	.007	.234	
Mean of the 5 trials.							.0884	.0986	.0104	.0068	.2043	

In each case, allowance has been made for 452 maunds of salt not included in the estimate for the volume of water. The state of the weather between the 31st May and 3d June, was such that I considered the intervening evaporation might be disregarded; but the days which followed have been reckoned in computing the averages after the 4th. The apparent increase in the results obtained after the 2d June, is perhaps attributable to the more perfect mixture of the water, especially that which the ground previously contained of higher specific gravity; and it is probable on that account that the last result (.234) was a more correct appreciation than the mean of the 5 experiments, or even of the 3 last. Comparing these of the 4th and 7th Jnoe, there would appear an intervening evaporation of .496 *per diem,* and between the 7th and 13th .338. The former is no doubt much beyond the truth, owing to the cause already assigned; whereas up to the 2d June inclusive, the tide-gates continued open, and there were also several showers, which might have affected the surface disproportionally, from whence alooe my Hydrometer was supplied. The ground influence I have taken equally at 1 inch. The correction is trifling in this table, but I am not sure that it ought to have been introduced at all.

To sum up the general result of my experiments, I think it may be concluded from them, imperfect as they are, that a greater evaporation occurs in the neighbourhood of Calcutta, than Mr. Kyd's experiments had led us to suppose; and that the following rate for fresh water and even for sea water, not stronger than the ocean, is not too great. In the months of

January,	3 inches or p. diem	.1 instead of	1 inch as per Mr. Kyd's report, or p. diem	.032
February,	5	.18	2	.071
March,	7	.225	5	.161
April,	9	.3	7.5	.250
May,	9	.3	6.5	.194

If I have time and opportunity to continue and improve the series of my observations at Baliaghát, after the present rains, you shall hear from me again.

Calcutta, 25th July, 1829.

Note by the Editor.

In the foregoing paper,—which may stand as a record, that the difficulties and uncertainties which beset an experimentalist in operations upon a very large scale, are fully equal to those of what has been termed the THIMBLE SCHOOL, one point is wanting to render the facts of general application, we mean, the *state of humidity of the atmosphere at the time.*

Our journal has already devoted so many pages to the subject of hygrometry, one of the principal desiderata of which is the measure of evaporation, under all circumstances, that we much regret not being able to direct our friend's valuable information to this branch of our discussions. We hope, however, that in the ensuing season, he will complete the object by registering the depressions of the wet bulb thermometer in the neighbourhood of the salt marshes.

III. *Tables exhibiting a daily Register of the Tides in the River Húgli at Calcutta, from* 1805 *to* 1828; *with Observations on the Results thus obtained. By* James Kyd, *Esq.*

[From the Trans. As. Soc. vol. xvii. part I.]

Having kept a register of the day and night tides in the Húgli, at Kidderpúr, near Calcutta, since the year 1806, for which the nature of my business, and my establishment afforded me facilities; and the permanency of my gauge fixed at the dock-head rendering the results correct, beyond suspicion or doubt; I am induced to lay them before the Society, trusting that they may prove interesting.

To avoid the dry detail of a daily register, I have drawn the heights of the tides in maps, shewing the state of the river throughout the year, conveying at a glance, all that is requisite to be known for every useful purpose.

The Map No. 1, is made for the year 1806—7, and I have upon the same map, traced the tides for the year 1825—26; the phases of the moon in the latter year falling nearly on the same days, and thus enabling me to give two years together, and to shew the variation between them, at a distance of nineteen years.

Map No. 2, is similar to the first in principle, but has been chosen to shew three distinct inundations, that took place in the year 1823; the first of which was occasioned by the sea, and the second and third by the Ganges and Damoda rivers. The first was a very rare occurrence, happening not oftener, perhaps, than once in a century; the last nearly as rare: but the second occurs every sixth or seventh year.

Map, No. 3, consisting of twelve parts, one for each month, is a daily and nightly record of the river, for the said remarkable year 1822—23.

Map, No. 4, gives a comparison of the range of high and low water for successive years, from 1806 to 1827.

These tide-tables, formed from a register kept for twenty-two years, establish beyond dispute, the lowest fall and the highest rise of the Húgli, and thus form natural points for the construction of a river gauge, for the purpose of obtaining, at all times, the levels that may be required for the formation of canals, docks, wharfs, and drains. They also shew the height of the river at all times of the year, a matter of considerable importance in the formation of public works, especially as the variation is so great, at its different periods.

I shall now advert to the local causes which affect the tides in the Húgli. The maps commence with March, in the beginning of which the South-west monsoon sets in. With the South-west winds, the currents set up the bay of Bengal, and gradually raise the sea, at its head, several feet, raising with it the Húgli, long ere the freshes are felt. The dotted curve line in map No. 1, will shew this rising of the sea and river, by the wind and currents. This cause continues till October;

the pouring of the rivers into the bay of Bengal, during the months of August and September, and the change of wind at the end of October, give the currents a set in the contrary direction, and gradually restore the sea and the river to the state they were in, in March.

The effect of the two monsoons upon the currents, and the height of the sea, in the Bay of Bengal, may, therefore, be considered as that of two long unequal tides, during the year, eight months of flood, and four months of ebb.

In conformity with these periodical local causes, partial ones have a corresponding effect: thus strong southerly winds raise the tides in the Húgli, whilst northerly ones depress them.

The freshes, or floods of the rivers, are a prominent periodical local cause, operating upon the tides of the Húgli at Calcutta.

The Ganges begins to rise from the melting of the snow, as early as the beginning of May, but its rising does not sensibly affect the Húgli till the beginning of July; at that period, so large an accession of water is thrown into the Húgli, that its level is bodily raised both at high and low water. The last is so remarkable, that the low water of the freshes (neap tides) is higher than the high water (neap tides also) of the dry season, by several feet.

The Damoda and western small rivers, or mountain streams, contribute very materially to the swelling of the Húgli; and it is, probably, the influence of the Damoda, the Rupnarain, the Tongoracolly, the Hidgelee, and even the Balasore river, (the latter situated beyond the mouth of the Húgli,) that occasions the height of the low water, by their acting as a dam, and preventing the ebbing of the waters from the Ganges, and higher streams, quickly into the sea.

There is another local affection of the tides, the cause of which I cannot satisfactorily explain. In the North-east monsoon, the night tides are the highest, whilst in the South-west monsoons, the day tides are the highest.

A conjecture may be hazarded, that as, in both monsoons, the wind is generally higher during the day than in the night, that the wind in the South-west monsoon raises the day tide, whilst in the North-east monsoon the wind, during the day, withholds and depresses the day tide; but that is not entirely satisfactory, in as much as the wind cannot possibly be uniform, whereas the fact of the higher tides during the day in one monsoon, and during the night in the other, is beyond doubt; besides, the latter is very much more than the former, being as much as two feet, whereas the former is seldom more than one foot. The night tides in the North-east monsoon are also more uniform, in this respect, than the day tides in the South-west monsoon.

Should it appear from future observation that the wind be the cause, it will prove that the depressing effect of the northerly wind, has much more influence upon the tides, than the increase by the southerly ones; or it may be, that the absence of the wind leaves the tides more freedom to act. I come now to general causes.

The horizontal parallax of the moon invariably affects the tides; when that is high, the tides are high, and *vice versa*, to such a degree of correctness, that allowing for local causes, I could venture to construct a table for a year in advance, that should not vary two inches, from the actual tides.

When the parallax is highest, on the second, or third day, after the full or change of the moon, the highest tide will correspond with these days, as that is the natural period of its greatest height: should the parallax be decreasing, the highest tide will be on the day of the full, or change; and should the parallax be decreasing, and near to its lowest, and increase again after the natural period has passed, the highest tide will be on the fourth day, after the full or change of the moon.

The difference of effect between the high or low parallax of the moon, upon the height of the tides, is about two feet, frequently much more; and as its variation, as to the time, is shewn to be four days, this is of importance to all mariners, as enabling them, in cases of danger, to ascertain by their nautical ephemeris, the true state of the tides. No longer need they trust to the partial observation, and equally partial theory founded thereon of pilots and seamen, most of whom have a notion that the dark spring tides are always the highest, that the night tides are higher than the day tides, and that the highest tide must always occur on the second or third day after the full, or change; whereas the parallax of the moon will effectually supercede this uncertainty, and either warn a mariner with his bark on a shoal not to wait till the second day, and lose the springs, or save him from despair, because these days may have passed, and induce him to wait with confidence till the fourth day, after the full or change, for the highest tide, as the case may be.

The parallax of the moon will assuredly indicate the height of the tides all over the world; this general cause, therefore, must be applicable at all places. The following abstract will be useful, as conveying a general summary of the tides of the Húgli. From the point of lowest low water in the dry season, to that of the highest high water in freshes, is twenty feet ten inches.

The greatest mean rise of tide from low to high water mark, takes place in March, April, and May, and is fifteen feet ten inches.

The greatest mean rise of tide from low to high water mark, in the freshes, is ten feet.

The smallest mean rise of tide takes place in the freshes, and is, at neap tides, only three feet six inches.

The smallest mean rise of the tide in the dry season, neap tides, is four feet.

From the lowest fall of the river, to high water mark, neap tides, in February, is eight feet.

From the lowest fall of the river to low water, in the freshes, neap tides, is twelve feet*.

The river is at its lowest, in the beginning of March.

The river is swollen by the freshes in July, August, and September, and part of October.

At the beginning of November, although the freshes are out of the river, it is upwards of three feet higher at low water, than in March.

The river is in the most quiescent state, during the months of November, December, January, and February; during these months the night tides are higher and more rapid than the day tides, and there are, on some occasions, bores at night.

The strongest flood tides, and the greatest mean rise of the tides, are in March, April, May, and June. The day tides in these months, are higher, than the night tides.

The strongest freshes are in September.

In July, the strength of the flood tides is counteracted by the freshes, and this, therefore, is a moderate month, as regards tides. The bores also are moderated as a consequence.

In August, the flood tides are overcome by the freshes, and the bores are moderate; should there be a high parallax of the moon, however, the great height of the sea in this month, will cause a considerable bore.

In September, the freshes are at their height; there is no visible tide off Calcutta, the ships do not swing up, and the river water is perfectly sweet, far beyond Saugor in the open sea. The high parallax of the moon at the equinoxes, with the great height of the sea, produces a heavy bore in this month.

The Bores. The bores in the Húgli occur only on the highest, or at alternate spring tides: their appearance may, with certainty, be predicted by the season of the year, and the parallax of the moon. During the months of November, December, January, and February, or on the periodical ebb of the sea, when the currents are settting down the Bay, the tides, as may be supposed, are languid, and consequently during this period there are no bores†.

As soon as the south-west monsoon sets the currents up the Bay, the sea begins to rise, the tides become strong and high, and bores follow in their train; whenever the parallax of the moon is high on the springs during the south-west monsoon, bores will certainly make their appearance; and when strong southerly winds are added, and freshes withheld, the height of the bores will be increased.

It must be remembered, that the height of the bore, is actuated by the peculiar form of the sands, and the direction, and set of the tides, in any particular reach

* During the inundation in September, 1823, the low water stood at eighteen feet six inches, the tide having ebbed only fifteen inches on that day. The difference between this low water, and the high water (neap tides in February, viz. eight feet,) is ten feet six inches!!

† Except very rarely—In twenty-two years, I have known but three instances. Agreeably to the statement of local causes which accelerate or depress the tides, it will be obvious, that during the North-east monsoon, if the winds which blow the waters down the Bay, be more than usually moderate, and the moon's parallax be high, there may be a high tide, and with it a bore; and this, agreeably to the third local cause, will happen at night. These night bores are particularly dangerous, as they are very rare, and, consequently, unexpected. They may be guarded against, by always considering it possible for them to occur during the North-east monsoon at night, upon a high parallax of the moon. Agreeably to the same local cause, it is fortunate that the bores at night, during the South-west monsoon, are not so high as during the day.

of the river; for instance, where the channel is straight, with deep water, from side to side, and no sand-bank, there will be no bore at any time; but a mere swell on the coming in of the tide. This is the case at the lower part of Garden Reach, opposite the Botanic Garden. This is the case also off Calcutta at Howrah Ghaut, where the back channel having lately filled up, the main channel is now confined between high banks. It is only where the main channel lies on one side, with a low sand on the other, that the bore shews itself upon the latter. This, a very few years ago, was the case opposite to Calcutta, and there was at that time an enormous bore, but which, as above explained, exists no longer.

IV.—*On the first Introduction of the Lithographic Art into India.*

[In a letter from the Superintendent of the Government Lithographic Press.]

In our 2d Number, we published a short notice on the rise and progress of the Lithographic art in India. New to the subject ourselves, we were not judges of its correctness, nor did we doubt but that it gave a fair and faithful account of the subject. We have understood however that such is not the case, or at least that the claims of one individual deserving of more extended notice are past over with slight mention, while the honor of introducing the art into this country is given to one who, whatever his other merits, is not entitled to this particular credit.

We have been favored with a sight of the following letter, written on the occasion, by the gentleman alluded to, and we gladly avail ourselves of the permission of the person to whom it is addressed, to publish it. Our first wish is, of course, to repair our unintentional mistake; but we think, independent of this consideration, our readers will find it sufficiently interesting.

My dear Sir,

You have expressed a wish to obtain some particulars regarding the establishment of the Art of Lithography in India; and as you are a member of the Lithographic Committee, I feel it as well a matter of duty as of pleasure to comply. With regard to the use that you may wish to make of this communication, I have only to say that as far as I am myself concerned you are left perfectly at liberty.

In a letter such as this, written during the few moments of leisure left me by the duties of my department, and under a hundred interruption , you will not expect to find more than a simple detail of facts; which, if it appear rather frequently to be occupied with my own proceedings, will not, I hope, be more so than the nature of the subject demands. For the style of a private letter I shall not make any apology: suffice if I am intelligible.

Early in the year 1821, I returned to Europe for the benefit of medical advice, having unfortunately lost both health and hearing by exposure to the inclemency of all weathers in the discharge of my professional duties, during the Pindara and Mahratta campaigns of 1816, 17, 18, and 19. Finding from the opinion of the most eminent practitioners in London, that the recovery of my hearing was hopeless, I proceeded to Scotland. Soon after my arrival there I had a relapse of my fever, and for the sake of obtaining the advice of my former preceptors, I repaired to Edinburgh. After a partial recovery I was on the eve of departure (intending to make a round of visits to the families of my several Indian friends), when I accidentally met with an old schoolfellow Mr. Alexander Forrester.

In the course of conversation he mentioned, that amongst other occupations he had been practising Lithography with some success, and asked me if I knew any thing of the art. To this I replied in the negative, adding that, from the little I had read of it, it appeared to be of no particular interest or importance. Perceiving that I had no very precise notions on the subject, he offered to exhibit the whole of the process to me, and explain the rationale; and we accordingly adjourned to his printing office.

To render the exhibition more striking, he requested me to write on the transfer paper any word or sentence. Being done, it was transferred to the stone, and in a few minutes several impressions were struck off.

All this was effected with such precision and rapidity as to draw many expressions of admiration from me. It immediately occurred to me, how peculiarly adapted the new art appeared to the wants of India, more especially in printing the

Oriental character, a thing yet only partially effected with type*. Having given utterance to my thought, my friend replied, that the same idea had often struck him, but that he doubted whether the extreme heat of the climate would not prove an insurmountable obstacle to its successful practice in the East, and that, were it even otherwise, the introduction would be attended with too much labour, and too much expense to permit him to engage in the project. He then threw off some impressions from highly finished works, both of writing and of drawing, with which I was greatly delighted; and in fact, the more I saw of the capabilities of the art, the more I felt impressed with the desire to impart it to India. In the course of the evening it was finally arranged, that giving up all idea of my projected tour of visits, as well as of every other recreation or pursuit, I should immediately commence the study of Lithography under Mr. Forrester, and having obtained from him a competent knowledge of the art, that I should immediately embark for India. In pursuance of this resolution, after properly qualifying myself, I purchased the requisite apparatus, and sailed for Calcutta, where I arrived in 1822.

You will readily believe, that on my landing here, my first inquiries were directed to learn how far, if at all, I had been anticipated in my project, and whether Lithography had been yet practised as an art in this country. And you may estimate the satisfaction with which I learned, that I was the first in the field, and had no competitor to dispute with me the merit, (whatever that may be,) of first introducing an art so admirably suited to the circumstances and condition of the country. Some days afterwards, however, on showing some of my productions to the late Dr. Jamieson, he, although approving of them, expressed his fears, as many of my friends had done at home, that the art could never be practised in India with any thing like efficiency, adding that M. Savignac had been making some attempts but without success.

I immediately called upon M. Savignac to ascertain exactly what progress he had made. I found that he had taken a solitary impression of a chalk sketch of a head, the stone having, by the injudicious pressure used, been broken. He had reunited the two pieces by means of a cement, and had made another sketch. All this had been done with materials received from Europe, he being entirely ignorant of the method of preparing them, the real secret of the art. In short he knew nothing of Lithography, not even the most elementary part of its processes; as an instance, I may mention, that long after the period I am speaking of, he would sometimes etch a fine drawing that had cost weeks of labour with pure nitric acid, and finding that rather too powerful he dispensed with acid altogether, failing thus in both cases. How then can it be said, that he was the first to introduce Lithography into India? So far from it that he was not himself in possession of the art, he never even thought of any thing beyond a chalk drawing. And I do not believe that he is at this day a lithographer. Before he possessed a press, I was studying the art, with a view to practise every branch of it in India that is practised in Europe. I had embarked my property in the scheme; had sacrificed all my private arrangements, and had at last reached this country, a proficient in the art, with a most effective apparatus, not only before he had done any thing, but even before he had himself acquired, what it would appear from a statement recently published, he had been the first to impart to others. And that I was the first to apply, or even think of applying Lithography to the printing of Oriental characters, one of its most important applications, is, I believe, a statement that no one can controvert.

Soon after my interview with M. Savignac, the Governor General honoured me with a private audience, and I then took the opportunity of proposing the establishment of a Lithographic press for executing Government work. Mr. Adam acknowledged that he was quite unacquainted with the nature of the art, but judging from my descriptions he was pleased to express a wish that it should be brought before Government in a public shape. I immediately drew up a prospectus; stating the advantages to be expected in the formation of a Government Lithographic establishment; and a committee was immediately ordered to enquire into and report on my proposition.

The opinion of the committee was highly favourable to the plan, and Government, in consideration of their unqualified approbation of the measure, determined to establish a lithographic press, and to give me the superintendence of it. My labours now became more arduous than ever, for I had to instruct subordinate agents in all

* In the case of the Nishki, Nagri, and Bengali characters. Of these the two last are perhaps best suited to type. The elegant Nastálik can have justice done it only by Lithography.

the several details; and above all, to form a body of native printers. For many months too I was compelled to perform with my own hands much of the most laborious part of the work. I have been not unfrequently engaged in this way from 6 o'clock in the morning till 1 o'clock the following morning, having scarcely time even for my meals, and for recreation or refreshment out of doors none whatever. Let it then be judged, to whom in reality belongs the credit of having introduced Lithography into India. The exertions I have described could not continue long; they brought on a relapse of my fever, (which had very nearly proved fatal;) yet not before I had sufficiently instructed others, so as at least to enable them to carry on the current business of the establishment during my illness.

Sometime after this a Lithographic press was established at Bombay, chiefly on the statement of its advantages set forth by me in a prospectus which I forwarded to the Honourable Mountstuart Elphinstone, then Governor. It did not, however, as I had predicted in a private letter, succeed, owing to the inexperience of the gentleman under whose superintendence it had been placed. The Bombay Government at length adopted the suggestion I had repeatedly urged, and sent round the superintendent here to acquire from me a competent knowledge of the details. He resided with me for some weeks, and on his return to Bombay I was gratified to learn that every thing was going on favourably, and that the establishment was in the fullest sense effective. His death, however, which occurred soon after, threw every thing into confusion, and the establishment has been in consequence abolished by order of the Court of Directors.

My friends, observing the success and efficiency of the Government establishment under my direction, strongly urged the expediency of my forming one for the accommodation of the public at large; I accordingly wrote to England for the necessary apparatus, and on its arrival, I placed the whole under the direction of Mr. G. Wood, to whom I offered half the concern for the trouble of management. This is the Asiatic Lithographic Company's establishment, and it forms with the Government one, the only two effective establishments at this moment in India: of their capabilities I leave you, who have seen both, to judge; and I would ask you, with these facts before you, to whom you consider fairly due the credit of having introduced this art into the country. I have shown that even in point of date I was *the first* to take any steps towards this important object; while no one can doubt that I was not only the first to establish an effective press, but the only one.

As to the claim set up in Paris in favour of M. Semelet as being the first to apply Lithography to the printing of Oriental characters, it will, probably, be considered sufficient for me to state, that in August 1822, I exhibited before Dr. Lumsdaine and a crowd of natives, specimens of oriental Lithographic printing. Whole works in the Persian character were published at the Government press and at my private establishments, long before the subject was even thought of in Paris. I may add as connected with this notice, as well as affording a good instance of the great utility of the Lithographic art, that I printed several copies of the declaration of war with Ava, at a time when the Avanese and Burmese character could not so much as be read in Calcutta.

Govt. Lith. Press,
August 1829.

Believe me, &c. &c.
I. N. Rind.

V.—*Examination and Analysis of some Specimens of Iron Ores, from Burdwan. By* H. Piddington, *Esq.*

[From the Asiatic Researches, vol. xvii. part 1.]

In the following analysis of iron ores from Burdwan, much care has been taken to ascertain correctly the presence and quantity of phosphate of iron and manganese, which two substances principally affect the qualities of the iron when smelted. The process was conducted in the humid way, and the separation of the manganese was obtained by Mr. Faraday's method, digestion of the oxides in a solution of muriate of ammonia with sugar.

Between Jamde and Sukhraj. No. 7. Sp. Gr. 3,144.

Phenomena with the Blowpipe.—Acquires a metallic tarnish and a slaggy porous appearance, becomes magnetic: with borax or charcoal, fuses into a dark and dirty green glass. The blue laminæ burn with the scintillation peculiar to iron, become porous, and have a metallic tarnish; they appear to be a deut-oxide of iron.

When calcined, the pulverised ore, which is of a yellow brown, changes to a deep chocolate red, probably from the privation of the carbonic acid.

Constituent parts.

Water and carbonic acid,	8,50
Silex,	4,00
Alumine,	4,75
Carbonate of Lime,	5,15
Deut-oxide of Iron,	76,00
Oxide of Manganese,	1,55
	99,95

Note.—This specimen probably contains from 28 to 60 per Cent. of Iron, for the portion analysed was found, by digestion in nitric acid, to acquire 8 per cent. in weight, probably from the peroxidation of the blue laminæ.

No. 2.—*No Label with this Specimen. Sp. Gr.* 3,081.

Phenomena with the Blowpipe.—Becomes magnetic with a metallic tarnish, fuses with horax into a clear bottle-green glass.

Constituent parts.

Water,	5,75
Silex,	3,20
Alumine,	0,40
Lime, with a trace Mag.	1,00
Oxide of Manganese,	4,00
Peroxide of Iron,	85,30
	99,65

Note.—I refer this specimen to scaly red iron ore, or iron froth of Jameson, vol. iii. p. 208.

No. 3.—*No Label with this Specimen. Sp. Gr.* 3,400.

Phenomena with the Blowpipe.—Becomes magnetic, and fuses with borax into a very dark and somewhat dirty green glass.

Constituent parts.

Water,	6,25
Silex,	8,50
Alumine,	0,50
Lime, Phosphate of Iron,	Trace
Oxide of Manganese,	0,0
Peroxide of Iron,	84,50
	99,50

Note.—Ochry red iron ore, or red Ochre of Jameson, vol. iii. p. 210 (?)

No. 4.—*Mal Chaiti. Sp. Gr.* 3,141.

Phenomena with the Blowpipe.—Becomes magnetic, and acquires the metallic tarnish: with borax on charcoal, fuses into a pitchy slag.

Constituent parts.

Water,	6,00
Silex,	4,50
Alumine,	1,75
Carbonate of Lime,	3,35
Oxide of Manganese (red),	16,00
Peroxide of Iron,	68,00
	99,60

Note.—The large proportion of Manganese in this specimen is remarkable; but the process used for obtaining it leaves no doubt as to its identity, for the solution of muriate of Ammonia will not dissolve oxide of iron. It may be found useful to mix with other ores, which may thus afford better steel than they otherwise would. See Jameson, vol. iii. p. 232.

No. 5. Paolta Kanowa, Sp. Gr. 3,587.

Phenomena with the Blowpipe. Scintillates, becomes magnetic, and acquires the metallic lustre; with borax on charcoal, fuses, with slight ebullition, into a very opaque green glass.

Constituent parts.

Water,	7,00
Silex,	7,90
Alumine,	0,60
Lime,	0,00
Phosphate of Iron,	Trace
Oxyde of Manganese,	10,25
Peroxyde of Iron,	74,00
	99,75

No. 6, no Label with this Specimen, Sp. Gr. 2,857.

Phenomena with the Blowpipe. Becomes magnetic, and externally, of a metallic lustre; with borax on charcoal, a dark enamel.

The pulverised ore, like No. 1, is of a pale yellow brown, changing to a deep chocolate red in calcination.

Constituent parts.

Water and Carbonic Acid,	9,50
Silex,	27,50
Alumine,	1,50
Oxyde of Manganese,	9,00
Deut-Oxyde of Iron,	51,00
	98,50

Note.—Like No. 1, this specimen acquires weight (about 8½ per Cent.) by digestion in nitric acid: it is certainly too poor an ore to be smelted, unless under very favourable circumstances; but trials might be made of its effect on the qualities of iron produced from mixtures of it with other ores. there seem to be grounds for supposing, that silica sometimes combines with iron in the metallic state.

No. 7, Deser Gerh, Sp. Gr. 3,645.

Phenomena with the Blowpipe. Becomes magnetic, and assumes the metallic tarnish; with borax on charcoal, a dark coloured enamel, studded with bright gold spots, resembling avanturine; the fragments translucent, and of a bright golden green.

Constituent parts.

Water,	6,00
Silex,	3,75
Lime,	0,50
Alumine,	0,50
Phosphate of Iron,	0,90
Oxyde of Manganese,	1,50
Peroxyde of Iron,	86,00
	99,15

Note.—The very beautiful appearance produced by the blowpipe, may probably be owing to the conversion of phosphate of ironinto phosphuret of iron,by the combustion of the charcoal support.

The process used in the foregoing analyses differs from those indicated by books; I have therefore subjoined a memorandum of it for the satisfaction of the scientific chemist.

1. Weigh the pulverised ore at the temperature of the atmosphere, and calcine, at a low red heat: the loss indicates the water; and if there is change of colour (from yellow to deep red brown) probably of carbonic acid.

2. For 100 grains of the ore take 1½ oz. muriatic acid, boil it gently over a lamp in a covered vessel for twenty minutes, add four ounces of water, and boil again for a few minutes; this dissolves every thing except the silica, alumina, phosphuret of iron (if any exists), sulphate and phosphate of lime: filter and wash the residuum perfectly, and calcine it; its weight is that of the silica and alumina—it may be tested for phosphate and sulphate of lime if necessary.

3. To separate the alumina, boil on the residuum sulphuric acid, diluted with thrice its weight of water: this will dissolve the whole of it, and leave the silex untouched.

4. Evaporate the muriatic solution at a gentle heat; when nearly dry, pour upon it about half a pint of well boiled distilled water, transfer the whole to a jar or flask, and keep it closely stopped for twenty-four hours: if any precipitate forms, it is phosphate of iron, which may be separated as usual.

5. Drop sulphuric acid into the solution; the lime, if any, will precipitate as a sulphate; separate and calcine at a low red heat, and by the scale of equivalents, the quantity of carbonate of lime may be known.

6. Precipitate the solution by one of caustic soda; filter, wash, calcine, and weigh the residuum, which consists of the mingled oxydes of iron and manganese.

7. Digest these in nitric acid, with a gentle heat; allow it to remain exposed to the air till nearly dry, calcine again at a red heat, stirring it often, and weigh it: if any increase of weight has taken place, oxygen has been absorbed, and this must be allowed for in the results.

8. To separate the oxydes of manganese and iron, boil them in a solution of muriate of ammonia, with a little sugar; the whole of the manganese will be dissolved and the iron left, (it has been ascertained by independent experiment, that no oxyde of iron is taken up, prussiate of potass will satisfy the chemist, that it is manganese:) precipitate the manganese, by water of ammonia, cautiously added, and filter; if the liquid has any colour, a portion of the oxyde has been re-dissolved by the excess of ammonia, and will precipitate on allowing it to evaporate; when the liquor is perfectly limpid, the whole has been obtained, and may be collected as usual. This is Mr. Faraday's process for their separation.

VI.—*Machines for Irrigation.*

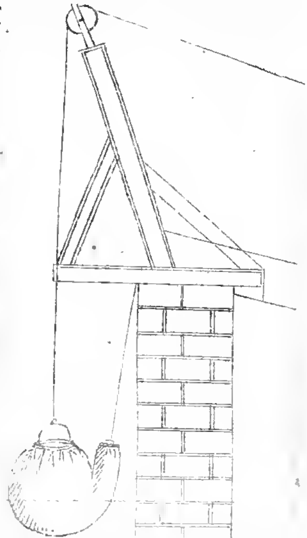
To the Editor of Gleanings in Science.

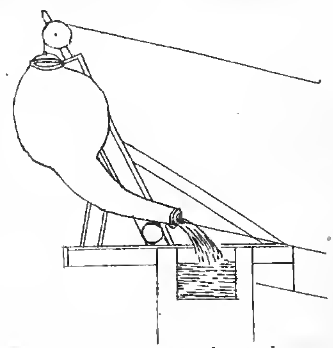
Sir,

The following extracts from Buchanan's Mysore, descriptive of the usual modes of raising water in that country, and giving data for calculating their respective performance, may not be without use to some of the readers of the Gleanings; and will I hope draw from your correspondent H. D. E. some further calculations of the comparative value of these machines for raising water, and of any others that may be commonly used in India.

The single *Yatam* of Buchanan, *Denkhi*, or *Denklee* of Hindoostan, and *Dhap* of Rungpore, will be familiar to most of your readers, being the commonest means employed for raising water in Bengal and Behar, and generally in India, where the springs lie near the surface of the ground. The double *Yatam* of the coast I have never seen used in Hindoostan. Buchanan's *Copily* or *Puckally* may not be very easily understood from his description; it is the common *mót* of the Deccan, and not met with, that I have observed, north of the Nerbuddah; I consider it a great improvement upon the *mót* of Upper India, and I shall be happy if I make it better known.

The advantages of the Deccan *mót* over ours, are, that it requires the attendance of only one man, that it raises more water, and discharges the whole that is brought up; and the wear and tear of the bag is much less. The *mót* of our provinces requires the attendance of two men; one to drive the bullocks, the other to pull in and empty the bag. The Deccan *mót* is entirely managed by one man, who drives the bullocks; the bag empties itself, and the general introduction, therefore, of it into our north-western and central provinces, would save one person's labour at each of the many thousand wells constantly working for seven months in the year. Our bag is small, and still it is a difficult matter to discharge its contents, a large proportion of which is always lost, and a considerable delay attends the emptying of it; the bullocks either not lifting the bag high enough, or pulling too far and strong. But by the contrivance to the Deccan *mót*, of the second bag or tail, to which is attached a separate rope, that draws over a roller at the top of the well, the bag empties itself, and must be entirely discharged by the time the bullocks have completed their pull; and without any loss of water. The rope from the tail of the *mót* is attached to the lower bar of rather a deep yoke, that the tail may be pulled in nearly on a level with the surface of the inclined plane on which the bullocks walk; and in consequence of the double ropes, the bulloeks are obliged to re-aseend the plane backward, lest the ropes should be entangled. Buchanan appears to think this awkwardness of movement objectionable, but I have not observed any difficulty attending the practice ; bullocks are very easily broken into stepping baekwards, and less time, I imagine, is lost in this manuer than by the double turn at top and bottom, that our cattle are obliged to make. The lower turn, with cattle under training, is not unfrequently attended with aeeidents, as they are often liable to turn round before the *mót* can be landed, and its weight falling down with a jerk, drags the bulloeks over, or breaks the ropes, and often maims, or makes them very shy. The double *mót* emptying itself, may be made of much greater capacity than the single one, which is of less size than the power of the bullocks is equal to, or otherwise, the labour of lnnding it would be too heavy for the man or woman whose business it is. The wear of the bag in the act of landing is considerable; the only wear sustained by the other, from friction, is by the tail in passing over the lower roller. The accompanying sketches will, I believe, give all the further information that can be required. Buchanan has no drawing of the *capilly* ; but Heyne has given one without any description, and the construction of the machine is not very distinct from his print. In descending the plane, the driver usually sits upon the upper rope, assisting the draught, I suppose, by his weight ; and in ascending, takes the under rope in his hand, to prevent its becoming entangled with the bullock's feet. The ropes should be made of twisted hide, being, beyond comparison, stronger and cheaper than the best rope exposed to the wet and rubbing, consequent to their use in sueh work.



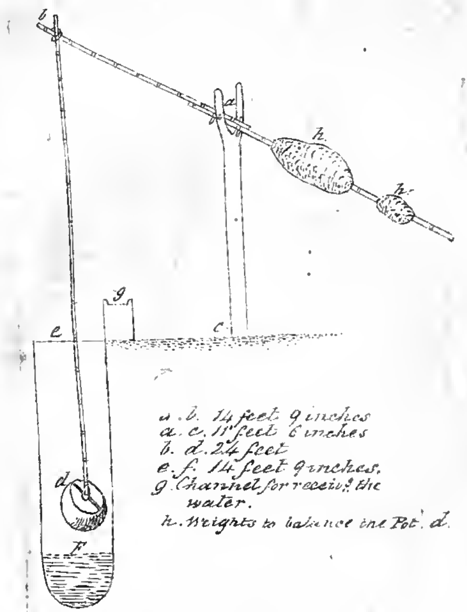


Professor Robison's double plunger can only be applicable to open pieces of water ; and besides, though a very simple machine, it is, I fear, too complicated for introduction, exeept under European superintendenee. I nm not aware of any means of raising water from deep wells, so effectual and universally practicable, as by the single and double *mót*, and I would beg to bring it to the notice of that zealous and seientific body, the Agricultural nnd Hortieultural Society of India, who will doubtless soon set an example to the native landlords, by ereeting one or two of the most approved machines for raising water, in their garden at Allipore ; instead of, ns now, raising water by the clumsy mode of baling, and earthen pots carried on men's heads. For raising large quantities of water from *jhils* and tanks, to heights of little elevation, such as is mostly required for Indigo fields and Faetories in the lower provinces, I should eoneeive the Professor's Plunger admirably adapted. The keep of the oxen employed at the wells is in this country almost nothing, the refuse of a garden, or of the fields, being ample provision for them; and the labour of collecting fodder, amply repaid by the return of manure.

I am, &e. F. A.

" *Bangalore.*—In this country the water for supplying gardens is generally raised by a machine, called *Yatam.* In the lower Carnatic the machine is wrought by a man who walks along the balance ; eoming before the fulcrum, when he wants to sink the bucket, and going back again when he wants to bring up the water.

Another man in this case attends to empty the bucket; but in this country, one man standing at the mouth of the well, performs the whole labour.—I have made no actual experiments to ascertain by which of the two methods, the same number would raise the greatest quantity of water; but it appears to me that the plan in use here, is the most perfect. At Madras the man who walks along the lever is in danger of falling, and the man who empties the bucket, of being hurt, for it must come up between his legs as he stands fronting the end of the lever: and although the bucket there, is much larger than the one in use here, I have observed that the workman was never able to empty more than two-thirds of its contents, owing to the awkward position in which he stood. The machine from which the drawing was taken, consisted of a lever or balance A. B. 14 feet 9 inches in length. This rested on a fulcrum A. C. 11 feet 6 in. high, a rod or bamboo B.D. 24 feet in length, by which the workman raises and lowers the bucket D. containing 789 cubic inches. Depth of the well from the surface of the earth C. E. to the surface of the water F. 14 ft. 9 inches; height of the end of the canal for conveying away the water G. 3 feet; Total height to which the water is raised, 17 feet 9 inches. The far end of the lever is loaded with mud I. I. so as exactly to counterbalance the pot, when full of water. The average time which the workman took to raise a pot of water, was 15 seconds: by this means, therefore, a man can in an hour raise about 671 gallons, to the height of 17 feet 9 inches. If the depth of the well be less, as is usually the case, the quantity raised by the same labour will be much greater, but in what proportion I did not ascertain."—*Vol. I. page* 263.



"*Colar.*—In this immediate neighbourhood, the *Yatams* that are wrought by men, walking backwards and forwards on the lever, are preferred. There are here two kinds; one in which two men walk on the balance, which has a bucket containing 40 seers, or $9\frac{784}{1000}$ ale gallons, and which can raise this five men's height, or 26 ft. 3 in. In the other kind, one man only walks on the lever, and can raise 32 seers or $7\frac{787}{1000}$ ale gallons, from the depth of three men's stature, or $15\frac{3}{4}$; for the men here being in general small, $5\frac{1}{4}$ feet are reckoned the ordinary human stature. The people of this place, reckon, that the same number of men will raise more water by the larger *yatam*, than by the smaller one; and much more by their small one, than by the *yatam* which is wrought entirely from below: of this, however, I am doubtful.

The machine here is equally used with that described at Bangalore. I examined one while it was at work, and which was wrought by two men on the lever. It raised water only eight ft. and at each time 35 seers only could be emptied from the bucket.—It drew water six times in the minute, and consequently raised 3066 ale gallons in the hour, or 1022 gallons for each man; but at Bangalore each man can raise 671 gallons to more than double the height. I have seen the single *yatam* drawing water from about eight feet deep, at the rate of seven times a minute; by which means a man will raise 1175 gallons an hour."—*Vol. I. page* 294.

"*Veneatacotag.*—I observed one of the machines for raising water called a *yatam*, which was made on a better construction than those above the Ghats. It was wrought by one man, who stood on a plank running parallel, to the lever, and placed on one side; so that the side of the lever passed parallel to his face, and he was in no danger of being hurt by the bucket coming up between his legs, as happens when the man's face looks toward the end of the lever. The lever was made of a bamboo, and the weight was a large stone fixed by a swivel. The bucket was made of an excavated piece of wood, shaped like the half of a Cheshire cheese, and when full of water was lighter than the stone, which, of course, raised it without any exertion of the labourer. From a well 16 feet deep, the man raised four buckets in a minute; each containing $209\frac{3}{10}$ cubical inches, or about 178 ale gallons in the hour." *Vol. II.—page* 461.

Great Balapura.—" In some places of this vicinity, the ground for sugar cane is watered by the machine which the Mussulmans call *Puckáli*, and the natives *Capilli.*—It consists of two bags of skin raised by a cord passing over a pulley, and drawn by two oxen or buffaloes, descending on an inclined plane. The great imperfection of this contrivance seems to be, that the cattle are forced to re-ascend the inclined plane backwards; but it appears to be a manner of raising water very capable of being improved, so as to become highly valuable; one man manages both the cattle; but these work only one half of the day, so that the *puckáli* requires the labour of one man and four beasts. The cultivators here, reckon that one *puckáli* will raise as much water as nine men working with the largest *yatam*, on which two men work the lever, or as seven men, each working a single *yatam*. This seems to confirm my opinion of the superiority of the last mentioned machine. The cost of the cattle is not reckoned to be more than that of one man, as they get no other provision than the straw of the farm, which they convert into manure, and which would otherwise be lost."—*Vol.* I. *p.* 356.

Madhugiri.—" A *capilli* which I examined, the water being 19 feet 8 inches below the surface, emptied its bucket, on an average, once every 36 seconds, and at each time brought up $32\frac{85}{100}$ ale gallons of water. One man and two oxen could work it for eight hours in the day, and thus draw up daily 26,280 gallons.—Double the quantity may be had, from the same well, by a double set of cattle; stops however, frequently intervene, that very considerably diminish the quantity actually raised."—*Vol.* I. *p.* 387.

VII.—*On the Manufacture of Writing Paper.*

Various are the materials on which mankind, in different ages and countries, have contrived to embody their sentiments; as on stones, bricks, the leaves of herbs, and trees, and their rinds and barks; also on tables of wood, wax, and ivory; to which may be added, plates of lead, linen rolls, &c. At length the Egyptian papyrus was discovered; then parchment, cotton paper, and, lastly, the common, or linen paper. In some places and ages they have written on skins of fishes; in others, on the intestines of serpents; and in others, on the backs of tortoises. There are few sorts of plants, but have at some time been used for paper and books; and hence the several terms, *biblos, codex, liber, folium, tabula, tillura, scheda, &c.* which express the several parts of the plant on which they were written. In Ceylon, for instance, they wrote on the leaves of the talipot; and the Bramin manuscripts in the Telinga language, are written on leaves of the *ampana*, or *Palma malaribarica*. Hermannus gives an account of a monstrous palm tree, called *codda pana*, or *Palma montana molabarica*, which about the 35th year of its age, rises to be sixty or seventy feet high, with plicated leaves nearly round, twenty feet broad, with which they commonly cover their houses, and on which they also write; part of one leaf sufficing to make a moderate book. They write between the folds, making the characters on the outward cuticle. In the Maldive islands, the natives are said to write on the leaves of a tree called *macaraqucan*, which are a fathom and a half long, and about a foot broad. But the most remarkable is the *xaqua*, which has something in it extraordinary; its leaves are so large, and of so close a texture, that they cover a man from top to toe, and shelter him from rain and other inclemencies of the weather, like a cloak; and from the innermost substance of these leaves, a paper is taken; being a white and fine membrane, like the skin of an egg, as large as a skin of our vellum or parchment, and nothing inferior for beauty and goodness to the best of our paper. Paper is chiefly made among us of linen, or hempen rags, beaten to a pulp in water; and moulded into square sheets, of the thickness required. But it may also be made of nettles, hay, turnips, &c. as tea is, or of any thing that is fibrous. The Chinese paper is so fine, that many of the Europeans have thought it was made of silk; not considering, says DuHalde, that silk cannot be beat into such a paste, as is necessary to make paper; but it is to be observed, that the same author afterwards speaks of a paper, or parchment, made of the balls of silk worms; and the like, we are assured by others, is done at Cathay.

The incalculable advantages which the moderns have derived from the art of printing, would have been only imperfectly known, but for the invention of linen rag paper. It has been observed by a French writer, that the dispatch of the processes of paper making is so great, that five workmen in a mill, may furnish sufficient paper for the continual labour of 3000 transcribers. The European process of making paper admits of three divisions; viz. the preparing of the rags, the forming of the sheets, and the finishing of the paper. The succession of the several processes is as follows: 1st, the rags are washed, or dusted, if they are dirty; then sorted into many qualities proper for different purposes. 2. The rags are bleached, to render them white; but this operation is sometimes deferred to the next stage of the process. 3. The washing engine of the paper mill is employed to grind the rags in water, till they are reduced to a coarse or imperfect pulp, called *half-stuff*, or *first-stuff*, in which state the bleaching is sometimes performed, or at other times it is bleached in the washing engine during the grinding. 4. The *half-stuff* is again ground in the beating engine, and water added in sufficient quantity to make a fine pulp; which being conveyed to the vat, the preparation of the rags is completed, and the pulp or stuff is ready for making the sheets. 5. This is done by a workman, who takes up a quantity of pulp upon a mould of fine wire cloth, through which the water drains away, and the pulp coagulates into a sheet of paper. 6. Another workman takes the sheet of paper off from the wire mould, and receives it upon a felt; he then covers it over with a second felt, evenly spread out; and continues this operation, which is called *couching*, till he has made a pile of sheets called a *past*, containing six quires. 7. The past of paper, with the felts, is placed in the *vat-press*, and the whole is subjected to a strong pressure, to press out the superfluous water, and give the paper a solidity and firmness it would not otherwise have. 8. The pile of paper is removed from the vat-press, the felts taken out from between the sheets, and they are pressed again by themselves for a certain time, in a screw press. 9. The sheets are taken from the press, and hung up, five or six together, on lines in the drying loft, till dry. The paper is now made, and only requires to be finished; but it should be observed, that the greater number of the processes of finishing are only performed upon fine writing paper, common printing paper being ready for packing up when dried. 10. The paper, in five or six sheets together, is dipped into a tub of fine size, and afterwards pressed, to force out the superfluity; it is then dried again in the drying loft; but in printing paper, this process is rendered unnecessary, by sizing the stuff whilst in the engine, and adding certain ingredients. 11. The examination of each individual sheet of paper is made, all knots and burs are removed, and the bad sheets taken out. 12. A very large pile of paper is made, and pressed with immense force, to render the sheets flat and smooth. 13. The pile is taken down sheet by sheet, and another made, and without turning the sheets over; by this means new surfaces of the sheets are brought in contact with each other, and the pile being again subjected to the press, the surface of the paper is improved. This operation is called *parting*, and is repeated two or three times for the best papers. The paper is now counted into quires, and packed up into reams for market. Thus, one of the dirtiest and least promising of articles is converted into one of the most beautiful and even delicate; and thus a material is made by art, capable of bearing our thoughts and feelings through time and space: which in its rude and unimproved state, would appear only fitted to be cast forth or trodden under foot. G. W.

VIII.—*Additional Notice on Cooling Wines.*

In our last number we re-published a letter from Dr. Walker of Oxford, on the means of producing artificial cold, with reference to cooling wines. The experiments having been followed up in Calcutta, a short account of some practical results may not be unacceptable to our readers, as affording them instructions how to obtain with certainty, and at a small expense, one of the greatest luxuries in a hot climate, where ice cannot be procured.

The salts employed in producing artificial cold, are saltpetre *(shora)*, sal-ammoniac *(noshádeh)*, and Glauber's salt: the latter may be obtained by solution and crystallisation from the native salt called *C,hara nún.*

To produce the greatest effect, the proportions are, to 16 parts of water, five parts of saltpetre, five parts of salammoniac, and six parts of Glauber's salt. The salts should be finely pounded and quite dry.

At the temperature of 80°, saltpetre and sal-ammoniac will each, singly, produce a cold of about 25 degrees, when dissolved in water to saturation; that is to say, water at 80° will be lowered to 55° by saturating it with either of them. At the same temperature, Glauber's salt will give a cold of 20 degrees. But it must not be supposed that the combination of the three salts will produce 25 + 25 + 20 or 70 degrees of cold. The reason of this is, that when water has dissolved a certain quantity of one salt, it cannot dissolve as much of a second, or a third salt, as it would do of each singly. The greatest degree of cold which can be produced by the three salts in combination is about 56 degrees, that is, water at 80° will be lowered in its temperature to 29°, or three degrees below the freezing point. A much less degree of cold, however, is sufficient for every useful purpose. One part of saltpetre, and one part of sal-ammoniac, to three parts or measures of water, will produce a cooling mixture of 40 degrees; and three quarts of this cooling mixture, in a pewter vessel, cased with wood, will cool a quart bottle of water or wine, much beyond what any *abdár* can effect with saltpetre alone; and as cold as claret, for example, can be made, without destroying the flavour of the wine. The wines which can bear the greatest degree of cooling, are champaigne and hock; and these may be cooled down to the freezing point, by the most powerful mixture above mentioned.

Saltpetre and sal-ammoniac compose the best cooling powder, as they can be recovered by evaporation. But the cheapest mixture is that of saltpetre, and Glauber's salt. We shall presently explain how the latter is to be obtained. The experiment to be mentioned in the sequel, was made with the refuse of saltpetre from the Company's refining works at Ishera, which costs three rupees per maund, and with Glauber's salt.

Into a tub, lined with sheet lead, and capable of containing 10 or 12 gallons, four gallons of water were poured, at the temperature of 78°; to this were added seven seers of Glauber's salt, when the water fell to 60°. Seven seers of dry saltpetre in a state of powder, were then put in, when the temperature of the mixture became 43°. This mixture was capable of cooling five or six bottles of wine; but the object of the experiment being to ascertain in what time a bottle of water placed in a cooling mixture, attains its maximum of cold, or how long it may be kept in that state, in a proper vessel, such as above described, only one bottle of water, with a thermometer placed in it, was put unto the tub, and examined every quarter of an hour, for several hours in succession. It may be proper to mention that the tub had a lid fixed to it, to exclude communication with the external air.

After the first quarter of an hour, the bottle of water was cooled down to, 47°

2d quarter,	45	8th do.	45
3d do.	45	9th do.	46
4th do.	45	10th do.	46
5th do.	45	11th do.	47
6th do.	45	12th do.	47
7th do.	45		

From the above, it will be observed, that in half an hour the water was cooled down from 78° to 45°; and remained stationary at that point for more than two hours. At the end of five hours it had risen 10 degrees, viz. from 45° to 55°, and even then was 23 degrees colder than what it was at the commencement of the experiment.

Another experiment was made at the same time in another tub, by keeping the cooling mixture in a constant state of agitation by means of a churn; but the only effect of this, was to bring the water in the bottle to its maximum of cold in a quarter of an hour, instead of half an hour, by bringing fresh portions of the cooling mixture constantly in contact with it. It is obvious that the same effect will be produced, whether the cooling mixture be driven round the bottle of wine, or the bottle of wine be driven about in the cooling mixture, as the *abdárs* do in the *tás*: but many people believe that wine is spoiled by too much agitation or shaking. The plan, therefore, of agitating the cooling mixture, instead of the wine, has the advantage of cooling the wine without disturbing it. But in fact, if the cooling mixture be 35 or 40 degrees colder than the fluid to be cooled, no agitation is necessary. It is sufficient to allow the bottle to remain in the cooling vessel for an hour.

It may be observed, that water does not taste very cold unless it be cooled down 30 degrees. It is obvious therefore, that saltpetre alone never can produce this degree of cooling. In the common way of cooling, also, in a leaden *tàs*, much cold is wasted; as the great surface of the leaden vessel is in immediate contact with the warm air which carries off the cold. Whereas, a leaden or pewter vessel cased with thick staves of wood, is protected from the external air by the wood, which is a slow conductor of heat or cold.

It may also be observed, that the cooling mixture, to be effective, should considerably exceed the fluid to be cooled in quantity. To cool a quart bottle of wine, the cooling vessel should be capable of holding seven or eight quarts of water; into this, pour three quarts of water, then add three quart measures of saltpetre, and sal-ammoniac, or saltpetre and Glauber's salt, half and half, that is, one and half measure of the one, and one and half measure of either of the other two. The vessel will then be about two-thirds full; and when the bottle of wine is introduced, the cooling mixture will rise up to the neck of the bottle. It is essential to the success of the experiment, that whatever fluid is to be cooled, should be under the surface of the cooling fluid. If only half the bottle is immersed, the upper half of the wine will remain quite hot as when put in; while the lower half may be cooled down 30 or 40 degrees, according to the power of the mixture. The cooling vessel will answer very well if made of a cylindrical shape; but if made in the shape of an inverted cone, or bulging out in the upper half, so that the largest body of the cooling mixture may surround the upper part of the bottle to be cooled, the desired effect will be most surely attained, as the cold descends, but will not ascend.

The *C'hára nún* is procurable in the Bazar at the rate of two Rs. a maund. It is in lumps (apparently a thin cake broken up), of considerable hardness. It is of a dirty grey colour, and its structure appears similar to that of calcareous stalagmite, being apparently formed by the deposition of successive layers. We have thought it necessary to be thus minute in our description, in as much as the appearance of the article, being so very different from that of Glauber's salt, as our readers have most probably seen it, they would be doubtful whether they had obtained the proper substance. It is to be added, that as purchased, it does not reduce, but actually raises the temperature (about 10°). The truth is (and the reader will see the reason of it in the following extract), that the salt is in the anhydrous state, or deprived of its water of crystallisation, in which case, as it must absorb water, heat is given out. The proportion of the water of crystallisation in the crystals of this salt, is very great, being 55 per Cent; so that 44 seers of the anhydrous kind, if dissolved and crystallised, should yield 100 seers. In this case, the cost would be less than one rupee the maund. There is however a partial difficulty in crystallising it in the hot weather or rains, owing to the circumstance of water dissolving a maximum of the salts at 91°. So that supposing a solution evaporated over the fire, till it begin to deposit crystals, and then allowed to cool, the crystals will be re-dissolved and disappear; the difficulty may be obviated by lading out the crystals, while the solution is still on the fire. Or a saturated solution being made at 91°, and allowed to cool all night to the temperature of sunrise (about 80°) will deposit crystals; which being removed, and the solution again saturated, the operation may be repeated, and this, without any expense of fire wood. This method we have found to answer, but the process is tedious. One hundred parts of water will dissolve 50,65 parts of anhydrous salt, at 91°. The result is a solution of 150,65 parts, which may be considered as 36 parts water, and 114 the crystallised salt. Such a solution cooled down to 80° will deposit 30 parts of crystals, or one-fourth of what it contains; cooled to 77°, it will deposit 40 parts; at 64°, 56 parts; and at 56°, 78½ parts. So that in the cold weather, the crystallised salt may be manufactured very conveniently, and without expense of fire wood. For 50,65 seers of the anhydrous salt, being put into 100 seers of water, of a temperature of 91°, and the solution being afterwards cooled to 56°, which may be done at that season of the year, nearly two maunds of crystals will be obtained at once. These estimates relate, however, to the pure salt; there is a large proportion of sediment in the Bazar article.

The following account of the manufacture of this substance, is extracted from Dr. Buchanan's unpublished reports. "The salt called *Khari*, is made there, at no great distance from the Ganges, about four cosses east from Singiya, a factory of the Company's. The saline earth, from whence this also is made, is called *Reher*, and effloresces on the surface of several places of Purgannahs Besara, Godasangr, Bhatsala, Jaruya, Partaul, Lai, Rati, Chhapra Maker, Goya, Sangrampur, Maharal, Barui, Dangsi, and Barel, in the districts of Saran and

Tirahut. It is scraped as usual, and collected at the furnace. A little rice straw is first placed on the ground, and covered with the saline earth to about four inches in thickness. The straw is then burned; and the burnt matter is covered with a foot of straw, and that by four inches of saline earth, when this straw also is burned; and the same is repeated seven times; after which the heap is covered with some fire wood, which is burned. The burnt saline matter (Bani) is then put into a cistern of clay, and about three thousand seers, or six thousand pounds of water are poured upon it. Next day the workmen take out the uppermost part of the matter, which had been put into the cistern, and from whence this water has washed the saline parts, and add as much more water; and this is repeated again and again, until all the salt has been extracted, and nothing remains in the cistern but brine. This is then allowed to flow from a cock, and is evaporated in from thirty to forty earthen pots, placed in a row over a trench, which serves as a furnace, the fuel being put in at one end, and the smoke coming out at the other. The evaporation is carried to dryness, but the *khari'* turns out of three different qualities, which is attributed to circumstances in the burning, that are beyond the control of the operator. When the operation has succeeded well, they procure a whitish salt in grains, which is called *phul khari'*, and that is given to cattle. It sells at eighty-eight seers (seventy-two sicca weight,) or pounds one hundred and seventy-eight and a quarter for the rupee; when the operation has less success, a more impure salt called *sinder khari* is procured, and is used in the preparation of red lead. It is sold at ninety-six seers for the rupee. When the operation is least successful, a very black salt is procured in a solid mass. It is called *chamari khari*, being used by tanners, and sells at one hundred and twelve seers for the rupee. All these I have refined into a very fine purging salt, which in its crystals entirely resembles Glauber's salt, but its taste is not near so strong nor disagreeable: and I would recommend, that the Commercial Resident should be directed to prepare a quantity sufficient for the Hospitals. It would come much cheaper than the purging salts imported from Europe, and is not inferior in quality to the best of them. Should he be doubtful of skill to conduct the process, he might send a quantity of the crude salt to the Company's Apothecary at Calcutta, by whom the operation would be, no doubt, more properly conducted."

IX.—*Queries respecting the Manufacture of Caoutchouc.*

We have been requested by a friend to give insertion to the following queries, which have been drawn up, we believe, by some one connected with the manufacture of water proof cloth in England. We need not point out to our readers the great utility that may be the result of any information they are capable of communicating.

What are the trees which yield Caoutchouc?

Is it produced from any other genera of plant besides *Hevea* (as *H. guianensis**), *Euphorbia* (as *E. punicea*), *Urceola* (as *U. elastica*), *Jatropha* (*J. elastica**), *Ficus* (various species of)? Are there not certain species of plants which yield caoutchouc of a much better quality than others? The former yielding a caoutchouc which remains permanently tough and elastic, though kept for any length of time; the latter, yielding one which approaches more to the nature of glue or birdlime, than of good caoutchouc; or which, although sufficiently tough and elastic, when newly prepared, gradually undergoes decomposition upon being kept, becoming soft and glutinous, or becoming converted into a brittle substance, having somewhat the appearance of rosin; or which becomes soft and adhesive like pitch, and remains permanently so, on the application of a very moderate heat?

Does not the *Urceola elastica* generally yield caoutchouc of the best quality?

Is not the caoutchouc obtained from the *Hevea guianensis*, also of tolerably good quality;

Is not the caoutchouc obtained from the different species of Ficus which yield it, universally of bad quality, being soft and clammy, and easily destroyed even by a gentle heat?

* The *Hevea guianensis* and *Jatropha elastica* are one and the same plant; and both names have been changed for *Siphonia elastica*.—Ed.

At what age do the various caoutchouc trees begin to yield caoutchouc? Is the caoutchouc prepared from the juice of a young tree inferior in quality, to that prepared from the juice of an old tree?

At what season of the year are the caoutchouc trees commonly wounded, for the purpose of obtaining from them the juice?

Is there any particular season of the year at which the juice is considered to be ripe, to yield caoutchouc of a better quality than the juice drawn from the tree at any other season? If this be the case, what would be the consequence of preparing caoutchouc from a juice not fully matured? Would the product of caoutchouc be smaller than usual? Would it be of inferior quality?

What quantity of juice is afforded on an average by a full grown tree?

What is the average proportion of caoutchouc in this juice? at what price can the manufactured caoutchouc be delivered by the grower?

Describe the process of preparing caoutchouc from the juice. Is heat at all employed in it? Are any precautions taken to have the caoutchouc unmixed with the other ingredients of the juice? Is the caoutchouc coagulated by any means, and this directly separated from the juice? or is the entire juice evaporated down to a dry mass?

Caoutchouc is imported into the country in two very different states. In the one it generally has the form of bottles, shoes, &c. the other of thick cakes. The former kind of caoutchouc has a reddish brownish, or blackish colour; sometimes also reddish white: the latter is usually white or yellowish white. The former generally inodorous, or has only a weak vegetable odour; the latter is sometimes inodorous, but in general, it has more or less of a very offensive odour, somewhat resembling that of rancid cheese, or of animal matter in a state of putrefaction.

Whence arise the differences? we are inclined to believe, not only the caoutchouc in these two cases is the product of a totally distinct genera of plants, but also that the process of preparing the brown or bottle caoutchouc, is quite different from that of preparing the white or cake caoutchouc.

It would probably lead to some useful result if a person who has some skill in vegetable chemistry, were to make a careful observation of the process of manufacturing caoutchouc, as practised where the caoutchouc tree is cultivated, watching it closely from the drawing off of the juice from the tree, to the complete dessication of the caoutchouc. It is probable, that he might be able to detect some imperfection in the process, which would easily admit of remedy; and that he might thus be able to suggest a modification of the process, which would yield caoutchouc of a purer quality than that which is generally delivered in commerce. The variations in the modes of manufacture, and the experiment which it would be worth while assaying, will of course be best judged on the spot, where he has an opportunity of observing the local process of the manufacture, and of familiarising himself experimentally with the habitudes of the recent juice.

We would however recommend all the following trials to be made.

1. Prepare caoutchouc from the juice, by the ordinary process practised among the natives.

2. Evaporate a quantity of the juice to dryness: and preserve the residuum. The evaporation should be performed either by simple exposure to the sun, or in a vessel placed over another filled with boiling water. For the properties of the caoutchouc would certainly be injured, if the evaporation were conducted in a temperature exceeding that of boiling water.

3. Boil the juice for from half an hour to an hour, and collect the solid portion which separates. The juice of the caoutchouc appears to contain always a considerable quantity of a substance possessing the properties of albumen, and which is perhaps the most dangerous of all its extraneous ingredients. In this trial the albumen will certainly be coagulated by the heat, and thrown down in a state of intimate intermixture with the caoutchouc.

4. Dilate the juice with four times its bulk of cold water, and observe if the caoutchouc rises to the top. If it does, wash it several times, by agitating it with water, then coagulate it by a slight heat or otherwise. The principle object of this trial is to ascertain if the caoutchouc may be separated by simple dilution of the juice with water, while the albumen (and other extraneous ingredients) will still remain in a state of solution.

5. Mix the juice with four times its weight of water, and $\frac{1}{20}$th of its weight of caustic potash, and boil the mixture for an hour. Let the solid portion which se-

parates be repeatedly boiled in fresh quantities of water, in order to remove from it the whole of the alkali. Then collect it. The object of this experiment is to ascertain if the albumen will be held in solution by the alkali, while the caoutchouc alone will be precipitated.

N. B. This experiment may be repeated with several different portions of alkali, both greater and less, than that we have recommended.

6. Mix the juice with twice its bulk of strong spirits; agitate the mixture thoroughly for half an hour; then collect the matter which separates.

7. Mix the juice with as much pure distilled vinegar as communicates to it a pretty strong sour taste. Agitate the mixture thoroughly; then collect the solid matter which separates, and carefully wash out the whole of the vinegar from it with water.

8. A similar experiment may be made by adding very dilute sulphuric acid to the juice, but in this case the greatest precaution must be taken, not to leave a trace of the acid in the precipitated caoutchouc.

9. Make thick varnish with *recently prepared* caoutchouc, by cutting it into small pieces, and mixing it with four times its bulk of the best spirits of turpentine, stirring the mixture at intervals for twenty-four hours, until the materials seem to be completely incorporated. Then put the varnish into a bottle, cork it up, and seal it.

It is of some consequence, that the caoutchouc employed in this trial should be as little coagulated as possible, because it will be in a proportionate degree more easily divided and dissolved by the oil of turpentine.

The caoutchouc which is separated by merely dilating the juice with water, (if this is found to succeed,) would be extremely suitable for the purpose; or this failing, the caoutchouc which is separated from the juice by the addition of alcohol or strong spirits.

10. Fill some bottles with the juice the instant it issues from the tree, cork them up, and seal them on the spot.

A specimen of caoutchouc prepared by each of the methods which have been mentioned, as well as by any other method which an observation of the local processes may suggest, should be transmitted to England for the purpose of being further examined there. And in order that the products of these various trials may be comparable with one another, it will of course be necessary that they should be all made from juice of the same kind, and that the same quantity of juice should be taken for each trial.

The quantity of juice taken for each trial may be from 1 to 6 pounds, according to the convenience which is possessed for experimenting. The caoutchouc prepared in all these trials should be dried, if possible, without the application of artificial heat, or even without being much exposed to the sun. It should then be packed up in a piece of matting, and directions be given for its being kept in a part of the ship where it will be rather dry than damp, and where it will be exposed occasionally to the air: for it is apt to become mouldy when kept excluded from the air.

X.—*Scientific Intelligence.*

1. Novelties in Science. 2. Miscellaneous Notices. 3. New Patents. 4. New Publications.

Under the above titles we propose to give a series of articles, noticing every novelty, whether of fact or doctrine, that may appear in the European Journals; and we solicit towards the proper execution of the task, the co-operation and contributions of our friends and correspondents. Notices of new publications on subjects of science, or of new editions of established works will find their place. And though, we shall not attempt to give an account or even list of all the new patents, yet we hope to present our readers with concise notices of all that are likely to be generally interesting, or that describe processes, which tend to any important change of manipulation in any of the arts.

As our work has already reached its 9th number, we shall be obliged to go back a little; and if we should admit amongst our novelties some things not so very new, yet as the great majority of our subscribers, owing to the heavy charges attending in India the acquisition of knowledge, have not the means of keeping their acquirements up to the level of modern improvements, we are persuaded they will not object to this partial retrospect, especially as it shall only include subjects of real interest or importance; and even those more fortunately situated with regard to the facilities of receiving supplies of books may not object to have their memories refreshed.

1.—Novelties in Science.

1.—*Mr. Tredgold's new Theory of the Resistance of Fluids.*

Those of our readers who are acquainted with the value of the several theories as yet proposed on this subject, will be interested to hear that Mr. Tredgold has proposed a new one, which as far as we can judge, certainly appears to agree better with experiment than that generally received at present. The importance of the subject may be in some degree estimated from the fact of the French Academy of Sciences, having offered a premium for the best series of experiments on the resistance of fluids. We shall endeavour to give a concise, but we hope, intelligible account of Mr. Tredgold's theory.

The resistance of fluids, Mr. T. divides into three parts, 1. The displacement of the fluid before the body can occupy its place, *(direct resistance;)* 2. The re-establishment of the fluid in the vacancy left by the body; *(minus pressure of the fluid;)* and 3. Friction.

The *direct resistance* of water, as measured by the height of a column, Mr. T. finds is equal to $\frac{l\, v^2\, Sin.^3\, d}{2\, g}$: v, being the difference between the velocity of the fluid and that of the body; a, the angle which the surface of resistance makes with the direction of the motion; and g, the space described in a second by the velocity due to the force of gravity in vacuo $= 32\frac{1}{8}$ feet. The *minus pressure*, he finds expressed by the formula $\frac{v^2\, Sin.^2\, c}{4\, g}$: c, being the angle the retiring surface makes with the direction of motion; and v, and g, as before. The combination of these two expressions gives $\frac{v^2}{4\, g}$ $(2\ Sin.^2\, a + Sin.^3\, c)$ for the value of the resistance exclusive of friction. When the body is a cube, or a cylinder, with the ends perpendicular to the direction of the motion, the angles being 90° the above becomes $\frac{3\, v^2}{4\, g}$

The friction he finds to consist of two parts; that due to the body, and that due to the particles of the fluid itself. The first he supposes to be directly as the square of the velocity, and as the surface; in which case f being put to signify the height of a column equal to the friction on one foot surface, moving with a velocity of one foot per second, this will become, when distributed over the whole section s of the moving body $\frac{f}{s}$ and putting $l\, p =$ the surface of the friction, the expression will become $\frac{f\, l\, p\, v^2.}{s}$ The friction of the fluid itself, he considers as affecting the value of l by a quantity A v. A being a coefficient depending on the nature of the fluid; the whole friction will then assume the form $\frac{f\, p\, v^2\ (l + A\, v)}{s}$ and consequently the whole resistance will be $v^2\ \frac{(2\, Sin.^3\, a + Sin.^2\, c}{4g} + \frac{f\, p\ (l + A\, v)}{s}$. The second part of the correction for friction may be neglected in the case of water.

From the first expression he finds the resistance exclusive of friction on the following bodies, will be in proportion to the numbers set opposite them.

A Cylinder with flat ends,	1,
A Cylinder with the hind part a hemisphere,	0,833
A Cylinder with the fore part a hemisphere,	0,600
A Sphere,	0,433

Mr. T. gives a very full account of comparisons made between his formula, the old one, and experiment. In the comparison made with Bossut's experiments (Hydrodynamique, Tom. ii. p. 411. or Robison's Mech. Phil. Vol. ii. p. 295) the accordance is truly surprising, while the difference of the old theory, from experiment, is sufficient to show that in practice it was worth nothing. Thus, when the angle with the direction of motion was 90°, the resistance by experiment, by the old theory, and by the new, is made 1000. When the angle is reduced to 6°. the resistance is by experiment 3999; by the new theory (including friction) 3578; or (excluding) 3341; by the old theory (excluding friction) 109.

In a comparison made between the results of his formula, and the experiments made by Dr. Hutton on the resistance of the air, the agreement is likewise very close; and still more in another series in which the body was a ball, 2 inches in diameter. The resistance increasing according to the velocity from 006 oz. to 1700 oz. exhibits in but two instances, a discrepancy amounting to $\frac{1}{10}$ and in one of these it is plus, in the other minus. And although the coincidences are not so close in some other comparisons made, yet there is, upon the whole, a sufficient agreement to speak strongly for the practical value of the formula.

2.—*Proposed Improvements of Daniel's Hygrometer.*

Mr. Jones, an instrument maker of London, proposed a modification of this instrument, tending to lower its price and make it more portable. It consisted in dispensing altogether with the two balls and connecting tube, while the internal thermometer, which was much enlarged, was used not only to indicate the temperature of the dew point, but also as the surface on which the dew should condense. It was in fact a thermometer with a bent stem and a large bulb, to the lower part of which, ether was applied, so as to diminish the temperature of the mercury; and on the upper part of the bulb, which was free, the dew it was expected would deposit itself, when from the evaporation of the ether, the temperature should fall to that, at which the moisture in the air was condensible.

The description of this instrument was honored with a niche in the *Philosophical Transactions,* an bonor it was certainly not entitled to; for Mr. Daniel having had such an instrument constructed by Mr. Newman, with two small thermometers inserted into the bulb, he found there would occasionally occur a difference of temperature amounting to 7° between the lower part of the bulb, subject to the cooling influence of the ether, and the upper one left free. This was a result which every one must have expected, and it was confirmed by the account the purchasers of the instrument gave, who in fact found that no dependence whatever could be placed on it.

Mr. Fogo of Leith, has recently returned to the subject, and in a late number of Brewster's Journal, he recommends the simplification of the apparatus as too expensive and bulky for the ordinary purposes of metereology. He considers the two balls and connecting tube entirely superfluous, and indeed it does appear an indirect mode of proceeding to apply the cooling substance (the ether) not to the ball to be cooled, but to another connected with it. There is here a double evaporation; the ether evaporating from the covered ball, and from the one connected with it. This renders the process more tedious certainly. Mr. Fogo asks why should not the ether expended on the covered ball, be used to produce a direct effect on the thermometer. He proposes, in fact, that a common thermometer should be used of a cylindrical shape, the lower half to be enveloped in cambric muslin, confined by a fine silver wire, to prevent the spreading of the ether on the upper half, which might vitiate the experiment. The lower half being dipped in ether, the thermometer will begin to fall till having reached the temperature of the dew point, dew will be deposited on the upper half, which being a bright reflecting surface, will instantly show the slightest dulling.

In the general principle, Mr. Fogo's modification is the same as that of Mr. Jones, and must therefore be subject to the same objection. Mr. F. notices this, but considers the difference of size of the thermometer as a sufficient answer. There is no reason why the bulb should not be as small as that of Daniel's arrangement; and as in the latter, only half the bulb is immersed, it is evident that the objection is equally good or bad, as made to either instrument.

A different modification has been suggested by a Mr. Cumming, in a late number of Brande's Journal. A glass tube is proposed to be employed, open at both ends; a thermometer being suspended within the tube, has its bulb surrounded with sponge, which is also in contact with the sides of the tube. Ether being poured on the sponge the temperature falls, and if the air be not very dry, moisture will be deposited on the outside of the glass tube, where the sponge is in contact. The thermometer is then read off, and must give the temperature of the tube on which the dew has condensed, and consequently the temperature of the dew itself. If the air be dry and the action of the instrument sluggish, a blast of air driven by a small bellows through the tube, will accelerate the process. Mr. Cumming's arrangement appears to us to be convenient; it can easily be tried by any one who has one of those thermometers enclosed in a cylindrical glass tube (which are so common in India), a little ether, and a bit of sponge. If the tube be not open below, a piece may be easily cut off with a file.

Our readers are sufficiently aware, that the observation of the dew point is the only certain means we have of measuring the humidity or dryness of the air. Every other hygrometer is only useful in as much as it enables us to determine this point, however indirectly. The observation of the dew point must then always be the test of accuracy of every hygrometer: it enables us in fact to form a standard, with which we can compare the indications of every instrument proposed for measuring the humidity of the air. Daniel's hygrometer offers certainly, a neat and elegant method of performing the experiment. Yet it must be confessed, that it is too expensive ever to come into general use. Mr. Cumming's modification of the arrangement, places within the reach of most observers, the means of making observations which the scientific metereologist must frequently have recourse to, whatever instrument he use for the ordinary indications. D.

3. *Mr. Wheatstone's Microphone.*

This instrument consists of two plates of metal, of one inch in size, so as to cover the cavities of the ears; each plate has a wire about one-eighth of an inch in diameter, rivetted at one end at right angles into its centre: the wires being about eighteen inches long, are bent round, a little distance from the plates, and brought together side by side at the extremities, where they are united by brazing, and jointly filed to a point. When this instrument is used, the plates are put over the ears, the spring of the wires holding them with sufficient force against the head; the point or apex of the microphone, is then to be brought into contact with any part, the phonic vibrations of which are to be ascertained. The vibrations are conducted along the wire, and being communicated to the plates which close the ears, these vibrations are reciprocated by the enclosed volumes of air, and the nerves of the ears powerfully affected. By the use of this instrument, the vibrating parts of the sounding board of a guitar, harp, or other instrument, can be at once ascertained, and the irregular sources of noise and mechanical action in machinery discovered, when every other trial has failed. So powerful is the effect of the microphone upon the ear, that by it the most uninitiated can at once be made to perceive the effect of chord and discord.

2.—Miscellaneous Notices.

1. *Dr. Thomson, and Professor Berzelius.*

Dr. Thomson, the well known author of a System of Chemistry, in 4 vols. having recently published a new work, entitled, *An attempt to establish the first principles of Chemistry*, consisting chiefly of determinations of the proportionate weights in which bodies combine, or as they are called chemical equivalents; Professor Berzelius of Stockholm has published some very severe strictures on it, not hesitating even to accuse the Doctor of falsifying results, or as a more homely word would better express it, of *screwing*, so as to bring them to the standard of his theoretical notions. Dr. Thomson has published in the Annals for March his answer—giving various particulars of his analyses and details of his mode of proceeding, with which we shall not trouble our readers.—" *Non nostri est tantas componere lites.*" One remark only we shall make. The editors of the Annals, and Dr. Thomson himself, seem greatly scandalised at the charge of Berzelius as implying a *mala fides*, or want of common honour and honesty. But " 'twere to consider too curiously to consider so."—We know in how gentle, yet persuasive a manner the genius of hypothesis can mislead her votaries; and when we refer to the history of the many serious scrapes into which the unwary amongst them have fallen, from the tricks of this Will o'the wisp, we cannot but smile at an attempt to magnify the finding 1 or 2 per cent. + or— of any ingredient, into a crime similar to that of feloniously and burglariously entering the houses of any of the King's lieges.

We consider Dr. Thomson's attempt to prove that all bodies combine in weights which are multiples of hydrogen, to be useless, if not mischievous ; and for this simple reason, that as from the imperfection of all human science and experiment, and the smallness of the unit (hydrogen,) every determination must necessarily come within such a quantity as the lover of hypothesis thinks may be neglected—may be in fact added or subtracted—nothing is gained ; while if the addition or subtraction be made in the direction of the actual error, something is lost, and the accuracy of chemical research impaired. Lord Bacon says, "*Homo naturæ minister et interpres ; tantum facit et intelligit, quantum de naturæ ordine re vel mente observaverit ; nec amplius scit aut potest.*" It may be said, however, that there is an advantage in having the numbers on the hydrogen scale, whole numbers, which is unquestionably true. But we may satisfy ourselves with using the nearest whole number without necessarily adopting the doctrine, and we may retain the more correct determinations to refer to when required. A Mr. Rainy, a pupil of Dr. Thomson's, has in some former numbers of the "Annals," stated well grounded objections to Dr. Thomson's fundamental experiments on the proportionate weights of hydrogen and oxygen, showing that they by no means warranted the conclusion of these weights being in the exact ratio assumed. The course hitherto found best suited to the cultivation of science, has been first to accumulate a mass of correct experiments, and then to theorise. By pursuing this course astronomy has become what it is, a monument of the sagacity of man. By pursuing an opposite course, geology has till lately been a bye word and term of reproach amongst all men of judgment. Let the chemist make his analyses unfettered by any hypothesis, and the theorist may then speculate on them with safety : but to attempt to establish the first principles of chemistry, by running after fanciful notions, which it is impossible to prove, and which if proved, are of no use, is to say the least a waste of labour. X.

3. *Overflowing Springs.*

It has long been understood, that two sheets of water, capable of furnishing constant springs of wholesome water, rising above the soil, flow under the basin of Paris. The one is between the chalk and green sand, and furnishes gushing springs wherever it is allowed to escape, provided it be in a place in which the two-thirds which enclose it, remain intact. The other sheet of water, lower than the first, is, nevertheless, capable of rising higher. It is equally wholesome, and is also capable of furnishing constant springs. Its position, however, has not been ascertained until lately ; but it has been at length found by labourers working at the wet-dock of Saint Owen. The water rose in the first instance with great noise, and, after several applications of the pump, was brought to the level of the soil, on which it flowed impregnated with a great quantity of green sand. In a short time, however, it became pure, and eventually threw up a jet of considerable height, and consisting of a large quantity of water. It has been recommended to bore for this water in the least elevated parts of the French capital ; such as the *Hotel de Ville*, the *Palais Royal*, the *Jardin des Plantes*, &c.

A well has been bored in the Duke of Northumberland's grounds at Sion, to the depth of 535 feet. The first twenty feet bored through, consisted of loose gravel and sand ; to this succeeded strong blue clay, to the depth of 410 feet ; next ten feet of green sand, and then between thirty and forty feet of loose chalk ; and finally, very firm and hard chalk, to the depth. A strong spring was found in the green sand, but it was not powerful enough to rise higher than thirty feet from the surface. The next spring was found in the solid chalk, and the two springs united, now rise to the height of five feet above the surface, and the water flows over at the rate of five gallons per minute, of a temperature of 55.° Fahrenheit.

3.—New Patents.

1. *Patent Metallic Caissons.*

[Messrs. Buckland and Smith, 3 Furnival's Inn, London.]

The Caisson is a hollow metallic box, open generally both at the bottom and top, the thickness of the sides proportioned to the strength and gravity required, and the mode of uniting being by dovetail. It is proposed that each caisson be seven feet in length, five feet in height, and from two to five feet in width, according to the nature of the work in which it is to be used. Caissons constituting foundations should be closed at bottom, and in raising one tier above another, each layer would become united to those immediately above and below it, by commencing the alternate vertical courses with a half caisson.

The results of various calculations of the comparative expense of granite and cast iron caisson works, give in some cases 20 per cent. in others 30 per cent. and even 50 per cent. and upwards, in favor of the caissons; and the advantage in saving of time, which in works on the coast is obviously of the highest importance, it is estimated will be at least four-fifths in favor of the latter.

The interior in every case is filled with liquid lime and rubble, or other suitable material to be found on the spot, so as to form a solid mass, girt with metal.—*Patentee's Advertisement.*

2. *New Threshing Machine.*

A portable threshing machine has been invented by Mr Rider, a mechanic and small farmer, who resides upon the Wallop estate, in the parish of Westbury, Wilts. The inventor is recommended not to exhibit the machine publicly until he has obtained a patent, or entered a caveat, which may be in a week or two. The principle of action is simple ; and it is calculated, that with the power of one man, it will make three hundred effectual strokes in one minute. If the experiment (which will be publickly made) prove successful, the utility of this machine will be great to farmers who have either uplands, or lands at a distance from their farms ; as this machine can be removed with as much facility as a winnowing machine, and its cost will not exceed £8 or £10.—*New Monthly Magazine, No.* 99.

3. *Sir R. Sepping's Patent for an improved construction of Masts and Bowsprits.*

The object of the patentee is to make large masts with small balk, in more numerous pieces, by which their cost will be considerably less than when made as usual of large Riga fir, in fewer parts of greater dimensions. The head and the heel of each of the masts of this new construction, are to be made of the same shape, in order that they may be turned upside down in case of accident. The pieces of balk are to be connected by trennels, coaks, bolts, and hoops.—*New Monthly Magazine, No.* 99.

4 —New Publications.

1 *Organic Remains.*

Mr. Charles Morrens has just published a pamphlet, entitled *Revue Systématique des Nouvelles Découvertes d'Ossemens Fossiles faites dans le Brabant Meridional,* with Lithographic plates.

This pamphlet contains facts and observations highly interesting to the student of geology. The researches and discoveries made by the author, prove that there formerly existed in this country, not only animals like those of the equinoctial regions, but also other species, such as still exist near the pole. The fossil bones discovered in several places belong to animals of the following species : the badger, the elephant, the hippopotamus, the whale, sparrows, waterfowl, reptiles of various kinds, tortoises, lizards, toads, and various fishes.

The quarries of St. Gilles, Milsbrock, Suventhem, Woluwe, and in the environs of Brussels, have furnished the greater part of these bones, which appear to be antediluvian.—*For. Qu. Rev. vol.* iii. *p.* 337.

2. *Baron Cuvier's Regne Animal.*

Baron Cuvier has just published a second edition of his valuable *Regne Animal distribué dúprés son Organisation,* which originally appeared in 1816, in 4 vols. 8vo. The work is now increased to five volumes, of which the two last, containing the crustacea, spiders and insects, are composed, as in the first edition, by M. Latreille.

As a necessary accompaniment to this, M. Guerin, a Parisian artist and naturalist, has just commenced the publication of an *Iconographie du Regne Animal de M. le Baron Cuvier,* in the same size as the book which is to contain a representation of one of the most remarkable species of each genus drawn from nature. It will be comprised in twenty-five livraisons of ten plates each, beautifully engraved, and all of which will pass under the inspection of Messrs. Cuvier and Latreille. The coloured copies are executed with extraordinary care. Some sets are also printed in quarto. —*For. Qu. Rev. vol.* iii. *p.* 329.

GLEANINGS

IN

SCIENCE.

No. 11.—*November,* 1829.

I.—*On the most eligible Form for the Construction of a Portable Barometer.* With a Plate.

It has been often made a subject of regret amongst those who take an interest in the enquiries of the metereologist, that there is no popular and full account of an instrument in such general use as the barometer. Were such a task well executed, and the different forms of the instrument, as usually made for sale, accurately described; their respective advantages and disadvantages clearly stated and compared with their prices; there is little doubt but it would be found highly useful. To the person desiring to possess a barometer, and wishing that it should be capable of answering the purpose for which he requires it, it is surely of importance to be made acquainted with all the peculiarities of the instrument, and of the modifications which may be induced by any particular method of constructing it. It is not uncommon to see a person, for want of this information, burden himself with an expensive instrument, which proves, when it comes to be used, totally unserviceable; while all are misled so far as to think a perfectly accurate barometer may not be had for about one third of the sum generally paid, or even less.

In the absence of such a work, I propose to give, what perhaps may not be without its use, a description of a barometer which I have employed for the last five years, and which has fully answered my expectations. I consider its advantages to be, greater portability, less liability to accidents, and greater economy in the first cost; while no difficulty is found to attend the applying new tubes, when in the course of time the old are broken. Excepting in some instruments constructed by Berge, this operation is not easily performed; so that when once the tube of one of these expensive instruments is broken, there is no repairing it without sending it to England. This is a serious objection.

A barometer is by many supposed to be a very mysterious and complicated machine; whereas, in theory nothing can be simpler: and the nearer this simplicity is adhered to in practice, the more useful is the barometer likely to be. After all the talent and mechanical contrivance that has been brought to bear on the subject, I am inclined to think the old and simple arrangement of the tube standing in an open vessel of mercury, is not only the best in every other point of view, but also the most portable. Or if I were to allow of any possible improvement on it, it would be the form generally used on the continent, where they give the tube the syphon shape, and by that means dispense with the cistern. The former arrangement I have now used for five years, during which period I have constantly been on the move; and I can give my testimony to its superior portability, durability, accuracy, and above all, economy.

Those commonly called Englefield's portable barometers are, as every one who has used them knows, subject to derangement by the leakage of the mercury from the wooden cistern. This, it is true, when the evil goes no further, may be corrected by comparing them with a standard; but as a person travelling cannot carry with him, or have access on every occasion to a standard of comparison, and as the leakage is still liable to occur at any time, observations made with such a barometer are always liable to suspicion. But the evil is even greater than this, for the leakage may continue till the mouth of the tube within the cistern is exposed: air now enters and dislodges the mercury. The consequence is a lengthening of the column; but if this increase in the column should happen, as it often does, to be counterbalanced by the fall occasioned by the sinking of the surface of the mercury in the cistern, a comparison made with a standard would fail to detect any derangement. The observer has his confidence restored, and is only finally undeceived (when the mischief is irremediable) by the intrusion of more bubbles of air occasioning the column to lengthen, when from other considerations perhaps he knows it should be diminishing. The instrument has now become useless, and the exorbitant sum originally paid for it lost, unless he will send it to England: for in none of these barometers that I have seen is there a contrivance to allow of the purchaser supplying a new tube. The vexation in fact attending the use of one of these barometers is not to be told, it must be felt; and to those who may think I have exaggerated the objections, I would recommend to make a journey through any difficult and wild country furnished with one of them; and if they do not obtain ample proof of the justice of my strictures, they will be more fortunate than I have been.

The objections, however, which I have stated, do not apply to all portable barometers. Troughton's for instance, are excellent, and in every respect indeed perfect save that of price, which is enormously high*. Berge's are very good and cheaper; but in a set of six that came to this country, and which I saw opened, not one arrived in order, the tubes were all broken, a proof they could not have been very portable. The wooden cisterns which have their several screws fitted in England become deranged in this country from the great differences in the hygrometric state of the air; add to which, though cheaper than Troughton's, they are still dear†. Dollond makes very excellent instruments to order: their price, however, I have reason to think is also high‡; as indeed must be, while made on the plan generally adopted: that which I am desirous of recommending, having but little work about it, must be cheap; while I am of opinion, that in durability and portability, they exceed any yet made.

Mr. Newman, an instrument maker in London, has lately brought out a barometer, in which he thinks he has successfully obviated the objections to Englefield's plan, that of continual variation in the zero point, without any means of detecting it. He has substituted an iron cistern for the wooden one, by which leakage being entirely prevented, as he says, none of the derangements I have described, can possibly arise. But as the iron cistern has still a wooden lid, there is left a possibility of derangement, and that there is something more than a mere possibility, will be evident from the fact that I saw one of these instruments which was purchased for the late Dr. Abel, at a price of 200 Rs. and in which there was a considerable bubble of air. The instrument was in fact unserviceable. A second of these instruments I saw in the possession of another gentleman, who spoke favourably of it. But I afterwards understood from him, that being packed by the maker in a *neat leathern case*, with a strap attached for buckling round the body‖, the case leather which was made to fit *too neatly*, in the dry weather contracted, and in the efforts made to extract the instrument, it was broken. The maker having sent no spare tubes, an expensive instrument was thus rendered totally useless. This is an objection that does not apply to the form I am advocating. In this, the only expensive part is the brass-scale, which is always applicable to another tube.

* 200 Rupees a piece.

† About 300 Rupees the pair.

‡ 350 Rs. the pair, I think.

‖ It is extraordinary that in making instruments for the Indian market, the artists of London will not advert to the difference in the habits of the two countries. Every thing appears to be made on the supposition that we are to carry the instruments ourselves, and is accordingly so contrived as to pack in the smallest compass—The consequence is, that in the great and sometimes sudden atmospheric changes in this country, more inconvenience, annoyance, and even injury, is produced, than would be the consequence of their having twice or even ten times the bulk. In India there are no difficulties as to carriage;—the great source of our troubles is small and curious packing obtained frequently by means of intricate divisions, fastened only with *glue*. Instrument makers should be told, that common glue will not resist a single rainy season in India, and thin or light wood work is of no sort of use. Every thing intended for this country should be massive and substantial, without any reference to portability.

The plan is, as I said before, the old one of a tube standing in an open vessel of mercury. About six years ago, I sent to Messrs. Dollond & Sons the particulars of what I wanted, and the following is a copy of their charges.

	£	s.	Rs.
2 Brass rods, with divisions from 17 to 31 Inches, 2 Brass verniers,	3	0	37
12 Barometer Tubes, filled and boiled, 7*s*. each,	4	4	50
12 Ditto ditto empty, 4*s*.	2	8	29

These have been in constant use for five years, and there are yet remaining the two brass scales and tubes. So much for the economy.

The tubes were 33 inches long, and of an internal diameter of $\frac{1}{16}$ inch, (I would recommend $\frac{1}{8}$ as preferable.) In consequence of the smallness of their diameter, they stood lower than tubes of the full size; and as I found them differ in this respect, I compared each of them with the standard tube which I had, $\frac{3}{16}$ inch, internal diameter. The error being once found, remained pretty constant, but they were subject to a small uncertainty of 01, or 02 inch, owing to the smallness of the bore occasioning sluggishness in the motion of the mercury. For this reason I would recommend any person ordering a similar apparatus from England to fix the size at $\frac{1}{8}$ of an inch at least. This will be ample, and yet not too large to make the tubes less portable. Tubes of the size I used, might almost be thrown down without breaking, or the mercury spilling.

The brass scales had two rings attached by screws, in which the tube was made to fit tight by means of a leather collar, with a couple of finger screws for tightening the rings; these finger screws being loosed, the scale was easily moved up and down on the tube, and by means of a projecting heel, correctly made to coincide with the surface of the mercury in the open cistern, the apparent deviation being doubled by reflection, it scarcely required the assistance of a magnifying glass to effect this: with the latter, however, the coincidence was easily made quite perfect.

The following apparatus was used for the purpose of setting it up, and quickly making it perpendicular.

A triangular box of eight inches side, with three wooden foot screws, afforded stowage to the several loose articles required for setting up and adjustment; and served also as a steady and convenient stand on which to fix it during observation. On one side of this box was attached a square pipe, made of deal, about six inches long, which by means of a wooden screw could be made perfectly fixed and steady. Into this square pipe fitted the case of the barometer, which was made of thin deal board, inch broad, and put together by wooden pins, being strengthened with square wooden dies at the top and bottom, about one inch long, on which the sides of the case were screwed, they being continued about an inch beyond it. At one extremity it opened at the end by the front being cut away for about six inches, which piece, being afterwards affixed to a similar square die, conveniently fitted into the projecting part, and held the cover quite firm. The barometer case, which it is evident was a hollow prismatic rod, being inserted into the square tube affixed to the triangular box, was secured by a wooden screw, and rendered perfectly firm. The cistern cup, which was a small wedgewood ware crucible, being now placed on the top of the box, which had been previously levelled by the foot screws, was filled with mercury from a bottle, which, as well as the cup, had been packed in the triangular box. The straining the mercury from the oxyd, which invariably collected on its surface in travelling, I suppose from the motion, was simply and conveniently effected by means of a notch cut in the lower edge of the cork, which being half withdrawn, the mercury passed through the small opening in the cork bright and clean as though it had been filtered through a paper funnel. The tube, with its brass scale attached, were now taken out of the case, and being set up in the mercury, was held in its place by means of a projecting piece attached to its wooden case, in the edge of which was cut a notch, something the shape of the Greek letter Ω, and a small wedge of deal, which was always at hand in the waistcoat pocket, being introduced in front of the tube, it remained perfectly firm, and allowed of the adjustment of the scale, which was moved up or down till the projecting heel was even with the surface of the mercury.

I have stated, that the triangular box was levelled by the three foot screws previously to setting up the barometer. This was effected by means of a plummet, formed of a silk thread with a musket ball attached, which passed through two small wooden studs attached to the side of the barometer case, the hole in the lower one being made something larger, in order to allow of a judgment as to the thread hanging freely in the centre. The place of the cistern on the triangular box, being fixed originally by experiment, was marked, and the cistern always carefully placed on the mark, so that it was at once known, when the plummet hung freely in the centre of the lower hole, that the barometer was vertical. By moving a little to the right or left, the scale was easily aligned with the plummet in one direction, and its verticality proved, while in the other they appeared parallel, which in dimensions of nearly three feet, allowed of the verticality being confirmed, certainly within half an inch, a deviation which would not occasion an error of more than 1000th of an inch. This confirmation was only resorted to occasionally when any suspicion of derangement occurred, though as it did not occupy a quarter of a minute, there was no reason why it should not always be practised. I should add, that it was this susceptibility of check at the moment of observation, which induced me to prefer the plummet to a spirit level, which I at one time thought of applying.

The barometer being now vertical, and the scale adjusted to the surface of the mercury, the height of the column was observed and read off. A little time was allowed it to settle, when it was again read off: the latter reading being recorded. If much difference occurred, a third reading was taken : and lastly the temperature of the mercury in the cistern was observed, being assumed to be the same as that in the tube. The tube was now removed from the cistern, the mouth closed with a cork, or piece of leather tied over it, and replaced in its case. The mercury was returned into the bottle, which with the cistern were repacked in the box, and the case being removed from the square tube, the latter was also separated by means of its screw, when the whole became portable again.

I have given the above description, because I think it may be of use to those who are able to procure unmounted tubes. They may in this way convert them into portable barometers at a very small expense of means, and in any part of the country, however unskilful the workmen; I have therefore at the hazard of being prolix tried to make it as clear as I could. To some I fear the use of such an apparatus may appear to involve a very tedious and troublesome operation. But I can assure them that so far from it, the whole could be performed in much less time than they will read the details. I had, however, a great convenience in performing all these little operations by being enabled to place the whole apparatus on the top of a box, which invariably accompanied me for other purposes, and which made the height so convenient as to allow me to adjust every thing without any constraint of position, one of the greatest enemies to expedition, and I will add, to accuracy of observation. And the improvement I have suggested at the commencement of this paper, having occurred to me, and been adopted, (though for want of tubes of sufficient length, I was prevented doing justice to it,) I found the operation so shortened, that I could see no objection whatever remaining on this score.

But it is to be considered, even supposing the reverse were the case, which is the just economy, by devoting a little more time to a work to finish it so effectually that it shall not require revision, or for the sake of expedition content ourselves with observations that are of no real value, and from which no correct result can be obtained? An objection is sometimes heard, as being made to accurate methods, that they consume time; but the very reverse is the truth, they save time in the end. At least, it is on such a conviction that I found my recommendation of the instrument I have described.

The circumstance of the tube being entirely exposed, and visible in all its length, I consider one of the most valuable features of this kind of barometer : at the moment of observation it is seen without any trouble, whether or not the barometer is in perfect order. So that should an air bubble by any means occasion a break in the column, the mischief is at once detected and remedied before a whole series of observations has been vitiated, and the time and trouble expended on them lost; or still worse, opportunities of observation passed away that cannot be recalled. And what is still more important, the error is not only detected, but may be corrected on the spot, if a few embers can be got for the purpose of reboiling the mercury in the tube. A barometer which may not at any time, at the moment of observation for instance, and without trouble, be scrutinised, and if found defective, corrected, cannot be considered otherwise than imperfect. No person who values his time will consent to be encumbered with such an instrument.

There is still another objection to the barometer, as ordinarily constructed, which does not apply to this form of the instrument. This is the limit to their scale occasioned by the case whether of wood or brass, which is necessarily closed for a certain length. Troughton's are divided only down to 16 inches; Berge's to 17; and some I have lately seen by Messrs. Gilbert and Co. only to 21. When I ordered my brass scales from Dollond, I had them divided down to 17 inches, which I imagined, then, would be the limit of their travels upward. However it proved otherwise, for both scales have visited elevations where the mercury sunk 1½ inch below this point. At each place an observation was made, and the record permanently preserved, by making a mark with the point of a penknife on the scale opposite the summit of the column. At leisure the quantity was accurately measured, by which it fell below the division marked 17. In a similar case, where a barometer of the more usual conscruction had been employed, the enterprising traveller was deprived of the satisfaction of knowing to what elevation he had ascended: the mercury sunk down and disappeared within the closed part of the case.

As before observed, I always thought that the instrument would be something more convenient in use, if the tube were fashioned into a syphon shape; and the little experience I have had of such tubes, strengthens the opinion I had previously formed in their favor. The straining of the mercury, the filling and emptying of the cistern, are all got rid of by this means; as also the trouble of carrying so many additional pieces. Nor do I think the difference in charge could be very great, or if it were, the purchaser could order straight tubes, and himself give to them the syphon form. This was the course I pursued, but the tubes not having been ordered with reference to this operation, were deficient in length, so that I was not able to give the plan a fair trial.

An objection is sometimes made to the syphon barometer, that the scale being fixed, the motion of the mercury is reduced to one half of what it would otherwise be; while if the scale be moveable, two observations are required instead of one, and consequently there are two chances of error instead of one. To the first objection I answer, that the scale ought not to be fixed; and to the second, that it is so far true that two observations must be made, I will not say instead of one, for I assert that it is the impossibility of making the second observation that renders so many barometers useless, except as mere playthings. This remark does not apply to Troughton's, Dollond's, or Berge's.

Under the idea that some of your readers might wish to order from England a barometer such as I would recommend, I have prepared a drawing of one (Pl. IV.) and shall give a description of such part of it as appears to require it. Fig. 1 represents the instrument without its case. The tube is 42 inches long, and $\frac{1}{7}$ of an inch internal diameter. It is bent into a syphon shape, so as to leave one leg $32\frac{1}{2}$ inches in length, the other $8\frac{1}{2}$. A brass scale 31 inches in length, divided down to 15 inches, to tenths of an inch, is attached to it in the manner shown in the plate, with the addition of a screw ring set on at the height of 14 inches from the heel. The apparatus shown in the plate, as attached to the heel, is a screw for slow motion; the *modus operandi* is obvious. When the heel of the scale is brought near the surface of the mercury, in the shorter leg, the clamping nut *a* is fastened, when the screw *b* will be found to act on the scale, and adjust it to the greatest nicety. To the heel of the scale should be attached a thin plate of brass, bent so as to embrace the tube, from the alignment of which with the surface of the mercury the most correct adjustment may be made. A small piece is cut out of the heel of the scale for the purpose of hitching on the shoulder of the ring; but it would I think be preferable to make the latter, with a projecting piece to take under the heel of the scale, which should be left even. Another inadvertence in the elevation I may notice. It is the position of the slow moving screw *c*, which to allow the instrument to pack conveniently, ought to be in front. There will be no difficulty in making these slight alterations. They are corrected in the section fig. 2. where the same letters refer to the same parts.

The scale I would have only divided to 10ths of an inch, the vernier subdividing to 100ths, it will be so much cheaper, and I am convinced that estimation assisted by a magnifier will subdivide the 100ths of an inch as accurately as is required, fully as much so as any barometrical scale I have seen. The reading is made by this means more simple and direct, and in observations which like these are sometimes required to be taken in a hurry, that is a point of some consequence.

On the continent, the stop-cock which used to be fixed at the bend, has given way lately to an improvement suggested by Gay Lussac, of forming the bend into a capillary tube, thereby preventing those shocks to the mercury which occasion so often the intrusion of air bubbles, and even of arresting the progress of the latter in a large tube; this is a decided and a great improvement, but such tubes as I used were already capillary, and scarcely required any reduction in their caliber. Whether such a modification would be advisable in a tube of $\frac{1}{7}$ inch diameter I can scarcely give an opinion. If Dollond or any of the superior makers be employed, it might be perhaps left to their judgment.

As to the manner of setting up such a barometer, I would propose the following improvement of the apparatus described above.

The eistern and supply of mercury being got rid of by the syphon form, the triangular board may also be dispensed with. For the security of the glass tube, a similar wooden case will be required as in the case of the straight one, formed in the same way, of thin deal boards attached by screws to two square dies, and secured in the middle by small screws or nails; or as I think might answer better, a square case of tinned iron, which should be painted inside and out : this would be perhaps stronger, and certainly as light as the wooden case, while it would be of smaller dimensions, and therefore more convenient. As the shorter leg of the syphon is only $8\frac{1}{2}$ inches while the longer is $32\frac{1}{2}$, I would have this case made broader below to the above length, and narrower above; in fact made to fit exactly the instrument : the use of this difference of dimensions will be afterwards seen, Fig. 3. Whether made of wood or of iron, the front should open for the purpose of inspecting the tube. A ring may be so affixed to the case, that when hung up with the front open, it shall be perpendicular. As the hanging position is not however favorable to observation, and as in very many situations there may be no convenient support to hang it on, I would propose to have three stays attached to it, by means of which it may be not only steadily supported, but also adjusted in a perpendicular position; a silk thread with a bullet or other small weight being attached inside the case, to indicate the correctness of the adjustment. These stays may be either of wood; or hollow, of tinned iron; and they may be so fashioned, that when put together they will form a prismatic rod of two feet in length, and of such dimensions as to fill up exactly the deficiency in the upper part of the case, thus completing a uniform prism. (Fig. 4.) They may be attached by means of rings or hollow shoulders fastened to the barometer case in a way so obvious, that any attempt at explanation would only render it obscure. The bottom of the case would rest on the ground, as well as the supports or stays, and thus render the barometer perfectly firm. In order the better to distinguish the surface of the mercury and edge of the sight, the inside of the case should be painted black.

A barometer such as I have described, and so fitted up, will I am confident give more satisfaction than any that has yet appeared in the market.

D.

Postscript.

Since writing the above paper, I have met with the following notice in a late number of the Revue Encyclopedique, of an improvement in the syphon barometer which I think is well worthy of notice : I shall therefore make no apology for offering the passage to your readers.

" M. M. Savart and Arago made a report on a new kind of barometer proposed by M. Bunten. Since the first application of the barometer to the measurement of heights, the ingenuity of observers, as well as of instrument makers, has been applied to modify it in a thousand different ways, and this chiefly with the view of rendering it more portable. Amongst these modifications Gay Lussac's appears to us one of the happiest*. Its lightness, its convenience, the accuracy of which it is susceptible, have been fully appreciated. We must however admit, that in particular circumstances, it is liable to derangement; and that on foot, on horseback, and more especially in a carriage, if the position of the barometer be horizontal, bubbles of air are likely, nay certain, to intrude into the larger column. This is the defect which M. Bunten has proposed to remove, and which he has accomplished without sacrificing any of the advantages of M. Gay Lussac's invention. For this purpose he constructs the barometer in two pieces, joined afterwards together, the upper one being drawn out into a capillary tube, which is inserted within the lower; through which capillary tube the mercury in its ascent as well as descent must necessarily pass. But a bubble of air, if it make its appearance, as it always follows the inner surface of the tube, is at length stopped by the re-entering angle formed at the junction of the two tubes; reversing the instrument for carriage, the bubble naturally escapes. This improvement has removed the only objection to which Gay Lussac's barometer was liable, and this without rendering it more fragile."

By a curious coincidence, soon after reading the above description, I had an opportunity of examining one of these barometers made by Bunten, and I must acknowledge that it very nearly answers my idea of a perfect barometer. The instrument was remarkably well finished, contrary to the general opinion on this subject, which seeks to find some palliation for the bungling and expensive contrivances which are applied to instruments of science, as fabricated by London artists, in the ela-

* The syphon tube with capillary bend.

borate and useless finish given to them. The glass tube has been already described; it is enclosed in a brass one, which equally serves the purpose of a scale, and of protection in carriage. The nonii, for there are two, move by rack work. The scale is of metres and commences at the middle, so that the two readings are + and are therefore added together. The nonii subdivide to decimals of a millimetre, one subdivision being ,004 inch. The instrument is suspended by a ring affixed to the top, and so adjusted as to hang perpendicularly. This instrument is eminently portable, convenient, and accurate; and to evidence the latter point, it may be mentioned, that on comparing this instrument with a very fine Troughton's, the difference was found too trifling to deserve consideration.

But though I highly approve of it, and think it the best I have yet seen, I must be allowed to make the following remarks:—1st, That the outer brass tube which conceals the greater part of the glass one, is an objectionable arrangement. For, as I have before remarked, unlimited confidence cannot be placed in observations made with an instrument in which the state of the column cannot be seen in its whole extent at the moment of observation. I may add, that the above arrangement adds to the expense.

2dly, I object to the rack work, as more expensive and more troublesome than a sliding spring clip, and not so accurate. It is greatly inferior to a slow motion screw, and I believe fully as expensive.

3dly, And lastly, I object to a hanging barometer, particularly where, as in the syphon barometer, both extremities of the scale are to be read off. The time lost in endeavouring to make a correct observation with such a barometer, can only be correctly appreciated by those who have themselves made the attempt. So that upon the whole, I am inclined to think the arrangement in the plate the least exceptionable; but with regard to the "fundamental features" of Bunten's barometer, i.e. contraction of the tube at the bend, if it be a large one, and the construction of the principal leg in two parts, with a projecting capillary tube from the upper into the lower half, concerning these vital improvements, there can be but one opinion. Whether our English makers will consent to adopt it, or if they do, whether they may not charge so high as to render it too expensive a delicacy for those who are likely to require it, is a point that must be considered by those who are disposed to make trial of, and to order such an instrument.

II.—*On the Tides of the River Húgli.* With a Plate.

The attention of the scientific public has recently been directed to the phenomena of the tides which prevail in the Húgli river, by the presentation to the Asiatic Society of the analysis of a most elaborate register of daily tides kept for many years.*

The subject appears highly interesting in its general bearings; when we reflect that the power of this tide has had such influence in determining the formation and peculiar appearance of our lower delta; and consider the superior advantages which our position at Calcutta appears to enjoy in its navigable facilities, over most parallel positions in the embouchures of the other great rivers of the globe. There is then little doubt, that any one who can add to the information already before the public, will be excused his presumption, should he venture to intrude with his contribution, the result also of his own meditations on the subject.

I forward for the benefit of your readers, what I think will be considered to constitute a valuable addition to our information on this subject, viz. an abstract of a tide register kept daily at Mud-point on Ságur Island, between July 1828† and July 1829 [Pl. V.] It has not, unfortunately, the advantage of Mr. Kyd's analysis, of being a general view derived from the observations for many years. It was procured by my own private means, and to elucidate some views which I have long entertained, and which are now confirmed. It remains now to compare this abstract with the data furnished by Mr. Kyd's tables.

* Published in our last number. It is to be regretted that we could not on that occasion, from their size and number, give the tables, which are lithographic plates.—Ed.

† The registry commenced on the 10th July 1828, and did not furnish the extreme tide of the month. The other paths likewise are influenced by this circumstance, especially the centre or mean-path.

All who have devoted any attention to the subject will be aware of the absolute necessity, in order to draw any general conclusions from such varying phenomena, of reducing the observations to mean results. I have essayed to make this reduction both with the Ságur and Calcutta tides, as will be seen in the annexed charts.

The method of making this reduction may, perhaps, be matter of opinion. My first idea was to deduce the mean level from the average of the spring extremes, flood and ebb, compared with the average of the neap extremes, flood and ebb; thus striking an average from the tides of the full and new moon, which are necessarily not of the same extent. On comparing a result from such data, however, with the result which is obtained from the general mean of the whole monthly floods and ebbs, there appears to be too sensible a difference in the position of the curve of mean level obtained from the two methods; and I therefore incline to give preference to the general mean, which is that described on the annexed charts. At Ságur either method will give a satisfactory result, the difference being scarcely sensible.

With the Ságur tides, the first essential consideration is to fix as base of the system, what may be called the universal mean level of the whole year, or perhaps what is the same thing, (where the position of the observer is so near the mouth of the river,) the general mean level of the tides of the bay of Bengal; and this I have supposed not to be very different from what I have assumed as the zero or base-line of the Ságur chart No. 1.* We then become sensible, on viewing the phenomena laid off on this base, what is the real and true influence to be ascribed to the prevailing winds of the two monsoons, and what the effect of the discharge of upland water during the freshes. The path of mean level is our certain guide to this.

In January we find the monthly mean level 1ft. 1in. below zero. In June the curve reaches 1ft. 7in. above zero. The level appears again to sink with the subsiding of the southerly winds to August; and what is at first view singular, does not again rise with the greatest discharge of the freshes in September to the height attained by the influence of the southerly winds of May. The influence of the freshes in 1828-29, was evidently at its maximum before the Equinox; but I fancy further observations would show this maximum to vary, as at Calcutta, between August and September; depending partly upon several circumstances, such as whether the first springs of September happen with a full or new moon.

The view of the Calcutta chart No. 2, is somewhat more varied. The path of mean level, which in the former chart No. 1, performs a kind of double curve, from the effects of the opposite monsoons, here presents one single undulation of much greater extent, bespeaking the preponderating agent upon its levels, to be the dry or full state of the upland rivers. The lowest state of the river appears to happen in January, in the height of the northerly monsoon. The curve then rises with the approach of the southerly monsoon till it has reached + 1ft. 8in. in May. The rise is then progressive to its extreme elevation in August or September at + 6ft. 9in. From this amount + 6ft. 9in. however, some little must be deducted for the depression created by the north winds of January; the total influence of the freshes cannot therefore be said to amount to the whole of the above quantity.

For the perfect connection of the two charts, it cannot but be regretted that we have not more satisfactory data from instrumental admeasurement, by which to assign the exact position of the base line of the one system of tides, upon the chart of the other system. Speculation might then give place to certainty; and the levels of Calcutta and its vicinity, relatively with the tides of the Sand Heads, would be known beyond doubt.

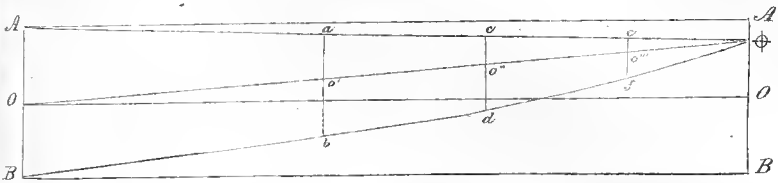
On the chart, No. 2, I have traced the path of mean level, which obtains in the eastern canal, which is connected with the series of salt lakes or marshes fed by the tides of the Bay, and which forms the eastern boundary to the town and suburbs. The extreme variation of the tide, which obtains in this canal throughout the year, is only four feet, and that due to season, or variation of mean level, only 2ft 4in. Now as this system of lakes has no communication with the river of any moment, (except by Tolley's nulla,) and communicates with a series of tide-creeks fed directly from the Bay, whose mean level is affected by inland drainage even less than this quantity 2ft. 4in., it seems natural to turn to this as the link to guide us to the solution of the difficulty of connecting the Calcutta and Ságur tide-systems.

* I have in the chart assumed the mean between the levels of April, May, January and February. I believe, however, it would be more correct to ascribe a greater influence to the southerly winds, than to the northerly; and that the proper base should be six inches, or more, below that assumed.

From every view of the question I have always been led to the same conclusion, that the highest tide in these marshes must be below the highest levels of the parent tides from which they are fed. *Prima facie*, it certainly appears bold to hazard a supposition that the waters of the Bay are frequently several feet above these waters, which are so far inland as 70 to 100 miles, taking the course of the creeks. But it cannot be otherwise, from the principle upon which they are fed, and their surface remains comparatively so constant.

Diagram 1st will perhaps explain this to your readers more clearly than words could express. The case to which it applies is the simplest form of a tide-channel, viz. that in which there is supposed no supply of upland waters.

FIG: 1



Let 0 be the mean level of the Bay tide, which rises and falls daily to A and B, respectively. At a certain distance up the creek, as at 0′, the daily variation of surface is found to be reduced considerably, measuring only *ab*, and still further reduced at the successive points 0″, 0‴, where the daily variation is, respectively, *cd*, *ef*. At last a point + is discovered, if the creek be sufficiently extended, where the daily variation disappears altogether, and we observe only a variation of surface between the full and new moons and their quarterings. The question then occurs, where are the levels of 0′, 0″, 0‴ relatively with 0. That they are above 0 is plain, because the last ebb cannot run as freely as the top flood; and the freedom of expenditure, must invariably be in favour of the flood.

The next question which strikes us, is, whether the flood levels *a*, *c*, *e*, must not rise every where to the same level A A, or whether it might not be supposed to rise even above A A, by the effect of afflux.

In the first case, the final point where the daily variation disappears + would be found on the line A A, and it would be simple to fill up the remaining curves of the diagram 0 0′ 0″ 0‴ + and B *b d f* +. But this, I believe not to be the case; and although I cannot fix with precision the exact place where + would be found, I must, from what data are before me, assign some intermediate level between A and 0, for the + of every simple tide, creek, or channel, where no inland supply of water exists. Nor is it difficult to reconcile this with the common principles of hydraulics. The only moving power to the fluid, is the variation A B, which we may suppose constant at the mouth of a long extended channel or creek. Now it is plain that this channel, being tolerably free for the passage of the tide, and not converging too rapidly*, before the tide can have reached any very remote point, the first cause of its flowing is removed, as the parent tide at the mouth, or A, has already subsided. This principle, which affects every intermediate point, according to its distance from the parent tide A 0 B, must at last leave + where the daily variation is at last dissipated below A A.

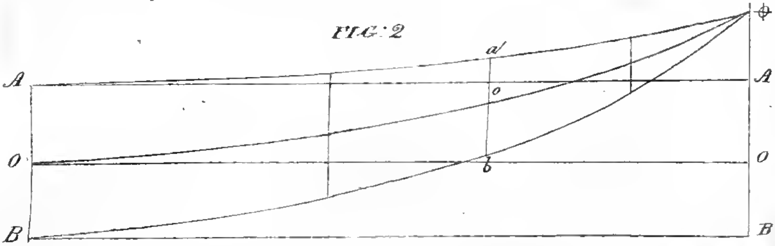
I find this confirmed in the Dutch table of tides, reduced to the Amsterdam guage or pile. The ordinary rise and fall in the Ye, near Amsterdam, an arm of the sea running into the land from the Zuyder Zee, being only 1ft. 6in.† while the ordinary rise and fall in the German sea is 5 feet. The mean level in the Ye, from these data, is several inches above the mean level of the German sea. The sea of Haerlem and the Dutch canals, of which we possess the levels, cannot be adduced as instances, both being under the influence of sluices, to keep down their mean levels. Many other cases, however, may be found much nearer to us, which appear to confirm this view.

* It is evident, that if the creek converged too rapidly, the principle of afflux would have effect in raising the top level, and such may still be the case in the first mouths of all these creeks, as well as of other rivers.

† Vide Lalande's 'Canaux de Navigation.'

The case just considered is the most simple, and of frequent occurrence in the backwater creeks of all large deltas; also in the many minor outlets of large rivers, which are only partially sensible to the influence of the upland freshes, and are, in some cases, closed entirely from the main river in the dry months. I have yet to adduce the more complicated case of a river subject to constant upland supply, besides the daily influence of tides.

FIG: 2



In a running stream it has already been laid down experimentally* and theoretically, that any obstruction or resistance offered to the stream, by river, or other contrivance, or by the meeting of an opposite current, will have the effect, by what is called afflux, of raising the level at the point where it is applied, and also of affecting the level for a considerable distance up the stream, to far beyond the additional level created by the obstruction at the point itself.

This principle alone is sufficient to warrant my placing the point + in the Hoogly tides, (diagram 2), where an inland supply of water always exists, although varying in quantity and force, above the level of A A. In proportion as the inland supply is withdrawn in the dry season, will the level of + approximate to A; and it is by no means certain, that in the dry months of April and May, it does not reach the level of A. Of this, however, we have not sufficient data to decide, nor, I am sorry to say, do we sufficiently know the limit of the tide at every season, to determine the elevation to which + rises in its extreme, when the freshes are at their maximum force. Perhaps some future observations may throw light on this desideratum. Meantime, we must rest contented with establishing the variations and relative levels at Calcutta, or the *a*, *ó*, *b*, of the 2nd diagram, one step only towards the general developement of the subject.

To assist us to the connection of the two systems of tides, we have an instrumental level struck between Calcutta and Diamond Harbour; and although no corresponding register of tides has ever been kept at the latter place, to afford us an intermediate chart, still, the lowest water mark has been ascertained within tolerable certainty. I have marked its position on the Calcutta chart No. 2, and if this lowest water mark correspond, as I believe it must, within some few inches of the lowest water mark at Ságar, or answers to the mark—8ft. 0in. on the chart No. 1, then will the dotted line, which I have drawn somewhat more than three feet below the base of the Calcutta tide system, represent the level of the Ságar base.

The differences of mean levels at Ságar and Calcutta, is thus found to vary between four feet, and nine feet, which is the slope between 0 and 0′ on diagram 2. for the two seasons relatively. The 0, at the extreme of the freshes, will be still one foot below the highest tide at Ságar. The 0′ in January will stand between three and four feet, below the high tide at Ságar, and I foot only above the mean level or 0 point of the salt lake tides, at the same tide, which is thus ascertained to be only two feet above the mean level at Ságar, and consequently, several feet at flood tide, below the flood tide at Ságar.

I must here check myself, before I am led, beyond the patience of your readers, into the host of reflexions which appear, as it were, to grow upon the curious facts adduced, chiefly relating to the peculiarities of our lower delta. I hope also, that I may not unnecessarily awaken any alarm by publishing the fact, that the high tides of the Sand-heads are so much above the level of the lake, in its highest state. As the Dutch of Amsterdam look to their Slaaper-dam for their protection against annihilation in their periodical inundations, so may we look to the great distance over which the tide has to travel, before it could reach the lake and low ground in its vicinity, as our safeguard against any like catastrophe. P. T.

* Vide Du Buat, Part 1, Sec. 3, cl. 3.

III. *Notice of Errors in* Arnott's *Elements of Physics, 3rd Ed.*—1828.

There cannot be a greater service conferred on the student or general reader, than the detection of errors in systems, or books of authority. Such works are almost always looked to as oracles, by those who are not masters of the subject they treat of; and in proportion to the reputation of the author, or the celebrity of the book, is the mischief which may be occasioned by their mistakes, their partial views, or false reasoning. In works on mathematical science there is less room for these corrections and criticisms, owing to the simplicity and small number of the general principles, which again, are such as are intuitively known to be true, and are always appealed to with the greatest confidence. But when observation forms part of the ground work,—when we have to build on the testimony, rather than the reason of man, the case is altered. In chemistry, in physics, in all experimental science, we have daily proof how little safeguard the greatest names afford against the intrusion of error. So, when we find the name of Davy sanctioning the opinion, that the air in a barometer proceeds not from the tube, but from the mercury; when we find Danniel unwarily asserting, and afterwards endeavouring to maintain, that, in correcting for the expansion of mercury in the barometer, we should allow for the expansion in the glass tube; when we find Thomson offering a theory of latent heat, the result of which would be, that to raise water to a higher temperature, we should lower the fire; when we find Ure giving us as the measure of the heat contained in a body, the number of degrees which its temperature may be above 32°, as if there were no heat at that point; when we find these and similar errors, all propped up at one time or another, by the greatest and most illustrious names in science, we may well have distrust of humbler men, and satisfy ourselves of the danger of trusting too implicitly to these miscalled systems. We are convinced that a very useful book might be made, by collecting the mistakes and paralogisms which are found, not only in the writings of these system-mongers, but even in those of the first philosophers of the day. We do not speak of those grosser errors, in which eagerness to make a discovery, outruns discretion, and which form the great justification the uninitiated have, in throwing ridicule on science and on scientific men. We do not mean such dogmas as "all nature alive;" for these are ephemeral errors, which are born one day and die the next. We allude rather to those mistakes in experimenting, arising either from defective observation, from pursuing a wrong course, from the warping of the mind by preconceived notions, or from that mixture of speculation with observation so common with many men, which would always lead them rather to guess an answer, than interrogate nature for one. If to these sources of error, to which the greatest men are more or less prone, we add those arising from half knowledge, we shall be persuaded that there is much in our modern books deserving of correction, and that a list of their errors would be longer than, perhaps, their authors suppose.

These observations have been prompted by the following communication from one of our correspondents, noticing some errors in *Arnott's Physics*, a work which though but recently published, has already reached a third edition. It is highly spoken of, both by reviewers and readers; our correspondent is, therefore, the more entitled to their thanks in noticing them, as well as to those of the author, who will, doubtless, take the opportunity of another edition being called for, to correct them. We must add, that we have not seen the work ourselves, and cannot judge, therefore, whether its general character is deserved. Our remarks have been perfectly general, and had no reference, whatever, to this work in particular.

p. 22. The author assumes as an established fact, that sulphuret of mercury consists of equal numbers of atoms of sulphur and of mercury; without explaining the reasons which have led chemists to infer the relative numbers of atoms in any substance.

33. He countenances the abandoned scheme of strengthening wood, by condensing *(crushing)* it.

39. He states that Caoutchouc becomes *permanently* elongated by too much stretching; unaware of the recently established fact, that its loss of contractibility is occasioned by the loss of latent heat, and capable of being restored by heating.

51. States that a cannon ball, or a shrapnel, if burst into fragments during their flight, would still retain their previous velocity; although it is clear, that it must be speedily diminished by the increased resistance of the air, to the increased surfaces.

64. A *new* theory of the spinning top, according to which, its line of rotation ought always to be perpendicular to the surface on which the apex turns, whether that surface be horizontal or not!

74. Describes gravity as a force acting in a line *directly* to the centre of the earth, which (setting aside cases of local attraction) can never happen, excepting at the equator and poles, owing to the oblate form of the earth.

84. He describes the swing wheel of a common clock as having 60 teeth, one of which escapes the pendulum at each beat; whereas there are only 30 teeth, one of which clears the pendulum at the end of every 2 beats.

88. Describes the balance wheel (balance) of a watch as being at each beat, carried to the extremity of its vibration, by *its own momentum*, (this would be a *perpetual motion*;) forgetting the use of the *mainspring*.

88. Ascribes the slow going of watches in hot weather, to the dilatation of the balance-wheel, [balance,] (which can have no perceptible effect,) instead of the true and universally admitted cause, viz. the diminution of rigidity in the balance-spring. This is a glaring blunder in a popular treatise on physics.

104. "A cannon ball having very great *velocity*, passes through a ship's side, and leaves but a little mark; while, one with less speed, splinters and breaks the wood to a considerable distance around." For *velocity*, read, *momentum*. A cored carronade shot, weighing only 30 or 40lbs. and moving with very great *velocity*, is more destructive than the solid shot of the same piece, though weighing 68 or 70 lbs. and impelled with much less velocity: the latter, pierces a moderate hole; the former, lays two ports into one. A strange mistake for a sailor!

104. He countenances the obsolete idea of injury occasioned by the "wind of a ball."

106. "On firing a cannon, the gun recoils with as much momentum in it, as the ball has." Read, with many times as much: little of the powder has been ignited when the ball quits the muzzle, and the gun is repelled by the explosive force of the remainder acting on the opposite air as a *point d'appui*.

121. He states that the Pisan tower was intentionally built with an inclination; contrary to the best authorities.

191. He states, that the roof of Westminster Hall approaches the limit of possible width without support; although it is not to be compared to the Exercise House at Petersburg.

220. and 346. He advances the novel assertion, that *atmospheric pressure* strengthens the joints of the animal frame: divest them of their tendinous and ligamentous appurtenances, and the capsular ligament would soon be stretched, and then forced in between the bones of the joint, the bones of which would thus be shewn to adhere simply by its intervention.

259. "The Ganges river 1800 miles from its mouth, is only 800 feet above the level of the sea; to fall which distance, it requires one month."

305. He admits without question the old idea, that the vegetable world compensates for the consumption of oxygen by the animal.

334. And, throughout the work, he gives the common sucking pump, the name of the "lifting pump," which is a machine totally different in form and principle.

354. He speaks of the ocean as being of uniform density, though aware of the compressibility of water.

361. He speaks of the "boiling" of fish oil and tallow at 600°: read *decomposition*.

383. Reasonings on warlike weapons, which would go to prove that mankind ought to use only their *natural* weapons for mutual offence.

391. "This process is used for making ice in India," meaning Leslie's apparatus: and P. 393, he does not remark, when talking of the loss of heat by radiation, that *this* is the process actually used in India.

393. He calls the hair hygrometer a toy; and Danniell's, a correct and simple instrument; and never mentions the most convenient of all, viz. the moist-bulb thermometer. The first has lately been shewn to possess excellencies which were overlooked by its inventor; and the second, though correct, is expensive, fragile, and inconvenient for ordinary observation in hot climates.

395. "It is the rain periodically produced in *mountainous* regions, which causes the extraordinary annual overflowing of many great rivers." Applicable to the Nile only and I'rawadi.

417. "The reason why the trade winds at their external confines, which are about 30° from the sun's place, appear almost directly *east* and become more nearly *north* and *south* as they approach the central line, is * * *". The *reason* might have been spared, as the fact is the direct contrary of what is here stated, for reasons too obvious to be delivered.

461. " A water screw has been applied at the bow or stern of steam-boats, to propel them in canals, where there was no room for side wheels." Side wheels are totally prohibited in canals, not for want of room, but on account of the destruction which they occasion to the banks.

472. In the professedly musical section " tone" is used for " note" or " musical sound;" whereas the former word is now universally limited to the designation of a particular musical interval, or the *quality* of sound in any instrument.

477. " All sounds which have simple relations to each other are remarkably agreeable to the ear," and *vice versa.* Now so far is this from being true, that the unison ($\frac{1}{1}$) and octave ($\frac{2}{1}$) which have the simplest relations, are heard as only one note, or produce particular effect; and the minor fourth ($\frac{4}{3}$), which has a simpler relation than the major sixth ($\frac{5}{3}$) and major third ($\frac{5}{4}$), is inferior to them, particularly to the last, in sweetness.

480. An unintelligible mechanical account of the reasons which have led all mankind to adopt the same seven steps in ascending through the major key: the truth seems to be, *that those notes which afford the greatest number of concords among themselves*, and whose intervals place them at nearly equal distances from one another, have been selected.

508. " Deafness ensues when the eustachian tube is obstructed, as by *wax!*" Read *mucus*, or swelling.

570. " A bath must propel the blood from all the external veins of the body towards the cavity of the chest, which the pressure cannot reach: it is this effect which, in part, causes the feeling of thoracic oppression, experienced by persons on first plunging into water, which feeling is usually attributed altogether to the cold." Therefore, a *warm* bath, or a tight pair of breeches or boots, ought to cause thoracic oppression. It is not pleasant to observe errors of this kind, in a work which was the first to refute Dr. Barry's idea, that the circulation through the veins is carried on by the expansion of the chest.

587. Dr. Arnott's mistakes on the subject of speech, are too numerous for correction, excepting by the writing of another treatise on the subject: but he states, at p. 587, that the easily distinguishable elements of speech, are about 50 in number; and that no single language contains more than about half of them. A moderate acquaintance with the tone pronounciation of living languages, and some attention to the subject, will enable any one to discriminate, at least, 100 distinct sounds, including the usual vowels as separate from those not usually enunciated, and also long vowels, as separate from short vowels. Of these, the English language will be found to contain 53, (omitting provincialism), Persian 55, Hindoostanee 61, and Gaelic no fewer than 71.

587. His physiological account of the formation of voice, is exceedingly superficial.

637. The pneumatic tractor would be a *dangerous* instrument, were it not *inapplicable.*

August 11*th*, 1829. D. B.

IV.—*On the Wernerian and Huttonian Systems of Geology.*

To the Editor of Gleanings in Science.

Dear Sir,—I have no wish, at this time of day, to revive the obsolete discussion relative to the Wernerian and Huttonian theories of the earth; my attention, however, has been directed to the subject of my present letter, by a sentence which appeared in No. 6, page 179. of the Gleanings. In reference to some interesting facts detailed in the said No. you remark, that in each case we are struck with "the triumphant nature of the answer, which the knowledge of these facts would have enabled the eloquent defender of the Huttonian theory, to give to those who could so little weigh the value of the rival theories." The facts alluded to are certainly most interesting, and far be it from me to detract from their importance. Neither do I feel at all inclined to detract from the merits of Dr. Hutton,—he was, indeed, a splendid and original genius: and well worthy of such a defender as Playfair. *In a practical point of view*, however, we are indebted to the followers of the Huttonian school, principally, for the great additional light which they have thrown on the origin of the Trap Rocks, and for the information which they have given us relative to granite veins, &c. I here, of course, only allude to the followers of Hutton *strict-*

ly so called; for be it remembered, that the most important part,—or rather, I ought to say, the most original and characteristic feature, of the theory of this great man, was the arrangements which it embraced for the destruction and regeneration of worlds; and that its distinguished author "saw nothing in the phenomena of Geology, but the ordinary operations of actual causes, carried on in the same manner through infinite ages, without the trace of a beginning, or the prospect of an end." And, however much his theory may be calculated to captivate the imagination—however much we may admire the vastness of the genius which framed it, and the acuteness of the arguments by which it has been supported, I am convinced that the Editor of the Gleanings in Science will entirely concur with Buckland, when he says, "that such views are, if possible, still more at variance with the conclusions of Geology, (as a science founded on observation,) than they are with those of theology." Do not imagine that I wish, by these remarks, to express my belief in the Wernerian theory. No one can be more aware than I am of its numerous imperfections, of the many unphilosophical opinions which it embraces:—but at the same time, the name of Werner must be dear to every Geologist; and I cannot help thinking, that if we candidly peruse the writings to which the discussion of these rival theories gave rise, we shall conclude that the Wernerians, so far from being *little qualified to weigh the merits of the question, had in the state of knowledge, at least*, an equal show of reason on their side, and that Playfair found in Dr. Murray an opponent worthy of himself.

The originators and defenders of these rival theories have passed away, and, while their friends and cotemporaries are *still* amongst us, the discussions in which they took so keen a part, have become little more than matters of history. The characteristic features of both theories have been lost sight of, except by one or two of their most devoted supporters; and the best of our present Geologists have set about collecting facts with a perseverance, which has nobly rewarded their exertions, and which has tended to throw a strong light on all the phenomena of Geology. In the self same number of the Gleanings, in which the sentence, formerly quoted, occurs, you mention the experiments of *M. Ganal*, relative to the formation of "artificial diamonds!" Here, then, is the hardest known substance formed in the laboratory of the chemist:—in the great laboratory of NATURE what may not have been effected by similar agencies! My only object in alluding to this circumstance, is to prove to you, that the Wernerians, also, might have found among our late discoveries, many wonderous facts in support of their opinions. But I am far from wishing to argue in favor of that theory, (still held by some, and those not the least distinguished of our cotemporaries,) which gives to our globe, exclusively, a Neptunian origin. On the contrary, I am willing to allow that many rocks, but more especially those of the overlaying trap formation, have in all probability owed their formation to an igneous origin, or, if you prefer the expression, to a Volcanic Agency, how generated, or how modified, it is not necessary to enquire. These remarks may appear to you trite and common-place; I do not, however, write for the practised Geologist, but rather with the view of guarding your *un-Geological* readers against an impression to which the sentence quoted might give rise,—an impression which might lead them to suppose that the Huttonian theory, with all its extravagances, had again come into fashion, and that a science, the importance of which has now been universally acknowledged, had again dwindled into a system of vague hypothesis.

But I may be asked, by those to whom I have more particularly addressed myself, what theory has been substituted in the room of those which have been exploded. To this I can only answer that Geologists, now-a-days, are not mere closet speculators,—that they have betaken themselves to the study of nature herself; and that, in the most remote countries,—among the most inaccessible mountains and valleys of the earth, they have been collecting facts, and making observations; and that they have been endeavouring to trace in the structure of our globe, that same unity of design which pervades all the works of the Creator; and, that it seems agreed on all hands that, without adopting any particular theory, we must patiently wait till the future progress of discovery shall have cleared away the mists which at present involve the subject, before we can pretend to form any decided opinion relative to what has been termed the "theory of the earth."

I may remark at the same time, that our best modern *theorists*, without pretending to draw any general and sweeping conclusion, have confined themselves to a much more simple question, a question which merely refers to the igneous or aqueous origin of particular rocks and suites of formations, and that the best modern observers seem inclined to adopt a theory which, *without infringing on the doctrine of final causes*, embraces the most unexceptionable dogmas both of Hutton and Werner,—a theory which, in the words of Conybeare, " admits of the operation of a volcanic agency beneath the pressure of an incumbent ocean, but which does not in any degree question the Neptunian origin of rocks, which have evidently been formed in the bosom of that ocean." It is now very generally allowed, that the trap rocks are of volcanic origin, and many too give a similar origin to the granites : and while on this subject, I cannot but allude to an opinion which has been of late most ably supported, and in favor of which every day's experience is adding new facts. The supporters of the opinion just alluded to, contend that whole countries have been raised to their present elevated position, by some expansive force which operated from below; and that the granites, more especially, have been *upheaved in a solid form*, thro' superimposed masses of overlying rocks. Supposing this theory to be true, we can easily conceive that that *cause*, which operated in elevating so large a body of matter already formed, might have rendered fluid the strata, which, lying in an inferior position, it might first have acted upon, and that, too, under an enormous pressure. In this way, we might account for the circumstance that the *true* granites rarely occur stratified; and in the same way, we might explain the phenomena of granite and porphyry veins, &c. &c. while the chemical constituents of the granites, their mode of occurrence, their similarity, or rather identity, in point of composition with the gneisses, the circumstance of their passing by insensible degrees into all the rocks with which they are associated, would all seem to indicate that the cause which *originally* formed, what have been called, the primitive rocks, was *one and the same*. We might, in this way, reconcile Dr. Macculloch's theory of the igneous formation of the *true* granites with the appearances observed in nature. If the present elevated position of the granites be attributable to an expansive force which operated from below, *at a period posterior to their first formation*, I should also conclude, with some of our latest geologists, that *this period* must have been posterior to the formation of some of the newer marine deposites. I could adduce many instances from Indian geology in proof of such an opinion.

The enormous extent of the few distinct formations as yet discovered in India, and the ease with which, in most instances, we can trace their limits, give us many advantages. The more recent of the supermedial rocks, the oolites, and chalks are, as far as we yet know, wanting in this vast continent; and a characteristic feature in its geology, is the enormous extent of its *lower* granite tracts, many of them distinguished, though on a small scale, by all the wildness and ruggedness of outline of an alpine country; a circumstance which might, perhaps, be attributed *to the absence of that superimposed weight of newer rocks, which might, in other countries, have presented an obstacle to the full operation of the supposed expansive force, especially in such situations where its power was weakly exerted;* while, among the Himalaya mountains, we may suppose, that this force, *operating without opposition, and in all its energy*, raised to their stupendous height the pinnacles of our globe. But I have no room at present to indulge in such a discussion, and shall content myself with remarking, that the fact alone, that many of the enormous masses of primitive rocks, constituting entire countries, are skirted, or I may say, *isolated*, by newer formations corresponding exactly with each other, even at the greatest distances, is a strong presumptive evidence, that the said primitive rocks were elevated, or rather, I ought to say, forced through a superjacent formation, the remains of which are still found skirting the mountainous countries which now occupy a central position in respect to such newer formations. The skirting belts of these newer rocks are frequently very narrow, and the above opinion is certainly a more rational one, than if we were to believe that so many *local causes should have operated at one and the same time*, and should have formed rocks precisely of the same nature, and characterised by the same kinds of organic remains; and that, too, in the different positions, often far distant from each other, in which such newer formations are found.

I cannot conclude this, I fear too lengthened epistle, without alluding to the most interesting experiments of Sir J. Hall, relative to the influence of sea salt as an agent in causing the fusion and consolidation of the incoherent parts from which he supposes our newer rocks to have been formed; and, in connexion with this subject, it is worthy of remark, that the saliferous sandstone formation, to which his experiments in this case more particularly refer, is almost always associated with the trap rocks, and, indeed, in some cases passes into them, and that, as before stated, the trap rocks are now pretty generally allowed to be of volcanic origin. The saliferous sandstones are characterised by a nearly total absence of organic remains, a circumstance which ought not to be lost sight of in considering this subject.

The above remarks may have some slight degree of interest to those of your readers who are as yet unacquainted with Geology. If so, they are at your service—and I shall conclude with an expression to you individually, of my warmest thanks for your exertions in the cause of Science. Your periodical has given a new impetus to that spirit of enquiring, which has of late manifested itself so strongly among the Company's servants. For myself I may say "*non nostrum tantas componere lites;*" but though an inactive, I shall still be an interested spectator of the progress made by the Indian School of Science, and am

Your constant reader,

OMICRON.

Note by the Editor.

We must take merit to ourselves for the remark we hazarded, if only for having produced the preceding letter, which will we think be considered highly interesting by our readers. While we give our correspondent every credit for the candour and ability with which he has combated the tenour of our remarks, we must, however unwilling to differ from one so capable of forming an opinion on the subject, confess that we still think all the philosophy, if not all the talent, was on the side of the Huttonians. We are afraid, that if we were to make a full profession of our geological faith, our correspondent would find us tainted with more heresies than we should care to acknowledge at this early period of our editorial labours.

A very full discourse has been recently delivered by Dr. Fitton, before the Geological Society of London, on quitting the chair of President; in which he has taken a view of the recent progress, and present state of Geology. Dr. Fitton seems to be something like ourselves, an admirer of Playfair; and he makes favorable mention of his views in different parts of his speech. In particular, the very dogma alluded to by our correspondent, "that he saw nothing in the phenomena of Geology, but the ordinary operations of actual causes, carried on in the same manner through infinite ages," appears to be not very far removed from the conclusion at which Dr. F. has arrived, that, "the more recent views, which regard a certain class of causes as having ceased from acting, will probably give place to an opinion that the forces from whence the present appearances have resulted, are in Geology, as in Astronomy, and in General Physics, permanently connected with the constitution and structure of the globe." Our correspondent will find a notice in our present number, of the opinions of Baron Ferussac, which appear, also, to favor Playfair's views. We mention these particulars merely to show our correspondent, that there is not in the geological world that unanimous rejection of the Huttonian views, on this particular point, which he assumes. For our own parts we lay little stress on opinions, however great the authority by which they are supported; and notwithstanding our admiration of Playfair, we should be as sorry to pass for an exclusive Huttonian as for a Wernerian. Our remark was, we think, natural in one who had read with attention, the very acute and subtle arguments by which Dr. Murray proves so satisfactorily, that no such source and supply of heat, as the Huttonians wanted for their theory, could exist, while he finds no difficulty whatever, in having on his hands an ocean of water, covering the globe to the height of five miles above its present surface, however unable to say what had become of it.

V.—*Remedy for Cholera.*

The following remedy for Cholera is taken from the work of Hermannus Vander Heyden, a physician of Ghent; dated 1653. It is sufficiently curious to deserve a place amongst our Gleanings, and as it is on a subject fearfully interesting in this country, our readers will, we doubt not, thank us for the sound advice which it contains, as well as for the remedy itself, which we believe to be such, as will be approved of by competent judges.

The second discourse of the disease called by Physicians, Cholera.

I shall in the next place come to speak of the disease called Cholera, which as it is most swift and violent in its progress, so is it usually also most dangerous, and destructive, so that I could not forbear, with all the speed I could, to give an account of the nature of this disease, to the end that the fury of it (which is sometimes so great, as that it takes away a man in the space of four and twenty hours, (or at least, brings him to death's door,) may be made known to all, and chiefly to such as *are destitute of any sudden means of help,* so that they may in due time *provide themselves of convenient remedies,* which I do the more willingly, because I have long since observed, that betwixt the excessive flux in the diarrhœa, and this disease, which we call cholera, there is some affinity, in so much, that sometimes it may be a doubtful business how to distinguish the one from the other.

Now this disease, called cholera, is, a continual and violent voiding, both of choler, and other humours, and whatsoever a man takes in; and that, both upwards and downwards, by vomit and stool; the violence whereof is so great, as that nature being not able to bear it, the patient's strength must thereby necessarily be brought extremely low, and hence you may easily perceive how it differs from a diarrhœa with gripings of the guts, and from other fluxes. The cause of this disease, I have (in my French treatise) shewed to be, rather the malignant quality of the humours, and of the things taken in, than the acrimony of cholera; which very thing seems to have been also insinuated by ancient authors, where they tell us, that our medicines are to be varied according as that cause of it is different, whether it proceeds from a hot or from a cold cause. And even Alexander Trallians professeth, that he had recovered divers that were at death's door, by administering *wine* unto them. Erasistratus, in the like manner, considering, that there was respect to be had in this case, as well to the crudity, as to the acrimony, affirmeth, that nothing is more effectual in the cure of this disease, *than wine* diluted with water: which is also commended by Galen in his book concerning phlebotomy. But for as much as in this so violent a disease, these things seem not to be at all sufficient for the cure of it; we must, therefore, have recourse to *laudanum Theophrasti;* of which, people that are of ripe years, may take a pill of four or five grains in weight; and children, of one grain only; and so, proportionally in all other ages; yet this is not to be administered, unless the patients have before purged sufficiently, both upward and downward; least, otherwise, something should be retained, which should rather have been purged out; neither, yet, is the taking of it to be deferred so long, as that the patient is fallen into convulsive fits, and his excrements are of the *colour of whey;* WHICH ARE EVIDENT SIGNS THAT NATURE IS SPENT IN HIM; for then it must be taken with ALL SPEED: and in case the party cannot keep it, but that he vomiteth it up again whole immediately, you may then either wash that in wine, and give it him again, or else you must give him another fresh one. And in case he does retain it, and yet his evacuation cease not, you must then give him another, of the quantity of two or three grains; yet for the most part, one pill only is sufficient to do the business; and to the end that he may keep it, and not vomit it up again, he must chew in his mouth a slice of citron, the peel taken off, and rolled about in a little sugar, if he please. Other opiates are not in this case so proper, neither are they so easily retained; yet where this cannot be had, you may give him some Rondeletius in his pills, or else some of my pectoral opiate, which I have set down in my French treatise, concerning the cough, to the quantity of half a scruple. And to the end, that no man may be mistaken, I shall here set down the composition of this *laudanum Theophrasti,* as it is usually made among us; although some of the ingredients, being put in, are so small, and so disproportionate a quantity to the rest, (as, for instance, of unicorn's horn,) seem to confer little to the business.

Receipt for laudanum Theophrasti.

Take an ounce and a half of *Spec. Diambre,* infuse this in half a pint of aqua-vitæ rectificata, and set it upon the embers, or in the warm sun, for the space of twelve or thirteen days, that is, till the infusion be sufficient; in the mean time, often stirring it about. Then leaving the powder behind, add to the infusion two drams of *opium thebaica,* dissolved in a dram and a half of the *juice of henbane.* The next day after, add to it mummy, red coral prepared, and eastern saffron, of each one scruple, and of white amber half a dram, and of unicorn's horn, and oriental musk, of each four grains. And having brought this to the consistence of a thin poultice, you may again (if need be) for some certain days together, add some small quantity of aqua-vitæ rectificata; and having so done, you must lastly dry it into the consistence of a solid mass; out of which, you may make up your pills as occasion shall require: which composition, seeing it is of so great efficacy, as that it is a certain, and indeed the only remedy in this disease, I have always advised many great personages, and others, that have dwelt far from any city, and have had great families, that they should never be unprovided of one or two of these pills, and of the weight aforesaid; least they should be suddenly snatched away by the violence of this disease, before any can be new made, or be sent for to the neighbouring cities. I have also in like manner always persuaded my friends, that whenever they take any journey, seeing these pills are not every where to be had, they would be sure to carry with them one or two of them. The same care did I also take, above twenty years ago, for my sons, when I sent them abroad to the university; instructing them in my way of proceeding in the cure as well of this disease, as of the griping diarrhœa before spoken of, and also in the manner of preservation from and cure in the plague: wherein, notwithstanding, I advised them to consult the physicians of the place, that so the cure might be the more certain and speedy.

As concerning diet, I have spoken very largely in my French treatise; where I have said, that for their ordinary drink, the sick parties must take water and verd-juice, of each a like quantity mixt together; that is to say, the quantity of a cup and a half of either, with the yolk of an egg boiled in it, with a small quantity of sugar. And if the party be not very thirsty, he may then take a draught of red wine; or where that cannot be had, of old white wine: with which, if need be, you may mix water, with a little cinnamon boiled in it; you may allow your patients a draught of small ale, so it be clear. He may also have made him several kinds of broths, of the aforesaid verd-juice; or white wine, with water, sugar, and the yolk of eggs mixed with it; restorative jellies also; and a toast of white bread in wine mixed with water, and with a little sugar put upon it, are here of very good use. And whatsoever he takes either of meats, or drinks, he must take them cold; and least, by drinking too much, his evacuations should be the more violent upon him, he may sometimes allay his thirst, in some measure, by taking two or three spoonfuls of wine and water, mixed together in equal quantities, with a little sugar, and juice of citron added to it, to make it relish the better. He may also take a slice or two of citron (the peel taken off,) rolled about in sugar. Conserves of red currants, and of berberries and the like, which are both cooling and astringent, and also pleasant to the taste, are here of good use also.

VI.—*On the Distress and Exhaustion consequent to Exertion at great Elevations.*

To the Editor of the Gleanings in Science.

Sir,

If the following observations are deemed worthy of a place amongst your "Gleanings" I shall feel obliged by their insertion. They relate to a subject which, comparatively speaking, but few individuals have had an opportunity of observing to any great extent, viz. the difficulty of respiration, and consequent fatigue and inability to proceed above a few paces at a time, up an ascent, in situations of considerable elevation above the level of the sea. I am not aware of any thing having been written on this subject, beyond the facts just mentioned; and having been placed in situations favorable for repeated observations relating to it, I determined on making such remarks as might possibly tend towards its elucidation. One of the first ideas which occurred to me on first experiencing this feeling, was, to try the state of my pulse; and certainly, whatever its remote cause, it appears to be connected with the rapid circulation of the blood; as I found my pulse rise from 64, its usual rate, to 160 beats in a minute; the inspirations being in proportion. This effect was produced in me, I think, at about the height of 12 000 or 13 000 feet, where, unless the road was unusually steep, I was enabled to take 30 or even 40 paces without being under the necessity of stopping from fatigue, (particularly felt in the loins and legs,) and the impossibility of making further progress without resting; but whatever the distance I was enabled to accomplish at one time, whenever my pulse rose to 160, I was compelled to rest: and a very short one restored my strength. As I gained a greater elevation, the number of paces I could take, decreased in proportion; till having ascended to the heighth of about 17 000 feet, the ascent being rather steep, I found myself quite unequal to the exertion of advancing even six steps, without being completely exhausted; and without the assistance of two men (Bhoteahs) accustomed to travel at such elevations, and a *jabbu* (an animal bred between the Tartar *yak* and common cow), to whose tail I tied myself, (it being too weak, from want of food for three days, to carry me, as was intended,) I should never have reached the summit of the pass, which proved by barometrical measurement to be 17 800 feet, the column of mercury being only a little more than 15 inches—and even with their combined-aid I did not accomplish it without very severe fatigue. This sensation is experienced by the natives, though in a less degree; and they attribute it to the poisonous exhalations of a plant, (the monks-hood,) which grows at the height of about 12 000 feet, and perhaps somewhat higher; but so far from its being found at the height I ascended to, nothing of the vegetable kingdom was to be seen, not even a blade of grass or patch of moss; which I remarked to them. Even the natives are said to suffer so much, as sometimes to fall down in a state of insensibility; and this I believe would have occurred to me, had I exerted myself, so as to have caused my pulse to rise above 160. The height to which this may be raised, will, of course, vary with the constitution and habits of the individual, as I tried the pulse of a sepoy (an inhabitant of the lower mountains,) which rose to 172—mine never exceeded 160 on level ground. In descending, however elevated, I never experienced any difficulty in breathing, however far I walked.

I am not aware that any one save myself, ever tried the state of his pulse in similar situations; my trials have been frequent, and made with a watch with second hand. But although I have proved the extraordinary increase of the pulse to be connected with the feeling, this circumstance in itself cannot be considered as a satisfactory elucidation of the matter; much yet remains for discussion, regarding the cause of this increase of the circulation. That the rarefied state of the air at great elevations, being insufficient for the due supply of the lungs, will be pronounced to be the sole cause by many of your readers, is probable enough; as it may be said, that the lungs require a certain quantity of air to keep them in proper action, and that in proportion as it is rarefied, so much oftener must the person breathe, and consequently so much more must the pulse increase. But if this were the true solution, how happens it that the breathing is not affected, or the pulse accelerated, when a person is stationary and undergoing no fatigue at the same elevation, which I have proved to be the case; or even when he is walking along level ground, or in descending? This last was proved by my companion and myself, descending in about 20 minutes, running when the ground admitted, a distance which had taken us a full hour to ascend; without experiencing any inconvenience in breathing, beyond what we should have felt in lower situations. That the rarefaction of the air is one cause of this debilitating feeling cannot be doubted, and the difficulty lies in explaining the fact of no inconvenience in breathing being felt at the height of 17 800 feet whilst in a state of rest, with that of every particle of strength being completely prostrated in taking six paces on my ascent to this great elevation. It would be a natural supposition that the inspirations must be in proportion to the density of the atmosphere, and that the state of a person's pulse might be told on knowing the height of the column of mercury which was supported by the air we breathed, for the pulse and the breathing will, I imagine, always bear a certain proportion to each other; but this supposition I have proved to be erroneous, and that, however elevated the place, the usual number of inspirations are in a state of rest just sufficient for the due supply of the lungs and support of the circulation, as the air is at the level of the sea. If then, notwithstanding its rarity, the air proves to be an equivalent to that at the level of the sea in a state of rest, how is the circumstance to be accounted for, that I could not take six steps, which was also the case with my companion, without being completely exhausted, and that my pulse rose from 64 to 160. The breathing I conceive may act on the pulse or *vice versa,* as I presume the latter is the case with a person labouring under the effect of violent fever; and the increased circulation may require a proportional supply of fresh air; but the feelings which I experienced, did they derive their source from the immediate increase of the circulation of the blood, or from the want of a due supply of air to the lungs, as the air, notwithstanding its tenuity, was sufficient in a state of rest? We must suppose they were occasioned by the former. Supposing the inference I have drawn from the facts, which I have related, should be correct, the next question is, What is the cause of this rapid circulation? As at the level of the sea, so at this elevation, we must suppose it to have its origin from violent exertion. But when six paces taken at the rate of 60 or 70 in the minute, produce a more powerful effect on the system than perhaps some hundreds taken at the utmost speed in the former situation; and as this effect is not produced in walking on level ground; must it not arise from the weight of the body which a person has to raise? Remove this, a measure I have frequently had recourse to by fastening a rope round my body, and causing a couple of men to assist in pulling me up, and the fatigue is comparatively trifling.

Having traced the subject so far, to the best of my ability, I shall not further trespass on your pages with any crude attempts to bring to light the ultimate cause of the feeling, but leave its further elucidation to some abler pen, should the facts which I have related draw the attention of such to the subject.

J. M.

VII.—*On the Produce of the Soil, and Rent of Land, in Hindustan.*

In the following table, which has been drawn up from data collected in zillah—— I have exhibited the produce per English acre of the several kinds of grain grown here. The Wheat and Barley are expressed in bushels and pounds; the other produce in pounds avordupois. The table, it will be seen, is the result of experiments conducted in twelve several divisions of the district.

Div.	*Wheat. Bush. lbs.*	*Barley. Bush. lbs.*	*Gram. lbs.*	*Rice. lbs.*	*Jowar. lbs.*	*Bajra. lbs.*	*Goojye, mix. of Wh.&Bar lbs.*
1	18 .	40 . 18	1757,4	790,	561,92	210,72	421,44
2	21 . 53	17 . 19	1627,	1162,58	842,	—	985,24
3	17 . 4	23 . 42	583,64	900,8	—	632,1	—
4	12 . 60	22 . 26	749,76	1387,24	464,	657,	916,72
5	13 . 16	12 . 27	870,	864,	—	263,	—
6	15 . 20	22 . 11	1221,	842,	513,3	526,8	885,88
7	14 . 7	34 . 3	1404,8	1794,2	—	—	—
8	28 . 31	54 . 10	1295,	1701,	—	—	1487,5
9	18 . 38	25 . 24	1061,	1558,	517,44	517,	—
10	18 . 55	35 . 2	1137,36	941,76	1233,6	812,4	—
11	20 . 5	29 . 45	1327,	1109,	—	707,	—
12	24 . 37	—	1155,	1109,	—	1231,	—
Aver.	18 . 42	28 . 48	1182,41	1179,96	688,71	617,44	919,35

The average value of the gross produce of the average of Wheat, Barley, Gram, and Rice, at a mean price between the average of the 10 previous years, and that of the present, gives per acre.

	Wheat.		Barley.		Gram.		Rice.	
	s.	*d.*	*s.*	*d.*	*s.*	*d.*	*s.*	*d.*
	21	0,8	15	2,67	15	11,44	14	4,75
Rent at ⅓	7	0,2	5	0,89	5	3,81	4	9,58

The average returns in fold for the seed are as follows,—but it must be mentioned that the quantity of seed sown, has been, perhaps, incorrectly stated: and the results must, therefore, only be taken as an approximation to the truth.

Wheat.	Barley.	Gram.	Goojye.	Rice.	Bajra.	Jowar.
8,93	11,43	9,36	7,13	13,51	167,5	125,72

The average produce of wheat in England is stated in Loudon's Encyclopedia of Agriculture at 28 bushels, Seed 3½ bushels, an 8fold return—Of Barley the average produce of *Great Britain* at 28 bushels, seed 2¼ bushels, 12⅔ return. In the same work it is stated, that the ordinary return of wheat in Savoy, is from 3 to 5 fold—near towns from 5 to 7: and that before the revolution 4½ fold was regarded as the average return of wheat in France.

Assuming the wages of a common day labourer in England at one shilling—he could purchase (when at 60*s.* the quarter) 8½lbs. of wheat. With the corresponding sum of 8 annas in this district (at the exchange of 2*s.* to the rupee, which has been used in the above calculations,) a labourer in this district can purchase 52lbs. of wheat;—the relative prices are, therefore, as, 1 in England to 6½ in this district. The wages of a common day labourer in this district are about one anna per diem, or 1½ pence English—which is at the rate of 1 here to 8 in England. The value of money, therefore, to the cultivating classes may be assumed as about seven times greater in this district than in England.

Taking the average, therefore, of the rents of the four grand articles of produce, —Wheat, Barley, Gram, Rice; we have 5*s.* 6½*d.* per acre, which multiplied by 7, gives 38*s.* 9½*d.* the value of the rents per acre in English money at Indian value.

But this is not altogether a fair criterion. Out of 100 parts, allowing 57 to be cultivated with these articles, 38 parts with the inferior small grains and millets, and 5 parts with Cotton and Sugarcane, we gain the following results.

	s.	*d.*		*s.*	*d.*	
57 at	5	6½	=	315	10½	average, 4*s.* 6¼*d.* × 7 = 31*s.* 7¾*d.* comparative value of each free acre.
38 at	2	3	=	85	6	
5 at	10	3	=	52	6	

But the actual rents are higher : from the most careful investigation and conclution on all the data I could gain, it appears that the average proportion of the gross produce taken as rent, is not less than ⅗ds instead of ⅓d. The average rate of rent as yet determined in this district, amounts to 6*s*. 5½. per acre : the average of 39 estates in another district gives 8*s*. 5*d*. and in a third (this last is a good deal supposition,) 7*s*. per acre, which multiplied by 7 for a comparison between this country and England, gives respectively 45*s*. 0⅝*d*. 58*s*. 11*d*. and 49*s*. per acre.

Out of the rent of the land, the public direct land-tax is 75 per cent. by the regulations. On 100 acres therefore at 7*s*. per acre, or from 700*s*. the public tax amounts to 595 shillings, leaving 105 shillings to the proprietor, to defray all expenses, to make good all calamities of season, to furnish the interest of money horrowed, (never *less* than 24 per cent.) to yield all profits, to discharge all indirect taxes, (stamped papers, &c.) and to indemnify him for all police charges and responsibility.

Perhaps the above accounts may be of use to those who entertain so strongly the idea of the advantage to be gained by the colonization of Europeans in this country. It has been remarked, that the returns in fold are somewhat doubtful : they are not however *very* wide of the truth, and they shew an average return not much inferior to that of England. It is in the prices that the main difference exists : and if a labouring European in this country could be contented with the common food, and clothing and housing used by the native, he would, no doubt, be as well off (always excepting the climate) as in England. If a European proprietor could be contented with 25 per cent of the rents of an estate, and square his ideas of comfort, and the education of his children, to the same standard, there can be no doubt, that he could feed himself and his family. But beyond this (even putting out of view the question of climate) he could do little with all his skill, industry, and capital. It seems to me, at least, very doubtful, whether with all these be could ensure so much larger a produce from the land than is now attained, as would at all reimburse him for the expenditure.

In Bengal, where the permanent settlement prevails, I suppose the proprietors understand the proportional higher value of their land, and would exact a proportionally higher price for it : so that capital laid out there in the occupation of land, would, perhaps, not be much more fruitful than it promises to be in the upper provinces.

I am inclined to think that the produce of land in this country has been underrated, and reckoned more by the money rents it yields, than by its actual quantity ; and a comparison made between 6 or 7 shillings per acre in this country, and 50 or 60 in England, whereas it is in the value of silver that the main difference lies.

The average rent per acre on the whole of France is 11,075*s*. (reckoning the franc at 10*d*.)—*Revue Encyclopedique*.

	s.	*d.*
In Hanover the average rent per acre, is	9	6
In the Campagna di Roma,	15	0

Loud. Enc. of Agr.

It would appear, therefore, that there is as much improvement required in those parts of the continent of Europe as in this country.

H. S. B.

VIII.—*Statement of the Expense attending the Manufacture of Raw Sugar* (Gúr) *in the districts of Saharunpúr, and Muzafarnagar.*

In collecting the following particulars from the people of the Pergannahs above mentioned, the object was to ascertain at what rate per cent. the *Zemíndars* or proprietors of sugarcane, manufactured the *gúr*, calculating from the time the cane arrived at the mill, until the *gúr* prepared from it, was sold into the hands of the *Banias*.

Had there been time to have made the investigation, it would have been more satisfactory, and the result obtained more correct, could the manuring and ploughing the ground, and the setting, watering, cutting, and carrying of the cane, have been taken into account ; but my short stay at each camp did not admit of these minute enquiries, nor were the people, on all occasions, very willing to enter into a detail of the expense.

The account of the number of people employed who were in constant attendance on the mill, and of their wages ; the statement of the hire of the mill and pans for boiling the juice; of the number of days the mill works; its produce during the 24 hours; the actual cost and tear and wear of bullocks; the price the *Zemíndar* sells his *gúr* at; and the produce per pucka *Biga;* must not be received as perfectly exact, but may be considered as being rather below than above the truth.

About 20 days before the season for making *gúr* commences, the *Zemíndar* generally obtains an advance in cash from some monied *Bania*, either of his own or some neighbouring village; and in general where this advance is received, the lender of the money becomes the purchaser of the *gúr*. And at the end of the season, when accounts are settled, the produce is sold at such a rate as to give the *Bania* his full interest. In a fair year, 24 seers of *gúr* for the rupee is considered as a good price for the *Zemíndar*, and at this rate the wages of the servants, always paid in *gúr*, and the raw juice of the cane, have been calculated.

The mill for bruising the sugarcane which is used in this part of the country, is so well known as hardly to need description.

It is sufficient to state, that it is a very clumsy machine, consisting of two principal parts, viz. 1st. an upright block of wood or stone, generally the former, firmly fixed in the ground, and rising from 3 to 4 feet above it. It is hollowed out in the shape of a cone, and receives the second piece, viz. a beam of wood about seven feet long having its lower end conically shaped to fit loosely the hollow of the block before mentioned, leaving room enough between them to admit of the pieces of cane being thrust in, and subjected to pressure. A pair of bullocks are attached to this beam by a yoke, and communicate motion to it, walking in a circle of above 10 feet in diameter; but from the confined space in which they move, and the immense friction, a great deal of power is lost, and labour wasted: such a mill costs, if of the best kind, 16 rupees. Four pair of bullocks, and eleven men, are required to work it to its best advantage, keeping it going day and night. Many *Zemíndars* have no mills or pans for boiling the juice of their own, and the practise of hiring both is so common, that the subsequent calculations have been made on the supposition that both have been hired.

It has been assumed, 1st, that the hire of a mill for the season is, ...Sa. Rs.	3	8
and of two pans for boiling the juice, ...	5	0
2dly. That the average price of such bullocks as are used in the mill is per pair, ..	30	0
3dly. That the mill in 24 hours produces on an average eight maunds of *gúr*, consuming about 1⅓ *Bigahs Kacha* of sugarcane.		
4thly. That the mill works 60 days.		
5thly. That eleven people attend the mill, they are as follows:		
	srs.	*ch*
Two *Pedehs*, who keep the cane from being thrown out by the action of the mill, they receive between them per diem, ..	4	5
Two *Mútiahs*, who cut the cane into pieces from 4 to 6 inches long, and fill the mill with it, at per pan, per diem, ..	2	5
Two labourers, who attend to keeping the mill in order, renewing ropes, replacing wedges, &c. and giving assistance whenever required, at per diem, per the pan, ..	3	10
One Carpenter for repairing the mill, ..	2	6
Two *Peindiahs*, who form the *gúr* into masses of three or four seers each; they receive per diem between them, ..	2	7
Two *Jhokas*, who attend to feeding the fire under the boilers with the refuse of the bruised canes, at per diem, per pan, ..	2	14
Village servants receiving for each cast, viz. *Kumhárs*, *Lokárs*, *Chámárs*, *Dhobis*, *Darzis*, and *Brahmins*, between all per diem, ..	3	15
6thly. That a *pacca bigah* of the plant, produces on an average 16 maunds of *gúr*, and,		
Lastly, that the *Zemíndar* disposes of his *gúr* at 24 seers per rupee.		

The number of servants and their wages are known, and the rent is assumed from enquiry. From these data the expense of manufacturing the *gúr* per cent. has been thus calculated. *Gúr*, selling at 24 seers for the rupee, the hire of a mill for the season at 3-8 is equal to, ..	*srs.* 84	*ch.* 0
Hire of pans at the same rate, for the same time, at 5 rupees, ..	120	0
Wear and tear of four pair of bullocks, equal to the price of one bullock, per annum 15 rupees; but as the sugar season is short, allow six months, or half, = 7 8 or, ..	180	0
Expense for the season,	384	0

		Srs.	Ch.
The mill works for 60 days, which gives the daily expense, being a portional part of the expense for the season,	.. ..	6	6
Servant's wages per diem,		21	14
Total expenditure per diem,	..	28	4
Quantity of *gúr* made do. do.	..	320	0
Net produce of the mill,	..	291	12

And charge per cent. on the manufacture, 8 seers 13 ch. or above 8$\frac{3}{4}$ per cent. About 30 *pacca bígahs* of cane are generally consumed by one mill during the season: this the *Zemindars* often positively deny, but by cross questioning them, it has appeared that 30 *pacca bígahs* is not too high an average to allow to one good mill. The Játs have, so far as I have observed, the finer crops, prepare the ground more carefully, use the best bullocks, and produce the greatest quantity of *gúr*. The village of Mandelh, near Shamli, has been known to raise canes producing 24 maunds of *gúr* on the *pacca bígah*. Shamli is a great mart for *gúr*, and excellent sugar pans are made there, which are sent all over the country.

IX.—*Experiments on Evaporation performed at Vera Cruz, in* 1818-20.

In the preceding number of the Gleanings I gave an account of some experiments (conducted on a large scale) to determine the amount of Evaporation in the neighbourhood of Calcutta, the results of which appear to differ from those hitherto received. I was curious to compare them with some which I had formerly made on the opposite side of the globe, and as your readers may also wish to make this comparison, I shall here present them with my conclusions, tabulated for better reference.

Experiments on Evaporation at Vera Cruz, in 1818—20.

Year and month.		Mean temperature.	Monthly evaporation in inches perpendicular.						Daily evaporation.
			A. D.	A. S.	W. D.	W. S.	B. S. 1.	B.S.2	W. D.
1818									
September,	..	80,75	11,4	..	..	2,2	..	..	..
October,	..	77,57	16,	..	..	3,02	..	..	..
November,	...	73,51	9,2	8,7	..	2,55	...	..	..
December,	..	70,35	..	..	..	1,8	..	..	
1819									..
January,	..	72,11	..	..	..	1,64	..	..	..
February,	..	74,93	..	..	...	2,02	..	..	..
March,	..	76,98	..	..	..	3,066	1,435	..	..
April,	..	77,08	13,	..	..	2,1	1,14	..	..
May,	..	80,97	15,85	13,24	..	2,91	1,41	..	...
June,	..	80,16	12,03	10,42	5,46	4,56	1,72	..	..
July,	..	80,97	11,82	10,22	5,86	4,04	1,6	..	,182
August,	..	80,86	12,02	10,26	5,24	4,56	1,72	..	,19
September,	..	79,83	13,71	11,87	6,70	5,11	,345	1,86	,17
October,	..	76,35	20,62	..	10,42	7,09	22,45	2,85	,223
November,	..	75,56	20,52	..	6,18	4,64	1,91	2,62	,34
December,	..	71,77	15,86	..	5,	4,01	1,78	2,76	,206
1820									,16
January,	..	70,30	11,85	..	2,76	2,45	1,113	1,26	,09
February,	..	74,02	15,32	..	4,64	3,76	1,69	2,06	,16
March,	..	72,97	18,32	14,31	7,41	5,31	2,27	4,09	,24
Mean an. tem. & total evap. for 1 year,		76,74	180,92		59,67 in 10mo.	50,54	21,148		,196 for 10mo.

for 12mo. say 70 in. for 12 mo. say ,191

Explanations and Remarks.

A. D. was a tin cylindrical vessel, 5 inches deep and 4,08 diameter, placed on a wooden rail, elevated 10 feet above the *Azotea*, or flat roof of the house I lived in, filled up every day with a glass measure, dividing a cubic inch into 50 parts, by which I reckoned the evaporation.

A. S. was the shallow top to the above, placed on the same rail, and filled up daily in the same manner.

W. D. a tin cylinder of 5 inches deep, and inches 4,08 diameter, placed in my window, (always open, and opposite to another, also open,) and very rarely reached by the sun : it was filled up daily as above.

W. S. a canister top, similar to A. S. in depth, and inches 2,40 diameter, placed by the side of W. D. and filled as above.

B. S. 1 and 2 were two shallow canister tops of diameter inches 4,35 and 1,65 placed behind the two shutters of the same window, where the air had little motion, and filled as above. As there was every day either a sea breeze, or a *Norte*, the agitation of the water exposed a larger surface than the area of the vessels to evaporation, by wetting all the space inside between the edge and the surface of the water. This in common *Brisas* would average a quarter of an inch, and would make the result about 2 tenths in excess. But in days of *Norte*, the winds were so violent that it was necessary to reduce the level 1 or 2 or more inches below the edge of the deeper vessel, and reject the shallow one as useless. At such times the correction for excess would be an average reduction of half the apparent evaporation. The season of *Nortes* prevailing through eight months, commencing with October, during which they blow about half the number of days, we must take off for *Nortes* 30 inches, and for *Brisas* 21 inches, from the aggregate of 180 inches of A. D. which reduce the loss to 127 inches, or per diem ,347 inch. This cannot be considered accurate, and is probably in excess of the truth ; but, notwithstanding its position by the sea shore, and the great quantity of rain, (nearly 100 inches) which falls in the season, Vera Cruz must be subject to a higher degree of evaporation than Calcutta ; from two causes, the almost constant sunshine and infrequent day-showers during the Rainy season ; the heavy falls of rain coming with the land wind of the night ; and the frequency and violence of the *Nortes*, which in a few hours bring down the temperature 15 or 20, and sometimes 25 and even 30 degrees of Fahrenheit, and produce an extraordinary dryness in the atmosphere.

W. D. and W. S. were affected in the same way, but in a less degree, especially the latter. The corresponding correction would probably be an abatement of 15 per cent. from the former, and 10 per cent. from the latter, which makes them respectively as follows.

W. D. $59\frac{1}{2}$ inches per annum, and	,162 per diem	}	Mean ,143
W. S. $45\frac{1}{2}$,, ,,	,124 ,,	}	

Until the middle of April, 1819, W. S. stood in the open window of a room, which had no free current of air: hence the great difference in the corresponding months of the two years ; a small evaporation in that room, shut up, gave only 0,917 in June, and 1,04 in July, 1819, or *per diem* ,03 and ,034 respectively ; and ,032 *per diem* from 30th September to 2d October. G.

Note by the Editor.

The above we consider a valuable addition to our correspondent's former paper on the subject. It is both curious and instructive, as showing the very great variation in the quantity evaporated, occasioned by differences of situation, and even by an increase or decrease of depth in the fluid to be evaporated, all things else being alike.

From the meteorological tables kept at Benares, an attempt was made to determine the ratio of evaporation at different temperatures, of which an account will be found in the Oriental Magazine for 1827. For the sake of comparison with those of Calcutta, and Vera Cruz, we here subjoin a few of the Benares results.

The mean temperature in the shade for four years, was	79°.	
The depression of the wet bulb thermometer,	9°.	
The annual evaporation, was	64,5	inches.
or monthly,	5,4	ditto.
and daily,	0,180	ditto.

Taking the average of four years, and selecting the periods most at variance, or the hottest, coldest, driest, and most damp, were obtained the following.

	In December and January.	In March.	In April and May.	In July and August.
Mean temperature,	62°,3	79°,4	91°,2	84°,4
Depression of wet bulb,	6°,0	15°,3	20°,3	2°,0
Monthly evaporation, inches,	2 ,55	7 ,3	13 ,9	3 ,0
Daily ditto,	0 ,085	0 ,243	0 ,463	0 ,1

From these and the classification of such data throughout the register, the following table was framed.

Temperature of the air.	Depression of the wet-bulb thermometer.								
	0°	5°	10°	15°	20°	25°	30°	35°	Tempe.
60°	0	0,05	0,11	0,17	0,23	—	—	—	
70	0	,07	,13	,20	,27	0,33	—	—	
80	0	,09	,18	,27	,35	,44	0,53	—	
90	0	1,2	,25	,37	,50	,63	,75	0,86	
	Decimal parts of an inch evaporated in 24 hours.								

These numbers apply to small shallow surfaces of water sheltered from the wind. It seems probable, that the wind on a large scale may more than compensate for increase of surface. The Vera Cruz average takes in the influence of wind, otherwise the climate of that place must be drier than Benares, which is very unlikely.

The amount of evaporation is given by Dalton and others, in an inconvenient expression of "the number of grains per minute for a given surface;" by converting these, however, into similar terms, we have the following contrast, at a wet bulb depression of 20 degrees.

Temperature.	Aqueous Tension centesimal.	Evaporation by Dalton.	Evaporation by above table.	Ratio.
60	0	0,42	0,23	1,8
70	10	0,52	0,27	1,8
80	20	0,65	0,35	1,8
90	30	0,76	0,50	1,5

Whence it appears that the actual evaporation daily from a set of four years experiments, turns out little more than one half the quantity given by Dalton's formula. We have, ourselves, made some direct experiments on the subject, which, as far as they go, confirm the above curious result.

X.—*On the Chinese Lacker.*

In our 6th No. we published the contents of a letter from a gentleman in Calcutta, to a friend in China, containing enquiries regarding the nature of the Chinese Lacker, and a suggestion, whether it might not be the same as the Burmese varnish, of which a specimen was sent for the purpose of trial and comparison. The reply which was also published in that number, was not so full or satisfactory as could have been wished; and indeed the most interesting and important of the queries, the identity of the two substances, was left altogether unnoticed. The following letter, which has been recently received, in a great measure supplies these deficiencies; and we think will be read with interest. We wish however, we could *print* the specimens sent by the writer of the letter, as they throw more light on the subject than can any description. The inferior sort or No. 1, of the Chinese lacker, was, we thought, a little better than the Burmese, but very little; the others decidedly so; but the best or No. 5, was exceedingly beautiful, and its lustre can only be compared to that of polished metal. The preparatory coating, of which also a specimen was sent, appeared to us very similar to the size coating given by painters to carriages before laying on the proper colour. As far as these specimens go, they appear to us to decide the question in the negative, as to the identity of the two lackers; but whether justice has been done the Burmese varnish is a question worth considering; it is particularly to be adverted to, that the Chinese have five sorts, of which the worst is probably not much if at all better than the ordinary Burmese varnish. These five sorts are all drawn from the same tree, the only difference being the order in which they are drawn, that taken first being best, and so on. It is possible that the Burmese may not be aware of this circumstance, and it might be worth enquiry to ascertain whether there be any difference in the quality according to its priority of flowing. This point may be even worthy of attention, though the specimen of the tree which is premised in the letter should prove that the two are not the same, inasmuch as it might occasion the same improvement of the Burmese varnish, as it has evidently of the Chinese.

"There is only one kind of lacker used in China, which is opaque; but there are five degrees of quality; the first drawn from the tree being the best, and the quality gradually diminishing in goodness, in proportion to the quantity extracted—it is used in its pure simple state.

Previous to laying on the lacker, the wood is covered over with a thin coating of clay, (of which a specimen accompanies.) The clay is used mixed with water only. When dry, and the surface polished, it is coated over with the common lacker, and dried in the shade, when it appears of a dull black color. It is then coated again, and dried in the shade. This process is repeated as often as desired, each time with lacker of a better quality than the preceding; the surface being rendered smooth or polished before each coating. The last coating is dried in the sun, which is the only process employed to render it transparent.

The transparent black is produced by the pure lacker without any colouring matter, or being dissolved in any oil or spirit. When another colour is wanted, the colouring matter is mixed with the lacker used for the last coating; which, like the black, is rendered transparent by being dried in the sun.

Specimens are sent herewith of the different qualities of Lacker, No. 1. being the most common, and No. 5. the best.

In general four coats of lacker are deemed sufficient for any colour, except black, to produce which, of good quality, five coats are required; the finest quality of lacker being used for the last coat.

I send herewith pieces of lackered wood, shewing the different grades of the lackering process.

I send also a piece of wood lackered over with the Rangoon lacker, sent on here last year. It was used in the pure state like the Chinese lacker; and the manufacturer observed that it required about four times as much exposure to the atmosphere to dry, as the Chinese lacker, and when dry it is very inferior in point of transparency. Indeed it remains soft, and rubs off. (When it reached Calcutta it was quite hard; the nail had no effect on it. ED.)

I am promised a specimen of the Lacker tree, which shall be forwarded to you by the earliest opportunity."

XI.—*Miscellaneous Notices.*

1. *Volcano in the Himmalaya.*

In a late vol. of Brewster's Journal appeared an account of a Volcano in the Himmalaya,—which was received with much interest both in England and in India. No proof of the actual occurrence of such a phenomenon was brought forward, however, the information given, being confined to the description of an appearance, something like smoke issuing from the vertex of a peak on the Rungpore frontier. No light or fire had ever been seen, and the conclusion that it was a volcano, appeared to those who have seen much of these mountains not at all probable.

The following extract from a letter (we believe by the author of that notice) relates to the same peak. The writer had been within sixty miles of it.

"The high snowy mountain said to smoke, certainly the more I see of it, the more I feel puzzled as to what can occasion the appearance. Whatever it is, snow, vapour, or smoke, it comes out of a deep rent in the top of the hill. It always blows from West to East, that is, it shows itself when the wind is from the Westward, but is not visible when the wind is from the East; I have had no view of it on a clear day within the hills yet."

Our readers will remember a similar appearance related in Bishop Heber's Journal, as communicated by Mr. Traill, at Almorah. The following, which we have extracted from the Journal of an excursion in these mountains, made by a friend, refers to the same circumstance.

"Having travelled so much in these mountains without ever hearing any mention by natives of fire or smoke issuing from any of the snowy or other peaks, it was not without surprise I learned from Mr. Traill, that the mountaineers aver, that Nunda Debee, one of the large snowy peaks visible from the summit of the Almorah ridge, frequently emits smoke; they attribute the fact to the circumstance of their god cooking his dinner; and they call the peak his *chúla* or kitchen. The frequency of hot springs within the snowy range, is certainly a remarkable feature, though I can by no means allow of their necessary connection with volcanic phenomena. They are found in formations of every character. And what is more important, we are to consider that in no case do their peaks exhibit an outline at all volcanic, being almost all sharp spiracles, and in no single instance that I know of truncated cones. Amidst the innumerable peaks existing in the remote and inaccessible, as well as invisible depths of this vast region of snow, it is impossible to say, what may or may not exist, and therefore it may be said, that we cannot speak possitively as to their non occurrence. It is at the same time curious, that no specimens of rocks of volcanic, or even trap origin, have yet been found within these mountains; even granite is very rare."

2. *Syphon Hydrometer.*

The Journal of Science and Arts has given a description of two Syphons applied to the measurement of the specific gravities of liquids, invented by Mr. Meikle. The first consisted of a double syphon, *a*, into one leg of which a column of pure water was introduced, and into the other, a column of the liquid whose specific gravity was required. The chief difficulty with this instrument was in cleaning the interior tubes, where the liquids might be apt to become mixed.

a

An improvement afterwards suggested itself to the inventor; a common Syphon *b*. of two equal legs was made to dip into two adjacent glass vessels containing the liquids: but in this form of the instrument, allowance must be made for the difference of the capillary action of the tube upon the two liquids; besides which, it is necessary to have sufficient liquid to fill a glass cylindric tube, which is sometimes inconvenient.

b

A third form still presents itself, possessing greater advantages than either of the foregoing.

It is that of *a syphon of three legs.* Into the upper orifice of it, the required liquid may be carefully poured, and very little will suffice to fill the tube half way up. The whole instrument is then to be plunged into a vessel of water, which enters at the lower orifice, and by the intervention of the air, elevates a proportionate column of the other liquid, the height of which, and of the water, may be read off on a scale graduated upon one, or all of the tubes. The allowance for capillary attraction need now only be applied for to the water tube, and will therefore be a constant number: and indeed it will be reduced nearly to nothing, inasmuch as the water cannot enter the lower orifice of the syphon without affecting the level of the liquid in the other branch, on account of the air between them, and the same force which raises the capillary water in one column, raises a proportional weight of the liquid in the other tube. It all follows that the height of the capillary column will only be half as much as it would be in an open tube.

c

Q

3. *On making Ice.*

The experiment of Professor Leslie, in which, by means of sulphuric acid and an air pump, he froze water in the middle of summer, is doubtless familiar to most of our readers. An apparatus on this principle, but on a very large scale, was brought out to this country sometime ago, with the view of establishing the manufacture of ice in some quantity, for the use and consumption of the good people of Calcutta. We had not an opportunity of seeing it ourselves, but we have understood that the air pump was to be worked by a small steam engine. Whether it ever received a fair trial we know not, but it is generally supposed, that the experiment has failed. The only account we could ever get of the matter is that the ice produced, was not only in small quantity, but so imperfectly solidified, as to render its transport to any distance precarious. We wonder it did not occur to the conductors of the experiment to mix common salt with it, by which the temperature would be reduced at once from 32° to 0°. Such an improvement of the power of the machine might have insured the success of the experiment. Suppose for instance, two lbs. of ice were the produce of the sulphuric acid and air-pump; and allowing that only half this quantity was real solid ice, the other half being water of the temperature of 32°, mix half a pound of muriate of soda, and the result would be, a mass of three pounds at the temperature 13°. Or as muriate of lime might be imported from Europe at a very trifling charge,—suppose $1\frac{1}{2}$ lbs of the latter, mixed with the 2lbs of slush ice, the resulting temperature on $3\frac{1}{2}$lbs should be 5°.

Such was the view which suggested itself on first hearing of the failure of the experiment and relinquishment of the plan. Whatever doubts might have arisen as to its justness, are removed by the opinion of one every way qualified to judge. Mr. Walker, in the communication which is republished in our 8th number from the Philosophical Magazine, gives it as his opinion, that artificial ice is most economically and effectually applied, by mixture with saline ingredients.

4. *Table of the Comparative Tensions of Aqueous Vapor, for 30 degrees of depression of a wet-bulb thermometer, and from 30 to 120 degrees of Temperature.*

Depression.

Tem	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
30	83,8	70,0	56,2	43,8	32,7	22,8	14,3	6,8	0,0						
40	87,8	77,1	67,2	57,7	48,8	40,6	32,9	25,7	19,3	13,5	8,0	2.7			
50	90,7	82,3	74,0	66,5	59,5	52,4	45,8	39,5	33,4	27,8	22,3	17,5	12,9	8,7	4,7
60	92,6	85,6	78,7	72,2	66,1	60,0	54,5	48,9	43,4	38 3	33,4	28,7	23,3	20,4	16,6
70	94,0	88,2	82,5	76,9	72,3	66,1	61,2	56,4	51,8	47.3	42,9	38,9	35,0	31,4	27,5
80	94,9	90,0	84,8	80,0	75,3	70,6	66,4	62,1	58,1	54,1	50,0	46,4	42,8	39,3	36,2
90	95,6	91,3	86,9	82,8	78,7	74,7	70,8	67,1	33,3	59,7	56,1	52,6	49,2	46,1	43,0
100	96,1	92,4	88,8	85,2	81,5	78,0	74,6	71,0	67.8	64,4	61,2	58,2	55,1	52,0	49,1
110	96,5	93,7	30,0	86,9	83,7	80,5	77,3	74,3	71,2	68,5	65,5	62,7	60,0	57,4	54,7
120	96,9	94,0	91,0	88,4	85,7	82,7	80,0	77,1	74,4	71,7	69,3	66,7	64,2	61,6	59,2

Depression.

Tem	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
50	1,0														
60	12,9	9,6	6,5	3,3	0,3										
70	23,0	20,6	17,4	14,3	11,1	8,6	5,7	3,1	0,04						
80	32,6	29,4	26,4	23,2	20,5	17,6	15,1	12,7	10,4	7,9	5,6	3,3	1,0		
90	40,3	37,3	34,5	31,7	28,7	26,1	23,8	21,1	18,8	16,5	14,4	12,2	10,0	7,9	5,8
100	46,3	43,4	41,0	38,3	35,7	33,3	30,9	27,4	26,2	24,0	22,0	20,0	18,0	15,9	13,8
110	51,9	49,4	47,0	44,4	42,0	40,0	37,5	35,5	33.3	31,2	29,0	27,1	25,2	23,2	21,2
120	57,2	54,7	52,3	50,1	57,8	45,6	43,4	41,4	39,5	37,4	35,5	33,6	31,8	30,0	28,0

The above is inverted from P's table page 81, of the March Number of this work, in which the following error of the press was discovered; for temp. 80, and tension 10, read depression 24.1. instead of 25.1. C.

5. *Whirlwind.*

April 15th, 1828. About 4. 30. P. M. in the vicinity of Dacca, after a heavy squall of wind which commenced at N. W. and afterwards shifted to every quarter of the compass, accompanied by a heavy shower of rain and hail, and tremendous peals of thunder without a single flash of lightning, which lasted for nearly an hour, I observed a very dense cloud to the S. and E. from which a column appeared to be descending. On first perceiving it, it had descended but a short way from the cloud in a slanting direction towards the earth, the part nearest the cloud being the thickest, and the lower end gradually tapering away to a blunt point; it however increased rapidly in circumference, and gradually approached the earth, the point swelling out to nearly the same size as the rest of the column, and appearing, except at the upper extremity, (which was of the same denseness as the cloud) like a fine white smoke, rather denser towards the middle than at the edges, which were clearly and beautifully defined. After watching the formation for about a minute and a half, it reached the ground in the middle of a grove of trees, and immediately a cloud of dust was raised in the air, mingled with bamboos, mats, &c. and the whole dissappeared; the upper part appearing to be drawn up into the clouds, and the lower falling to the ground. As it had the appearance of what is generally denominated a water-spout, I immediately proceeded to the spot, which was about two miles off, fully expecting to find that a large quantity of water had fallen. To my great surprize I found there had not a drop fallen, and that the phenomenon in question, was a whirlwind which had descended in the centre of a number of fishermen's huts, twelve of which were entirely destroyed. The devastation was entirely confined to a space of about 50 yards in diameter, and those houses beyond the range of it, and absolutely almost touching the other, had merely the thatch of the roofs ruffled; the roofs of the houses in the centre of the spot where the wind had struck, were crushed flat on the ground; those towards the side were thrown down outward. I say struck, for it evidently appeared that the wind had caused the same effect as a gun when fired at the earth, which throws the dust up on every side of the space through which the charge passes. I can only account in this way for the appearance of the houses, and suppose that it was part of those at the edges which were thrown outwards, that the rebound carried into the air: some of the people were thrown down flat, but no lives were lost. It had exactly the same appearance as those which are frequently met with at sea, and from observations which I have frequently been enabled to make on the latter, I am inclined to think, that in general, they are merely of the above description, containing no water; the sea appears to be generally much troubled under them, but I have never observed, though at times very close, that any quantity of water has fallen when they break. L.

6. *Tabasheer.*

Our readers are aware, that several very interesting papers and notices have been published by Dr. Brewster in his Journal, and in the Philosophical Transactions on this curious production of the vegetable world. In a letter to a correspondent, in Calcutta, who had sent him numerous specimens, he mentions as a desideratum, the possibility of procuring it in a fluid state. The following are the observations of a gentleman who has had many opportunities of observing this substance.

"The Tabasheer may be procured in a state resembling jelly. It must also be in a completely fluid state in the first instance, but as almost all bamboos contain a large quantity of water between their joints, it would be difficult to determine when it was, and when it was not held in solution. In the state of jelly in which I have seen it, I think it might be pressed into the shape of a lens; but it would not, I suppose, answer Dr. B.'s purpose, as he probably requires it to be made from the substance in its crystalline form." D. S.

XII.—*Proceedings of Societies.*

I. Asiatic Society.

A meeting of this Society was held on Wednesday the 2d Sept.—Sir Edward Ryan in the Chair.

The following gentlemen were elected Members of the Society:

Major Caldwell, Rájá Banwari Lall, Babu Asutosh De, Babu Rájchander Das, and Babu Syámalál Thákur.

A letter was read from Mr. Avdall, presenting part of a brick from Babylon.

A letter was read from Lieutenant Craigie, presenting sundry old coins, in the name of the Bégam Somrú.

A letter was read from Mr. Williams and Dr. Adam, presenting thirteen copper weapons found in the earth near Fatehgerh.

A letter was read from Babu Sibchander Das, presenting a Gorac,h Danda, being an iron instrument with rings, said to have been invented by Gorac,h Nátha, a celebrated sage, from the vicinity of Guzerat. The instrument is a kind of puzzle, in which an iron rod is passed and repassed through a series of rings. The inventor is said to have died at Górucpúr, which derives its name from that circumstance.

A letter was read from Mr. Martin, presenting a pig with two heads, and two young Kangaroos in spirits.

A copy of the Alif Leilla, printed at the Lithographic Press, was presented by Mr. Wood.

A letter was read from Captain Ruddell, forwarding a very large collection of Thibetan Manuscripts on the part of the College Council.

A copy of the work of Ribarius on Surgery, found in Nepal, was presented by Mr. Hodgson.

A Tagala Dictionary was presented by the Count di Vidua, and a copy of the Englefield Vases, by Mr. Grant.

Captain Herbert presented the printed Meteorological tables for June and July.

Copy of an inscription found in a temple near Allahabad, was presented on the part of Mr. Boulderson.

An account of a journey from Nepal by Tazedo on the frontier of China, by a Cashmíro-Bhótiah, interpreter to the traders on that route, communicated by Mr. Hodgson through the Hon'ble W. B. Bayley, was read.

Account of Hindu Sects, second part, was presented by the Secretary.

Class of Natural History and Physics.

Wednesday, 26th August.

Honorable Sir E. Ryan in the Chair—A letter was read from Mr. Hodgson, dated Nepal, 23d July, accompanied by specimens of a certain earth, which Mr. H. states "to have been brought from a considerable distance in the mountains, and used in the Khár Kháneh to assist the fusion in some metals." The secretary, (Mr. Ross,) stated that on first inspection, this earth appeared to him to be the bituminous marle, which is used as a flux for ores of copper in Thuringia, where it abounds as a matrix of the ore; but the very small proportion of lime precipitated by means of oxalate of ammonia, from a solution of the earth in muriatic acid, does not justify the conclusion of its being marle. Subsequent examination led Mr. R. to believe it to be earth, forming the bottom or sides of some of the natron lakes, which are known to exist in certain parts of the Himalayan range of mountains:—a complete analysis of this earth is, however, promised for the next meeting.

A large collection of rock specimens, supposed to have been obtained from the vicinity of Simla, was presented by Mr. Calder, on the part of Dr. Govan, but no specific account of them has yet been received.

Mr. Calder also presented on behalf of Mr. Hardie, some specimens of lias lime-stone, with organic remains, illustrative of his paper on the Geology of central India.

A letter was read from Captain Franklin, dated Jubbulpore, 12th July 1829, detailing the progress made by him in the prosecution of his interesting geological investigations, and what still remains for him to accomplish.

2. Medical and Physical Society.

Saturday, 4th July.

A. Ogilvie, Esq. in the Chair.—A specimen of Tincture of Hill Rhubarb and of Tincture of Hyosciamus (Henbane) prepared by Mr. Royle at Saharunpore, were submitted.

A notice of two Hindu Sculls, with preparations, was presented by Mr. J. Tytler.

A paper entitled, Remarks on disunited fracture, and the mode of treatment pursued in such cases by Mr. Amesbury, (London,) was presented by Mr. Raleigh.

A paper entitled, Memoranda on Cholera, was presented by Mr. Dickson, Assistant Surgeon His Majesty's 30th Regiment.

Mr. Tytler's notice, and Mr. Dickson's Memoranda were read and discussed by the meeting.

Saturday, 1st August.

A. Ogilvie, Esq. in the Chair.—Dr. Storm was elected a member of the Society.

Letters were read from Messrs. Laughton and Scoular, withdrawing their names from the list of the Society's members, on the plea of inability to afford the expense of subscription, &c.

Mr. T. E. Baker's Essay on the Art of Preserving Health in India, was presented in the name of the author.

A model of a Hydrocephalic Native Child, was presented by Mr. Grant.

Extract of a letter from Mr. Spilsbury, on points of professional interest, were read by the Secretary.

Mr. Raleigh's paper on disunited fracture, was then read and discussed by the Society.

Saturday, 5th September.

A. Ogilvie, Esq. in the Chair.—Dr. Irvine was elected a member.

Dr. A. B. Webster withdrew his name, pleading inability to pay further subscriptions.

The following papers received since the last meeting, were submitted by the Secretary.

An account of Epidemic Fever, lately prevailing among the troops at Hansi, by Dr. A. Murray.

A case of Fracture of the Cranium, successfully treated by Mr. Cameron.

A case of Traumatic Tetanus, with observations, by Mr. Grant.

An account of the production and effects of Malaria, in the valley of Udayapúr, by Dr. Hardie, and a paper on occlusion of the biliary ducts, by Mr. Twining.

A copy of Brook's History of St. Helena, was presented on the part of Mr. M. Ritchie.

Mr. R. M. Martin presented a collection of Crania.

Dr. Stewart's case of Delirium Tremens, Mr. Grant's of Traumatic Tetanus, and Mr. Piddington's Observations on Gulunchu and Cat Caringa, formerly presented, were then read and discussed.

XIII.—*Scientific Intelligence.*

I. Novelties in Science.

1. *Figure of the Earth.*

In a paper inserted in the Philosophical Magazine and Annals of Philosophy for Feby. 1829. Mr. Ivory finds, that out of forty experiments on the length of the pendulum made in different parts of the world, 34 are represented tolerably by an elliptieity between $\frac{1}{300}$ and $\frac{1}{293}$.—That $\frac{1}{289}$ is too great for the 34, and does not even represent the remaining 6.—The great discrepancy is in the tropical observations; thus the length of the equatorial pendulum by a mean of 6 experiments

is		39,01230
By a mean of, ...	9	,01330
Ditto, ditto, ...	15	,01605

This irregularity is particularly remarkable near the Equator, as the following comparison will show.

				Long.
Maranham,	...	..	39,01173	44. 21 W.
Rawak,	..	..	,01479	131. 1 E.
Galapagos,	..	..	,01717	90. 0 W.
St. Thomas,	..	..	,02074	6. 45

These places are all so near the Equator that they may be reckoned to be upon it.

These irregularities do not extend beyond the tropics; and even within them, there are only a few that do not belong to the same surface. Of the other observations Mr. Ivory seems to think, that the first step should be to scrutinize these observations, before coming to the conclusion that the earth is really different from a solid of revolution.

In a former communication he had shown, that the best measurements of arcs of the meridian in different countries were (within the limits of error) accordant with such an ellipticity. In the number for March he returns to the subject, and attempts to show that the few measurements of perpendicular arcs yet made, also support an ellipticity of about $\frac{1}{300}$; thus he finds the difference of longitude between Dover and Portsmouth to be,

		m. s.
Geodesically,	..	9 42,4
By Chronometers,	..	9 42,9

The latter result would be 9 42,1 (*m. s.*) if two observations that appear irregular are rejected. Again, the result of the recent measurements made on the continent to determine the value of a perpendicular degree, have shown the difference of longitude (on an ellipticity of $\frac{1}{300}$) between Marennes and Geneva, and Marennes and Padua,

	m. s.		*m. s.*
To be Geodesically	29 2,2	and	51 57,3
By Astronomical observation, they are	29 1,1	and	51 56,1
Difference, ..	1,1		1,2

A difference, which as he observes, is certainly within the limits of error. He thinks that we have then, as yet, no warrant from the results of observation, to call in question the opinion which has been so generally received, till lately, viz. that the earth is a solid of revolution, and consequently the meridians regular ellipses. We may also conclude with perfect safety, that the ellipticity is very nearly, if not exactly $\frac{1}{300}$.

In connection with this subject, we may notice a very interesting paper by M. Nicolle, in one of the early volumes of the Journal of Science, N. S. in which he attempts to prove that the celebrated and much discussed discrepancy in Mechain's latitudes of Mont-jouy and Barcelona, was to be explained in a much more simple and obvious manner than had yet been attempted. Those of our readers who are familiar with the *Base du Systeme Metrique decimale*, are acquainted with the history of this question, which, with Zach's results, has been hitherto such a stumbling block to the unqualified admirers of the *Cercle Repetiteur*. Mechain, *the most faithful of observers*, as he may be well called, found that his best and most unexceptionable results obtained at Mont-jouy and Barcelona, gave a difference of latitude 3″,8 more than the actually measured distance (on any probable figure and dimensions of the spheroid) gave. The distance on the meridian was only one mile, so that no solution of the error could be sought for in any probable inaccuracy of measurement. Local attraction, and many other causes have been assigned, but none of them even plausible, excepting Mechain's own explanation, which attributed the error to the instrument, and to the impossibility of determining with such small means, quantities so minute. His letters to Delambre on the subject are extremely interesting, and show what a hold the subject had taken of his mind, and what a source of mortification and sorrow it became to him.

M. Nicollet's paper is exceedingly ingenious. He attempts to show first, that the star in the observations of which the greatest discrepancy is observable, ζ *Urs. Maj.* is a double star. That the circumstance was not known to, or allowed for by Machain, and that being visible in the telescope of the circle, as an ill defined single star, the wire was made necessarily to bisect some point between both stars. He also brings forward other auxiliary considerations connected with the progress of Astronomy, attempting to show that when the observations are reduced by applying modern corrections and the above allowance made, the great discrepancy of 3″,8 is entirely got rid of. It would be impossible to do justice to his paper in the limits we are confined to; it is, as we before remarked, exceedingly clever and ingenious. Unfortunately however, for the success of the opinions it promulgates, we have in the March number of the Philosophical Magazine, a letter from a French correspondent, who at once demolishes M. Nicollet's carefully reared fabric. He shows from Mechain's own letters, that he not only knew the star ζ *Urs. Maj.* to be double, but saw it so in the telescope of his circle, and describes the lesser star to be, *when he brought the wire on the centre of the large one*, about two diameters of the wire below or about 12″ And the other arguments he summarily disposes of by taking *the same stars*, observed *at the same season of the year* at both places, (though in different years,) in which case, all doubt as to the coefficients of the different corrections must vanish. As a useful memorandum, we shall here give the result of his comparison.

The difference of Latitude by	α Polaris,	Sup.	Pass.	61,″19
	Do.	.. Inf.	Pass.	59,44
	β Urs. min.	.. Sup.	Pass.	62,65
	Ditto	.. Inf.	Pass.	63,16
	ζ Urs. maj.	.. Sup.	Pass.	58,10
	Ditto	.. Inf.	Pass.	64,88
	β Gem.	..		57,83
By measurement it was found to be				59,33

These results prove, that no latitude deduced from observations made with this instrument can be depended on to so small a quantity, as the admirers of the instrument would have us believe.—And they lessen the force of the objections made to the regular figure of the earth, founded, partly on similar discrepancies in arcs of greater extent. The English irregularities have, as far as we recollect, been plausibly referred to local attractions acting on the plummet.

2. *New System of Water Power.*

A description with plans of a mode of supplying public works with water as a moving power in all situations, and at all seasons of the year, has been lately published in Scotland by an engineer of the name of Thom. It promises to be of the greatest advantage to this country, not only as affording a cheap substitute for steam power, but as lending a powerful aid to the amelioration of the condition of the working classes, and perhaps of becoming the means of removing the intolerable nuisance of smoke in large towns. This plan of water-power, which has been adopted at Greenock on a beautiful water-fall of 512 ft. height above the level of the sea, is capable of universal application throughout the island, and is rendered complete by the contrivance of a series of self-acting sluices adapted to every site and to every state of weather, all the invention of the engineer, who had the honor of first submitting the plan to the public. The success of the method at Rotbsay in the Isle of Bute, where it originated, induced a company of patriotic gentlemen in Greenock to lend themselves to the undertaking; and their works, now nearly finished, form one of the greatest wonders of art in the country. The rains collected from a number of barren hills in the neighbourhood of Greenock, are collected into a large natural reservoir at their base, and conveyed along the face of mountains, carried across deep ravines, and conducted along the edges of rocky precipices in a gently sloping aqueduct, about six miles long, to the brow of a hill surmounting the town; thence the water is led along in small aqueducts or lades to the mills, (which are situated on the face of the hill,) amounting to thirty-three in number, and (from their various heights, being placed successively below each other,) yielding a power arising from the extent of their falls, equal to that of 2000 horses, (as appears by the report of the Company's engineer,) and if certain improvements be afterwards made as indicated, they may be made to yield a power equal to that of 3000 horses, a mechanical power far exceeding that of the great manufacturing town of Glasgow, and its populous vicinity. The water collected into the great natural reservoir with some small auxiliaries, is drained from about 4890 acres of ground, it covers about 300 acres, in which the water stands, about forty-six feet deep, and it is capable of containing about 300 millions of cubic feet of water, or of discharging 600 millions of cubic feet annually; so that besides supplying the town of Greenock amply with water for culinary purposes to the amount of 50 millions of cubic feet annually, the reservoir can furnish 2464 cubic feet of water per minute, for 310 days (the working days) in the year, for the period of twelve hours a day. The most astonishing circumstance regarding this immense public undertaking is, that they can afford to give their water so cheap to the people who take their mills, that the price of a horse's power is reduced to about the twentieth part of what it would cost, were it derived from steam. The expense of steam engines and fuel, would by the general adoption of this plan throughout the country be entirely done away with; coals and many other articles of consumption, would be rendered cheaper; the smoke of public works would be abolished in a more effectual way than by burning; and the health and morals of the lower classes, the last, but not the least important of the advantages to be derived from it, would be improved by the removal of manufactories from confined situations in crowded towns to airy and salubrious situations in the country.—*New Monthly Mag. No.* 101.

3. *Mr. Motley's Arch Suspension Bridge.*

The admirers of mechanical ingenuity, and those who take an interest in the architectural improvements of the metropolis, will, we are sure, feel gratified by a visit to Mr. Motley's model of a wrought iron arch suspension bridge, now exhibiting at the Strand. The principle of Mr. Motley's improvement, for it is not, as he alledges, his invention (as the celebrated Schaffhausen bridge in itself proves), is to do away with lateral pressure, thence of the necessity of abutments by removing the pressure or weight of the bridge from, as at present, the top to the bottom of the structure, and resting it on a simple trussed beam or tension line. This tension line, which is ensured by a very ingenious apparatus of inflexibly jointed iron vertical bars and horizontal lines, (the arch being the reverse of our inverted one of our abutment bridges,) constitutes the carriage way of the proposed suspension bridge over the Thames, from Charing Cross through Scotland Yard to near King's Arm's Stairs. Corresponding with this tension line and parallel to it, is another on the top of the arch, which may be used as a floor for an arcade of shops, or as a foot way, according to the height or number of the arches. The advantages of such an arrangement are obvious. Its practicability has been borne testimony to by the highest scientific and practical authorities, amongst others by professor Lardner, Dr. Birkbeck, and Mr. Tredgold; and indeed is made evident by an inspection of the model itself.

4. *Anti-convulsion System of Geology.*

In a notice of a work entitled '*Lettres sur les Revolutions du Globe*,' in the section of the Natural Sciences of the '*Bulletin Universel*,' the Baron de Ferussac repeats an opinion maintained by him on former occasions in the same *Bulletin*, that the present state of the Earth's surface in a geological point of view, is the last, or rather the most recent, of a series of successive and gradual modifications; that, in fact, there have not been any revolutions, properly so called, on the globe; but an uninterrupted succession of phenomena diminishing in importance with the course of time, as the energy of the causes on which they depended have become less ; and the greater part of which still continue, ond are in a state of progression, but with less force, and on a more limited scale than heretofore. In a word, that the general laws of harmony have uot been disturbed on our globe, more than in any other part of the universe ; and that the explanation of geological phenomena, instead of being discoverable in a desolating theory of imaginory convulsions, is to be found in the natural consequences of the primitive state of the globe, and the necessary effects of the general laws to which matter is subject.

True it is, convulsions, violent ruptures of beds, their reformation, the change of place of substances, the consequences of a certain anterior order of things, cannot be mistaken. But these effects are far from being the consequences of disturbances of the established order,—of deluges in fact. The Baron asserts that the whole history of the globe is contained io a few lines formerly published by him, in which he maintained it was time for geologists to abandou the system of coovulsions of nature and of cataclysms ; to acknowledge the influence of natural causes, aod of the order and permanence by which the universal planetary system is governed.

The primitive volcanic fire and its consequences ; the formation of waters by the condensation of gases ; the sinking of their level in consequence of the infiltration effected in proportion to the refrigeration and to the thickening of the crust of the earth, and the diminution of the temperature on the surface of the globe, the effect of the same refrigeration, are the primary causes from which the explanation of the geological phenomena proceed, by a natural and easy concatenation.—*Lon. Mag.*

5. *Improvement of the Value of Life.*

It has been pretty generally suspected of late years, that either there is a manifest increase of the value of life, which is oot impossible, from the improved habits and more extended enjoyments of a people constantly advancing in civilisation, or that, as is also possible, the Northampton table, which have been hitherto the basis on which calculations connected with the value of lives have been founded, was constructed on insufficient data. The following table given by Mr. Morgan, actuary of the Equitable Insurance, which places this subject in the clearest point of view, is taken from the Westminster Review, No. XVIII. p. 411.

Age.	No. of Policies.	Died.	By the Table should have died.
20 to 30	4720	29	68
30 40	15951	106	243
40 50	27072	201	506
50 60	23307	339	545
60 70	14705	426	502
70 80	5056	289	290
80 95	701	99	95

We see by this table, that the value of life from the age of 20 to that of 50, is more than double what is given by the tables.

6. *Proportion of Sickness.*

The following table taken from the same work, p. 415, may interest those fond of statistic enquiries. It was drawn up by the Highland Society of Scotland, from 79 returns of benefit societies scattered through 16 counties, and it consequently applies to the class of labourers and mechanics who formed the members of those Societies. The total number was 104218, and the period in some iostances extended from 1750 to 1821.

Years of age.	Sickness.	Years of age.	Sickness.
21	4 days,	66	5, 4 weeks,
46	1 week,	67	6, 6 weeks,
57	2 weeks,	68	8 weeks,
63	3 weeks,	69	9 weeks,
65	4, 4 weeks.	70	10 weeks.

7. *Constant of Aberration.*

Mr. Richardson of the Greenwich Observatory, having reduced 4119 observations made there in the years 1825-6-7-8, with the two mural circles of Troughton and Jones, on 14 of the most favourably situated stars, finds this constant by Troughton's circle to be 20″,505, by Jones' 20″,502. This value is something greater than that proposed by Dr. Brinkley and Mr. Struve, but rather less than the values deduced by M. M. Bessel and Lindenau.—*Lon. Weekly Rev.*

8. *New Percussion Rifle, ignited by a Spring instead of a Lock.*

Lieutenant Colonel Miller is the author of this improvement, which is stated to combine the following advantages. 1, It produces fire more suddenly than the common lock. 2, It is less liable to get out of order; and 3, It may be constructed at half the expense of an ordinary rifle. A favourable report of its performance as tried at Woolwich is annexed.—*Phil. Mag. and An. Phil. vol. iii. p 277.*

2. Miscellaneous Notices.

The Atlas of India, published by the East India Company

This noble work, of itself a splendid monument of the munificence of the East India Company, is upon a scale of four miles to an inch, and taken from actual surveys, which when completed will form a map of India on one uniform plan. The project was first conceived by Colonel McKenzie, and a large portion of those parts already published were surveyed under his superintendence. The surveys on the northern part of the peninsula have for their basis the triangulation of Colonel Lambton, who extended a set of principal and secondary triangles over the whole country.

The sheets are published as they are completed; some of them have blank spaces, to be filled up as the surveys proceed; nothing being allowed to go forth to the world which is not founded upon actual survey. The following are the sheets already published.

Sheet 47 contains the surveys of Captain Hodgson and Lieutenant Herbert in the mountainous country comprising the northern part of the province of Sirmúr, and the principal part of Bissáhir.

Sheet 48 contains the surveys of Captain Hodgson and Lieutenant Herbert in the Southern part of the province of Sirmúr, part of Garhwál, and the Debra Dún. The flat country is from the surveys of Lieutenant White, Lieutenant Hodgson, Captain Colvin, and Lieutenant Blake.

Sheet 65 is principally the survey of Captain Hodgson and Lieutenant Herbert of the sources of the Ganges, and of Captain Hearsey and Mr. Moorcroft of the sources of the Indus and Setluj.

Sheet 66, is principally the survey of Captain Webb, of the province of Kamáun.

Sheets 69 and 70, contain the greater part of the province of Bandelkund surveyed by Captain Franklin, brother of Captain Sir John Franklin, R. N.

Sheets 42, 43, 58, 59, 60, 77, 78, 80, 81, 95, are surveys executed in the peninsula. —*As. Jour.*

Survey of the Heavens.

It will be recollected by our astronomical readers that in the plan published by the Berlin Academy in 1825, for the construction of a new map of the Heavens, it was proposed to survey a zone equal to thirty degrees in declination, namely 15° above, and 15° below the equator; and that this zone was to be divided into twenty-four hours of right ascension, each of which was to be assigned for the observation of one individual. Any astronomer wishing to be employed on the map, was to address himself to one of the members of the commission appointed for the object by the academy, who would assign for his portion any of the hours of the zone, not undertaken by another. This region was to be allotted to each observer for two years, and at the end of this time, if no real progress had been made, the commission was to be at liberty to appoint another observer. The whole was to be finished by January 1st 1829. We now read in some late numbers of the Antologia, that two astronomers have furnished the task assigned to them, namely, M. Inghirami of Florence, and M. Harding of Gottingen The Italian astronomer had undertaken the 18th hour, one of the most difficult, as it contains the greater part of the milky way comprehended in the zone: his map contains a list of nearly 7500 stars, of which only 1500 were set down in the catalogues of Bradley, Piazzi, Lalande, and Bessel; the other 6000 are the result of his own observations. Notwithstanding the vastness of the undertaking, M. Inghirami has been the first to complete his part of the map. These details are stated by M. Encke, Secretary to the commission, and are inserted in the Italian Journal. M. Harding, who was entrusted with the 15th hour, has only recorded 3000 stars. The results of the labours of the other astronomers are not yet known.—*For. Qu. Rev. vol. iv. p. 332.*

Keith Prize proposed by the Royal Society of Edinburgh.

The Royal Society of Edinburgh have determined to appropriate the interest of a sum amounting to £600, left them by the late Alexander Keith Esq. towards the formation of a biennial prize, to consist of a gold medal not exceeding fifteen guineas in value, together with a sum of money or piece of plate bearing the inscription and devices of the medal. The prize is to be adjudged to the author of the most important discovery in science made in any part of the world, but communicated by their author to the Royal Society, and published for the first time in their Transactions.—*Phil. Mag. and An. Phil. for May*, 1829.

Platina Coinage.

The coinage of money in Russia is of the platina found in the Oural mountains. The coin is dated Petersburgh 1828; and of the value of three roubles, or nearly ten shillings in silver. It is about the size of a sovereign, and so beautifully executed as to do great credit to the mintage and arts in Russia. On the one side is the Russian eagle emblazoned with its shields and bearings, on the reverse a central inscription, stating the amount of the piece, and round the border, the words, "2 zol. (*quasi* zolotniks) 41 parts of pure oural platina."—*New Monthly Magazine*, No. 99.

Geographical Society of Paris.

The Geographical Society at Paris have presented their annual gold medal, of the value of one thousand francs, to Captain Sir John Franklin, as a testimony of their sense of the importance of his second expedition to the shores of the polar sea.—*Phil. Mag. and An. Phil. for May*, 1829.

3. New Patent.

Mr. Langley's Improved method of fixing the Compass on Board Ship.

This is an American patent, and the following is the patentee's account of it.

"The object of my improvement is to dispense altogether with the binacle, in which the compass is ordinarily fixed, to cause it to answer all the purposes of a tell-tale, and to secure it against accidents from cannon-shot, the shipping of heavy seas, or any other cause of injury. The mode in which these ends are attained, is by cutting a hole through the deck of the vessel, at or near the place where the binacle is usually situated; this hole is cut through into the cabin, and within it, is placed the compass with its box, suspended in the usual way; and when so situated, it is completely out of the reach of cannon or other shot. In order to cause it to act as a *tell-tale*, the compass-box is made with a glass bottom, so that the card can be seen as perfectly in the cabin as upon deck. I also make the compass-card translucent, or semi-transparent, in consequence of which, it may always be lighted from below, and will be much more plainly seen at night than when lighted in the ordinary way.

The compass is defended at top by a very thick piece of glass; such as I have used, has been three-fourths of an inch in thickness, and this is also defended by a rim or band, projecting above the deck; the lower side of the box is also glazed; and I contemplate sometimes making the sides of the box of glass, should it be desirable to admit light in that way.

What I claim as new in the above described invention, is the fixing of the compass entirely within the planking of the deck of a vessel; and the mode of rendering it equally visible, both upon deck and in the cabin."—*Tech. Rep.*

4. New Publications.

1. *Elements of Mechanical Philosophy.*

A treatise on this subject by Dr. Lloyd, Professor of Natural Philosophy in the university of Dublin, has been recently published, of which a favorable critique is given in the 78th Number of the Quarterly Review. Such a work was wanted, and if well executed, we doubt not of its success.

2. *A new System of Geology.*

Dr. Ure, the author of the Dictionary of Chemistry, has published a new System of Geology,—a principal object of which appears to be to refute the ill considered objections made by sciolists to the book of Genesis on geological grounds. It contains a popular and well written account of the new Theory of Light; a theory which has been gaining ground lately, and promises to be generally adopted by the scientific world.

GLEANINGS

IN

SCIENCE.

No. 12.—December, 1829.

1. *On the Introduction of the Iron Chain Suspension Bridge into the Himmaláya Mountains.*

Those of your readers who have visited our hill provinces to the north-west, scarcely require to be told, that they constitute, perhaps, the most rugged and difficult country in the world. The difficulties of intercommunication are indeed very great, and no doubt, oppose a serious obstacle to the improvement of the people. In the Alps of Europe, which naturally are, probably, not much better, there yet have been formed magnificent roads in various directions, so as to render the labour of traversing the country comparatively easy, and thus to unite places apparently separated by nature. There arches of stone facilitate the passage from one precipice to another; bridges convey the traveller over, otherwise, impassable torrents; and where the nature of the rock forbids the attempt to construct a road, galleries are excavated through the very body of the mountain, and by a safe, though dark and dismal transit, render passable routes, where nature had seemed to fix an insurmountable barrier. In our northern mountains, again, with a few trifling exceptions, all is as it came from the hand of nature. Yet the Alps of Europe cannot be put in competition for value with our mountain provinces. They owe these improvements, rather to the circumstance of their being the high road into one of the finest countries in Europe, than to any settled plan of improvement, or to any well understood view of developing the resources of the country. Schemes of conquest, and projects of ambition have, perhaps, oftener prompted such works (and this in every country) than the more useful calculations of the political economist. But whatever the motives, the works are a positive gain: the destruction of the barrier against invasion, being more than compensated for by the increased facility of communication with the neighbouring countries. The sword will yet, it is to be hoped, be stayed in its ravages, by obstacles more difficult to be surmounted than "Alp or Appennine;" and in the mean time, the mutual intercourse of nations, as it affords to suffering humanity the principal solace for its destructive rage, so will it be mainly instrumental in forwarding that state of things in which, (if the prophecy be ever *literally* fulfilled,) "the swords shall be converted into plough shares, and the spears into pruning hooks."

Facility of communication within itself, and with neighbouring countries, is certainly one of the first steps in the improvement of a nation. What rank the people of our mountain provinces would have held in the scale of wealth and civilization, if their country had been more accessible, and they less shut up in their rugged hills, it were vain to conjecture; but it may safely be said, that a country with such resources could not have remained stationary at so low an ebb under such circumstances. With one of the finest climates in the world as it regards health; with a soil certainly not below the average, and having the great advantage of a cheap irrigation; with so many valuable natural productions, both vegetable and mineral; there seems only wanting improved means of intercourse within itself, and with the neighbouring countries of Thibet and Hindoostan, to raise it far above its present level. With a surface equal to nearly half of England; with such a range of cultivated productions as the vine, wheat, rice, hemp, flax, cotton, sugar, and opium; with copper, lead and iron mines; with inexhaustible forests, that include almost every production of the tropics or the temperate zone, the bamboo, the oak, the cedar (probably the three most valuable trees); the richest and most extensive pasturages; situated so conveniently for trade, between two countries, mutually in want of each other's productions, this tract does not yet yield a revenue of perhaps £30,000. Can it be doubted, but that good roads and bridges would do much for such a country?

Unfortunately, however, the people are too poor to undertake any thing of this kind themselves. These works must indeed in every country be the result of a certain degree of national prosperity, on which they again react; producing an accelerated progress. To carry into effect extensive schemes of public utility, requires capital; and this is unfortunately an element of improvement as much a desideratum in these countries, as the execution of the projects which can only be secured by it. In Europe the first impulse is given by the great landed proprietors and capitalists, who have their remuneration, the former in the improved value of their estates, the latter in the return made by the employment of their capital. In India we have no proprietors of land but the Government, nor any capitalists capable of comprehending the full value of projects of public utility, still less of rising to the conception of so paradoxical a thing (to them) as public spirit. It is by the Government, and by them only, that such works can be expected to be undertaken; and I may add, that in the double capacity of rulers, and landed proprietors, it is their interest to engage in them. Nothing can be clearer than that improved facilities of intercommunication allow of a country's resources being fully developed. As internal and external commerce increases; with it increase the wealth and comforts of the people. Population then takes a start, and with it must increase the productions of the agriculturist. Land acquires a new value, corresponding to the increased demand for its produce, and the means of the consumers to purchase. Thence a direct interest as landed proprietors; while as rulers, it is sufficiently obvious, that the more prosperous the commerce and agriculture of a country, the more productive will its revenue be; its revenue, I mean, as composed of ordinary taxes. In America it has been found, that the revenue has steadily increased with the developement of her resources; and it will be even found, that the rate of increase in the line of roads, taken as a single element of national improvement, corresponds *pari passu* with that of the revenue.

The value of a proper system of intercommunication, as a step in the improvement of a country, is indeed so fully acknowledged, as to stand in no need of illustration; were it a doubtful question, the practice of that magnificent people the Romans, might serve to reassure us. Even our Mogul predecessors could discover this truth, and their best princes made the construction of roads and bridges a principal object of their care. It is remarkable, how little we have done, when our means of accomplishing so much are taken into consideration. By a laudable innovation on the established maxims of a cruel justice, we have sparingly administered the punishment of death; and have in all, but the very heaviest offences, commuted that punishment for labour on the roads; our delinquents are therefore collected in prisons all over the country, and in considerable numbers. As their punishment is expressly declared to be LABOUR ON THE ROADS, and as their numbers are very great, the stranger might naturally conclude, that our roads, at least in the plain country, must be excellent. Our residents can tell a different story, particularly those who have travelled much; in no country on the face of the globe is the state of the roads so truly disgraceful, or so little indicative of the rule of an enlightened people. This is a truth the more provoking, because so little expenditure of labour would suffice to ensure all that is wanted*.

The utility of a few good lines of road in a mountainous country, is, if possible, more directly obvious, at least to the traveller, than in the plain country. But if roads are useful, bridges may be said to be absolutely necessary; for without them, there can be no intercourse whatever in a country so completely intersected by rivers; by rivers that are very seldom fordable, and never navigable. Ferries in such rivers are entirely out of the question; without bridges, therefore, the people must be confined to the narrow spots on which they were born, and all inter-

* I should be sorry to be so far misunderstood in any of these remarks, as to have it supposed, I mean to impute blame to any one; either to the officers of Government or to the Government itself. It is as far from possibility, that one man should be able to do the work of ten, as that 5 or 600 Europeans, be their intelligence, zeal, and activity, what they may, should be able to govern and perfectly manage in all its details, a country so extensive as this subject to our sway. The wonder is, that it has been done so well. The fault is in the system.

change of commodities, or other intercourse must cease. To give some idea of the number of impassable torrents by which the face of the country is covered, I may mention that on the road from Bhamaúri to Almórah, a distance of 44 miles, there are required in the rainy season three bridges. In the road from Almórah to Petórahgerh (40 miles), there are required two bridges all the year round. In the road from Petórahgerh to Lóhu-ghát (18 miles), one bridge all the year round. These are all military roads, and as the communication must be kept open, the fact is, that these bridges have been erected at the expense of Government.

The people of these hills, every where aware of the value of intercommunication, have exerted themselves to facilitate it, but their efforts, like their means, have been circumscribed; and, in general, their bridges have been rude and inefficient in a degree commensurate with their poverty. Their most simple erection is a single rope, or rather several ropes gathered together, stretched across a river, on which slides a wooden block, to which the traveller, being attached, is pulled across. This is a disagreeable, though not a dangerous method of crossing a river; but it is exceedingly tedious, and laborious to the attendants at the bridge; and has this capital objection, that cattle cannot pass such a bridge; they are necessarily dragged through the foaming torrent, where if they escape the two-fold danger of drowning or being dashed to pieces against the rocks, they are fortunate. Yet as even the worst communication is better than none, and as the poverty of the people cannot afford better, these bridges are numerous in the hills.

The next step, is the bridge of ropes, which is a little, and only a little better; being merely a huge rope ladder as it were, stretched across the river, on the spokes or steps of which the traveller has to pick a precarious footing, assisted by the two lateral ropes which he holds to steady himself. This kind of passage of a river is scarcely less disagreeable than the other; it is a little less tedious, and involves little labour on the part of the attendants; but it has the same disadvantage of being impassable to cattle. These bridges are generally made of rope, manufactured from the fibres (bark) of the *Málzan*, *(Bauhinia scandens)* a gigantic climber common in the mountains. It is a rope of great toughness; but exposed as it is, without any defence, to the action of the sun and atmosphere, it does not last many years. Instances have occurred of these bridges giving way, and the unfortunate passengers being lost.

The third step is the wooden bridge, and this alone is, perhaps, entitled to the name or capable of answering the purposes of a bridge. It is, however, of various degrees of efficiency, and some of its ruder and less expensive forms, are indeed scarcely preferable to the preceding arrangement. But whatever the care bestowed in its construction, one principle guides the workmen; a principle which shows little mechanical ingenuity on the part of these people, and which is the very opposite to that which directed the construction of the celebrated bridge in Europe, at Schaffhausen, over the Rhine. Instead of disposing their timber in a light frame work, judiciously opposing tension to stress, they use massy and heavy beams, the support of which, when the bridge exceeds a certain span, forms a great difficulty in the erection of such bridges. They are in fact only kept in their places by a heavy mass of stones and earth placed on them, and even this weight is not always a match for the great length of lever combined with the weight of the bridge itself, particularly when extra loaded by passengers; any twist or warping of the beams too, immediately weaken such a bridge. These objections will be better understood by considering the following short description of the mode of construction. A row of massy beams about 18 ft. in length, are laid in a direction, slanting upwards, on each bank, with not more than 6 feet projecting over. On these a second tier of beams, 6 ft. longer, is laid, the extra 6ft. projecting beyond the ends of the lower tier: a third and fourth tier, and even more, are added, each projecting 6ft. beyond the inferior tier, till the vacant space in the middle is so far reduced as to allow of spars being laid across it. The ends of the projecting beams are then secured by masses of stones and earth, as above noticed, and the whole is planked over and railed. Such a construction does not require its faults to be pointed out. Were it necessary, nothing could show them in a clearer light, than the very narrow limits to which such a bridge applies. A span of 100 feet is not without its difficulties. To erect one of 190 feet was considered a great triumph, and I believe no one thinks it possible to apply this construction to a bridge with a span of 250 feet.

It will be supposed that the bridges erected by order of our Government, mentioned in one of the preceding paragraphs, were on a superior principle; and that an opportunity was taken of showing the people the inferiority of their method of construction, as compared with those which the genius of improvement had so fully developed in Europe. It may, therefore, surprise your readers to be told, that so far from it, no attempt was made to bring to the consideration of the question any of the resources of modern science, or even of the most ordinary mechanical skill; and that, content to follow where we should have led, we blindly adopted the above clumsy and ill-contrived bridge, and that the six bridges mentioned, as necessary for keeping open the communication on the military roads, were all built on this model. In the usual course of things, experience at last made us wiser, so far at least as to be aware of the great expense of these bridges, both in original cost and repairs; and also in want of durability. About this time, the Shakespearian rope bridge, had attracted a good deal of attention, and those who, from travelling in the mountains, had personal experience of the want of bridges, as well as of the greatness of such a want, began to hope that such a bridge would be a great boon to these people; and being, as it was said, so cheap, might come even within their own means to erect them in various remote parts of the country. Having also a general resemblance in principle to their own rope-ladder bridge, it was thought that they might the more easily understand and appreciate such a bridge, than one requiring greater mechanical skill, or resources of art less available in their remote situation.

In a short time, several of these bridges were erected in various quarters, some at the expense of Rájahs, and some at the expense of Government; they were considered, we believe, to be a decided improvement on the wooden bridge above described; as being more convenient, more economical, and less confined as to the limit of span; and in fact, it was generally thought that this was the best adapted form of bridge that could be devised for these provinces; and that the introduction of it into such a country, was a real benefit. A little more experience showed the fallacy of this opinion. The durability of the rope was found to be less than half that of timber, while the expense of attending to the former, proved to be considerable. An establishment was required to tighten the ropes; to watch that none of the iron-work should be stolen, or ropes wilfully destroyed; and where the nature of the torrent would allow it, for taking them down during the dry months. In this latter case, a store-room was required in which to lodge the ropes. In considering all these particulars, it appeared that the difference of expense, which on a first view was 4 to 1, dwindled down as low as 8 to 5; while the greater security and convenience of the wooden bridges, was thought to be more than a balance for this small difference of expense. The Shakespearian bridge began now to be considered only fit for the smaller rivers, or for those situations where wood was not to be had; and for the larger spans the *sanga* was thought best adapted.

There were still some who could not but think the *sanga* or wooden bridge a very unskilful structure, and who wondered that European science could devise no better. The quantity of timber used in these bridges is enormous, and might certainly, if judiciously disposed, support four times the weight they are capable of bearing. It is indeed truly surprising that the subject of carpentry and framing of timber should be so little understood or appreciated, as to permit of such structures being erected under European superintendence. It being, however, taken for granted that these were the best wooden bridges, (although the plate and description of the Schaffhausen bridge, which is in every Encyclopædia, might have proved the contrary,) every new project for erecting bridges of a different material attracted the more attention; and this, assisted by the judicious puffery of the Calcutta newspapers, was the great secret of the short lived popularity of the Shakespearian. The late Mr. Adam, when residing at Almórah, took great interest in the question; and his attention being drawn to an account in one of the scientific journals, of a bridge erected at Geneva, constructed entirely of iron-wire, he thought at first that such a bridge might be preferable to any yet tried in the mountains. A gentleman there, I believe, wrote to Calcutta for some iron wire, to try the experiment; but I never heard the result, further, than that it was found too expensive a delicacy for the mountains, where from the number required, economy was a primary consideration. At length the iron-chain suspension-bridge was proposed, and this invention, so useful and so generally applied in Europe, promises to be not less so in these mountains. For economy, for durability, and portability, it has no equal. It requires scarcely any attention or repairs, and is applicable to the largest spans. The following extract from Mr. Ainger's lectures, at the Royal Institution, puts the subject in so clear a light, that I shall make no apology for giving the whole passage.

"The most striking circumstance in these bridges, is their great economy, as compared with ordinary or what are called insistent bridges. That economy arises from the power of a suspension-bridge to vary its curve, so as to adapt it to any variation or partial excess in its load; in consequence of which, the strength of the chains may be with great precision adjusted to any required strain, and no more: while in insistent bridges, the liability of the arch to a fatal derangement of its form from partial or excessive pressure, requires an enormous increase of weight, and of strength beyond what is requisite for the mere support of its load, supposing it were uniformly distributed.

"Iron, independently of its cheapness and extensive diffusion, is singularly and admirably adapted for the construction of suspension bridges. When it is considered that the greater part of the weight of these bridges arises from the chains themselves, it is evident, that the best material for this purpose is that which has great tenacity with small weight, and thus we find that iron is at the same time the most tenacious, and excepting tin, the lightest of the common metals. A square inch of good iron requires about 28 tons to separate it, and it will not be stretched or otherwise affected with less than half that weight." Mr. Ainger goes on to say, "Where, however, economy and portability are important objects, rope bridges will be found advantageous, and they have been during the last few years extensively introduced into British India by Mr. Shakespear, the Post Master General at Calcutta. They have produced great benefit in facilitating the transport of troops and baggage, as well as of merchandise and the mails. One of these bridges, 160 ft. in length, *is so light and portable, that it has been several times set up and removed in a few hours.*" The reader will presently see, that, in giving this opinion, Mr. Ainger was not acquainted with the real value of the Shakespearian bridge.

The merit, I believe, of first pointing out clearly the great superiority of the iron chain bridge over any that had yet been constructed of wood or rope, belongs to the present superintendent of that department. In 1828, nine bridges were sent up for the military roads about Almórah, before mentioned. The span was from 80 to 190ft. and the cost only 5,256 Rs. being an average of 584 Rs. for each bridge. Sundry spare articles, amounting in the whole to 707 Rs. increased the above average 78 Rs. making it 626. One of these bridges alone, the 190 ft. span, cost Government, when constructed of wood, 10,000 Rs. and what is still more to the purpose, it was, in less than three years, condemned by a committee as unserviceable. Their opinion was justified by the result; for the following year, two unfortunate men, having ventured to cross in spite of the repeated public notices that had been given, the bridge gave way, and they were lost. I believe the smallest of those bridges, as made of wood, did not cost less than 3000 Rs. and probably on an average, one with another, they cost 5000, or eight times the price of the iron chain bridges, which will probably last five times as long. The Shakespearian bridge was found to have in economy, only the advantage of 5 to 8; so that the iron-chain bridges will be FIVE times cheaper than these, contrary to Mr. Ainger's and the inventor's opinions. On this subject I propose to enter into a little detail.

The strength of iron has been found, by experiment, to be nearly SEVEN TIMES greater than that of the freshest hemp rope (of European manufacture). The weight then of two bridges of equal strength of iron and hemp, should be nearly equal, *i. e. while dry*. In practice, however, it is found that so many additional pieces are required in a rope bridge, as ties, slings, braces, &c. as to raise the weight of the rope bridge to double that of the iron, strength for strength. This in itself is a serious objection, independent of its reference to economy. The price of wrought iron is found to be from 10 to 12 Rs. per factory maund; that of good European hempen rope is 21 Rs. per cwt. or 56 seers, equal to about 16 Rs. per maund. We see therefore, that even on the supposition of equal weights, rope is dearer by one-third than iron: if we take double the weight of rope, as shown above to be necessary, a bridge of rope will be nearly three times the cost of one of iron.

Now, supposing a rope-bridge will last four years, (and this I think is a favourable supposition,) an iron one will last ten*; and as it costs only a little more than ONE-THIRD, the price is evidently more economical in the ratio of nearly SIX to one. It may however be said, that Europe hemp is dear, and that a cheaper material is on the spot (the *Malzun* rope), the adoption of which, would make a great difference in this estimate. I shall therefore, as the best answer to this objection, and as a means of showing the great superiority of iron, here compare two estimates, the one showing the expense of a bridge of 165 ft. span to be constructed of rope, manufactured in the hills; the other of an iron-chain bridge of the same span. That the latter estimate is not too favourable, will appear by the details I shall presently bring forward of what *has been actually done.*

* The duration of an iron bridge is assumed as above, to allow of every advantage to the rope-bridge in the comparison; but I believe, I might safely have said 20 instead of 10.

1. *Estimated expense of a Rope-bridge of* 165 *feet span.*

	R. A. P.			R. A. P.		
Standards, platform, &c.				470	0	0
450 fms. of 5½ inch rope, wt. 3200 lbs.	288	0	0			
750 4½ 3600	315	0	0			
410 2½ 500	100	0	0			
1000 ft. twine,	40	0	0			
400 netting,	44	0	0	787	0	0
Iron work, bars, bolts, thimbles, rings, 1,500,				318	4	0
Total, St. Rs.				1575	4	0
Or, Sa. Rs.				1,507	5	0

2. *Estimated expense of a Chain-bridge of* 165 *ft. span.*

	R.	A.	P.
Standards, platform, &c. as before,	470	0	0
Iron work, (say) 60 maunds at 10 Rs.	600	0	0
Probable expense of transmission to Futtehgurh,	50	0	0
* Carriage to Almorah, putting up and every possible contingency,	250	0	0
Total, Sa. Rs.	1,370	0	0

By these estimates the expense of the chain-bridge appears to be $\frac{1}{11}$ less than that of the rope-bridge.

But in reality the saving is far beyond this, as we shall be able to shew in a few words. It appears, that such a rope-bridge would only last two years, i. e. the rope-work; and even to last this time, it must be taken down and put under cover for eight months out of the 12. These circumstances make a wonderful difference in the comparative economy of the two erections; and to show the real amount of this difference, let us take a period of 10 years, a period which a chain-bridge certainly will outlast, and during which it requires no looking after.

Estimated expense of a Rope-bridge of 165 *ft. span for a period of* 10 *years.*

	R.	A.	P.
Five sets of ropes, each set as above 787 Rs. lasting 2 years,	3,935	0	0
Standards as before, ...	470	0	0
Iron work as before, ...	318	4	0
Establishment and expenses of storehouses at 250 Rs. per annum, ...	2,500	0	0
Total, Sonat Rs.	7,233	4	0
or, Sicca Rs.	6,911	14	6

This is SIX TIMES the expense of the chain-bridge. And that the estimate of the latter is not under-rated, will be evident from the fact, that a bridge of 170 ft. span was afterwards dispatched, weighing 66 mds. 36 seers, Fy. Wt. † and costing Rs. 737.

A particular advantage which the iron chain-bridge has over the rope-bridge, as applicable to these mountains, is in the facility of carriage. We have seen that the weight of the bridges are, strength for strength, as two to one. This is much, but it is not all. The size of the bars which go to form the chain may be varied at pleasure, so as to be portable in any situation, and by any means, whether by hill porters, ponies, mules, bullocks, elephants, or wheel carriages. For instance, suppose them one inch square and 10 feet long; such a bar would weigh 37 pounds avoirdupois, which may

* It was found that the actual expense of transmission to Bareilly amounted to about 50 Rs. The land carriage thence to the foot of the hills may be reckoned about four annas a maund, or even less. The hill carriage by porters *(the most expensive)* is at the rate of one rupee for every 30 seers (Baz. wt.) carried 44 miles. This includes return hire. Some of the bridges required by Government are much nearer than Almórah, one not more than five miles from the foot of the hills. The above estimate is certainly above the medium charge.

† The factory maund is ten per cent. lighter than the bazar.

be carried without any difficulty on the worst roads. A coil of 4½ or 5½ inch rope, the weight of which would be probably five or six cwt. is not so easily disposed of.

In point of durability, including attention and repairs, the advantage is equally great; a chain-bridge once properly erected, requires little or no care, and the expense is confined to an annual cost of paint, or coal tar; either of which, will effectually preserve iron for many years, however inclement the weather to which it may be exposed. The rope-bridges which were erected in Kamáún, on the contrary, required constant attention, and the maintenance of an extensive establishment, as well for tightening the ropes, as to preserve them from intentional mutilation. During eight months of the year, they were wholly removed and lodged in storerooms. Yet with all these precautions they are not expected to last in the hills above two seasons. In convenience, therefore, as well as in actual safety, iron must be always superior to rope, which from its flexibility, however well secured or supported by guys, is ill adapted for bridges. This is so obvious that it is unnecessary to insist more on it.

Thus we see that in economy, whether as regards prime cost, expence of carriage, expence of management, or durability, iron has a great and manifest advantage over rope as a material for bridges. In point of convenience, whether as regards facility of carriage, durability, or as requiring little attention, it is also greatly superior. The erection of bridges every year, and their reconstruction every two years, would be a seriously inconvenient arrangement, even were it not an expensive one. Lastly, there can be little doubt of its being the most safe material, in as much as it is, strength for strength, only half the weight; being scarcely subject to deterioration from exposure, it is not liable suddenly to become insecure in consequence of any unusual or unseasonable weather, or casual inferiority of material; and lastly, not being subject to the stretching and tightening of rope, it is not liable to suffer from any change in the figure of the bridge, or alteration of strain on the parts.

The above points have been so clearly established, and are now so generally admitted, that there is every prospect of this bridge being extensively introduced within the hills; and doubtless, it is one of the greatest boons the mountaineers have yet received from their European masters. Tables have been furnished, by which any engineer or executive officer may at once determine the expense of these bridges, according to their span and the weight they are required to carry. The following are the particulars of three different sizes which may, we think, be interesting to many.

Dim. of bridge.		Section of iron for chains.	Probable weight of finished iron work.	Prob. Exp. of iron work deliverable at the H. C. yard.	Estimated weight of platform.	Load at 20 lbs. pr. square foot.	Equivalent to men.
Length.	Breadth.						
ft.	*ft.*	*Sq. inch.*	*Fy. mds.*	*Sa. Rs.*	*lbs.*	*lbs.*	
100	6	2	30 to 35	360	5,000	12,000	80
150	6	3	40 to 50	480	7,500	18,000	120
200	6	4	60 to 70	720	10,000	24,000	160

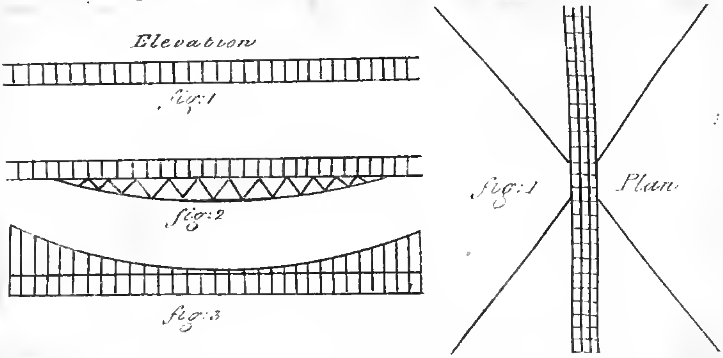
Note 1. For retired and less frequented situations, lighter bridges might be constructed at, probable, one quarter the above expense; the loads allowed for these being ample, and such as are not likely to be placed on any but military bridges.

Note 2. In order to facilitate the carriage of the iron, the bars can be made of any length considered most convenient, so as to admit of the whole being carried by hill porters if necessary; multiplying the numbers of joints must of course increase the expense in some measure; the above estimate supposes the bars not less than 10 ft. in length. Such a bar having a sectional area of one inch, would weigh 27 to 30 lbs.

Note 3. In calculating the comparative expense of iron, with that of any other material, the former may be assumed to last 20 years, at the expiration of which time, it might still be available for bridges of smaller span, or would at least be valuable and sell as old iron to be wrought up again. Its durability, however, will greatly depend upon its being occasionally cleaned and fresh painted or tarred.

It will be useful to mark also the several modifications of form of which the iron-bridge is susceptible. The most obvious and secure against vibratory motion, as well as the most economical, is that represented in the accompanying elevation fig. 1. next page. The chains are carried straight across.

The second, (Fig. 2,) which is applicable to greater spans, is nearly the same. The chains are less tightened, and the platform is supported by a light framing.



In the 3rd form, (Fig. 3,) fulcra or piers are erected, over which the chains are suspended. The platform is then attached to them by vertically suspended rods; the path or roadway passing between the supports. This form is applicable to the largest spans.

When the breadth of the stream is considerable, and the depth not very great, economy and safety will be best consulted by multiplying the number of curves, and having one or more supports in the bed of the stream: vibratory motion must naturally be expected where the length of chain is considerable and the platform narrow; but this may be obviated by applying horizontal chain guys in the manner represented in figure 1.

A Mountaineer.

II. *On some Petrified Shells found in the Gawilgerh Range of Hills, in April* 1823. *By the late* H. W. Voysey, *Esq. Assistant Surgeon His Majesty's 67th Foot.*

[From the Asiatic Researches, vol. xvii. pt. i.]

This remarkable range of hills is called, by Arrowsmith, in his last map, the Bindih, or Bindachull (Vindhya or Vindhyachala) hills. The same name is, however, given to a lofty range of hills on the left bank of the Godáveri, as it passes through Gondwána, and also to those near Guálior. I shall, therefore, distinguish them by the name of the Gáwilgerh range, particularly as, after repeated enquiries, I have never been able to discover that they were as above designated, either by the inhabitants of those hills or of the neighboring plains. They take their rise at the confluence of the Purna and Tapti rivers, and running nearly E. and by N. terminate at a short distance beyond the sources of the Tapti and Wardor. To the southward, they are bounded by the valley of Berar; and to the north, by the course of the Tapti. The length of the range is about one hundred and sixty English miles, and average breadth, from twenty to twenty-five miles.

On the Southward side they rise abruptly from the extensive plain of Berar, the average height of which is one thousand feet above the level of the sea, and tower above it to the height of two and three thousand feet. The descent to the bed of the Tapti is equally rapid, although the Northern is less elevated than the Southern side of the range. The outline of the land is generally flat, but much broken by ravines, and by groupes of flattened summits, and isolated conoidal frustra. The summits and the flat land are generally remarkably destitute of trees, but thickly covered by long grass:—in the ravines and passes of the mountains the forest is very thick, and, in many places, almost impervious. The inhabitants are principally Goanás, whose language, manners, and customs differ remarkably from those of the Hindus. At present, their chief occupation is hunting, and cultivating small patches of land, which produce a coarse rice and millet. In former years, the cultivation must have been very extensive, since there are the ruins of numerous hill-forts and villages, which derived their chief subsistence from the surrounding lands.

Many opportunities are afforded of studying the nature of this mountainous range in the numerous ravines, torrents, and precipitous descents, which abound in every part. A Wernerian would not hesitate in pronouncing them to be of the " newest floetz-trap formation," a Huttonian would call them " overlying rocks," and a modern geologist would pronounce, that they owed their origin to sub-marine volcanoes.

I shall not give them any other name, than the general one of trap-rocks ; but proceed to describe them, and state with diffidence the inferences which, I think, obviously present themselves on an attentive study of their phenomena.

1st.—The principal part of the whole range is formed of compact basalt, very much resembling that of the Giant's Causeway. It is found columnar in many places, and at Gáwilgerh, it appears stratified ; the summits of several ravines presenting a continued stratum of many thousand yards in length.

2dly.—The basalt frequently and suddenly changes into a wacken, of all degrees of induration, and, I may say, of every variety of composition usually found among trap-rock.

3dly.—Into a rock which may be named, indifferently, nodular-wacken or nodular-basalt, composed of nuclei of basalt, usually of great specific gravity, surrounded by concentric layers of a loose earthy mass, resembling wacken, but without cohesion, which, on a superficial view, conveys to the mind the idea of a fluid mass of earth, having, in its descent from some higher spot, involved in its course all the rounded masses it encountered, and, subsequently, become consolidated by drying. A very slight inspection is sufficient to detect the true cause of this appearance, which is owing to the facilities of decomposition of the outer crust, depending on difference of structure and composition. In none of the conglomerates, or pudding stones, do we observe any traces of this structure, and as it is common to the most crystalline green-stone, porphyritic green-stone, and those rocks usually denominated syenite, there can be little doubt, that it is owing to the developement of a peculiar concretionary structure by decomposition. In a small ravine, near the village of Sálminda, two thousand feet above the sea, I saw basalt of a perfectly columnar structure, closely connected with a columnar mass formed of concentric lamellæ, inclosing a heavy and hard nucleus. Near this ravine, I had also an opportunity of observing the gradual and perfect passage of the columnar basalt into that which has been called stratified, from the parallelism of its planes ; the composition being identical, and, without doubt, cotemporaneous. These changes and passages, from one rock into the other, are so frequent and various, as to render it impossible to refer the most of them to either of the rocks I have above mentioned as types. I shall, therefore, proceed to describe those which are distinctly marked, and their accompanying minerals. In external appearance, the columnar and semi-columnar basalt closely resembles that of the Gaint's Causeway, possessing the same fracture, internal dark colour, and external brown crust. It is equally compact and sonorous. It, however, contains more frequently, crystals of olivine, of basaltic hornblende, and of carbonate of lime. The fusibility of each is the same. Perhaps the basalt of the Gáwilgerh range more nearly resembles in every respect, that of the Ponce mountain in the Mauritius. This is, however, of very little importance, since every body, who has travelled much in trap countries, knows well what great changes in composition and structure occur, even in continuous masses. Among the minerals, calcedony, and the different species of zeolite, are rarely found in the columuar basalt, but they are of frequent occurrence in that which is semi-columnar.

The waken, or indurated clay, is as various in character and composition as the basalt, and unfortunately, I have no type with which to compare it, as in the case of the basalt of the Giant's Causeway. Its colour varies with its constituents, but is most usually gray. It is easily frangible, very frequently friable, and is almost always porous and amygdaloidal. It appears to be composed of earthy felspar and hornblende, with a considerable proportion of oxyd of iron. It is always easily fusible into a black scoria or glass, according to the quantity of zeolite which it contains ; of all the trap-rocks, it abounds the most in simple minerals.

They are—Quartz.

Calcedony and calcedonic agates, enclosing crystals of carbonate of lime.

Common and Semi-opal.

Heliotrope.
Plasma, or translucent heliotrope.
Stilbite.
Analcime.
Natrolite.
Icthyophthalmite.
Felspar.
Carbonate of lime and green earth.

I have never been able to discover in it, either angite or hornblende in distinct crystals. When the surface of the land is strewed with these minerals, it is a certain indication, that the rock beneath is wacken. With regard to the situation of this rock, I have rarely seen it on the summits of hills, but much more frequently at their bases and forming the flat elevated plains. I shall have occasion to advert to this rock again, when I proceed to describe the petrified shells.

The nodular basalt is, perhaps, the most common form of trap in this mountain range, as well as in other parts of India. It more commonly forms the surface than either of the rocks, and is as frequently seen on the summits, as it is at the bases of the mountains. It rarely abounds in minerals of any kind. It is the principal source of the rich, black, diluvian soil of India, commonly called black cotton soil; I have little to add to the former description of it. Its external structure is sometimes beautifully developed by decomposition; since in a mass of about six inches diameter, it is possible to count above twelve concentric layers; and on striking the necleus a slight blow with hammer, one or two layers more are broken off. It is owing to this facility of decomposition, that the annual rains carry down such vast quantities of alluvial soil from its surface, which is, moreover, always strewed with an abundance of nuclei in various stages of decomposition. It is owing to the difficulty with which the roots of trees penetrate this rock, that they are so rare on its surface, and never grow to any size; yet this circumstance does not prevent the *Andropogon contortum*, and *Nardus*, from growing in the most luxuriant manner; which sufficiently proves the fertility of the soil.

On ascending from the Tapti, I observed in a nullah, a groupe of basaltic columns, one of which was two feet in diameter, and six-sided. When near the summit of the flat table land of Illan, I entered on a pass, formed on one side by a perpendicular section of the rock, from twenty-five to thirty feet, and on the other, by a rapid descent of forty or fifty. The lower part of the section, as well as the pathway, composed of the wacken, or indurated clay of the kind I have before mentioned, of about ten feet in thickness; lying on it is a stratum of earthy clay, of different degrees of induration and purity, twenty yards in length, and of about two feet in thickness, containing great numbers of entire and broken shells. This possesses all the characters of a stratum, since the horizontal fissures are parallel, and are prolonged, with a few interruptions, through the whole extent. The accompanying sketch will serve to give a tolerably correct idea of the mode in which the stratum appears to overlie the lower rock, and to have been depressed by that which is superincumbent. The upper rock consists of about fifteen feet in thickness of the nodular basalt, or wacken. The nuclei being of all sizes, the vertical fissures, which are so remarkable in trap-rocks, are prolonged from both the upper and lower rocks into the shelly stratum, although there is no intermixture of substance.

The stratum is composed of a highly indurated clay, fusible before the blowpipe, into a fine black glass, and neither it nor the shells it contains, effervesce in acids. The shells are for the most part flattened, and belong either to the genus *Conus* or *Voluta*. It is not possible to conceive that so fragile a substance as a thin land shell should have been so completely flattened without pressure, unless it had been previously softened by some mode, which at the same time produced a sufficient degree of pressure to effect its flattening. I have attempted, in the annexed sketch, to give a representation of the degree of flattening, but I fear that it can only be well understood by the specimens themselves. Neither the rock nor its contained shells, effervesce in acids. Westward, the ground is covered by the debris of a shelly conglomerate, much more indurated and impregnated with green earth, exhibiting cavities and shells in relief; from the shape of the former, there can be no doubt of their having once contained shells. Some of the shells are entire, but are rarely flattened. The matrix appears to be siliceous, and, in some cases, approaches to imperfect heliotrope. It is not fusible before the blowpipe.

I may here mention, that in a report to the Marquis of Hastings, in June 1819, I mentioned the existence of shells in trap-rocks at Medconda, at a height of two thousand feet above the sea. The hill was composed of nodular trap, and lying on its surface, were numerous pieces of siliceous stone, containing shells of the genera *Turbo* and *Cyclostoma*; the specific gravity of the stone varied from 2,0 to 2,5. the shells did not effervesce in acids, although some of them preserved their external polish. Internally, some of the stones appeared to pass into flint, particularly those of small specific gravity, whilst their external surface effervesced in acids. Some of the small shells were completely changed into calcedony. Specimens of these shells are lodged with the Asiatic Society.

It is a remarkable fact, that the only remains of animals, hitherto discovered in India, should be found in trap-rocks, and under quite peculiar circumstances. 1st. They are found in situations where there are no indications of the former existence of lakes. 2dly, Both the shells and matrix are destitute of carbonic acid. 3dly, The former are in many instances squeezed flat without fracture, and, in some cases, completely commixing with their matrix.

These effects could only have been produced by the agency of heat, and consequently, the modern theory of sub-marine or sub-aqueous volcanoes, will best serve to explain the phenomena. These shells were deposited in the stratum of clay in which they are now found, and when forced up by the mass of wacken beneath, they were, most probably at the same time, covered by the nodular basalt. Thus we have heat to drive off the carbonic acid, and soften the shells under a pressure, which assisted the process, and at the same time flattened them.

I have too numerous collateral proofs of the intrusion of the trap-rocks in this district, amongst the gneiss, to allow me to doubt of their volcanic origin. I shall take an early opportunity of completing the history of the trap-rocks of India, for which I have collected materials for several years past.

III. *On the Measure of Temperature and the Laws which regulate the Communication of Heat.* By M. M. Dulong and Petit. With a plate.

[From the Journal de l' Ecole Royale Polytechnique, T. xi.]

§ 2. *On the expansion of Mercury.*

The importance of a correct determination of the expansion of mercury is a necessary consequence of the application of the barometer to the measurement of differences of level. Nor is the value of this datum much less obvious in a variety of other physical enquiries; so that there are few determinations of this kind which have given birth to more numerous researches. Yet notwithstanding all the precautions which observers have taken to ensure that accuracy, of the value of which they were fully sensible, there are few subjects in which examples of greater discordance in the results will be found. For instance take the following:

Expansion of Mercury from 0° *to* 100°.

By Dulton is,	$\frac{1}{50}$	By DeLuc,	$\frac{1}{56}$
Mr. Cavendish,	$\frac{1}{53}$	Gen. Roy,	$\frac{1}{59}$
Sir G. Shuckburgh.	$\frac{1}{54}$	La Lande and Delisle,	$\frac{1}{66}$
La Place and Lavoisier,	$\frac{1}{54}$	Dom Casbois,	$\frac{1}{67}$
Hallstroem,	$\frac{1}{55}$		

The greater number of these determinations were obtained by adding to the apparent expansion of mercury in glass, the expansion due to the latter, and as this has been hitherto more or less uncertain, the preceding results were necessarily affected by this uncertainty.

DeLuc, Casbois, and General Roy, have attempted a direct measurement of the expansion of mercury, by means of the change in the barometric column, produced by a known change of temperature. The results of this attempt were even still more erroneous. It would not be difficult to point out the reasons of this, had we time for an examination of the methods employed by each of these experimenters; but it would be necessary for this purpose to enter into tedious details, added to which the labours we are referring to have only a relation to temperatures below 100°; whereas, it is equally above this term that we require the expansion of mercury. It became then necessary to have recourse to new methods, and we accordingly made choice of one which we shall now describe, and which is, we think, susceptible of every desirable accuracy.

It is founded on the incontestable principle in Hydrostatics, viz. that when two liquid masses communicate by a lateral tube, the vertical heights of their surfaces are in an inverse ratio to their densities. If then we can contrive to measure the heights of two columns of mercury, contained in the branches of a glass syphon in a reversed position, one being surrounded by melting ice, while the other may be raised to any desired temperature, we can easily deduce the expansions we are in search of.

If h and h' represent the vertical heights of the two columns, the pressures of which correspond to the temperatures t and t' we shall have, putting d and d' the corresponding densities.

$$hd = h'd'$$

Now d and d' are in an inverse ratio to the volumes v and v' which the same mass of liquid would occupy in raising it to the successive temperatures t and t' Then

$$v' = \frac{v\,h'}{h}$$

from which we get for the mean coefficient of expansion between t and t'

$$\frac{h'-h}{h-(t'-t)}$$

Every thing then depends on the exact measurement of the temperatures and of the height of the columns. It is scarcely necessary to remark, that in this way we obtain the absolute or true expansion of the liquid, since as the shape of the containing vessel exercises no influence whatever on the pressure of its contents, neither can any expansion affecting them influence it.

Boyle was the first who proposed this method as a means of comparing the specific gravities of fluids. Other experimenters have thought of applying it to the measure of expansions, and it is probable, that in the lower temperatures it is as applicable as it is exact; but when we come to temperatures of 300° and more, it is certainly of very difficult application.

In order to render intelligible the following description of apparatus which we used, we have drawn an outline of it, (Fig. 1, Plate VI.) in which, however, the principal parts only are drawn; the reader can easily supply what is omitted.

The bent tube which contains the mercury, has two vertical branches A B and A′ B′ which communicate by a horizontal tube B B′ exactly levelled, and having the same diameter as well as thickness of glass throughout. We had taken the precaution to ascertain beforehand, that the pressure was readily communicated from one branch to the other, through the horizontal tube, and that the friction against its sides in no degree interfered with the re-establishment of the level when that had been disturbed.

Each of the two vertical branches is formed, as we may see in the figure, of two tubes of different diameters properly united. In using a smaller diameter for the lower tube, we diminish the quantity of mercury, and by using a larger diameter for the upper tube, we guard against any inequality of the capillary depression which might be occasioned, by the difference of temperature in the two legs.

The horizontal tube rests on a strong iron bar, M N shaped like a T and supported by its three legs on a very thick table. The upper surface of the bar has been worked quite true, and carries two spirit-levels placed at right angles, which are adjusted by means of screws placed at the four corners of the table. Close to each of the vertical tubes, there rises an iron arm carrying a ring with screw, which serves to hold the tube in a fixed position. Not to crowd the paper too much, we have only drawn the iron arm along side the tube A B It is terminated, as is shown in the sketch, by a small bent piece of iron, the point of which R is meant to serve as an index.

The bent tube being thus adjusted in all its parts, it remained, so to arrange the apparatus, as to communicate the requisite temperature to each of the columns. Nothing was easier in the case of the column A B, which it was desired should be at 0°. The object was effected by surrounding it with a large cylinder of cast iron, cemented below to the bar of iron all round, and filling the cylinder with ice up to the surface of the mercury in the tube. A small window F had been made and applied to the cylinder, which was occasionally opened when observing, in order to take out a little of the ice, so that the surface of the column might be better observed. Thermometers, which were immersed from time to time in the mercury, satisfied us, that every part of the column was actually of the temperature 0°.

That part of the apparatus again, which is meant to contain the bath for heating the column A′ B′, presented the greatest difficulties in its arrangement. It was necessary, that the bar M N should pass through this box, and that the space left between the bar, and the edges of the aperture, should be filled with a lute, impermeable to water. In order to fullfil all these conditions, we had a cylinder formed of copper, the bottom of which might be detached at pleasure. At the upper end, the edge is turned over at right angles, forming a support on which the cover fits; towards the bottom it carries two appendages in the shape of horizontal half cylinders, in which lies the bar M N—Fig. 2, a section of it made on a vertical plane, parallel to the direction of these appendages, will give an exact idea of this arrangement. The bottom piece is represented in Fig. 3,—it can be fastened to the box by means of steel screws, which are tightened as much as possible. Even this force is inadequate to prevent leakage; so that, small pieces of card have been necessarily interposed between the two copper surfaces.

The use of these appendages is to allow of the lute being applied at a distance from the source of heat. Still, the lute becomes heated, and would eventually give way, if it were not kept cool by a constant current of water.

The box thus constructed, is enclosed in a furnace, supported every way by iron bars. This furnace is shown in the sketch as if cut down through the middle, in order that all the interior arrangement may be seen.

We shall conclude our description by saying, that the copper cylinder or box is filled with fixed oil, which can be heated gradually up to the temperature of observation. The openings of the furnace are then all of them shut, and the heat dispersing itself equally throughout the mass, the temperature remains stationary for an interval sufficient to allow of all the observations being made. But that nothing may interfere with the exactness of these determinations, it is necessary that the copper cylinder should be full of oil, and that the heated column of mercury should rise but a very little above the lid. This latter condition, was easily brought to bear, by adding or subtracting, a short time before the reading was to be made, a very small quantity of mercury, by the aid of a dropping tube. The former again was effected, by filling the cylinder in the first instance, and by applying a pipe L Q the orifice of which Q was level with the lower surface of the lid, for the extra quantity occasioned by expansion to flow off.

Let us now turn to the determination of the temperatures, and of the heights of the columns.

The oil bath contains two thermometers, the one mercurial, similar to that we have already had occasion to describe, and in which the temperature is estimated by comparing the weight of mercury lost by the instrument, to that which filled it at zero. Such is the sensibility of that we used, that an increase of 1° in the temperature occasioned the loss of a decigramme of mercury. The reservoir D E of the same diameter throughout, being immersed to the same depth as the column A′ B′ indicated the exact mean temperature of the column.

The second is an air thermometer, the cylindrical reservoir of which D′ E′ situated like the preceding, is terminated by a very fine tube E′ G′ H bent into a horizontal direction on quitting the furnace. This tube is joined at H′ to a vertical tube a little larger, of a uniform diameter throughout, and which dips into a vessel full of mercury K′. In order to regulate this thermometer, the oil bath is heated nearly to the boiling point, the end of the tube K′ being left open. When the surplus air has been driven out by expansion, the extremity K′ is immersed into a saucer of dry mercury. During the cooling of the oil, the mercury rises a little in the tube. It is by measuring, during the maximum of temperature, the height of this column and of the barometer, that we discover the increase of elasticity in the air; whence, by a simple enough calculation, we can deduce the temperature as shown by the air thermometer. It is hardly necessary to add, that the tube had been carefully dried, and that in each measurement an allowance was made for capillary depression.

The indications of this thermometer add in no ways to the exactness of those afforded by the mercurial thermometer. But we thought it advisable not to lose the opportunity of again comparing the scales of the two instruments. The results deduced from this comparison are included in the determination of the mean results inserted in table 1.

We have now only to describe the kind of micrometer, which we used to measure the heights of the columns. This instrument (Fig. 4.) is composed of a thick ruler of brass A B on which slides loosely a piece of the same metal M N P R S carrying at its two extremities R and S two supports or Ys, in which turns the micrometer O O′ having a horizontal wire in the focus. To the micrometer is hung a very sensible spirit level, the graduated scale of which serves to adjust the optical axis. The piece of copper M N P R S is capable of the two motions; a quick one, by the hand, when the side screw C is loosened, and a slow one, by means of the regulating screw D. The whole apparatus turns on a vertical axis, which is supported on a triangular plate of thick brass, furnished with three foot screws.

The nature of this instrument, it may be seen, allows of our measuring the difference of height of two points, though not situated in the same vertical. For this purpose, it is only necessary, after having directed the microscope on one of the points, to turn it on its axis till it point to the other. It is then raised or depressed, the proper quantity which is measured on a scale engraved on the opposite face of the ruler, by the aid of a vernier, moved by the piece M N P R S The application of a micrometer screw would, perhaps, have been preferable, but for the dispatch required in our experiments; and as the vernier allowed of our estimating the fiftieth of a millimetre, it appeared capable of sufficient exactness.

To give to this instrument all the nicety required, it was necessary that the smallest differences of level should be appreciable, and that in turning the microscope from one direction to another, it should preserve its horizontal position, or at least that account might be taken of any derangement which might occur. The first point was attained by giving sufficient power to the microscope; and for the second, the great care with which the level had been constructed, and the solidity of its support, which was unconnected with the rest of the apparatus, may be considered to have fulfilled this condition. Nevertheless, we had determined before hand, the difference of height answering to a change of inclination of one division of the level, regard being had to the distance of the observed columns. This determination enabled us to correct those observations in which any derangement of the level had been observed.

The methods by which such instruments are adjusted, are too familiar to require any detail from us. It is well known, that by proper reversal of the microscope, as well as by observations in different azimuths, while the instrument is turned on its axis, the latter is adjusted to the vertical, and the former to the horizontal line.

We will now return to the observation of the expansion. The micrometer was placed on a slab of marble T supported by masonry, the axes of the instrument was then at equal distances from the centres of the tubes A B and A′B′ and of the index R—the height then of this point above the summits of the columns was easily measured, that is to say $r—h$ and $r—h'$ for the height of the index In order to be certain that refraction through the tubes had no effect in the vertical plane, we placed in the centre of each a small well defined object on which we directed the cross wire of the microscope, and we convinced ourselves that the coincidence of the wire was nowise disturbed, whether the tube was taken away altogether, or merely turned round.

We had still to determine r. Now this height must be constant in all the experiments, as the stem which carried the index was always surrounded by ice. To measure it we made use of a graduated vertical ruler, the zero of which was placed on the iron bar M N this ruler, originally made for a different purpose, and with the utmost care, gave results true to the 10th of a millimetre. The heights, however, measured by it are all in excess, because h, h', and r, should be reckoned from the axis of the horizontal tube; we had, therefore, to subtract from the height given by the ruler the semi-diameter of the tube.

To give an idea of the exactness belonging to these different operations, we may give the particulars of the measurements made at 100°. The height of the index, above the axis of the horizontal tube was in metres 0,5825 the heights $r—h$ $r—h'$ were severally 0,03855 and 0,02875; thus h was 0,54395 and $h—h'$ 0,00980, and consequently the mean coefficient of the true or absolute expansion of mercury between 0° and 100° $=\frac{1}{5550}$. We may see by this that an error of two or three tenths of a millimetre in the value of r would only affect the preceding denominator by 2 or 3 units. So that in consequence of the peculiar arrangement of our apparatus, the measurement in which least exactness was attainable, was precisely that which could never occasion errors that might not fairly be neglected; even the iron bar might have been a little disturbed by the action of the heat (although care was always taken to set it truly horizontal by means of the levels) without affecting much the final result.

It is in this particular, that our apparatus appears to be superior, both in exactness of result and simplicity of construction, to those adapted to the measurement of the expansion of solids. In fact, in these latter, the slightest derangement of the fixed point, during the very long interval of an experiment, not only affects the total length of the ruler or bar, but the expansion itself is also augmented or diminished thereby, inducing errors of the greatest moment. Whereas in our experiments it is evident, that even were the heights h and h' to become affected in the manner we have spoken of, still the quantity $h—h'$ which is the measure of the expansion, cannot be; for it is highly improbable to suppose that the instrument should be deranged during the very short time that elapses between the successive observations of the hot and of the cold column.

We have included in the following table, the mean result of a great number of observations made in the manner just described. The first column contains the temperatures deduced from the air thermometer, the second the mean expansion of mercury between the temperature of melting ice, and each of the temperatures found in the first column. The third gives the temperatures, such as they would be, supposing the expansion of mercury uniform, or in other words, such as would be shown by a thermometer constructed with this fluid, the containing vessel of which should have the same rate of expansion as itself.

Table 2.

Temperatures as shown by an air thermometer.	Mean expansions of mercury.	Temperatures supposing the expansion of mercury uniform.
0	—	0
100	$\frac{1}{5550}$	100
200	$\frac{1}{5525}$	204,61
300	$\frac{1}{5500}$	314,15

IV. *Observations on the Occurrence of Fresh Water Testacea in temporary Pools formed by Rain, and unconnected with permanent bodies of Water.*

[To the Editor of Gleanings in Science.]

Sir,

I wish to attract the attention of such of your readers, as may have opportunities and inclination to investigate the subject, to a fact which is of frequent occurrence in this country; and which claims notice from its extraordinary nature, although, I cannot find that it has yet been discussed. I allude to the faculty possessed by some animals formed for living in, and respiring water, of inhabiting places, which during a large portion of the year are not visited by moisture; and which, containing water during the season of the periodical rains only, have then no communication with any other body of water. From this fact, one of three conclusions seems inevitable—either that the animal in its perfect state, has the power of retaining sufficient moisture within itself to sustain vitality until the return of rain; that it is enabled to withstand the inclemency of the hot season, in a state of extreme dryness; or that its ova possess the same extraordinary faculty. The occurrence of the common *Planorbis*, which is scattered so widely over these provinces in every water, from the running stream to the smallest pool, in shallow tanks which are dried up during a large portion of the year, may not excite so much surprize, as it belongs to a division of trachelipodes which breath air; but the appearance in such situations of a species of *Paludinæ**, belong to a genus, which is known to breathe water only, cannot so easily be passed over. This species I have found in a ditch, at Banda, excavated for the enclosure of a compound, and carried across the slope of a rising ground without any attention to level, so that the ditch rises on both sides of a part where the rain waters collect from the slope of the ground enclosed. No water, but that supplied by rain, can possibly have access to this place. The superfluous waters fall down the slope beyond, whence, during very heavy rain, they proceeded to a ravine distant more than a quarter of a mile, where they fall perpendicularly more than 20 feet, and are thence carried to the river; so that it is not possible, that shell fish should have access from the river by this channel. As

* It is small, ovate-oblong, whitish-yellow, sub-hyaline, the lip at the base of the columella slightly produced; operculum calcareous. It floats on the surface of water when it has attained it by climbing, but sinks when alarmed. It has no objection to dead animal substances as food!

the ditch is entirely dependant on rain-water for its supply, it is liable, even during the rainy season, to drought, and during the remaining eight months of the year it is as perfectly parched as the neighbouring soil; yet in a very few days after it has become replenished with water, the little *Paludinæ* may be seen clinging to the stems of the grass which then grows in it.

Perhaps the thick calcareous operculum of the *Paludinæ*, which fits closely to the shell, shuts it in from all communication with the external air, and enables it to resist evaporation, its own fluids supplying it with sufficient moisture to sustain life in a torpid state, during the prevalence of drought. In this manner a *Bulimus** (a land snail), by means of a viscous fluid which it secretes, fastens itself to plane surfaces; between which and its shell, it interposes a thin membraneous layer, composed of the hardened viscus, and may be found adhering to rails or the trunks of trees; exposed to the fury of the hot winds, the animal only dissolving its barrier, and venturing forth to feed upon the herbage when the rains have fully set in.

According to my second supposition, the animal may be torpid in the earth, beneath the surface of the place where the pool stood, or in a state of extreme aridity, and may revive on the approach of moisture; as land shells, which have been for years in ablutions in a similar state, and, which is more to the purpose, naked polypi, which live in water, have been known to do. The retention of life under analogous circumstances by fish and insects, in the states of lawa and pupa, submitted to severe cold and congealed to a solid mass of ice, is not more extraordinary.

Under either of these circumstances a difficulty still remains. In what manner did the original progenitor of these mollusca reach the spot? We must trace its transportation to a period, the memory of which has been lost in the lapse of ages, and which perhaps was anterior to the first peopling of the country, before the cave excavated its present deep bed, and when it was spread in the form of a lake over the spot in question, and a large tract of the neighbouring country. Of the former existence of this lake, abundant evidence is afforded by the appearance of the surface of the country, and the vestiges of inundation, particularly at the top of the elevation on the side of which the ditch is cut, where, at the depth of 3 or 4 feet, occur numbers of specimens of shells of a species of *Unio*, coinciding exactly with one of the species which now live in the cave†. At a somewhat lower level *Melaniæ*‡, *Planorbes*¶, and other shells similarly circumstanced occur imbedded in the soil in profusion. In some instances these shells are invested with a concretion of sand and calc-tuff which sometimes wears a crystalline appearance.

Should it be found on enquiry that the *Paludinæ* conceals itself in the soil, (which discovery is only likely to be made by chance, in consequence of the smallness of the shell,) a close examination and subsequent immersion in water will test the truth of the first two suppositions.

It is possible, however, that the ova, which in general have been found to retain vitality under exposure to much greater degrees of heat and cold than perfect animals, and which may be in this species a gelatinous substance, might be dried up to a form capable of being transported by storms, by which bodies much heavier are removed; but in this case the growth of the perfect animal from the ova must be very rapid.

The shells which I have hitherto mentioned belong to Lamarck's class of *Mollusca*, or the univalves of authors; but at Hamírpúr I have observed *Conchifera* (bivalves) in a shallow tank near the Jumna, to which neither the waters of that river, nor of the Betwa have risen for a half century; which is 12 or 14 feet above the highest rise of the rivers in ordinary seasons, and which has no communication with any other water. There are small and fragile, almost membraneous, species of *Anodonta*, which I have never met with in rivers, but have found them in other tanks and ponds, to which no stream can have had access since the period when the diluvial waters

* Bulimus. Ovate-conical, right lip sharp, whorls flattened, white, stercorate, with a single red-brown band at the middle of the lower whorl. Length ¾ inch.
Species B. Gl. Sci. p. 264.
Icon. pl. viii. *f.* 4. The length is taken from the largest specimen discovered.

† Unio. B. Gl. Sci. p. 265. Icon. pl. vii. *f.* 1.
It is found imbedded indifferently in loam and beds of calc-tuff gravel. In one specimen which I possessed, from the latter deposit, part of the shell has been changed, throughout its thickness, into calcareous spar, which retains the original form of the shell, even to the rugæ on the exterior; the rest of the shell appears only to have been deprived of its gluten.

‡ Melania. B. Gl. Sci. p. 264. Icon. pl. vii. *f.* 8. and 9.

¶ Planorbis. B. Gl. Sci. p. 264. Icon. pl. viii. *f.* 14.

were drained from the plains of Hindustan. The fresh appearance of the epidermis, and ligaments, although I have never met with the living animals enclosed in the shells, shewed that they must have died recently, and forbid the notion that they could have lain even a single month exposed to the decomposing action of the elements, which in this country act with such force and expedition, reducing the strongest shells in a short space of time to a friable mass. Yet the tank in which these shells occur, while the rains prevail, is dry during the greatest part of the year.

In concluding this notice on extraordinary localities of animals, it may not be out of place to mention, that on the summit of the fortified hill of Callinjer, inaccessible on all sides to animals unadapted for climbing, the tanks excavated in the sandstone rock swarm with tortoises, one of which belonging to the genus *Trionyx*, (comprising the soft fresh-water turtles,) has been sent to me for examination. I find on reference to the sketches and descriptions which I made some years ago, of the Jumna turtles, that it agrees with the characters of the species called Goomeree by the natives, which is one of several species of Trionyx which occur in the Jumna. It was also recognized as the same by the servant who procured me my former specimens.

Unlike the genus Emys, the Trionyx is not in the habit of venturing to any distance from the water, but suns itself on its margin on insulated ledges of sand, and it is by no means calculated by its structure for such excursions. I can only suppose that, from its kindred having a place in the Hindu mythology, the breed has been conveyed to the spot by the Brahmins, who, in former times, had charge of the temples on the hill. One of the tanks is accounted so holy, that, as I am informed by the gentleman who had the kindness to procure for me the specimen I now possess, the natives refused to take a tortoise from it, alleging, as a reason, that the person who should violate its sanctity by such a deed, would shortly be punished by death for his temerity; but that should one be caught out of the water, no danger to the captor was to be apprehended.

W. H. B.

V. *Remarks on* Cancar.

To the Editor of the Gleanings in Science.

Sir,

In No. 8 of the Gleanings there occurs a short notice on the "production of *cancar*," which I do not exactly understand. Is the "nodular substance" alluded to the work of the "shell-fish?" and do these shell-fish belong to fluviatile or marine varieties? or do they, as I should infer from the notice, inhabit indifferently both the salt and fresh waters? to what genera do they belong? how are they attached to the "nodular substance," or rather to the "minute cellular coating of shell-work," which "surrounds the nucleus of iron clay," by a byssus, or how?—or are they simple adhering univalves? And with regard to the nucleus itself, I am still further at a loss to understand its nature, or *how it got there*. It seems to differ in its character from any substance with which I am acquainted. It "becomes softer and loses its shelly coating during the dry months, when the waters become salt, " and, " when burned it crumbles on exposure to the air, but again becomes hard on immersion in water," (fresh water I presume) The notice is certainly an interesting one, and I trust, I should be excused if, in submitting these queries, I express a hope that either your correspondent or some one else will afford the information desired, without which the value of the notice, as a contribution to science, is greatly lessened.

The question, however, to which your correspondent directs the attention of your readers is a most interesting one. Whence have been derived those widely distributed beds of *cancar*, which while they form a most perplexing, are at the same time a most distinguished feature of Indian geology? Your correspondent justly remarks, that it is "difficult to conceive that a substance of such general occurrence and uniform appearance can be the *detritus* of any stratum of limestone of prior existence to it. In short, the cancar must be considered as a distinct and separate rock formation, to which the name of *cancar* formation may be applied, till such time as we can discover its real place in the geological scale of rocks.

I do not mean to trouble you with a description of the different varieties of *cancar*, (and there are many,) but rather to offer one or two very general remarks relating to its natural history, more with the view of receiving information from some of your able correspondents, than with the hope of myself being able to impart any.

Cancar, as before stated, is very extensively distributed throughout Hindostan, and is a substance with which all your readers are familiar. It very generally occupies the banks and beds of nullas, and is seen forming small rounded swells and tumuli generally in lower situations. But it has been found, also, on the tops of some of our lower ranges of hills, and in situations very considerably elevated above the level of the sea. The *cancar* beds are not, generally speaking, very thick; in this respect, however, they vary much in different situations; they include very frequently, numerous imbedded masses, both rounded and angular, of various kinds of rocks, from a granite down to limestone; the last apparently of not a very ancient variety, and many of these imbedded masses must have been transported from a great distance. From an attentive consideration of these last, I conceive that many interesting conclusions might be drawn, relative both to the age of the formation, and to the agent which was employed in forming it. The *cancar*, as far at least as my information goes, has been seen overlying all the rocks as yet discovered in India; but as the series appears to be the most recent of these, (the alluvial and diluvial deposit of course excepted,) we can only conclude from the above circumstance, that the *cancar* must be of more recent formation than the series. I have not heard of any well authenticated instances of organic remains having been found in *cancar*, at all events I can say that such an occurrence is rare; and, though we are thus deprived of the best means of determining its age, and of tracing an analogy between it and similar formations in other countries, still the very circumstance appears to me to be a proof of its antiquity, as compared with the more ancient diluvial deposits. I fear, also, that this consideration must set aside the opinion hinted at by your correspondent. I have frequently found in the alluvium, resting immediately on the *cancar*, numerous shells belonging to recent genera, but none of these could be found in the *cancar* itself; and had this substance been formed in the way your correspondent supposes, numerous shells must have been found associated with it.

In examining this formation, care must be taken not to confound it with some more recent deposits of *calc tuff*, which are frequently met with in India, or with calcareous soils often containing imbedded portions of globular *cancar*. The formation in question must, from its nature, have been exceedingly liable to be affected by those great denuding causes which have been concerned on a large scale, in forming our diluvium, and which still continue to operate on a smaller scale, in forming our more recent soils. The details which you have given us (in No. 6. of the Gleanings) of borings made in Calcutta, &c. prove the great depth at which such imbedded portions are found. From borings conducted on a similar plan the greatest information might be derived, and I sincerely trust that, ere long, we shall be put in possession of further details connected with this interesting subject.

I observe in a note, page 220, of your periodical, the remark, "That *cancar* has perhaps some analogy with the *cornstone* of some of the English counties." I have not by me the vol. of Geological Transactions to which you refer, but if the *cornstone* be the same rock as the *cornbrash* of the English geologists, I confess that I perfectly concur in your remark. The absence in India of rocks of the oolitic series and others of the more recent suites of the supermedial order, has often been remarked by geological observers, and I have for some time been in the habit of considering the *cancar* formation as occupying the place of one or other of the numerous series of subordinate formations included in the order just alluded to. From the absence of organic remains, I have not been enabled to form an opinion as to the precise period of its formation. The *cornbrash* is classed by Conybeare and Phillips with the inferior oolites, and in its structure, &c. many points of similitude may be traced between it and the *cancar*. There are many other deposits, however, belonging to the oolitic series, with which this substance might be compared; for instance, the numerous beds of an argillo-calcareous, calcareo-siliceous, &c. nature which are found so abundantly associated with the English oolites: and while, from the very extensive distribution of the *cancar* formation, we must conclude that the cause which formed it must have been very general in its operation, it is at the same time very natural to suppose, that this cause under other, and probably more favorable circumstances, might have formed rocks of a more distinct and tangible nature. Its whole appearance gives rise to the idea of its having been formed in a hasty and confused manner, but in it may probably be traced *all the elements* of the oolitic system of formations. My principal object in addressing you at this time is to obtain, if possible, through the medium of the Gleanings, some information on this interesting subject; and to ascertain whether, or not, any organic remains have been discovered in the *true* cancar *beds*, and if so, to what genera they belong.

While on the subject of your Editorial notes, I may as well reply to a query contained in note 2d of the said page 220. Most geological writers describe greenstone as a primitive as well as a trapperian rock. The former is the primitive greenstone or trap of the Wernerians, and you may see a description of it in Jameson, &c. &c. Dr. Macculloch too gives a minute account of rocks "identical with greenstone and basalt," which occur associated with the granites of Aberdeenshire, and those he declares to be "varieties of the most decided granite." From the above circumstance he deduces many important conclusions. This paper is altogether a most interesting one, and is to be found in the Journal of Sciences and Arts, (old series,) I forget the vol. but I quote from extracts made on reading it. The *grunstein* of the Germans include both our greenstones and hornblende rocks, and the same remark may be made in reference to the *diabase, diorite,* and *amphibolite* of the French.—In some late arrangements indeed, primitive greenstone is described as a variety of hornblende rocks, "differing from the greenstone of the submedial order more in geological position than in respect of composition."—But in the said submedial order of these geologists is included a greenstone which occurs "lying upon and interstratified with" the clay-slates, gray wake-slates and greenstone slates, &c. Many of these greenstones perfectly resemble the greenstones of the overlying trap formation, especially such of them as occur associated with the argillaceous schists, under which term Dr. Macculloch very properly *includes both* the clay-slates and gray wake-slates. Perhaps the terms trappean-submedial, and primitive or superior greenstones might, in the present state of our nomenclature, be sufficient to distinguish these three varieties, more especially if the term SYENITE be, as has lately been recommended, restricted to a rock composed of compact felspar and hornblende, which occurs associated with the overlying rocks. In describing the different varieties of hornblende rock, it is difficult to convey in any one work, a precise idea of their nature. From pure hornblende rock down to syentic granite, a term adopted by Dr. Macculloch, and which I should be sorry to see exploded, we find them of almost every variety of structure: sometimes too, in the compound rocks, we find the hornblende replaced by actinolite, and this last also, we occasionally find nearly pure, forming an actinolite schist. These with hornblende schist, form but varieties of one great primitive series; and while I perfectly agree with you in thinking, that the term greenstone as applied to rocks associated with the granites might be changed with advantage, we must still lament the want of a sufficiently precise name to substitute in its place. I should be happy indeed to learn that this term could be confined to the overlying rocks.

With regard to compact felspar, it also is a primitive rock. The prino-sandstone of Dr. Macculloch (a rock included by Phillips in his inferior order,) is stated by Dr. M. to pass into comp. felspar, and he also describes comp. felspar as an occasional constituent of granite and gneiss. The *ensite* of the French has a base of compact felspar, and the white stone (*weisstein* of Werner) is a granular felspar, which is only a variety of the compact, while comp. felspar itself, either pure or with associated mica and quartz, &c. is not an uncommon primitive rock. It certainly does occur much more abundantly as a member of the overlying trap series, both as a simple mountain rock, and as a constituent of the compound varieties.

I shall conclude this most *digressive epistle* with one allusion to a subject of a very different nature, and earnestly would I call upon those of your readers, who take an interest in the science of oryctology, to give it a share of their attention. It is believed by many geologists, that before the existence of animals, our earth was covered with various plants belonging to the families of Cactus, Euphorbia, &c. &c. and that from the remains of these, our great coal deposits were formed. All who have travelled in India must have been struck with the appearance of large tracts which are occasionally seen covered with individuals, especially of the latter of these families. On spots apparently the least likely to afford nourishment to plants,—among bare and barren cliffs,—they seem as it were to spring from the solid rock, and often attain a size and woodiness of fibre which might entitle them almost to the appellation of trees. That vegetables similar to these must have formed the *first covering of our globe* (fitted as they are for that soilless and rocky state in which it must then have existed,) is a conclusion at which we almost naturally arrive, and an interesting comparison might be drawn between the fossil varieties found in Europe, and those which exist in India at the present day. With the example of the great Humboldt before us, much might certainly be done to elucidate a subject which his discoveries have made so peculiarly interesting.

I am, Sir,

Your obedient Servant,

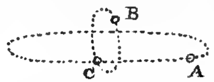
J.

VI. *Omission in the Remarks on "Arnott's Elements of Physics."*

Page 63, Arnott says, that, "The reason why a spinning top stands, will be understood here. While the top is perfectly upright, its point, being directly under its centre, supports it steadily, and although turning so rapidly, has no tendency to move from the place; but if the top incline at all, the side of the peg, instead of the very point, comes in contact with the floor, and the peg then becomes a little wheel or roller, advancing quickly, and with its touching edge, describing a curve somewhat as a skaiter does, until it come directly under the body of the top as before. It thus appears, that the very fact of the top inclining, causes the point to shift its place, and so that it cannot rest until it come again directly under the centre of the top. It is remarkable that even in philosophical treatises of authority, the standing of a top is still vaguely attributed to centrifugal force."

What makes the point "*come?*" The author has described the phenomenon instead of assigning the cause: and the fact itself is so obvious a corollary from the doctrine of centrifugal force, that the authors of the treatises in question have probably thought it unnecessary to illustrate the subject by any detail.

It may be explained by the analogy of planetary movements. Suppose a planet *A* moving in a circle round the common centre of gravity of itself, and of the two suns *B* and *C*, and in an orbit, the plane of which is at right angles to that of the small orbit described by the two suns, (in the capacity of a double star): then taking the sun C when at its greatest distance from the plane of the planet's orbit, the planet *A* will have to the sun *C* the same relation as any atom in the substance of a spinning top has to the centre of attraction in that part of the earth where the top is standing; in other words, the top's axis of rotation will take the plumb-line direction.



For, supposing the top's velocity of rotation to be gradually increased, and its particles gradually to yield to the centrifugal force thus engendered, and to separate from each other, they would describe special orbits in a plane parallel to the rational horizon, but for the earth's attraction, which would gradually draw them into a plane passing through its own centre of gravity; but as the cohesion of the atoms composing the top's substance is stronger than the centrifugal force, part only of the above consequence follows, and each individual atom performs a circle, the plane of which is perpendicular to the direction of the plumb-line,—the mutual cohesion of its atoms effecting in the instance of the top what the sun B does in the case of the planet, i. e. causing the moving bodies to describe circles perpendicular to the line of gravitation, which, therefore, coincides with the axes of their respective orbits. Therefore the axes of rotation in a top will always be vertical when no extraneous force is applied to it, such as originally setting it in motion with its axis inclined to the horizon, or inclined to the plane on which it whirls, so much as to bring that into contact with the side of the peg; and even when such force is applied, the centrifugal force gradually brings the axis of rotation into the vertical position. The subject does not deserve such a detail. D. B.

VIII. *Table shewing the market price of Grain &c. in lower Bengal from the year* 1700 *to* 1813, *extracted from authentic documents of one month in each year, for which, generally, the month of August was selected. Drawn up by* G. Herklots, *Esq. Fiscal of Chinsurah.*

The following Table will, we doubt not, be received with interest by all who take pleasure in statistic enquiries. We trust it will be followed by many similar communications. The two papers in our last number, with this and some others we have by us, will, we hope, excite those who have the means, to place more of such materials at our disposal. The subject of Indian statistics is untouched.—It is an extensive one, and its importance is equal to its extent.

	1700	1701	1702	1703	1706	1714	1715	1716	1717	1718	1719	1720	1721	1722	1723	1724	1726	1728	1729	1732	1733	1734	1735	1736	1739	1740	1741
Rice, best sort	40	30	40	40	60	80	40	45	40	35	32	40	40	30	32	32	40	22	30	20	20	25	25	24	24	25	24
Do. coarse,	70	40	60	60	90	120	50	55	50	40	38	45	45	35	35	40	45	30	40	35	35	30	30	30	30	32	31
Callaí,	60	60	70	80	80	120	50	60	40	35	35	40	40	37	40	45	50	28	35	30	30	25	25	24	24	25	23
Dál,	60	50	50	60	80	80	40	45	40	35½	30	32	30	30	30	32½	30	30	40	35	35	30	30	30	25	24	25
Bút gram,	70	50	70	60	80	130	70	70	50	45	40	40	40	40	50	55	50	30	40	45	45	30	30	30	28	30	27
Wheat,	30	30	50	50	80	90	45	45	45	35	22	30	25	25	25	27½	22	18	20	30	30	20	20	20	20	22	26
Ghí,	10	8½	7	13	6	5¼	6½	8	7¾	8¾	8½	8	8½	8¾	8¾	8¾	9	8½	8	10		10½	10½	10	10¾	10¾	10½
Mustard Oil,	4	3¼	3	5½	2¾	3½	2½	2½	4¼	6	4½	3¾	3¾	3½	3	3	6½	6¼	6½	6		6¼	6½	5	4¾	4¾	4½
Cowries,	32	32	32	32	34	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32		32	32	32	32	32	34

	1742	1744	1745	1746	1747	1748	1750	1751	1752	1753	1754	1755	1756	1757	1758	1759	1762	1763	1764	1766	1769	1770	1771	1772	1773	1774	1775
Rice, best sort	20	13	14	28	18	26	40	30	10	18	22	21	23	30	30	28	26	28	26	14	10	3	19	23	29	28	23
Do. coarse,	25	16	17	34	22	32	50	38	11	21	32	33	40	42	37	31	28	25	31	24	11	3¼	24	34	31	32	27
Callaí,	18	15	16	36	22	27	35	39	17	13	22	18	32	46	30	21½	38	28	27½	18½	14	4½	17	29	44	35	30
Dál,	20	14	16	40	32	34	45	47	27	22	22	24	42	35	32	16½	35	40	28	18	13	4	15	29	42	40	36
Bút gram,	24	16	20	43	40	40	65	70	20	25	23	30	59	42	28	32½	35	30	32	28½	15	15¼	18	35	50	55	57
Wheat,	20	14	15	22	25	35	28	40	14	20	16	19	34	18	23	24	58	20	26	16	13	4¼	17	32	51	45	36
Ghí,	11½	16¼	15	14	10¾	12	15	16½	15	15¾	14	15½	15	14¼	17½	17½	14¼	14	18½	22½	26	20			17½	16½	15½
Mustard Oil,	4¾	6½	8½	5	5¾	4¼	4¾	9½	8	9	5½	7 6	4¾	7 2	7½	7½	5¾	6	9½	10	12½	14			3	6¼	5½
Cowries,	33	36	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32			32	32	32

	1776	1779	1780	1781	1786	1787	1788	1789	1790	1791	1792	1793	1794	1802	1803	1804	1805	1806	1807	1808	1809	1810	1811	1812	1813
Rice, best sort	24	17½	20	26	24	22	12	17	18	16	17	17	26	35	34½	32	40	35	22½	33	32	26½	28	34	34
Do. coarse,	30	25	30	38	25	25½	14	24	24	20	24	29	37	38	40	42	50	40	26½	40	35	32	30	45	40
Callaí,	37	20	25	30	28	34	16	25	24	20	29	32	42	38	53	53	35	42	45	42	33	34	22	33	35
Dál,	36	14¼	17	32	36	27	23	27	40	30	25	29	54												
Bút gram,	42	42	45	55	44	40	30	50	90	48	36	48	82	58	40	40	32	49	49	39	32	29	34	58	32
Wheat,	40	40	22	28	24	30	19	24	25	22	26	22	45	33	44	40	40	53	38	32	34	26½	24½	45	34
Ghí,	12½	12¼	19	19	18	18	16½	17½	17½																
Mustard Oil,	5¼	5½	10½	9	13½	12	12½	12	12½																
Cowries,	32	32	32	32	67	66	74	72	68																

The first 6 articles give the number of seers per Rupee; the *Ghí* and Mustard Oil, the price per maund in Rupees; and the Cowries the number of *pans* per R.

VIII. *On the Calculation of Heights from Observations of the Barometer.*

To the Editor of the Gleanings in Science.

Sir,

The tables for calculating differences of level from barometrical observations, such at least as have fallen in my way, being in general spun out to such a length as to render them nearly as troublesome as the direct calculation from the formula; perhaps you may consider the ooe I have oow the pleasure to send you not altogether unworthy of a place in your Gleanings. In the first volume of the Memoirs of the London Astronomical Society, page 299, Professor Littrow has given as concise tables as I recollect having seen any where: but they are adapted to the French measures, and Reaumer's thermometer, and are, therefore, so far inconvenient. The formula he uses is,

$$N = 9436{,}966.\ (1 + {,}00284\ \cos.\ 2\ \phi)\ .\ (1 + {,}0025.\ (t + t')$$

$$H = N.\ \log.\ \frac{b'}{[1 + \cdot 00023\ (T' - T)]\ b}\ \text{in which}$$

b, t, T { express the height of the Barometer Temperature of the Air, and Temperature of the Mercury at the upper station.

b', t', T'—the same at the lower station.

ϕ = Latitude of the place.

H = Difference of level in Toises.

Now, neglecting the factor depeoding on the latitude of the place, as being too small to be worth attending to, except perhaps in the very nicest experiments, and supposing the barometers at the two stations to be in the first instaoce reduced to one and the same temperature, 32° for instance*, we shall have for English feet and Fahrenheit's thermometer, this simple formula.

$$N = 56055 + 67{,}05\ (F + F')$$

$$H = N.\ \log.\frac{b'}{b}$$, or in Logarithms

$$\text{Log. } H = \log.\ N + \log.\ (\log.\ b' - \log.\ b)$$

The following is a table of the Logarithms of N for every probable value of (F+F') the sum of the temperatures of the air at the two stations.

F+F'	Log. N.	Diff. for 1°	F+F	Log. N.	Diff. for 1°.
60°	4,77871	48,2	150°	4,82028	43,8
70	,78353	47,7	160	,82466	43,4
80	,78830	47,2	170	,82900	43,0
90	,79302	46,6	180	,83330	42,5
100	,79768	46,1	190	,83755	42,1
110	,80229	45,7	200	,84176	41,7
120	,80686	45,2	210	,84593	41,4
130	,81138	44,7	220	,85007	
140	,81585	44,3			

An example can hardly be necessary, except to show that the table gives the same results as other methods. Let us take the one given in No. 3 of your Gleaniogs, page 87: the two barometers reduced to the same temperature are:

b'=,7344 log. = 9,86593
b =,5372 log. = 9,730115

log b' = log. b =,13579
log. ,13479 = 9,13287
for F+F' = 105.°6 the table gives 4,80029

Feet 8573·5 = 3,93316

* A table for this correction, of limited extent, however, will be found io Daniel's Meteorological Essays, 2d ed. p. 372. I have extended it so as to be useful to the residents in mountainous regions, and perhaps you may think it worth printing.

which is precisely the height stated to have been found by geometrical methods.

While on the subject of barometers, I cannot help suggesting to those who keep registers, how much better it would be to reduce their observations to some standard temperature (32° for instance) and so record them: with a copy of the table sent herewith suspended near the barometer. This reduction would cost no trouble, no inconsiderable space would be saved in the register, and the observation, whether at the same time or at different places, might then be compared at once. It would be still better if the correction for capillarity were applied also, and this would be no additional trouble whatever, since each observer might incorporate the correction due to his particular instrument on this account, with that due to difference of temperature; one table giving both corrections: the latter, however, is of less consequence, if I am right in supposing that the tubes sent to this country are generally made of nearly the same interior diameter, about two-tenths of an inch. I am, &c. T.

Correction to be applied to the Barometer for expansion of Mercury.

Temp. of mercury.	In. 14	In. 15	In. 16	In. 17	In. 18	In. 19	In. 20	In. 21	In. 22
25°	+ ,010	+ ,011	+ ,011	+ ,01[illegible]	+ ,013	+ ,013	+ ,014	+ ,015	+ ,015
30	,003	,003	,003	,003	,004	,004	,004	,004	,004
35	— ,004	— ,005	— ,005	— ,005	— ,005	— ,006	— ,006	— ,006	— ,007
40	,011	,012	,013	,014	,014	,015	,016	,017	,018
45	,019	,021	,022	,023	,023	,025	,026	,027	,029
50	,025	,027	,029	,031	,032	,034	,036	,038	,040
55	,032	,035	,037	,039	,041	,044	,046	,048	,051
60	,039	,042	,045	,048	,050	,053	,056	,059	,062
65	,046	,050	,053	,056	,059	,063	,066	,069	,073
70	,053	,057	,061	,065	,068	,072	,076	,080	,084
75	,060	,061	,069	,073	,077	,082	,086	,090	,095
80	,067	,072	,077	,082	,086	,091	,096	,101	,106
85	,074	,080	,085	,090	,095	,101	,106	.111	,117
90	,081	,087	,093	,099	,104	,110	,116	,122	,128
95	,088	,195	,101	,107	,113	,120	,126	,132	,139
100	,095	,102	,109	,116	,122	,129	,136	,143	,150

Temp. of mercury.	In. 23	In. 24	In. 25	In. 26	In. 27	In. 28	In. 29	In. 30	In. 31
25°	+ ,016	+ ,017	+ ,018	+ ,018	+ ,019	,020	+ ,020	+ ,021	+ ,022
30	,005	,005	,005	,005	,005	,006	,006	,006	,006
35	— ,007	— ,007	— ,008	— ,008	— ,008	— ,008	— ,009	— ,009	— ,009
40	,018	,019	,020	,021	,022	,022	,023	,024	,025
45	,030	,031	,033	,034	,035	,036	,038	,039	,040
50	,041	,043	,045	,047	,049	,050	,052	,054	,056
55	,053	,055	,058	,060	,062	,064	,067	,069	,071
60	,064	,067	,070	,073	,076	,078	,081	,084	,087
65	,076	,079	,083	,086	,089	,092	,096	,099	,102
70	,087	,091	,095	,099	,103	,106	,110	,114	,118
75	,099	,103	,108	,112	,116	,120	,125	,129	,133
80	,110	,115	,120	,125	,130	,134	,139	,144	,149
85	,122	,127	,133	,138	,143	,148	,154	,159	,164
90	,133	,139	,145	,151	,157	,162	,168	,174	,180
95	,145	,151	,158	,164	,170	,176	,183	,189	,195
100	,156	,163	,170	,177	,184	,190	,197	,204	,211

Note by the Editor.

We have substituted a more correct table for that of our correspondent, which being taken from Daniel was affected by the error noticed in our last number, p. 323. The error in the extreme case was ,028 nearly 30 ft. in altitude. The expansion of the mercury had been diminished by the mean value of the dilatation of glass, whereas nothing can be more evident than that the latter should be neglected.

Our readers may see the reason of this in the paper of MM. Dulong and Petit, published in the present number. We have adopted their value of the expansion of mercury as stated p. 365 to be $\frac{1}{5550}$ for the centrigade thermometer which is equal to $\frac{1}{9990}$ for every degree of Fahrenheit, or in decimals ,0001001. There is a correction however (if we do not mistake the matter) which neither Mr. Daniel, nor any of the writers on barometric calculation have introduced into their formulæ. It is the correction which should be made for the varying temperature of the brass scale. Supposing it to have been adjusted at 60° which is I believe the temperature adhered to by English makers;—it is evident that at a temperature of 96° the brass scale has undergone a change of 36°. It may be said that this correction is small,—yet it is twice as great as that of glass, which Mr. Daniel has erroneously introduced, while this one has been altogether neglected. The French standards are graduated at 32°. Here, therefore, the difference would be still greater.

IX. *Corrections of, and Additions to, an Article on Ampullaria, in the Second Number of the Gleanings.*

To the Editor of Gleanings in Science.

Sir,

I request that you will have the goodness to give an early insertion to the following correction of an article which appeared in the 2d. Number of the Gleanings of Science, for February, 1829. In that number I stated, that no notice on the subject of the genus *Ampullaria* had, to my knowledge, yet appeared; whereas in the 12th number of the Zoological Journal, for April, 1828, (which, from some mistake, only reached me on the 16th instant,) I find a minute and interesting account of the animal, from the pen of the Rev. Lansdown Guilding, of St. Vincent's; setting my rough account (which was drawn up in December, 1828, from notes made in October 1827) completely in the shade. It is however gratifying to me to find that, as far as my description goes, it is supported in its more obvious points by the more finished account of my fellow-labourer; and that the *Ampullaria* of the East coincides with its brethren of the western hemisphere; the pedunculated eyes, situated at the exterior base of the tentacula, and the subretractile tentaculiform genæ*, being alike in both shells.

In the same paper Mr. Guilding has separated from the genus *Ampullaria* that of *Pachystoma*, under which it appears that our eastern *Ampullaria* should be ranged. Deep, however, as Mr. Guilding's knowledge of the subject is, I should hesitate to adopt the new genus, if on a more minute examination, and a comparison of the animal of our *Pachystomata* with Mr. Guilding's *Ampullariæ*, it appears that there is no essential difference in the characters of the animals. In this case, *Pachystoma* and *Ampullaria*, viewed separately, can hardly be looked upon as divisions of equal value with, or even proximate importance to *Paludina*, which Mr. Guilding places as the first genus of his family of *Ampullariadæ*, and which differs so materially from these two genera united. The mere thickening and partial reflection of the peristome of the shell, and the substitution of shell for horn in the operculum, do not appear to me to afford sufficient generic distinction: the shell *seldom* exhibits the first character until the animal has attained its full growth, and the operculum is often not preserved with the shell; so that it would be difficult to point out the place of a specimen in many cases.

In my notice on *Ampullaria* I mentioned that I had *Paludinæ* with calcareous as well as horny opercula. A curious analogical resemblance is exhibited by the former to *Pachystoma*, the peristome being thickened and subreflected as in that genus. Should the latter genus stand on the difference observable in the shell, a new one will be also required for these *Paludinæ*, the animal of which, I have satisfied myself by comparison with *Paludinæ* with horny opercula, to be essentially the same.

The Rev. Lansdown Guilding combats the opinion, that all the *Trachelipoda* are devoid of eyes, of which some of the land *Pulmonifera* undoubtedly are; and instances

* These are so remarkably like tentacula, that I set them down as such in my Journal on my first examination of the animal at Mirzapore, in October 1826. It was not until I had an opportunity of inspecting them more leisurely, a year afterwards, that I became aware of my mistake.

in support of his opinion on the *Strombidæ*, which he ascertained by dissection to be endowed with true eyes.

In *Ampullaria* these organs occupy the summits of the pedicles, and are not, as in *Helix*, mere specks on the back of the capitulum; they have a large central, circular black spot, surrounded by a whitish surface resembling the cornea. I found it difficult to persuade myself that the wary animal, which, at my slightest cautious movement to inspect it, shewed its sense of my presence, by quickly withdrawing itself, and whose staring eyes presented themselves so conspicuously, was not endowed with true vision.

Bundélkhand, October 28th, 1829. I am, Sir,

Your obdt. Servant,

W. H. B.

Errata in the Notice on *Ampullaria*, p. 52. & 53.

passim *For* " tentaculæ" *read* " tentacula"

P. 52. *After* "Melania" *dele* " and"

P. S. *A. reflexa* of Swainson appears to belong to the division with a horny operculum, and, therefore, cannot be identical with either of the species described by me. The only notice I had seen of that shell was in the Philosophical Magazine, vol. lxi.

Obs. Ampullaria is by the natives called *Talha,* and the shell is by them supposed to be efficacious in medicine, being used as a vessel to contain applications for the eyes, for which purpose it is sold in the Bazár of Allahabad. W. H. B.

X. *Miscellaneous Notices.*

1. *Further Notice of the Beetle described in the 8th No. of Gleanings.*

Among the miscellaneous notices in the 8th No. of Gleanings in Science, T.J. P. gives an account of an acid secretion from a species of beetle, which from the characters given by him, appears to be *Anthia Sexguttata* of Lamarck, belonging to the stirps *Geodephaga* of MacLeay, and to the family of *Brachinidæ.*

The natives in these provinces, term this beetle, from its conspicuous markings, *Cheh-gundeh,* and consider it venomous. It is probable that its powerful mandibles would easily penetrate the skin, and the caustic liquor mentioned by T. J. P. as ejected from its mouth, might, if it found its way into the wound, produce much pain and inflammation.

In the rainy season, this insect may be observed climbing trees in pursuit of the insects which form its prey, like the cognate genus *Calosoma* in Europe; during the hot winds it inhabits rat-holes, in company with a large species of *Blaps,* from which it is easily ejected by filling them with water.

I have observed the pungent smell alluded to by T. J. P. when the insect has been confined under a glass, and has been making violent efforts to escape. On such occasions the inside of the glass and the surface on which it was placed, were wet with a colourless liquor, mixed with a little brownish dirt, which appeared to be excrement. I cannot, however, give the vapour the praise of resembling aromatic vinegar; its odour was pungent and spicy, but unpleasant.

Several allied species belonging to the genus *Brachinus,* have the faculty of darting out, on being pursued or disturbed, a pungent visible vapour from the anus, with a crepitating report. This vapour blackens the hands, and reddens blue paper. Mr. Ritchie found it to be neither acid nor alkali, and soluble in alcohol and water. I have several species of *Brachinus* in my collection, taken in this part of the country. I have often caused them to crepitate by disturbing them, but I never perceived the visible vapour which is emitted by the European species. A friend of mine once observed a vapour emitted by a small beetle, with a crepitating sound, which I doubt not belonged to this genus. Many other beetles possess acrid juices which they eject from the mouth when disturbed. Some interesting particulars connected with this property will be found in Kirby's and Spence's Introduction to Entomology, vol. ii. page 246, in the chapter concerning the means of defence of insects; and in vol. 4. page 143, &c. in the chapter on secretions.

Bundélkhand. W. H. B.

Note. T. J. P. has omitted to notice in his description, the singular position of the trochanter in the hinder pair of legs, which characterizes all the families which have been formed from the genus *Carabus* of Linné; or to mention the number of joints in the tarsi, the knowledge of which is necessary to discover the place of an insect in the latest continental systems. The arrangement founded on the latter character is artificial, and has been proved to be founded in error by Mr. MacLeay: yet until a complete system on the new methud appears, recourse must be had to it.

2. *Manner of Hunting in District Daínajpúr.*

3. The following very singular method of killing wild animals, described by Dr. Buchanan, as practised by the people in the Daínajpúr district, may be interesting to some of our readers. It is quite new to us, and may be su to others.

"There are many deer in the vicinity of the Mohanunda, and of the lower parts of the Punabhobka and Tanggon; but scarcely any where else. They are so numerous among the long reeds and woods of these parts, that they are a nuisance, by destroying the crops. I saw none but the *C. axis* and *C. porcinus*; and in this class of animals the natives apply specific names so indefinitely that I cannot follow them as guides. There are no hunters who make a profession of killing these deer, and of carrying the venison to market, although no one would hinder them. The neighbouring farmers keep nets, and occasionally hunt, partly to save their crops, and partly to procure animal food. I went twice with them, and on one occasion took two deer, and on the other had no sport. I was a good deal surprised at the method. The net is made of whip-cord and may be about six feet wide, and each farmer brings a piece with him of 30 or 40 feet in length. All the pieces having been joined, they are set in a straight line, and are supported on one edge by poles which incline towards the direction from whence the game is expected to come, and lean on forked sticks; some persons then remain behind the poles with lances to kill or seize the game which comes into the net, before it can disentangle itself. The others advance from the net in a line parallel to it, and beat the grass and bushes and make a great noise. I expected, when they had set the net, that they would have gone in silence to a distance, and have roused the game as they advanced towards the net; but this they assured me would not answer; for the game always runs backwards in the direction whence the noise advances upon it. The game taken in these nets consists of wild hogs, deer, and tigers."

3. *Diminution of Intensity of Sound.*

To the Editor of the Gleanings.

Sir,

The insertion of the following extract from an article on Sound in the Encyclopedia Metropolitana, in your useful publication, may procure for the author of that article the observations he wishes for, from some of our scientific travellers in the hills.

I am,

Yours faithfully, I. S.

"The dimiution of the intensity of sound in a rarefied atmosphere is a familiar phenomenon to those who are accustomed to ascend very high mountains. The deep silence of those elevated regions has a physical cause, independent of their habitual solitude. Saussure relates, that a pistol fired on the summit of Mont Blanc, produced no greater report than a little Indian cracker, (petit petard de Chine) would have done in a room. We have ourselves had occasion to notice the comparatively small extent to which the voice can be heard, at an altitude of upwards of 13,000 ft. on Monte Rusa. Observatiuns on this point, in the elevated passes of the Himmaláya mountains, would be very interesting. They should be made by the explosions of a small detonating pistol, loaded with a constant charge, and the distances should be measured; for the voice loses much of its force from the diminution of muscular energy in rarefied air, and distances are extravagantly underrated by estimation in such situatiuns." *Ency. Metr.*

4. *Jasper from the Cásia Hills.*

A very beautiful specimen of jasper, with colour delineations, equal, if not superior to those of the most admired marbles, has been shown us as a production of the Cásia hills. The substance, there is no question, would be considered in Europe, valuable as a material for small fancy articles. The piece we saw had been polished by one of our stone masons, but a substance like jasper requires more powerful means of grinding and polishing than are applied to marble. The effect, however, was quite sufficient to show the beauty of the stone. The gentleman who forwarded it, mentions that it is found "projecting from the ordinary sandstone, about 1500 feet from the summit of the hill between Momlú and Lykúnchaú," and as far as he can learn no where else. It may be had, he believes, in large pieces.

The specimen in question has been placed in the hands of Messrs. Hamilton and Co. for the purpose of being cut and polished.

5. *Population of Górucpúr.*

The town or city of Górucpúr appears, by a statement prepared by one of the revenue surveyors, to contain 7237 houses, of which only 208 are brick. The population is 40023; of which 24766 are Hindús, 12973 males and 11793 females; and 15257 Musulmáns, 7819 males and 7441 females. The males are to the females as 110 to 100 amongst the Hindús, and as 105 to 100 amongst the Musulmáns. On the whole population as 108 to 100. The average of inhabitants for each house is 5,53. The proportion for Hindús is 5,57, for Musulmáns 5,47.

The cattle belonging to the town consists of 2700 bullocks, of which 1101 belong to 410 ploughs, the remainder to 233 carts, 315 ponies, 2890 cows and buffaloes, 7 elephants, and 420 horses.

6. *Quantity of Water raised by the* Dénci.

SIR,

In pursuance of the suggestions affixed to my letter inserted in the 9th No. of your Gleanings, I have endeavored to ascertain, as well as I am able, though I fear but incorrectly, the quantity of water which two men are capable of raising by a lever, from a well fifteen feet deep; and the result is, that two men will work alternately 10 hours, make five dips of the bucket in a minute, and raise about 10 seers of water at each dip, from a depth of 15 feet; the bucket, made of iron, weighing six seers.

The great inconvenience in drawing water by the lever is, the liability of the water, should the distance for it to be conveyed be great, being absorbed by the soil before it reaches its destination; unless worked with a rapidity which the natives are little inclined to perform, but for short periods. Hence, except for horticultural purposes, where labour is so fully remunerated by the value of the produce, this mode of irrigation is seldom resorted to; but where the drain is made of masonry, and the water conveyed to reservoirs, as at indigo factories, the mode might be adopted with cheapness and facility, in situations on the banks of rivers or tanks, where the height to be raised does not exceed 15 feet.

I remain,

Your obedt. Servt.

X. Y. Z.

XI.—*Proceedings of Societies.*

1.—ASIATIC SOCIETY.

A meeting was held on the 8th November; Honorable Sir Charles Grey in the chair. Dr. Smith and Mr. Stewart, were elected Members of the Society. The following letters were then read:

From Mr. Cosmo DeKöros, declining the monthly allowance offered him by the Society, and any assistance until his visit to Calcutta.

From Dr. Vos, presenting the 8th volume of the Transactions of the Batavian Society.

From Mr. Robertson, presenting a copy of his Translation of Roostum and Sohrab.

From Mr. Vaughan, Secretary to the American Philosophical Society, announcing the despatch of Nos. 2 to 9, of the 3d volume of the Society's Transactions and Elliot's Botany.

From Mr. H. T. Prinsep, forwarding a copy of the Astronomical Observations at the Madras Observatory;—also one announcing the remission of duty on 100 copies of the proceedings of the Physical Class of the Society, despatched for England.

From Babu Sib Chander Das, presenting a copy of the Hanumán Cheritra, an Astrological work.

The Transactious of the Royal Society of Edinburgh, vol. xi. part 1.—and of the Geological Society, 2d series, part 3d, of vol. ii. were presented by these Societies.

Rhodes' Work on the Religion and Mythology of the Hindoos, was presented by the Author.

The Meteorological Register, for August and September, was presented by Captain Herbert.

An account of the Jains, by the Secretary, was read in continuation of his account of the Hindu Sects.

The following Donations were received for the Museum :

A Malay Kreese, from Babu Mathuranáth Mallic.

The jaw of a Shark and snout of a Sword Fish, &c. from Dr. Burlini.

Two slabs of Sand-stone, from near Gwalior, from Mr. Beatson.

A number of Musical Instruments used by the natives of India, from Babu Rám-Comal Sén.

2. Medical and Physical Society.

A meeting was held on the 3d October, the president in the chair. Mr. H. Mackenzie was elected a member of the Society.

A letter was read from Mr. Rind, requesting that his name might be withdrawn from the list of members of the Society, on the same ground as those of others who had recently seceded.

A communication was presented by Mr. Breton, on the flexible tubes prepared in this country from Caoutchouc, with specimens of tubes.

A paper by Dr. Hardie was read,—on the production and effects of Malaria in the valley of Udayapúr.

3. Agricultural and Horticultural Society.

Wednesday, October 21st.
Sir E. Ryan in the Chair.

The following gentlemen were elected members of the Society, His Highness Shams ud Daúla Bahádur, Nawáb of Dháka ; Máha *Rája* Meterjít Sinh Bahádur ; Mr. G. Lamb, Dhaka ; Mr. J. W. Hogg, Mr. Geo. Wood, Mr. T. Beeby, of Calcutta ; Mr. T. A. Shaw, Chittagong ; Mr. R. Neave, Sherghatty ; Mr. J. Watson, Faridpúr ; Mr. D. Hare, Calcutta ; Sir Frederick Hamilton, Benares ; Mr. E. Marjoribanks, Santipore ; Mr. G. W. Trail, Kamaun ; Babu Conni Lál Tacor, Babu Goculnáth Mallic.

On the motion of Sir Edward Ryan, Dr. Carey was requested to accept the office of additional Vice-President of the Society.

Dr. Carey intimated his acceptance of the same, and returned thanks.

The Secretary stated that he had, at the request of the Society at their last meeting, applied to the several printing offices to ascertain for what sum they would engage to print the future Transactions of the Society, in parts of the same size and quality as the volume already published,—he now submitted the result ; upon which he was requested to procure specimens of the paper and printing from each of the offices.

Read a letter from the Proprietors of the Asiatic Lithographic Press, offering (if the Society would supply paper) to print extracts from the works on Agriculture, by Rajah Gunsham Singh, and Mr. Da Costa, (presented to the Society by Mr. Breton,) free of all charge, —provided the work should not be of any great length.

The thanks of the Society were voted on account of this handsome offer.

It was resolved, that the 1st volume of the Society's Transactions lately published, should be immediately translated into Bengalee, and printed.

A letter was read from the Secretary of the Madras Literary Society, acknowledging receipt of the 1st volume of the Agricultural and Horticultural Society, presented to the former.

A letter was read from Dr. Vos, forwarding a set of copies of a work published in Latin, at Batavia, by Dr. Blume, being a Synopsis of Indian Plants ; and which he begged to present to the Society, in the name of the Batavian Society.

Captain Jenkins forwarded, on the part of Captain Watkins, Treatises on the Vine, and Mangoe tree.

Read a letter from Mr. D. C. Smyth of Húgli, accompanying a number of mangoe trees for the Society's Garden, which he had taken great pains to obtain of the finest kinds known in his district.

Read a letter from Dr. Royle, Saharunpore, pointing out the great advantage which would attend the acclimating of foreign fruit trees and vegetable seeds in the garden on the mountains under his charge, if the Society would supply him with such; and offering to return the acclimated produce, and to assist the Society in every way which lay in his power.

Mr. Nathaniel Alexander presented to the Meeting a Paper by Mr. Wilkinson, of London, on Indigo, as a mercantile speculation, and some additions to his own essay on that subject, presented at the last Meeting.

The thanks of the Society were voted to the several donors and writers mentioned.

Sir Robert Colquhoun submitted the report of the Garden Committee; including the resignation of Mr. Mitchel, the gardener, of that situation; which was immediately accepted.

Sir Robert Colquhoun further reported, that the Garden Committee had taken a lease for two years of a piece of ground adjoining the garden, for Agricultural purposes;—a measure which was approved of and confirmed.

It was moved by Mr. Robison, and carried;—that the Garden Committee be requested to prepare, and lay before the Society at its next meeting, a report on the garden, exhibiting the objects and grounds, on which Government had been solicited to bestow it on the Society in 1827;—the rules then drawn up, and approved of, for carrying those objects into effect;—the sums which had since been laid out on the garden;—the objects and plans now pursuing by the Committee; and lastly, a distinct account of the present state of the garden; and how far, in the Committee's opinion, the expectations of Government and the Society have been realized.

The President suggested the propriety of the Society annually nominating a Committee of Papers, consisting of the Office Bearers of the Society, and five ordinary Members, before whom the Secretary should lay all such essays and papers, as had been read before the Society, for the purpose of enabling the Committee to select such as were most fitted for publication in the Transactions of the Society.

It was resolved accordingly, that the following Gentlemen form the Committee for the present year: Mr. Calder, Mr. Hurry, Captain Jenkins, Mr. Minchin, and Sir Robert Colquhoun, Bart.

On the motion of Sir Edward Ryan, Babu Dwarkanáth Tacore, and Radamadab Bonnerjí, were added to the Committee.

It was resolved, that the morning of the second Wednesday in January next, (the 13th) be appointed for the annual show of vegetables, and the distribution of prizes, in the Town Hall; and that the annual election of Office Bearers for the following year, be held on the same evening, and in the same place, after which there should be an Anniversary Dinner, at which, such members might attend as previously gave in their names to Messrs. Gunter and Hooper.

Wednesday, 25th November.

Sir E. Ryan in the Chair.

A special general meeting was held at the Town Hall for the purpose of considering the answer proposed to be given to a letter received from Government.

The following gentlemen were elected members of the Society, Mr. C. T. Glass, Lieut. Col. Frith, Dr. J. Clark, of Munghér, Mr. B. D. Colvin, of Calcutta, Mr. J. Marshman, of Serampore.

Read a letter from the Deputy Secretary to Government, to the Secretary to the Society, dated 20th October last, communicating the desire of the Right Hon. the Governor General in Council to promote the cultivation of Cotton and Tobacco of a superior description, as well as of the improvement of the quality of Raw Silk, and of other articles of raw produce, calculated for the Home Market; as also the disposition of Government to cooperate in such measures and arrangements as may appear likely to conduce to the above end; and requesting the Society to report, for the information of His Lordship in Council, the mode in which it may appear to them the aid of Government can be most usefully given, it being understood that it was not the intention of Government to interfere in any manner with the proceedings of the Society.

The President informed the Meeting, that the above letter had, at his suggestion, been referred, in the first place, to the consideration of the Agricultural Committee, which had, after due consideration, framed the draft of a reply to the Government, and which he now begged might be read, and the sense of the Meeting taken upon it.

The Secretary having read the proposed reply, it was unanimously resolved, that the draft reply now submitted, be approved of, and that the Secretary be requested to write in terms of it to Government.

The President submitted the draft of a circular which he proposed should be translated into French, Spanish, Italian, German, and two, at least, of the native languages of India, and circulated by the Foreign Secretary Mr. Piddington.

The draft circular was approved of by the Meeting.

CIRCULAR.

Sir—The Agricultural and Horticultural Society of India have directed me to request your kind attention to the present circular.

The Society are convinced that the freeest possible exchange of the natural productions of every country will be found in the end most conducive to the prosperity of all: and guided by these principles, they desire to offer both to societies and individuals, in every quarter of the globe, any of the Agricultural and Horticultural products of India, or any information relative thereto, which may be desired, in exchange for such as may be forwarded or communicated to them. It will be most gratifying to the Society if you can point out to them any desiderata which can be supplied from India, or if you can by any means forward to them seeds, plants, useful communications or suggestions. The Society will feel much obliged by your giving every publicity in your power to this communication.

XII.—*Scientific Intelligence.*

NOVELTIES IN SCIENCE.

1. *Analyses of the Tourmaline.*

M. Gmelin has analysed a great many varieties of this mineral; the method adopted is the following:—The mineral reduced to a fine powder is mixed with carbonate of barytes, and strongly heated. The mass is afterwards treated with a sufficient quantity of muriatic acid to dissolve it entirely, and the solution is evaporated upon a sand bath to dryness. M. Gmelin ascertained by direct statement, that at this temperature the quantity of boracic acid volatilized is so minute, that it may be neglected without any sensible error. The silica is obtained in the usual manner, by treating the residuum of evaporation, with water. Carbonate of ammonia is added to the solution, and after filtration and evaporation to dryness, the residuum is gradually heated to low redness. In this manner, no boracic acid can be lost, because it is combined with ammonia. The residuum, after being weighed, is washed with alcohol mixed with a little muriatic acid; the alcohol being separated, is burnt; the operation is repeated until the alcohol does not give a green flame. All the boracic acid, which is combined with the ammonia, is thus obtained. The residuum again heated and dried, and the loss of weight determines the quantity of boracic acid.

Tourmalines are divided by M. Gmelin into three classes, the first of which contains lithia:—I. Red Tourmaline from Râsna in Moronia, sp. gr. 2,96 to 3,02. II. Red Tourmaline from Peru in Siberia; sp. gr. 3,059. III. Celadon-green Tourmaline from Brazil; sp. gr. 3,079.

	I.	II.	III.
Boracic acid,	5,74	4,18	4,59
Silica,	42,13	39,37	39,16
Alumina,	36,43	44,00	40,00
Oxidulous oxide of iron,			5,96
——— manganese,	6,32	5,02	2,14
Lime,	1,20		
Potash,	2,41	1,29	
Lithia,	2,04	2,52	3,59 with Potash.
Volatile matter,	1,31	1,58	1,58
	97,58	97,96	97,02

2d. Tourmalines which contain potash or soda, or both together, without lithia, and without a notable quantity of magnesia. The following are the varieties—I. Black Tourmaline from Bovey in Devonshire, found with quartz and phosphate of lime; sp. gr. 3,246. II. Black Tourmaline from Eibenstock in Saxony; sp. gr. 3,123. III. Green Tourmaline from Chesterfield, North America, sp. gr. 3,102.

	I.	II.	III.
Boracic acid,	4,11	1,89	3,88
Silica,	35,20	33,05	38,80
Alumina,	35·50	38,23	39,61
Oxidulous oxide of iron,	17,86		7,43
Protoxide of iron,		23,86	
——— manganese,	0,43*		2,88
Magnesia,	0,70†		
Lime,	0,55	0,86	
Soda,	2,09	3,17	4,95
Loss in the fire,		0,45	0,78
	96,44	101,51	98,33

* with magnesia, † with manganese,

3d Tourmalines which contain a considerable quantity of magnesia. Four specimens were analyzed: I. Black Tourmaline from Kooringbricha, a province of Westmanland in Sweden; sp. gr. 3,044. II. Black Tourmaline from Rabinstein, n Bavaria; sp. gr. 3,113. III. Black Tourmaline from Greenland; sp. gr. 3,062. IV. Deep brown Tourmaline, from the mica slate of St. Gothard.

	I.	II.	III.	IV.
Boracic acid,	3,83	4,02	3 63	4,18
Silica,	37,65	35,48	38,79	37,81
Alumina,	33,46	34,75	37,19	31,61
Magnesia,	10,98	4,68	5,86	5,99
Oxidulous oxide of iron,	9,38	17,44	5,81	7,77
Oxide of manganese,		1,89	trace.	1,11
Potash, }	2,53	{ 0,48	0,22	1,20
Soda, }		{ 1,75	3,13	
Lime,	0,25	trace.		0,98
Loss in the fire,	0,03		1,86	0,24
	98,11	100,49	96,48	90,89

M. Gmelin is at a loss to what cause to attribute the deficiency in the last analysis. He thinks the tourmaline from St. Gothard should be again examined, especially as the loss in Bucholz's analysis is still greater.

2. *Specific Gravities.*

A communication appears in the Philosophical Magazine, for June last, pointing out what has been long known to every one, who has had occasion to examine them, viz. the little dependence to be placed on the table of specific gravities by Hassenfrantz, given in Thomson's System of Chemistry. The writer also shows that Hassenfrantz was equally erroneous in his calculated specific gravities of various compounds, and gives the true results deduced from the correct formula

$$\frac{(w + w')\, s s'}{w\, s' + w'\, s}$$

in which w, w' are the combining weights and s s' the specific gravities. It is to be remarked that, Hassenfrantz is not the only writer who has made a mistake on this point. In Aikin's valuable Dictionary (Art. ALLOY) the very same mistake is made, as shown by Dr. Ure, who points out the correct formula. This, with the many other gross errors we find every day in books, otherwise of the highest authority, should teach the student to examine and try every thing before he give implicit credence to it, however high the name by which it may come recommended. From the comparisons made by the author of this paper, it appears that in every case the calculated specific gravities are below the true, thus indicating condensation. But we would remark, that no hasty conclusion should be drawn on this subject: the writer does not give the specific gravities of the ingredients of the compound which he has used in his computations, so that we are at a loss to judge how far his results are likely to be correct. It is notorious, how little agreement there is in this determination as made by different experimenters, and were we to take the limits of some of the bodies he has included in his table, we might bring out at pleasure a result higher or lower than the experimental one. The subject is a very interesting one, particularly as connected with the prime equivalents or combining weights of bodies, and we think promises to reward any one who shall devote his time and attention to it.

3. *Expansion of Air.*

In the latter part of the preceding paper, the author takes notice of another error in a work of authority, (Brande's Chemistry.) He might have added Ure's Chemical Dictionary, and possibly many other works. Ure's error is the more extraordiuary, as he professes to give his table instead of one which will be found in Thomson's Chemistry, 6th edition, vol. 1, p. 29 which he represents as useless. Now the real state of the case is, that his own table being erroneous, is worse than useless, while Thomson's, though not so convenient in use as it might be, is strictly correct. The error in Brande was pointed out some years ago in the Annals of Philosophy by a Mr. Biggs. The error is this :—Air has been found to expand from a volume of 1 to 1,375, when heated from 32° to 212°, and again from 1,375 to 1,75 when further heated to 392° increasing by,375 of what its volume at 32° would be for each increase of temperature measured by 180° Fah. It is therefore concluded that for every 1° it expands ,002083 *of what the volume would be at* 32°. The words in Italics being lost sight of, occasions the error. Thus Ure and Brande assume that it expands ,002083 of the volume at 60° which is evidently at variance with the above law.

The rule for reducing the volume of air at a given temperature to what it would be at another, is as follows. Multiply the given volume by 448, increased by the resulting or new temperature, and divide by 448, increased by the given temperature, the quotient is the resulting volume. Or in symbols,

$$V'=V.\frac{440 \times T'}{448 + T.}$$

The table in Thomson's Chemistry before referred to, will furnish the proportionate volumes at different temperatures, from which, by a simple statement of the rule of three, the resulting volume may be found.

4. *On the use of Plumbago (Graphite,) instead of Oil, in Chronometers.*

In vol. xvi. of the Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce, appears an account by Mr. L. Herbert, of his method of applying plumbago to the pivots of the works in chronometers in lieu of oil, to which it is known there are many serious objections. Mr. Herbert obtained the gold medal for his discovery. His method of applying it, is as follows: Having obtained some of the finest plumbago or graphite, (as it should rather be called) from Mr. Langdon, the celebrated manufacturer of pencils, he pounded it in a metal mortar till it became so fine as to feel greasy to the touch. It was then diffused in water. Part sunk down and part swam upon the top in a kind of film. This was taken off carefully with the blade of a knife and set by. The same operation was performed with another portion, the film being set by for use, and the operation continually repeated until a sufficiency was obtained. The fine powder collected, is now to be bruised with the back of a spoon, and again diffused in water; if pure, there will be now no sediment, the whole floating on the surface. The washing is to be continued till this result is obtained, and then you may be sure the graphite is pure. Some pure alcohol is now to be poured into a glass—the pivot dipped in it, and then in the powdered graphite. Then put some of the powder into the pivot holes by rubbing them with a little taken on the finger till even with the plate, and the hole appears filled. Now insert the pivot, and turn it with the hand several times. Repeat the whole operation two or three times. In the case of a clock, there was difficulty in making it adhere to the jewelled pallets of the escapement, but this was obviated by applying it to the friction planes of the teeth of the swing wheel.

A sidereal clock being thus treated in January 1816, had been cleaned three times without the plumbago (graphite) being renovated. The friction surfaces were merely wiped with a fine piece of muslin, and in 1827, it was found to go as well as ever.

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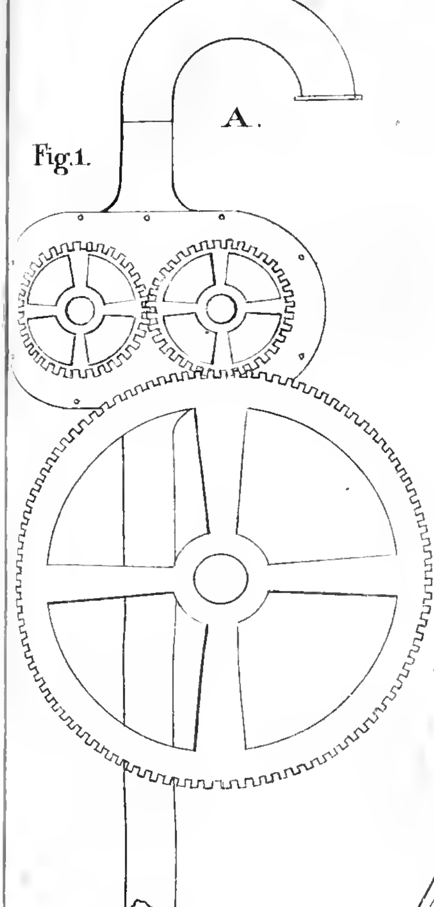
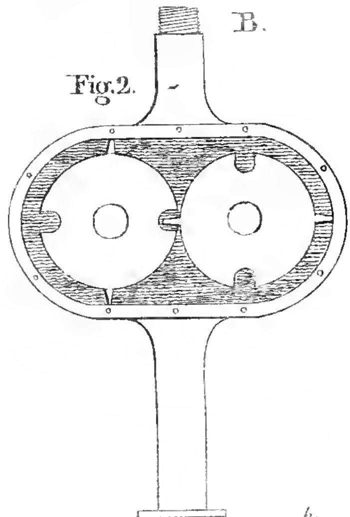
Fig.1.



The shaded part shews the water line.

Fig.2.



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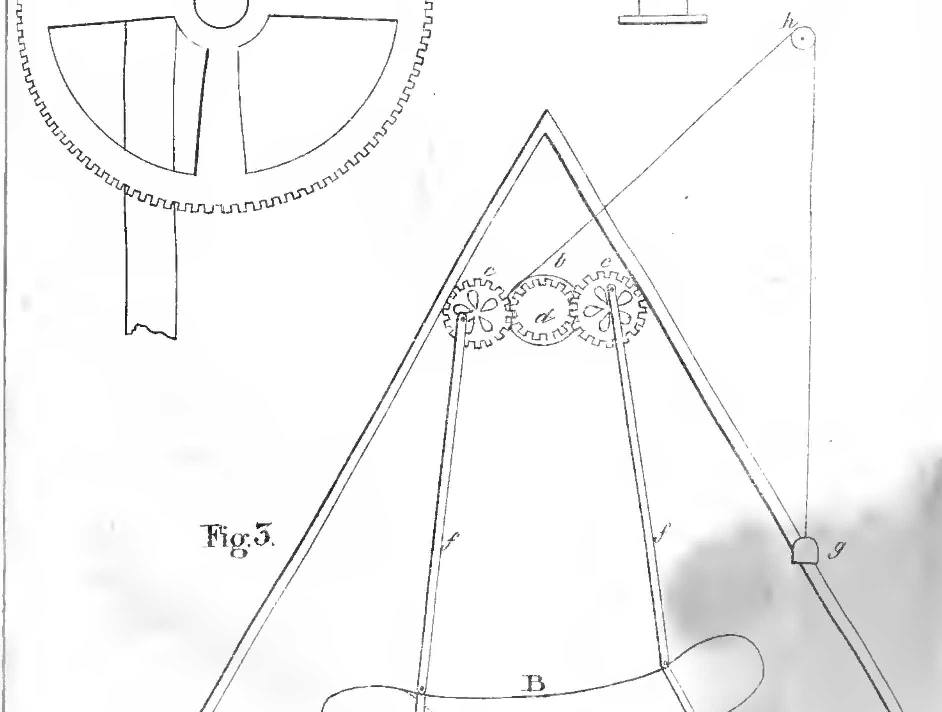

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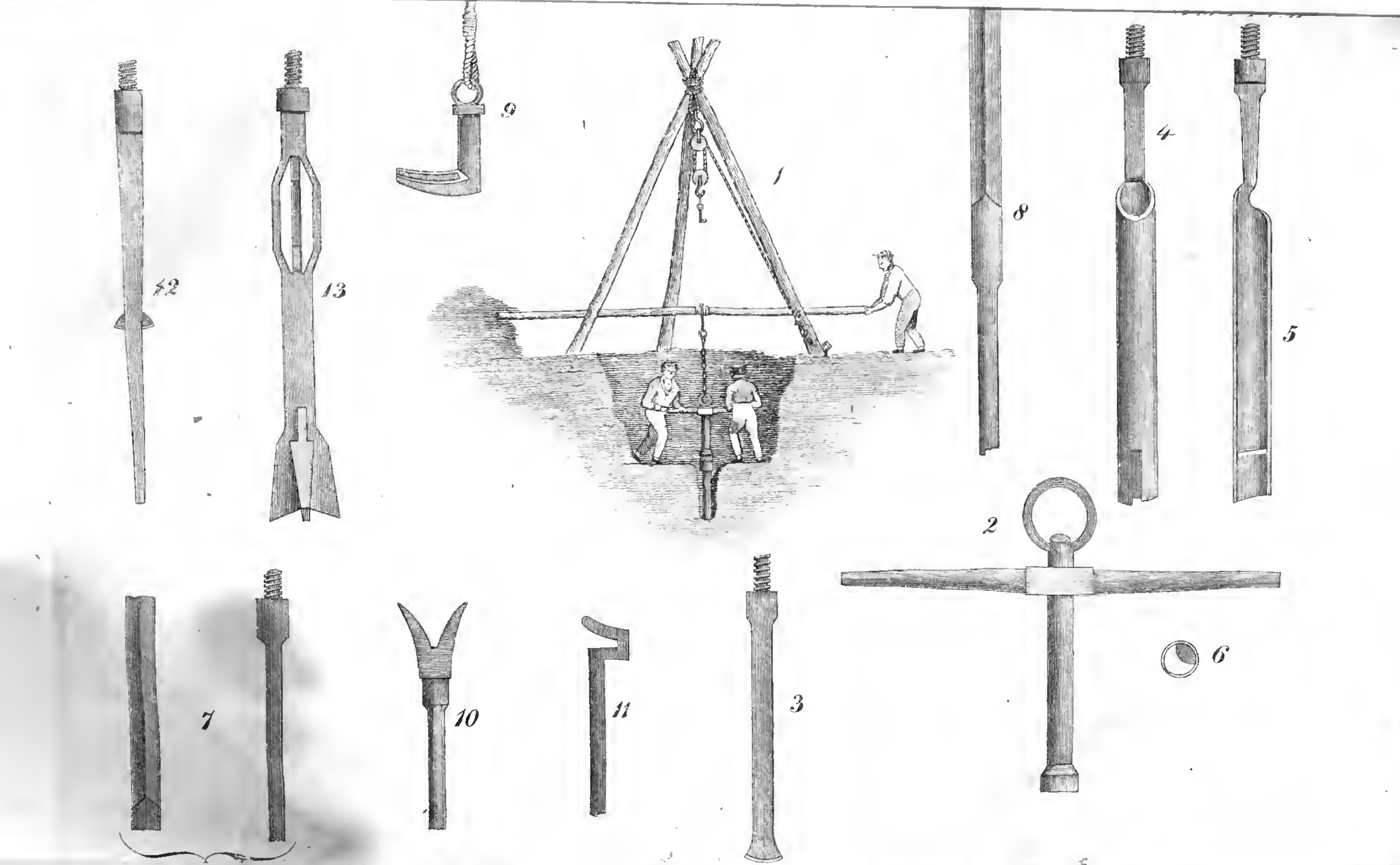

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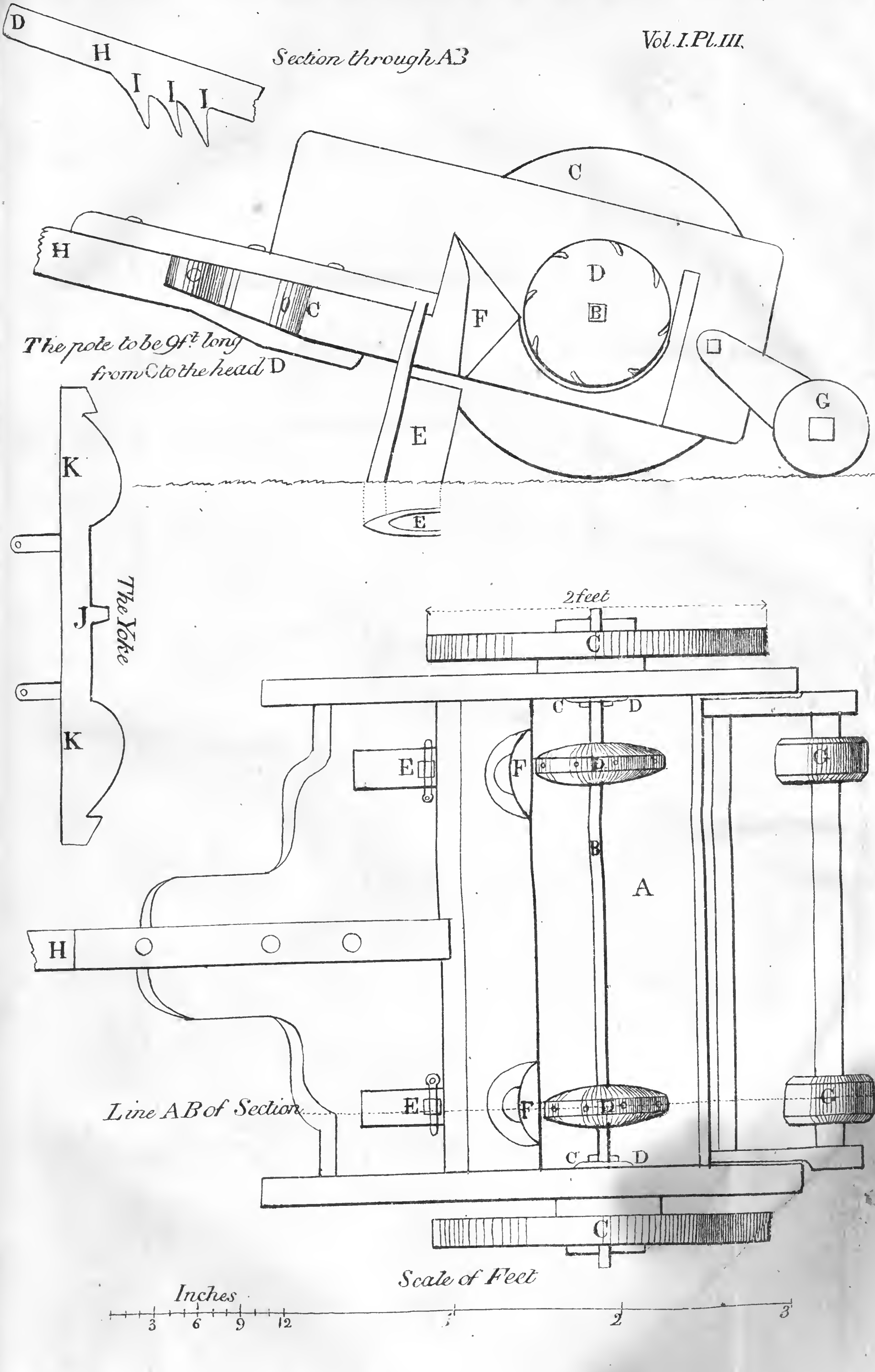

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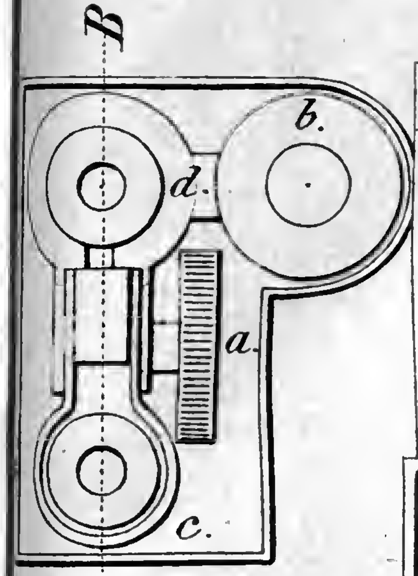

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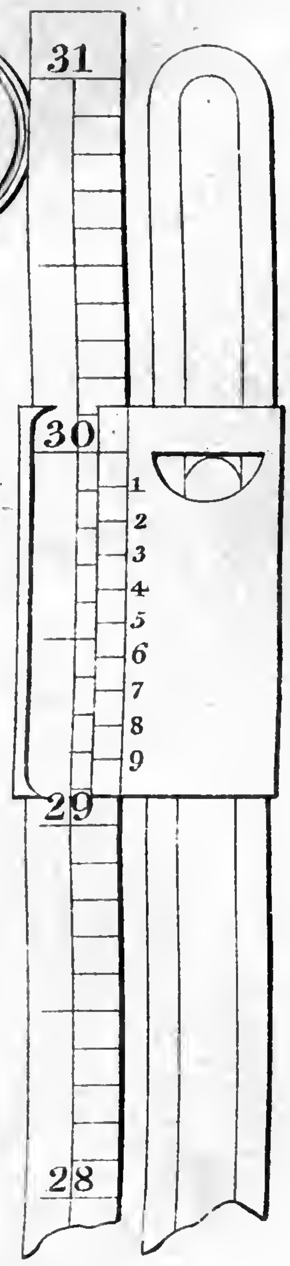

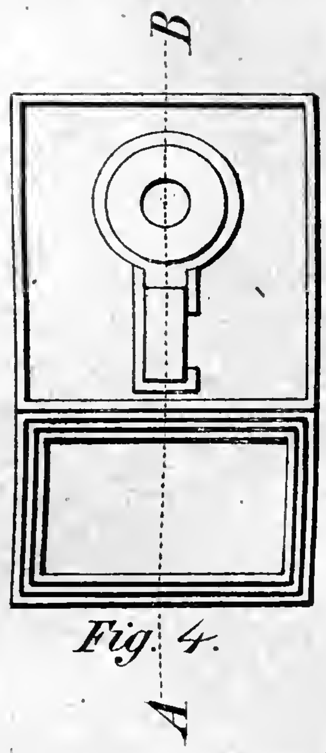

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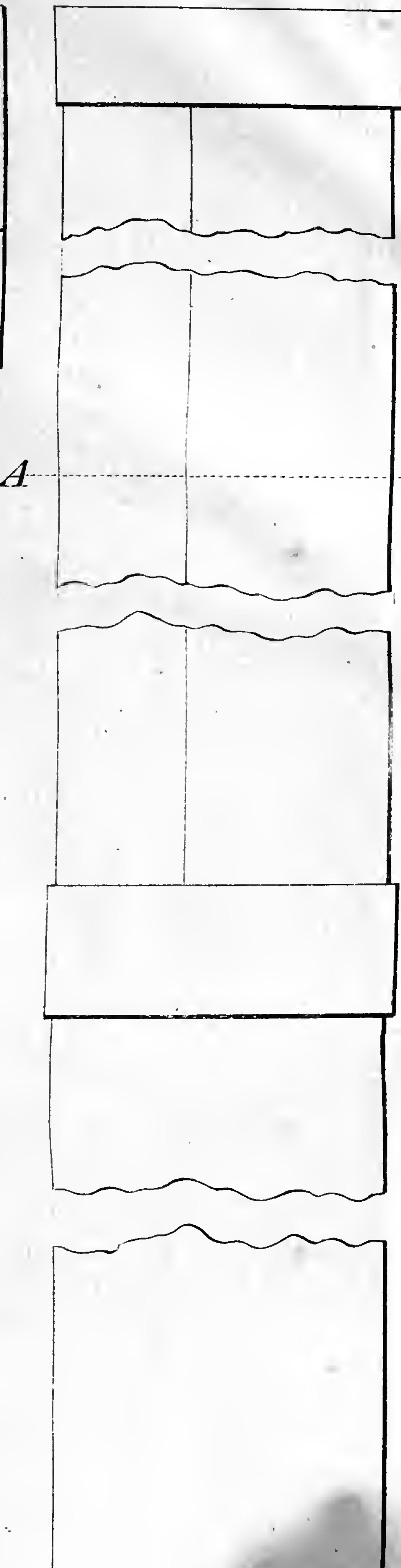

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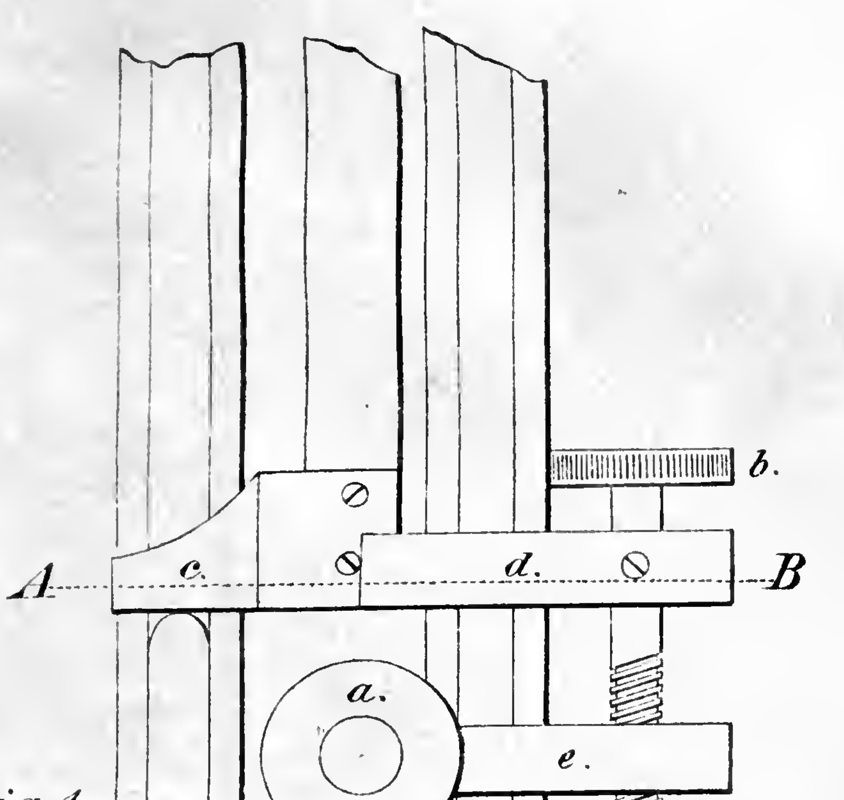

Fig. 1.

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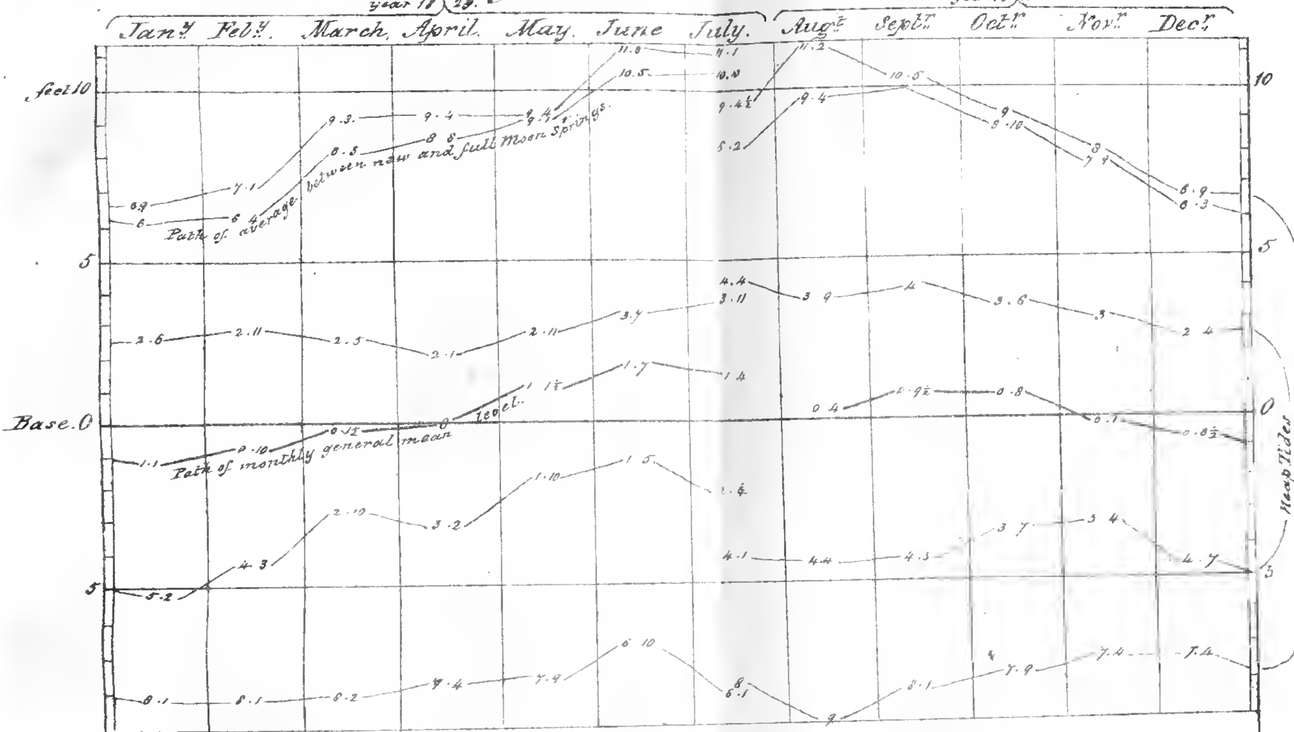

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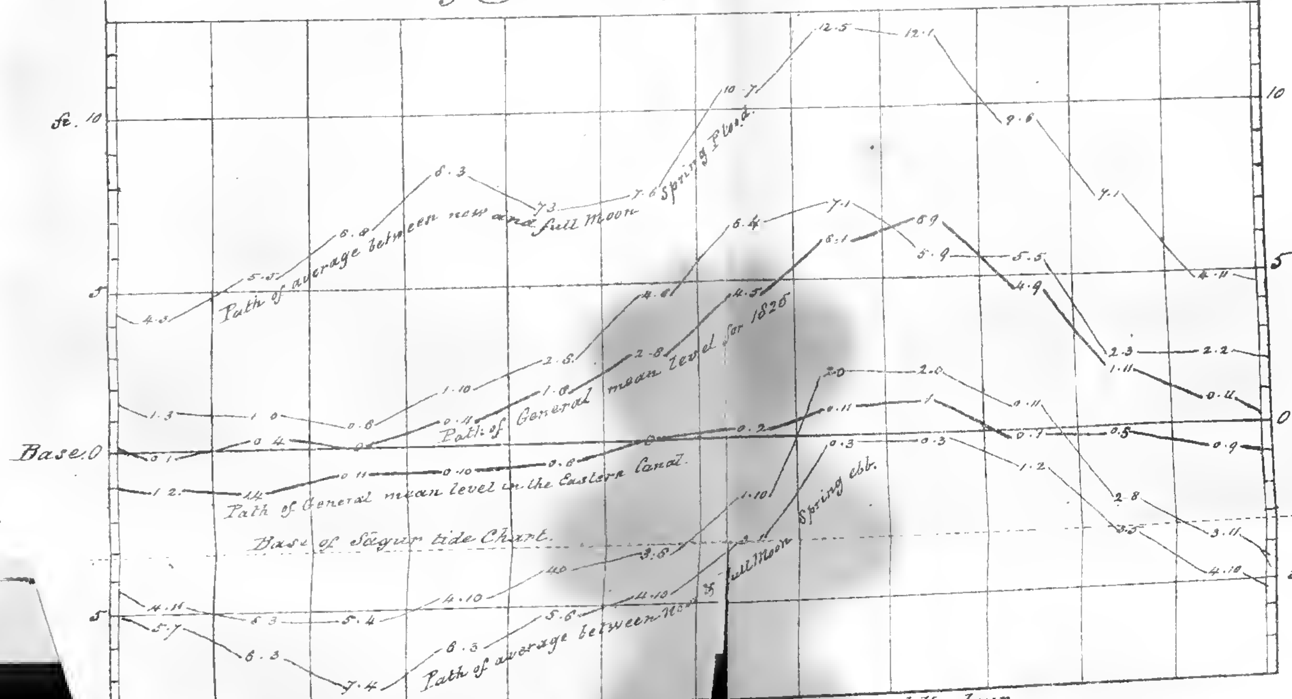

10.11. reputed lowest Water Mark at Diamond Harbour

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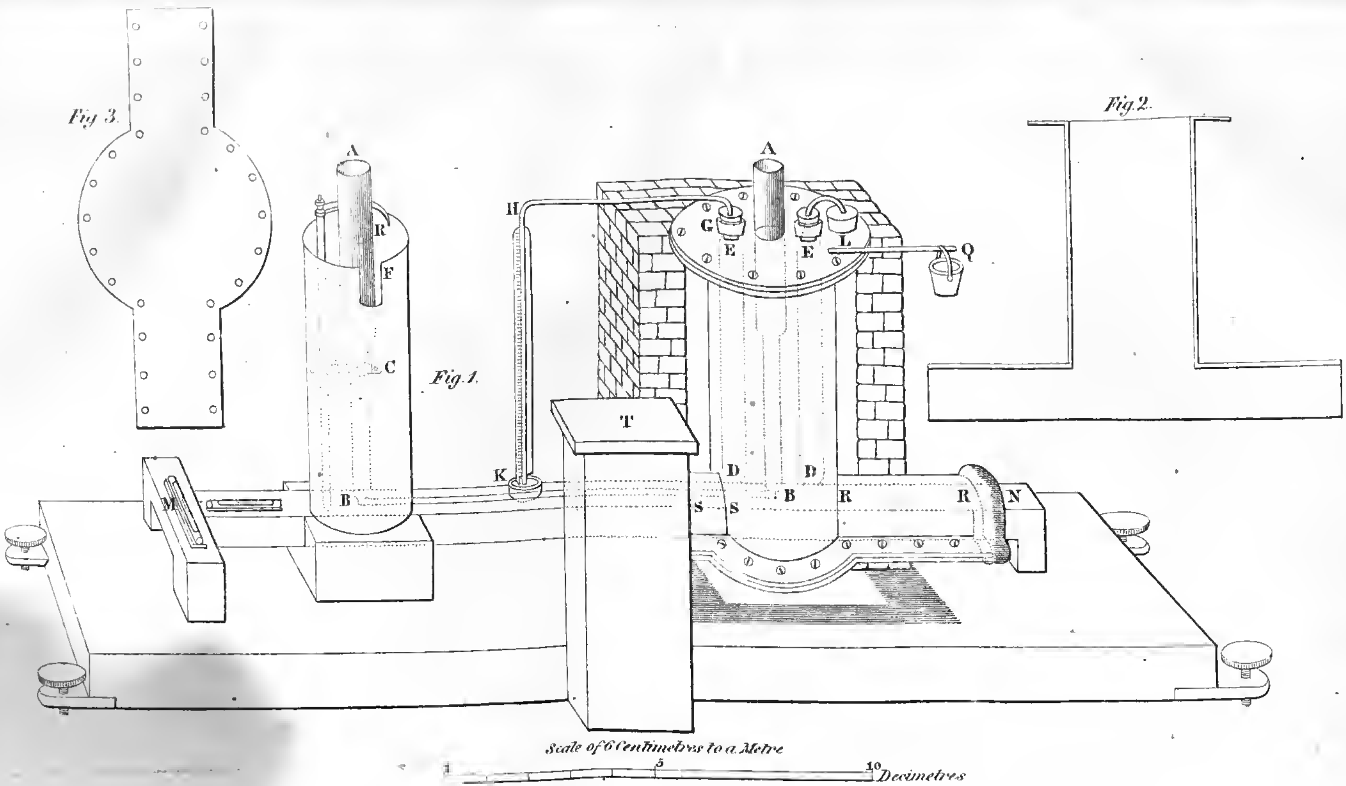
Fig 3.
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Fig. 2.
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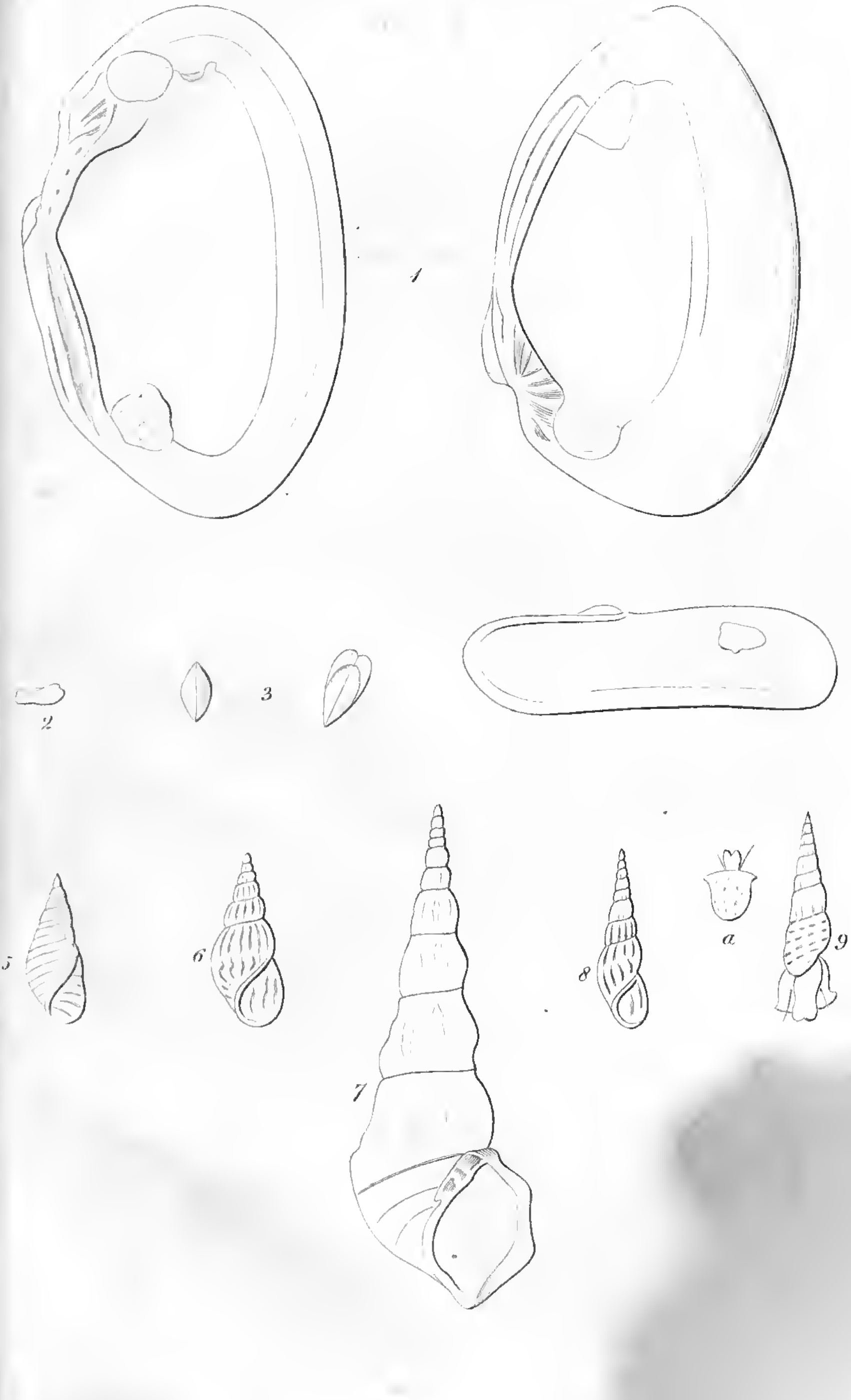

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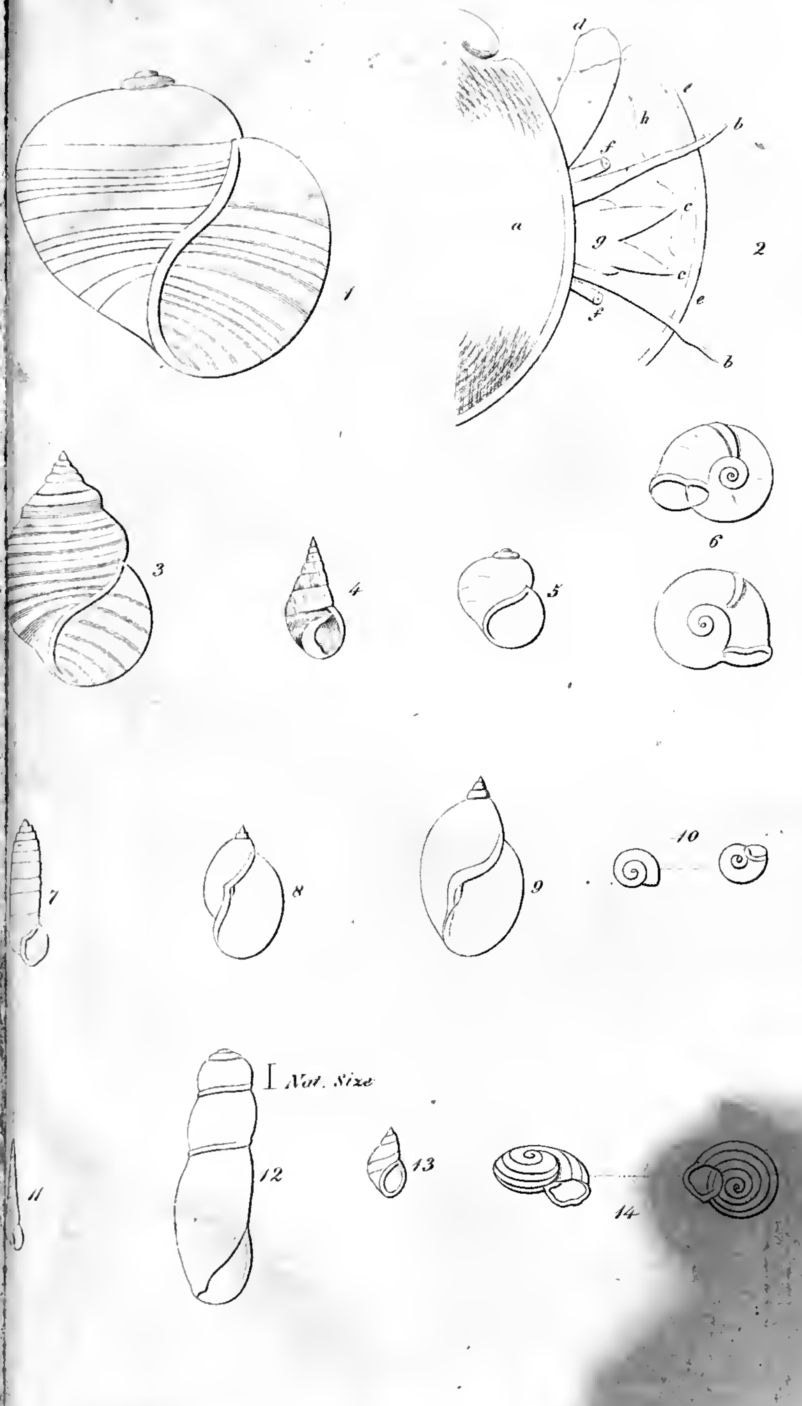

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