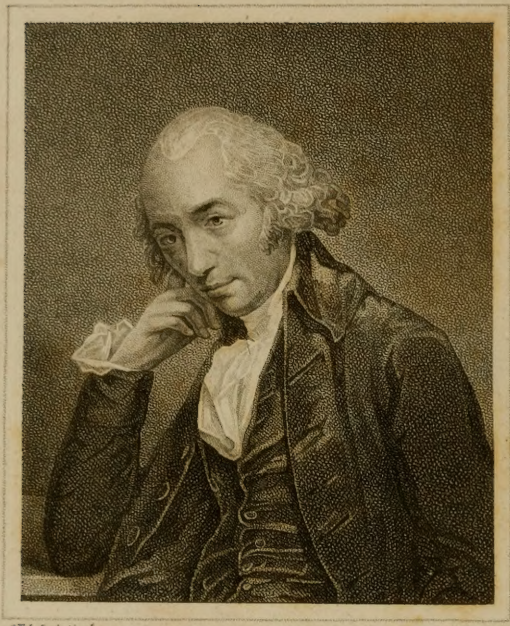

C. F. de Breda pinx.t E. Mackenzie sc.

James Watt, Esq. F.R.S.

Publish'd by A. Tilloch Jan.y 1. 1806

THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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1805.

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TWENTY-SECOND VOLUME.

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THE

THE

PHILOSOPHICAL MAGAZINE.

I. *Extract of two Letters from Captain* Von Krusenstern, *Commander of the Russian Expedition to Japan, dated the Harbour of St. Peter and St. Paul, July* 19, *and August* 20, 1804.

We arrived here on the 15th of July, and are now actively employed in unlading the ship and taking in ballast. Agreeably to our original plan, we ought to have proceeded directly to Japan; but as it appeared to me impossible that the business of the embassy could be terminated soon enough to return in the course of the same year to Kamtchatka, as several months would be necessary only for transporting the presents from Nangasaki to Jedo, I resolved to proceed first to Kamtchatka in order to unload the vessel there, and then set sail for Japan, where we otherwise must have remained the whole winter. The difference of two months later could be of little importance to the embassy; while, on the other hand, if the lading, respecting the value of which I had formed an improper idea, as from 1000 to 2000 per cent. may be gained on some articles, had remained a whole year in the ship, the half of it at least must have been lost; for already some of the articles have been spoiled by dampness. In regard to my voyage from Brazil to Kamtchatka, which took up five months and a half, during which, nine days excepted, we were continually under sail, and which in every respect was exceedingly fortunate, I intended to have transmitted to you a complete journal of it; but as Dr. Espenberg has told me that he proposes to send you one, and as there is reason to expect that his information will be more interesting than mine, which would contain rather nautical than historical events, I am happy to think that you will be informed by him of every thing that distinguishes our expedition from others of a similar kind. The account of our residence at the island of Nukahivah, respecting the nature and inhabitants of which nothing has yet been known in Europe, is the only thing new that you can expect. The

Sandwich islands are too well known for me to regret having been prevented by want of time from touching at them. The changes, however, which have taken place in these islands since the time of Vancouver, and which must be considerable, will be accurately described by capt. Lisianski, as he undertook to remain there at least a fortnight. It appeared to me of some importance to examine Easter Island: the information which Roggewein and La Perouse have given us respecting it (for the Spaniards have published no account of it) proves that it has experienced great changes. I consequently was desirous to ascertain whether the benevolent views of the French voyage of discovery in regard to this island had been accomplished, and therefore resolved to come to anchor there for some time; but the strong north winds rendered this impossible, or at least prevented us from doing it without considerable loss of time,—a sacrifice which in my present situation I could not venture to make. Capt. Lisianski, from whom we separated when we doubled Cape Horn, remained some days in the neighbourhood of it, but without coming to anchor, and without having any communication with the inhabitants, who probably, for want of canoes, of which they had some in the time of Cook and of La Perouse, did not come on board. My passage from this place to Japan will exhibit no variety, for on account of the lateness of the season I must use as much dispatch as possible to arrive in proper time at Nangasaki. But, if circumstances permit, I hope that my return from Japan will prove of some benefit to geography.

Journal of the Voyage from Brazil to Kamtchatka; extracted from a Letter of Dr. ESPENBERG, *dated the Harbour of St. Peter and St. Paul, August* 24, 1804.

'That we were obliged to remain at the island of St. Catharine from the 21st of December to the 4th of February, because the Neva stood in need of two masts, is already known to you. On the 4th of February we hove up our anchors, and as soon as we had got to a sufficient distance from the land we directed our course southwards. On the 25th we saw land at a great distance: it proved to be Cape St. John, the eastern extremity of Staaten Land. This land is exceedingly high, and in consequence of the great distance looked like a cloud. People unacquainted with nautical affairs, to which class our naturalists belong, were extremely sorry that the ship did not approach the land. I myself was at first in the same situation; but the captain assured me that the winds near the land are very changeable,

ble, and that gales and calms often take place. In consequence of these calms, vessels are often in danger of being driven on shore by the currents and breakers, and therefore it was not advisable without necessity, and merely for the sake of gratifying curiosity, to run such a hazard. At Staaten Land the current is remarkably strong. I was told by the captain of an American ship at St. Catharine, that he was once carried by the current through the strait of Le Maire in one night, contrary to his intention, and without knowing it. At one time he saw, to his great terror, the land on both sides; but not long after he found himself again in the open sea. If this be true, he was truly fortunate.

We did not pass through this strait, but sailed to the eastward, around Staaten Land. From St. Catharine to this place nothing remarkable occurred. Between lat. 46° and 53° south, we saw a great number of whales. One night the Neva struck against something, which in all probability was a whale. The greatest southern latitude to which we were obliged to proceed on account of the wind was 60 degrees. Whether we really doubled Cape Horn, or not, in the proper sense of the term, I cannot with certainty affirm. On account of the north-west winds, which blow here so incessantly, and with so much violence, the captain is of opinion that navigators cannot be sure of continuing their voyage with safety in the ocean till they have sailed round the whole of Terra del Fuego. It is well known that capt. Bligh, who advanced so far as 78° west longitude from Greenwich, was obliged to return and steer for the Cape of Good Hope, in order to reach the Sandwich islands. On the 20th of March we were opposite to Cape Victoria and the Straits of Magellan. Cape Horn is not entirely undeserving of its bad name: it was stormy enough, and the land very high. How often we experienced storms I cannot exactly say. The eye becomes accustomed to heavy seas, and, by habit, the howling of the winds ceases to excite alarm. During these storms the heavens were filled with clouds; in the course of one of them we were separated from the Neva, and did not see her again till we reached the Marquesas, where she arrived three days after us. The captain intended to remain some days at anchor at Easter Island, but the wind prevented us; and, as we had resolved to proceed first to Kamtchatka, he was unwilling to lose time to no purpose. We therefore directed our course to the Marquesas.

On the 6th of May, early in the morning, we saw Hood's

Island, discovered by Cook: towards noon we came in sight of the island called by Hergest Rious Island, which belongs to the group of the New Marquesas, and which Ingraham, an American, the first discoverer, called Washington's Island, and Marchand, a month later, *Isle de la Revolution.* The largest of all these islands, that to which we properly steered, and which by Marchand is called *Isle Baux*, but in the language of the natives *Nukahiva,* we saw towards evening.

On the 7th, at noon, we were pretty near to the shore. Our expectation was on the stretch as we approached it, but no canoe appeared; which was rather a disappointment, as, according to the accounts of all navigators, these islanders venture a great way out to sea. The captain suspected that the master of some American ship must have behaved ill to the natives, and that this might serve to account for their timidity. At length two boats were dispatched to explore the bay of Anna Maria, so called by Hergest. When these boats were about a verst distant from us, we observed a canoe making towards them. Our expectation was now at its height: we saw the canoe approach the first boat, and in a few minutes both of them rowed off together: our boat proceeded forwards, and the canoe, which steered for the ship, approached the second boat. We could now plainly perceive that all the people in it were naked: one of them, who was of a somewhat lighter colour, we took to be their chief or king; for we are told by navigators that the higher ranks have a whiter colour. This person stepped into our boat, and the crew of our ship all exclaimed, "The king! the king!" The boat and the canoe then both rowed towards the ship. We now observed something in the water near the canoe, which we at first believed to be an islander swimming; for we knew from books of voyages that they are very expert at this exercise. "A man swimming! a man swimming!" was repeated both in Russian and German. All hurried to the head of the ship, and one climbed up on the shoulders of another. On their nearer approach we discovered to our regret that the excellent swimmer who had afforded us so much satisfaction was an outrigger or cross pole placed over the canoe, which was not above a foot in breadth, to defend it from being injured by the rocks. When the islanders got close to the ship, the light-coloured person climbed up, and, to our astonishment, addressed us in English. We soon found that he was an Englishman, who had already spent five years in the island: he was almost entirely naked, having only a narrow girdle tied round his middle, and was tatooed on the breast. The canoe rowed past the ship, and the men addressed to us a kind of speech. The index finger of their right hand was always stretched out, and they moved it towards us nearly in the same manner as when a person threatens. Mr. Roberts, for such was the Englishman's name, informed us that this motion was an assurance of friendship.

At length one of the natives in the canoe took courage and clambered up the side of the ship: he was the king's brother. He was exceedingly timid: sat down at the Englishman's feet, grasped one of his legs, looked round with great fear, and pressed his face, as if ashamed, against the back part of the Englishman's thigh. He was followed by another. We endeavoured to inspire him with courage; patted him, and called him our *tayo*. We firmly believed that this word signified friend; but this is not the case. M. Fleurieu, the editor of Marchand's Voyage, must therefore pardon me for suspecting that he copied the following passage,—*Vous étes nos amis, et vous nous tuez,* from Bougainville. Their attention was much attracted by our fowls, and some small papajays from Brazil: they squatted down before them and stared at them with their mouths wide open. These people have limbs remarkably pliable. Very old men will often sit down on the ground without ever assisting themselves in the least with their hands. They do not stretch out their legs when they sit, but squat down with their knees bent like young children. This may be in some measure owing to the frequent use of coco-nut oil, with which they besmear their skin to keep it soft and pliable.

Towards noon we came to anchor in Anna Maria Bay, at the distance of about a verst from the nearest shore; which, however, was only a barren rock. The shore on the other side, which was about two versts distant, was covered with beautiful trees, and exhibited a most charming prospect, especially to people who for thirteen weeks had not seen land; for the view of Cape St. John scarcely deserves to be mentioned. We now saw two groups which had the appearance of water fowl, not far from the inhabited part of the coast. As they approached us we perceived them to be natives; and among them were some small children, who sometimes laid hold of their stronger neighbours with one hand, again let them go, and continued swimming alone. They surrounded the ship, and their number gradually increased till the whole place swarmed with them. They seemed highly gratified, kept continually laughing, and did every thing in their power, by gestures and tricks of every kind, to attract our attention. They threw themselves into all sorts of postures, lay sometimes on one side and sometimes on their back, elevated their legs, &c. The women in this respect did not yield to the men; and the object of their pantomime might easily be comprehended. When a piece of a coco-nut was thrown at them from the ship, or when any of the sailors spat down upon one of them, the astonished savage immediately became an object of laughter to the rest. The natives brought us coco-nuts, bread-fruit, and bananas. The two latter articles, at this season of the year, were scarce. When any of them had obtained, as the price of their wares, a small piece of iron, or an old nail, they burst out into an immoderate fit of laughter; the reason of which, as appeared, was, that they thought we had been most egregiously cheated. When they received nails from us, they stuck them into the laps of their ears, the holes in which were so susceptible of extension, that they did not seem to be incommoded by a large rusty nail. Mr. Roberts told us that there was a Frenchman in the island; but he cautioned us against him, as a man of a very bad character.

While we lay at anchor the king or chief of the bay, Tapeka Ketenue, came to us in a canoe, and among his retinue was the Frenchman. As I do not understand English, I was extremely glad; but this Frenchman had so much forgotten his mother tongue, that he was become a real savage. All that he was able to say was,—*Oui moi beaucoup François, Americanish ship, ah dansons la Carmagnole!* He would then laugh like a native of Nukahiva, to whom he had a great resemblance, as not only his body but the greater part of his face was tatooed. He understood English pretty well, as he had been accustomed to converse with the Englishman in this language. He told us, that he had come to the Marquesas in an American ship; that this ship had been on the whale fishery; and that from the whales coco-nut oil was obtained. The person called by the Englishman the king, though that title did not seem at all suited to him, was a man of about forty or fifty years of agé, tatooed over his whole body, except on the palms of the hands and the soles of the feet: he viewed his corpulent person with great satisfaction in the captain's looking-glass, and was highly delighted with the presents which he received.

Next day, the 8th, we went on shore: we were all armed; and the sailors, who had muskets, pistols, and sabres, kept

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the natives in awe. Having landed with great difficulty, on account of the strong surf, the people surrounded us with every token of joy: they ran round us singing and dancing, while Ketenue's paternal uncle, who, however, was always called his father, kept them in order with a long pole, but without ever striking any of them. We entered Ketenue's house and saw his whole family, consisting of his wife and daughters. He then conducted us into another house adjacent, but as it was *taabooed* none of the natives durst follow us. The place on which this house stood was elevated, and paved with stones. We here remained unmolested, and were regaled with the kernel of the coco-nut, and had some of the liquor to drink. Ketenue often paid us a visit, and always received a present: but the natives set so much value on their swine that we could obtain from them only five, which were all of a small size.

On the 9th we received information that a ship was in sight; and on the 10th the Neva arrived. On the 13th a large body of us, for we were accompanied by some of the crew of the Neva, went on shore well armed; and after viewing the *morai*, which is here called *wahitaaloo*, we paid a visit to Roberts and Ketenue. By an accident I lost a great many of my papers, among which were three sheets respecting Nukahiva. This, I know, is no serious loss; but I must make an apology for presenting you only with fragments.

I cannot comprehend why the beauty of the women of the Marquesas has been so much extolled. In regard to the face in general, I shall say nothing; but their persons are altogether ugly. They are small, and of low stature: their arms are proportionally thin, and the lower extremities thick and clumsy. When a female has attained to the full growth, that is to say, the age of fourteen or fifteen, her breasts are quite flaccid, and hang down. Children of the age of nine or ten came on board our ship, many of whom were married. All those who came on board were quite naked. Some of them had a cord tied round their middle, from which were suspended two leaves, one before and the other behind. On shore we saw several who had fastened round them a piece of cloth made from the bark of the paper mulberry tree; and others were painted yellow with the juice of the curcuma root. They were not much tatooed, and only on the arms and shoulders, with some transverse strokes above the lips.

The males are very fine men, of a good stature, and have well

well proportioned limbs. Though they do not exhibit prominent and athletic muscles, and their arms are rather like those of a well made woman, they gave proofs of very great strength. They ornament themselves in the most romantic manner. Many of them have a circle of feathers around their head, or of swine's teeth strung on a cord, on their toes and fingers; feathers, or small bunches of human hair, and other kinds of ornaments, around their neck. Most of them are more or less tatooed. The figures are regular, and have each a determinate name: some of them are attended with particular privileges; thus, for example, the Englishman had a figure on his breast which gave him a right, on certain festivals, to form part of Ketenue's suite. Those who have not this mark are not entitled to receive any pork. Some of the officers and most of the sailors on board our ship caused themselves to be tatooed. The natives, for the most part, go entirely naked.

Their chief food is the bread-fruit, which to me did not appear to be very savoury: but it was then unripe, and not in season. The tree has a good deal of similarity to the wild chesnut tree. They eat also bananas, coco-nuts, yams, pork, fish, and even human flesh. However incredulous I was in regard to the last point, it is certain that these people, who according to every appearance are so friendly and so mild, are real cannibals. The Englishman, and afterwards the Frenchman, who certainly had not entered into an agreement to deceive us, concurred fully in their accounts of this circumstance. We have several sculls, two of which I purchased, which were those of enemies whom they had defeated in battle and afterwards eaten. Some years, when the bread-fruit is scarce, a famine takes place: and on such occasions many of them kill their wives or children, and eat them; others undertake a warlike expedition against the enemy; that is to say, several of them creep imperceptibly in the night-time to the neighbourhood of the houses of the enemy, or conceal themselves behind trees or among the grass, and as soon as they discover any of the enemy, whether men, women, or children, they immediately attack them, carry them off, and devour them. Even when there is no scarcity, their expeditions against the enemy are continued, partly because they delight in them, and partly because they consider human flesh as a great delicacy, and prefer it to that of their hogs. They fight also battles in which one party is regularly pitched against another. Their weapons are slings, lances, and clubs. The two last are made of casuarino wood: one of these

these clubs I have in my possession. As soon as a couple of the enemy have fallen, the battle immediately ceases, because something has been obtained to eat. The Englishman, Roberts, cautioned us not to place any confidence in these islanders; to be always on our guard, and, when any of them offended us, to shoot them immediately: he assured us that this would produce no bad consequences, and that the rest would give themselves no trouble about it. Such are the islanders of the South Seas, so celebrated for their mildness and humanity! On the most friendly of these islands they are no better; and Cook, after being massacred, was publicly eaten by the natives of the Sandwich isles: nothing is clearer, notwithstanding the pains which capt. King and M. Fleurieu have taken to contradict it. It is mere folly to consider the man of nature, as he is called, as better and more benevolent than the man who has been civilized. Fortunately we had no disagreeable disputes with them. They feared us on account of our fire-arms, and considered us as *atuas*, or gods. None of them were ever struck by our shot, though we sometimes fired muskets or some of our cannon in the night-time, to frighten those whom we heard swimming around us, and to prevent them from injuring our cable.

These islanders spend their time properly in a state of indolence, and employ themselves only in dancing or ornamenting their persons. When any of them set about making tackle for catching fish, a girdle, or club, the work is speedily completed. On the whole, none of them, properly speaking, have any particular occupation. When we gave any of them work to perform, it was a kind of festival to them. They dragged away for us the wood which we had cut down; but they were most useful to us in filling our water casks. I do not know how other navigators could convey, without their assistance, large casks through the violent breakers; but it required five or six of our sailors to accomplish what one of these natives could do seemingly in sport. When a large wave came and threatened to dash the cask and man to pieces on the shore, the latter dived into the water, forcing down the cask along with him, so that the wave passed over both; after which the islander swam on quietly as before: on the approach of the next wave he did the same; and before we could believe it possible he arrived with the cask at our boat. The piece of iron which he obtained as a reward for this service he showed with a great deal of laughter to his companions standing on the shore, who then burst out into loud laughter

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also. They are remarkably fond of dancing, which forms the principal part of all their festivals. The most essential part of their dancing consists in a quivering motion of the hands: it is not disagreeable, and has in it something singular, which to me at least was new. Their music consists in beating in time with the right hand on the left arm. The sound was much louder than we were able to produce in the same manner. When they sing, they clap their hands in time in such a manner that the fingers cross each other and produce a full tone. A drum made of fish-skin was used also on these dancing festivals.

Their morals are of a piece with the rest. Parents and husbands sent their daughters and wives on board our ship to display their charms; and a piece of iron or a nail was sufficient to remove all their scruples. Those who have wives keep also a fire-maker, because this business is somewhat laborious. This fire-maker is the woman's second husband, and he pays great attention to her because his own interest is interwoven with hers. I was told afterwards by the Frenchman that the men are very jealous. This seems to be a contradiction, but it may nevertheless be explained.

Their expertness in swimming is really wonderful. Many of them swam off early in the morning to the ship, with their forenoon's repast, consisting of coco-nuts, which they ate in the water, and returned on shore late in the evening. Others had both their hands full of different articles, which they wished to barter with us, or which they had procured from us. These they held up, and swam for several hours merely with their feet. Some of them, for the greater convenience, had a piece of board with them. This board they held before them, and suffered themselves to be driven on shore by the surf: however dangerous this experiment may appear, none of them ever experienced any hurt by it.

Taaboo is the magic word here, in which are comprehended all their religious, political, and moral laws. But a clearer idea of the importance of this word will be conveyed by a few examples. When one says that this or that place is dedicated to the spirit of his father, the spirit Atua inhabits the place, and no person dares to pass over it; it is *taaboo*. When a person gives his name to a tree or to any other man, the tree or man is *taaboo* to all others: the spirit resides in both. In all the houses the place set apart for eating is *taaboo* to the women; and when any of the natives have got any thing which they are desirous to eat unmolested, they sit down in the place which is *taaboo*.

Swine's

Swine's flesh is *taaboo* to the women; but they eat human flesh when it is given to them. Ketenue and his whole family were *taaboo:* those who bore Ketenue's name were *taaboo*. The case was the same with the Englishman; so that no one durst do him any hurt. There is a particular ceremony by which people can make themselves *taaboo*. It consists in binding feathers around the head, dancing and singing, and declaring that they desire to have Ketenue's name. While this ceremony lasts, they are safe from all harm. One day it was *taaboo* for all the islanders to come on board the ship, those only who brought hogs to the ship were excepted. Those who break *taaboo* will be eaten on the first opportunity by the enemy. This they so firmly believe, that it keeps them in order. When it is discovered that any one has broken *taaboo*, he must make atonement for his fault by giving presents, such as hogs, &c. which the priest applies to his own use; or he is deprived of his land: if he has nothing, he becomes *kikino*; he is then unprotected, and always in danger of being eaten by the enemy.

Tapeka Ketenue is said to possess a great deal of land; he does not go to war, but there is nothing to prevent him if he chooses it. In times of scarcity he maintains a great many of the natives; for, as he possesses abundance of bread-fruit trees, he has large pits filled with fermented bread-fruit, which in this state will keep a long time. I cannot say much in favour of this kind of food; it has a disgusting sour smell, similar to that of fermenting wheat used for manufacturing starch. He has no power to command any person, but he and his family are *taaboo*; and as he is able to give maintenance to a great many, for the sea belongs to him, and all those who fish in it must bring him part of what they catch, he is still an important personage. He has also several houses. Roberts, who was well established, had received from him a house with coco-nut and bread-fruit trees.

[To be continued.]

II. *Some*

II. *Some Account of a terrible Hurricane which began to the Windward of the Caribbee Islands on the 3d of September 1804, and proceeded North-westwardly over the Virgin Islands and Bahamas on the 4th, 5th, and 6th, until it reached Florida, Georgia, and South Carolina, on the 7th, 8th, and 9th; and of a furious Gale from the North-east which prevailed at the same time, and proceeded South-westwardly until it met the former: showing that Storms of the most destructive Violence sometimes arise to Windward, and bear down every Thing before them in their Passage to Leeward.*

To Mr. Tilloch.

DEAR SIR, New York, April 2, 1805.

I INCLOSE you a copy of my letter to baron Humboldt on the hurricane of September 1804. It is intended to furnish facts for a more satisfactory theory of the American winds than we possess at present. My situation at Washington, the seat of our national government, and the great amount of ship news contained in our gazettes, have enabled me to make the collection of facts very extensive.

Yours truly and respectfully,
SAMUEL L. MITCHILL.

Mr. Volney, when he was in North America, sought information concerning its atmosphere, with an intention of forming a theory of the winds prevalent in the territory of the United States. At that time I was not able to furnish any facts worthy to be communicated to that able observer. Since his return to Europe I have had an excellent opportunity to collect the facts afforded by a most violent snow storm from the north-east, on the 21st, 22d, and 23d days of February 1802. These were published in the first Hexade, vol. v. p. 465, of the Medical Repository. From that inquiry it appeared that our most boisterous winter storms, accompanied with snow and a north-east wind, began to leeward, and progressed to windward from South Carolina to Maine, at the rate of about one hundred miles in an hour.

Since that collection of facts was printed, Mr. Volney has given to the public "his Picture of the Climate and Soil of the United States," in two octavo volumes, at Paris, in 1803. In the ninth chapter of this work he has ventured to give "a system of the winds within the United States." Herein he has treated of the winds from the north, north-east, and east; from the south-east and south; from

from the south-west; and from the north-west, in distinct sections. But finding there are some considerations in my piece on the gale of February 1802 which are not contained in his treatise upon our north-east wind, I take the present opportunity of inscribing it to you. In his short essay upon the south-east wind he closes his observations, for want of more facts, not choosing to supply their place by conjecture. I believe that, by reason of a hurricane which lately happened in the southern latitudes, bordering on countries in North and South America which you have visited, it is now in my power to communicate to you some additional facts on the south-east wind of the western hemisphere, and some very important information relative to its north-east and east winds.

Between the 3d and 9th of September 1804, there occurred in the Caribbee Islands, in the Bahamas, on the ocean to the north-east of these, and on the coasts of Florida, Georgia, and South Carolina, one of the most destructive storms that had ever raged within the memory of man. The agitation of the atmosphere and of the sea was so dreadful as to overwhelm and destroy an uncommon number of vessels, cargoes, and crews, both on the ocean and in port, and also to work great damage on shore. A current from the south-east swept all before it in its progress from the Caribbees. It had, however, various turnings, whirlings, and eddies, blowing in the most opposite directions, and veering almost all round the compass. Another current from the north-east met the former in about the latitude of Charleston or Beaufort. The two streams formed for a while an east wind, which continued until the south-east gale triumphed by its superior force. This conflict of the winds was accompanied by torrents of rain, by a retardation of the Gulf Stream, and by such an accumulation of water in the curvature of the coast between Florida and North Carolina, as to lay a great portion of the low shores and islands of Georgia and South Carolina under water. This storm, unlike the former one which I described, began to windward, and by violent propulsive force worked its way to leeward. I have reduced to something like method, the relations and facts as stated by navigators, and gathered from cotemporaneous publications.

A gale or hurricane of this sort happened in September 1782, as far north as lat. 42° 15′, and in long. 48° 55′. It began on the 16th, and destroyed many English ships, belonging to a fleet of ninety sail, then off the banks of Newfoundland, and bound homewards from Jamaica. It began

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at east-south-east on that day, and prevailed with greater violence than was ever before known on that part of the ocean, until about three o'clock the next morning, when, without the least warning, it shifted in an instant, and blew with such fury from north-north-west, that the oldest seaman in the fleet had never seen the like. The Ramillies, the Centaur, L'Hector, the Ville de Paris, and many other ships, the spoils of Rodney's victory in the West Indies, all perished.

The particular accounts will be given under distinct heads, classing the occurrences according to their appearance.

1st, *In the Caribbee Islands.*—On the 3d of September there was a hard gale at Martinique, so as to make vessels quit their anchors, drive ashore, &c. A number of vessels were driven ashore at St. Croix. Of thirty-two sail at St. Bartholomew's, only two rode it out. At St. Pierre's (Martinique), Mr. J. Anderson stated the wind to have been from the north-west and west-north-west. At St. Bartholomew's it began from the north-west, then blew from north, and at last got round to the south-west. On the 3d, 4th, and 5th of September, capt. Henry, on a voyage from Point-Petre to Philadelphia, was obliged to lie-to under Deseada for fifty-six hours. The gale was heavy, with rain and thick weather.

Captain Jones related that at Point-Petre there was, on the 4th of September, the most dreadful gale known for twenty years. There happened to be no vessels at Basseterre. But at Dominique every vessel had been lost.

On the 4th of September two brigs, commanded by captains Lovell and Glazier, were driven out of their ports; one at St. Croix, and the other at St. Thomas. The gale was so violent as to make them slip their cables with the loss of their best bowers. It lasted thirty-six hours, and was as severe as any ever recollected. At St. Thomas thirty sail were driven on shore.

Capt. Smith sailed from Demarara on the 21st of August, bound for New York. He was overtaken by the hurricane on the 4th of September. It blew from the southward and eastward. After being thrown on his beam ends, and losing his foremast and bowsprit, she was rendered so leaky as to be abandoned on the 8th as a wreck.

Capt. Boardman, on his passage from Guadaloupe to Newbury Port, experienced the gale from the 4th to the 7th of September. Capt. Day, on his passage from Berbice to the same place, was overtaken by the gale to the leeward of Tobago.

Capt.

Capt. Mountfort, from Demarara, gave information that the hurricane had not been felt there. Capt. Wood declared the like of Grenada.

The gale commenced at St. Thomas on the 4th of September, in the afternoon, and lasted three days. During this time it destroyed forty-two sail of vessels. Accounts from the windward stated that the British packet from Falmouth to Barbadoes had been lost. Guadaloupe, St. Bartholomew's (where thirty sail were driven on shore), Tortola, St. Kitts, Antigua (four sail driven on shore, a packet foundered at anchor, and much damage done to estates in the mountains), Eustatia, St. Martin's; and, in short, all the Caribbee Islands experienced a like fate, with the loss of many vessels, and much other property. There were four wrecks at Anegado.

At St. Kitts the hurricane began on the afternoon of the 3d of September. It blew at first from the north and north-west. On the 4th it shifted to the south-west, and changed frequently to the south, blowing with equal fury in all these directions. It was reckoned to be nearly as fatal in its effects, to shipping and to property on shore, as the ever-memorable one in 1772, and of much longer duration. The quantity of rain which fell was great and sudden, so as almost to deluge the mountains.

2d, *In the Bahama Islands.*—The gale was experienced at Turks Islands on the 4th of September. It prevailed in the Bahamas with extreme violence. No severer one was ever known. At Turks Islands all the vessels ran ashore except two, which put to sea. Most of them were totally lost. Capt. Rhodes, who put to sea, returned thither on the third day after, having sustained no other damage than the loss of one of his boats. The sea had broken into the salt-ponds, injured the dykes and canals, and melted large parcels of the salt in stacks.

But capt. Waite informed us that the gale was severely felt at Nassau, in New Providence, on the 5th and 6th of September. About thirty sail of small craft were driven on shore, but not much damage done to square-rigged vessels. Capt. Bakus, who was on his passage from Ragged Island to New Providence, experienced the gale on the 7th. The wind came first from north-east, then hauled to the west, and afterwards blew north-north-west, and then west again.

3d, *On the Atlantic Ocean, to the North-east and North of the Bahamas.*—Capt. Johnson encountered the gale on the 6th of September, in lat. 31° 5′, and long. 81°. It first blew from the north-east, and from that veered to west-

south-

south-west, north-west, and south-west. It was terribly furious, so as to damage his rigging very much, loosen his masts, and render his ship very leaky.

Capt. King, from Demarara, was invaded by the gale on the evening of the 6th, in lat. 21° 51′, and his vessel was thrown on her beam ends. He was forced to cut away her main-mast. Lost a man, who was washed overboard.

Capt. Messroon took the gale in the Gulf Stream, lat. 20°, on the 6th of September. The wind was then east-north-east, and continued so until the 7th, then it shifted to south-east. It was very severe, though he escaped without material damage.

In lat. 22°, long. 64°, capt. Beard was wrecked in the gale. It began on the 3d of September, continued during the 4th, and did not end before the 5th. He and his crew were taken off the wreck on the 9th.

On the 7th, 8th, and 9th of September, capt. Jenne, bound from Kingston, in Jamaica, to Baltimore, suffered a tremendous gale in lat. 33°, long. 74°. The wind varied between north-east and south-east.

Capt. Mood, on a voyage from Alexandria (Virginia), to St. Mary's (Georgia), was, on the night of the 7th, in the Gulf Stream, to the eastward of Charlston: the wind there was east-north-east, and so hard as to throw his vessel on her beam ends. She lay several hours in this situation. Several of his crew were washed overboard.

Capt. Miller, on a voyage from Martha-Brae, in Jamaica, oound for Wilmington (North Carolina), experienced the same gale the same night, on the inner edge of the Gulf Stream. It was so violent as to heave his vessel on her side as she was lying-to under her jib, to unstep her masts, and to tear up her deck. In this forlorn condition the crew were fortunate enough to save themselves by gettting on board another vessel.

Capt. Andrews, on his way from Charlston to Nassau (New Providence), encountered the most formidable part of the gale on the night of the 7th of September, in lat. 26°, long. 77°. She was thrown on her beam ends, her boom broken to pieces, her main-topsail and rigging carried away, and two men washed overboard.

The brig Augusta was on her passage from Savannah to New York when the gale began. She had sailed on the 31st of August, and had progressed no further than the Frying-pan Shoals, off Cape Fear, on September 7th. Being there exposed to its vehemence, they stood off shore as long as she could carry sail; but at half past two P. M. they were obliged

obliged to lie-to. The weather was turbulent all the night. On the morning of the 8th the rage of the storm was excessive, beyond what any person on board had ever experienced. It increased until two P. M., and continued all night with unabated fury. At day-light on the 9th she was about three and one-half leagues from the breakers on the Roman shoals at Cape Carteret. They were lucky enough to escape these, and to arrive about noon at Charleston bar, which was one continued breaker, so that no pilot could get out. They were forced to cast anchor on a lee shore, and with the help of two cables and anchors rode it out until the 10th, when she got into Charleston. Capt. Davidson, of this vessel, related, that in the fore part of the 7th, before the gale began, he plainly saw a brilliant star in the zenith.

4th, *In the Latitudes South of the Bahamas*.—Capt. Jaggart, who left Jeremie, in St. Domingo, on the 14th of September, declared, that the gale was not felt or known at that place at all. The captain of a Spanish schooner from Matanzes said, the gale was felt there, but not much damage done.

The British armed ships Theseus and L'Hercule took the gale first in north lat. 22° 12′, and west long. 63° 44′, on Wednesday, September 5, about eight o'clock P. M. They were then about sixty miles north-east of the "Square Handkerchief," and about one hundred miles north of the "Silver Quays." The gale was in the beginning from the north-east, and by degrees came round to the south-east. Its violence reduced them to the utmost distress. It lasted until Friday the 7th, at five P. M. They afterwards got into Kingston harbour, in the island of Jamaica.

Capt. Howe, from Porto-Rico, related, that the gale was experienced there on the 4th of September, and drove ashore every vessel at the west end.

Capt. Bennet sailed from St. Thomas on September 3. On the 6th, about thirty miles southward of Porto-Rico, he was assailed by a tremendous hurricane. The wind was south-south-east, but frequently varying. The Jamaica papers of the 8th contained accounts of considerable damage done on the south-east side of that island by the gale of the 4th. The north side did not feel it.

5th, *In the Latitudes North of Cape Fear*.—It appears that the gale did not prevail much to the northward of Wilmington (North Carolina). It was but slightly felt there. On the 9th, a small schooner and periago were driven on shore, but not materially injured.

The brig Wilmington packet, from New York, had been ashore on the Frying-pan, but, after taking out the cargo, was got off. The crops in the neighbourhood of Wilmington had not been injured.

Capt. Tilford, on a voyage from London to Baltimore, felt the gale on the 3d of September in lat. 39°, and long. 65°; it blew from east-north-east, and continued in the form of a strong and favourable wind until the 8th, when he made the Capes of Chesapeake. As soon as the gale reached land it grew more violent, and seems to have parted into two streams. By the assistance of one he then ran up the bay to the mouth of Patapsco in twenty hours. The other branch turned southward along the land toward Cape Hatteras.

Vessels from Europe, which had not got further south than lat. 39°, seemed to have escaped the hurricane.

6th, *On the Continent of North America, and the adjacent Islands.*—(A) In Florida the gale was excessively hard; at St. Augustine the tide rose to an uncommon height. Of nine vessels in the harbour only one rode out the storm.

(B) In Georgia. At Savannah the gale began on Saturday morning the 8th. The wind was from the east, yet varying between south-east and north-east incessantly. It was more dreadful than any that is recollected to have ever happened there. It commenced by slight wind and rain until about ten A. M., when it blew with uncommon violence. It was accompanied by heavy rain, and went on increasing until between six and seven in the evening. It did not cease until three o'clock in the morning of the 9th. The continuance was seventeen hours. The water rose to between eight and ten feet above the level of common spring tides. Houses and stores were blown down by the wind, and undermined by the water. Fences and trees were prostrated. Ships and vessels were stranded, and left high and dry upon the tops of the wharves. Great damage was done on the island opposite the town, and on Wilmington and Skidawa Islands. Fort Green, on Cockspur Island, was completely levelled; thirteen lives were lost, and all the buildings destroyed. The water was supposed to have risen from fifteen to twenty feet above the level of the fort. The surface of the land was considerably lowered and washed away. One of the national gun-boats was carried about eight miles from her moorings, and landed in a corn-field upon Whitemarsh Island. A cannon weighing 4,800 lb. was carried thirty or forty feet from its position. A bar of lead of 300 lb. was carried one hundred feet. Cases of cannister shot were carried from one hundred to two hundred feet, and muskets were scattered all over the island.

Such was the beating of the ocean against the shores, that at Savannah the rain which fell was of a saline taste. An experiment made by evaporating some of it, proved it to be highly impregnated with sea salt; this was probably derived from the spray of the sea. The water in the river was saltish at Savannah, and for fifteen miles above. Sand was blown into the upper stories of houses thirty feet higher than the surface of the earth.

At St. Simon's Island great damage was done by inundating the crops and drowning the negroes. The like happened on St. Catharine's, and on the other islands along the coast. At Sunbury the bluff was reduced to a perfect beach, and almost every chimney was levelled with the ground

Mr. Isaac Briggs, who was in the interior of Georgia, about twenty-three miles from the high shoals of Apalachy, on his way to Hawkins's settlement, on Tallapoosa river, arrived at the house of an Indian trader there on the 8th of September: here he was detained two days by severe stormy weather. In his letter to Mr. Jefferson he remarks, "that sometimes his ear could scarce distinguish an interval between the sound of one falling tree and that of another." The wind was north-east.

The gale was distinctly felt in the upper country as far as it is settled, which is to the distance of three hundred miles from the ocean. It was felt there as a strong wind which blew down the corn, but was not hard enough to prostrate trees. There it blew from the north-east, and began on the afternoon of Saturday the 8th. The rain did not begin until in the evening.

(C) *In South Carolina.*—At Charleston the gale was more furious and long continued than was ever known since the hurricane of 1752. It prevailed there on the 7th, 8th, and 9th of September, and exceeded, in violence and duration, the great storm of 1783. It began at Charleston on the 7th, about eleven P. M., and continued until Sunday morning, the 9th, at one. The wind was at first north-east. In the course of the morning of the 8th it shifted to the east, and in the afternoon to south east. It lasted for nearly thirty-six hours. But three or four of the vessels in the harbour escaped without injury. Many were much damaged, and several wholly lost. The whole of the wharves, from Gadsden's, on Cooper River, to the extent of South Bay, received considerable damage. Many stores

 were

were washed or blown down, and much property lost. Numerous houses were unroofed, and trees overturned.

On Sullivan's Island fifteen or twenty houses were undermined by the water, and carried away. Fort Johnson, which had been long in a tottering condition, was destroyed, so as not to admit the mounting a single cannon. The breast-work and pallisadoes of Fort Pinckney were washed away. From Fort Moultrie, near which the sea made a clear breach to the cove, every spot was covered with water.

At Jacksonburgh the crops of corn and cotton were much injured. The bridges were carried away between Charleston and George Town, and so many trees blown across the roads as to obstruct the stages for several days.

At May River all the crops, cotton, and negro houses, machines, &c. were completely swept off. The tide rose nine feet higher than the highest spring rise. On Hutchinson's Island many negroes, and some white people, were drowned. The like happened at Dawfousky and Broughton's Islands.

At Coosahatchie trees were thrown across the roads, and bridges carried away, so as to prevent intercourse through the country; that village was entirely surrounded by sea water. In Prince William's parish, Beaufort district, the storm was experienced in an awful manner. The sea formed a junction through the streams of Pocotaligo, Stony Creek, and Huspa rivers, in such a manner as to turn Scotch Neck into an island. Through the fields, at Sheldon to Motley, the water covered the plantations four feet deep on the high road and causeway leading to the meeting-house, rendering the roads impassable. Great destruction was made upon the crops of rice and cotton, and many animals of various kinds were drowned. Nothing but the high ground was visible on the roads of the Fishpond and Horse-shoe savannas.

The gale began at George Town (South Carolina), between three and four A.M. on the 8th of September. The wind was at north-east, and blew with increasing violence until midnight. It then changed to south-south-east, and abated little of its fury before the evening of the 9th. The rain descended in most profuse quantity the greater part of the time.

The gale extended to the upper part of the country as far as the mountains, to a distance of two hundred and fifty miles. It blew from the north-east, and was so violent at one hundred miles from the sea board, as to blow down

forest

forest trees in great numbers, so as to render the roads impassable for carriages.

From a consideration of all these details it appears that the gale extended from beyond the latitude of Tobago, in 12° north, to the latitude of Wilmington (North Carolina), northward of lat. 34°, sweeping a tract of ocean at least twenty-two degrees in extent. It probably exceeded by far these limits, as capt. Tilford felt it as far north as 39°. It appears also that it reached from the longitude of the windward islands, in 60° west of Greenwich, to the mountains and back settlements, travelling over a surface of as many degrees in that direction. And its prevalence was, in all likelihood, much more wide and diffusive than has come to my knowledge.

The gale in the islands blew from north-north-west, and even from the south-west, but, as it approached the coast, got round to the eastward, and varied between north-east, east, and south-east. It arose to windward in both the north-eastern and south-eastern quarters. In this respect it widely differed from the great north-east snow storm described in Med. Rep. Hex. i. vol. 5. p. 465, which began to leeward *. It seems to have taken about four days for the

* So did the one which is described in the following account:—But these snow storms from the north-east do not seem always to blow the whole length of the coast between the Gulf of St. Lawrence and the Bay of Mexico. The winter of 1804-5 was the most rigorous that had happened since that of 1779-80. One of the snow storms which occurred during the latter winter, illustrates at once the fact of their beginning to leeward, and of their limited extent in certain cases. It also shows that they prevail at different places with very different degrees of violence. The weather had been intensely cold during January; the quicksilver had sometimes been as low as 8° above 0, and frequently down to 11 and 14°. After this uncommonly severe weather, the atmosphere rapidly became warmer, the mercury rose to 46° in the course of a few days, and immediately a thick and heavy fog overspread the ice on the rivers, and the snow on the earth. This continued until the 26th, when the cold increased again. About four in the afternoon of that day, snow fell at Washington, and there was a mingled fall of snow and rain at George Town (Maryland). This storm was felt at New York city in the fore part of the evening, and not until eight P. M. by the ship Favourite, then off Boston harbour. At Newbury port the newspapers state it to have begun on Sunday morning, the 27th. By a comparison of the facts it will be found that this storm began at least four hours sooner on the Potowmac than in Boston harbour. The difference of time was no less remarkable on its cessation; for it had ceased so entirely at Washington on Sunday night, that the weather had cleared up on Monday morning the 28th. In New York it continued until Tuesday morning, and lasted at Boston until Tuesday evening. Though the storm was not of long duration at Washington, and the fall of snow was moderate there, yet it was far otherwise in New York and Massachusetts. The quantity of snow which fell in both those places was uncommonly great. Many vessels were wrecked and lost on the coast of

the south-east current (from the 3d to the 7th) to force its way along from Tobago and Barbadoes to Augustine, Savannah, and Charleston. In like manner, by comparing the times of its commencement along the Fredish coast, it is evident that the north-east current blew violently near the Frying-pan shoals at half past two P.M. on the 7th; that it began at Charleston at eleven the same night, and did not become formidable at Savannah before ten in the morning of the 8th, consequently it did not begin at Charleston until eight hours and a half after it began at the Frying-pan, nor at Savannah sooner than the nineteenth hour and a half subsequent to its commencement at the same place. Hence, on comparing this storm with the one before alluded to, it is evident that this, which had its rise to windward, was not near so rapid in its progress as that one which took its origin to leeward.

From all these facts and considerations there is reason to believe, that this gale, consisting chiefly, as it advanced toward the Continent, of currents from the north-east, east, and south-east, was the trade-wind diverted from its ordinary course, and blowing with a force prodigiously augmented over a tract considerably to the northward of its usual limits. The two columns of agitated atmosphere moving obliquely toward each other, appear to have met and expended their combined forces upon that bend of the coast which forms the front of North Florida, Georgia, and South Carolina. At present I know too little of atmospheric movements to determine what was the particular rarefying or expanding cause that, on this occasion, put the windward air into such destructive commotion, made it rush with such resistless impetuosity to leeward, and more particularly determined it to quit the intra-tropical regions, and exert its whole strength upon a part of the Continent so far to the northward as that which lies between St. Auguline and Wilmington, betwixt the latitudes of 29° and 34° north.

of Massachusetts in particular; and in the latter, this snow storm from the north-east lasted thrice twenty-four hours, while in Maryland it did not at farthest continue more than half the time, and certainly with less by far than half the violence.

III. *Twenty-*

III. *Twenty-second Communication from Dr.* THORNTON *relative to Pneumatic Medicine.*

To Mr. Tilloch.

Case of Consumption cured by Hydro-azotic Gas.

June 18, 1805,
No. 1, Hinde-street, Manchester-square.

JOHN HUGHES, æt. 18, helper to Mr. Cozens, livery stable keeper, City Arms, London-street, had all the marked symptoms of a decline. He had a very bad cough; used in 24 hours to spit up near half a pint of discoloured matter; had colliquative sweats; hectic fever; great debility: appetite good, and yet reduced in flesh to a mere shadow. These strong criterions of consumption had been progressively increasing for more than half a year, when he applied to me for advice. I pursued in this case the same plan as had saved Mr. Gregory, of Berners-street*, viz. the inhalation of hydro-azotic gas, with tonic medicines; and in two months all these alarming symptoms vanished, and the lad was restored to health.

Observations on this Case by Dr. Thornton.

1. Consumption is deemed a fatal disease; and the practice universally pursued in England has rendered it, I believe, still more destructive.

2. In Dr. Rush's works, the able professor of Medicine in Philadelphia, the best plan of treatment is laid down.

3. Bark (at which our practitioners are so alarmed) is recommended, with other tonic medicines.

4. Consumption must be considered either as a defluxion of the lungs, or as an abscess, or ulceration thereof.

5. If a defluxion, as in other gleets, bark is advisable: if ulceration, the treatment of the constitution should be as in other wounds.—Bark is there universally recommended.

6. Dr. Rush has recorded some cases where persons shot through the lungs have recovered. Captain Christie, of Liverpool, lately applied to me for advice. In a sharp engagement with the French, a ball entered the sternum, and lodged under the scapula. The wound in the lungs healed, and he felt afterwards only debility from the great loss of blood sustained.

7. The objection against bark, myrrh, and wine, is the *cough*. Colds are, I confess, aggravated by such treatment,

* This cure is recorded in Number 13 of our Magazine, p. 95.

7. The

and the incipient stage of phthisis; but in the subsequent stages the only chance is, I speak from wide experience, the practice I have here recommended.

8. Inflammation of the lungs is prevented by the hydro-azotic gas.

The lad is now before me; is fat, looks well, and has been cured a twelvemonth.

IV. *On the Action of Platina and Mercury upon each other.* *By* RICHARD CHENEVIX, *Esq. F.R.S. M.R.I.A. &c.*

Freyberg, June 3, 1804.

ON the 12th of May 1803, I had the honour of presenting a paper to the Royal Society, the object of which was to discover the nature of palladium, a substance just then announced to the public as a new simple metal. The experiments which I had made for this purpose led me to conclude that palladium was not what it had been stated to be, but that it was a compound of platina and mercury.

It was natural to suppose that a subject so likely to spread its influence throughout the whole domain of chemistry, and which tended even to the subversion of some of its elements, would awaken the attention of philosophers. We find accordingly, that it has become a subject of inquiry in England, France, and Germany; but the experiments which I had recommended as the least likely to fail, have been found insufficient to insure the principal result; and I have had the mortification to learn that they have been generally unsuccessful. I have even reason to believe that the nature of palladium is still considered by chemists, at least with a very few exceptions, as unascertained; and that the fixation of mercury by platina is by many regarded as visionary.

The first doubts were manifested in England; and Dr. Wollaston very early denied the accuracy of my inquiries. But as he has not published his experiments, I have had no opportunity of discussing them. His opinion, however, must have such weight in the learned world, that I should have neglected a material fact in the history of palladium if I had not mentioned it in this place.

In France the compound nature of palladium has been more generally credited. When the National Institute was informed of my experiments, a report was ordered to be made

* From the *Transactions of the Royal Society* for 1805.

made upon them, and M. Guyton was the person appointed for the purpose. He repeated some of the experiments, and produced some of his results. His general conclusion was the same as mine.

Messrs. Vauquelin and Fourcroy then undertook the subject, and they were led by it to the confirmation of the recent discovery of M. Descotils. The existence of a new metal which that chemist had found in crude platina, received great sanction from their experiments; and thus the discussion upon palladium has established a fact which will be considered as interesting, but which would be much more so, were we not already overburthened with substances which our present ignorance obliges us to acknowledge as simple.

No sooner were these celebrated chemists convinced of the existence of a new metal in platina, than they concluded that it must play a principal part in the composition of palladium. Shortly after this, in a note to a letter from M. Proust to M. Vauquelin, in which M. Proust expresses his astonishment concerning all he has read upon palladium, Messrs. Fourcroy and Vauquelin further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new substance does or does not play a principal part in the formation of palladium, could not be ascertained at the time my experiments were made, because the new metal itself was not then known. But from all that Messrs. Fourcroy and Vauquelin have stated, in such of their different memoirs upon this subject as I have seen, the grounds of their supposition have not appeared. May we not refer their opinion, then, to that common propensity of the mind, against which M. Fourcroy has himself warned us with equal justness and eloquence on another occasion, namely, a proneness to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of universal influence.

Messrs. Rose and Gehlen* were the first among the German chemists who instituted experiments upon palladium; and M. Richter has also published a paper on the same subject.

The first attempt of Messrs. Rose and Gehlen to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their re-

* *Neues Algemeines Journal der Chemie herausgegeben von Hermstadt,* Klaproth, Richter, Scherer, Tromsdorf, und Gehlen. *Ersten bandes funftes heft.*

sult was precisely that which I had observed when my operations failed altogether, and which of course was the most frequent. This method was repeated twice. The second time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more successful than the former.

Their third experiment was what they have called a repetition of that in which I had obtained palladium by passing a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following:—They dissolved 150 grains of platina with 450 of mercury, and added a solution of hydro-sulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was redissolved. The liquor was then filtered, and to that part of it which contained the redissolved precipitate an acid was added. From this process they obtained a yellow precipitate weighing 91 grains; and 50 grains of this, exposed to a strong heat, left 3-8ths of a grain of platina. They obtained no palladium from that part of the precipitate which had not been redissolved; and the result of the experiment was complete failure.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those metals as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former paper.

The sulphuretted hydrogen gas which Messrs. Rose and Gehlen presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered

delivered by M. Vauquelin at the *Ecole des Mines* in Paris, when he was professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydro-sulphurets, was redissolved by adding an excess of them.

It is moreover well known that there is a strong affinity between potash and the oxide of platina, and also that when those substances are brought together in solution, a triple salt, but little soluble, is the result. It was to avoid these difficulties that I had employed uncombined sulphuretted hydrogen gas; for the method adopted by Messrs. Rose and Gehlen appearing to me to be the application of two divellent forces, I presumed that it would produce a separation. The result of their experiment, which, it appears from their paper, they had not anticipated, shows the necessity of the precaution I had used. The operation which they performed to unite platina and mercury was, in fact, nearly the reverse of that which they supposed they had repeated from me, and might have been applied perhaps with a better prospect of success towards the decomposition of palladium.

Messrs. Rose and Gehlen seem, in many parts of their paper, to question my having fused platina; and inform us, that although they had exposed this metal in the furnace of the royal porcelain manufactory of Berlin, in which Wedgewood's pyrometer ceased to mark the degree of heat, they could not accomplish its fusion. Many of my friends in England have, however, seen the buttons which I obtained, and which were not few in number. The flux which I had used was borax. But no mention is made in any one of the operations of Messrs. Rose and Gehlen of borax having been employed.

In many of their attempts they obtained an irregular and porous mass, which of course was of a specific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of specific gravity, which I had observed, was owing to the same cause. It is true, not only that I had very often obtained such a mass, but that I had frequently also observed no diminution whatsoever in the specific gravity of the button which resulted from my operations. But all those upon which I had founded the conclusions alluded to by Messrs. Rose and Gehlen were performed in the following manner, and have been repeated since. A Hessian crucible was filled with lamp-black, and the contents pressed hard together. The

lamp-black

lamp-black was then hollowed out to the shape of the crucible as far as one-third from the bottom, leaving that much filled with the compressed materials; this lining, which adhered strongly to the sides of the crucible, was made extremely thin in order not to obstruct the passage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick mass of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fused, or varied according to other circumstances at pleasure. In general the axis of the cylindrical hole was about three or four times the diameter of the basis. After withdrawing the rod, the crucible was about half filled with borax. Upon this was placed the metal to be fused; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of course perpendicular to the axis of the cylindrical excavation at the bottom of the cover. More borax was then added to cover the piece of metal, and another quantity of lamp-black was presssed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this state it was exposed to heat in a forge, in which, upon another occasion, I had, in the presence of Messrs. Hatchett, Howard, Davy, and others, completely melted a Hessian crucible lined and prepared in the same manner. The fuel which I used was the patent coke of Messrs. Davey and Sawyer. In the present experiments I moderated the heat so as not materially to injure the crucible, and, upon taking it out of the fire, the lining was generally found so compact and so firm that it remained in a solid mass after the crucible was broken. When the metallic cylinder occupied the space at the bottom, it was natural to suppose that it had been fused; because in no other state but that of liquidity could it have run into the mould. In order, however, to prevent all objections, I had the precaution to make the hole of a different diameter from the metallic cylinder, and to observe whether the necessary change in the shape of the latter ensued. If, after such a test, repeated as often as required, I perceived that the metal did not vary in its specific gravity, I thought myself authorized to conclude that it was exempt from air.

M. Richter says that he had hoped to have put himself in possession of a considerable piece of palladium by repeating, with minute accuracy, the process which I had recommended as the best. He precipitated a mixed solution of platina and mercury by a solution of green sulphate of iron; and, after varying the subsequent operations, to which he submitted

submitted the product he had obtained by this method, he was led to the following important conclusions, amongst others of less consequence:—1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly, That mercury is capable of entering into combination with platina, so that it cannot afterwards be separated by fire. From the first of these conclusions it is evident that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

In addition to the chemists above mentioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attempts to form this combination; and M. Klaproth, in a letter to M. Vauquelin, published in the *Annales de Chimie* for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Messrs. Rose and Gehlen, as well as M. Richter, had conceived from my paper a reliance on the success of their experiments, which no words of mine had authorized, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance. M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure; and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success. The course of experiments which I had made, as well before as after reading my paper to the society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently seven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in stating them to

have

have been one thousand. Amongst these, I had four successful operations. I persevered, because, even in my failures, I saw sufficient to convince me that I should quit the road to truth if I desisted. After all my labour and fatigue I cannot say that I had come nearer to my object, of obtaining more certainty in my processes. Their success was still a hazard on the dice, against which there were many chances; but till others had thrown as often as I had done, they had no solid right to deny the existence of such a combination. On this foundation none, I believe, have established such a right. Messrs. Rose and Gehlen do not say how often their experiments were repeated; but it is probable that if they had been performed very often, these authors would not have neglected to mention it. M. Richter states his merely as preparatory to more extensive researches; and M. Tromsdorff, as well as M. Klaproth, mention little more than the fact. If the German chemists have concluded against my results, they have done so without just grounds, and without having bestowed upon them that labour and assiduity for which they are usually so remarkable.

In this state of uncertainty the compound nature of palladium received an indirect, but a very able, support from some experiments of M. Ritter, the celebrated Galvanist of Jena. M. Ritter had ascertained the rank which a great number of substances hold in a Galvanic series, arranged according to the property they possess of becoming positive or negative when in contact with each other. He had established the following order, the preceding substance being in a *minus* relation to that which comes next: Zinc, lead, tin, iron, bismuth, cobalt, antimony, platina, gold, mercury, silver, coal, galena, crystallized tin ore, kupfer nickel, sulphur pyrites, copper pyrites, arsenical pyrites, graphite, crystallized oxide of manganese. He had the goodness to try palladium in my presence, and found it to be removed, not only from what I believed to be its constituent parts, but altogether from among the metals, and to stand between arsenical pyrites and graphite. This result led M. Ritter into a new and general train of reasoning, and induced him to undertake the examination of a great number of alloys, and of a variety of amalgams. He considered the subject as a philosopher, and his operations were those of a consummate experimentalist. It would be doing him an injustice to attempt an extract of his ingenious paper, which contains a series of the most interesting experiments. I shall merely observe for the present purpose,

that

that it very rarely happened that the mixture of two metals bore any determinate relation to the same metals when separate; that in every case the smallest variation in the proportions produced the most marked effects; and that M. Ritter has furnished us with an instrument calculated to detect the presence of such small quantities as have hitherto been considered as out of the reach of chemistry. As palladium presents a very striking instance of the anomaly, to which all compounds seem to be more or less subject, by being removed altogether from the series of simple metals, this may serve to support the other proofs of its compound nature.

One of the principal objections of those who dispute the truth of my conclusions with respect to palladium, is grounded upon the repeated failure of all the methods I had made use of in forming it; but this cannot be of very great weight, when we consider the uncertainty of many other operations of chemistry. The most simple are sometimes liable to fail; and the easiest analyses have often given different products in the hands of different chemists, who yet enjoy indisputable and equal rights to the title of accuracy. The progress which we have made in some parts of the science has not removed the obstacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it; yet this often tends to show nothing more than contradictory results, and consequently the fallibility of the art.

But a recent case has occurred which is perfectly analogous to that of palladium. A few years ago, professor Lampadius, in distilling some substances which contained sulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain; and, after many fruitless attempts, abandoned his researches, and confined himself to stating the fact to the chemical world. Little notice was taken of it, and not much interest was excited by an experiment so likely to fail. Some time after this, Messrs. Clement and Desormes obtained the same result, and attempted to produce the substance a second time. They performed a vast number of experiments; but their success bore no proportion to their diligence and zeal. They published an account of their process and its consequences, but gained little credit, as no person was fortunate enough to produce the same substance. Many disbelieved the experiments altogether, and denied the existence of such a combination; whilst others, less inclined to doubt, attributed its formation to fortuitous circumstances which

which might never again occur together. In February 1804, professor Lampadius, in distilling some pyritized wood, though with a different intent, obtained the same substance. As he had it now in his power to observe the phænomena that attended its formation, he discovered, and has communicated to the world, a method of producing it which never fails. Since his late paper upon the subject, as the necessary precautions can be followed by every chemist, Messrs. Clement and Desormes have obtained that credit to which their experiments had, in truth, always been entitled; and the formation of what professor Lampadius terms his sulphur-alcohol is no longer a result of chance, or accounted for by being supposed one of those subterfuges to which human pride resorts, in order to spare itself the confession of human weakness.

The observation of any new fact becomes a matter of general concern, and truly worthy of philosophic contemplation, then only when its influence is likely to be extended beyond the single instance to which it owes its discovery. Whether water were a simple body or a compound, could have been of little importance as an insulated fact; but, connected with the vast chain of reasoning it gave rise to, it opened a new field for genius to explore. If in the present case our researches were to be confined merely to ascertaining whether palladium were a simple metal or a compound, all the advantages likely to arise from the facts observed during the inquiry would be lost; and an object of the most comprehensive interest would thus sink into a controversy concerning the existence of one more of those substances which we have dignified with the name of elements. It was in this point of view that Messrs. Richter and Ritter considered the subject as far as they went, and a few facts are stated in my first paper in support of the opinion that palladium is but a particular instance of a general truth.

By taking the reasoning on this subject, then, in its widest extent, we shall be led, I think, to the following conclusion,—that metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple; but to prove this must be a work of great time and perseverance, and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends

 their

their different properties as to make them apparently one, has taken its full effect, it will not be easy to separate them; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis, therefore, does not promise much success; and the labour of synthesis is sufficient to deter any individual from the undertaking.

[To be continued.]

V. *An Account of some analytical Experiments on a mineral Production from Devonshire, consisting principally of Alumine and Water.* *By* HUMPHRY DAVY, *Esq. F.R.S. Professor of Chemistry in the Royal Institution**.

I. *Preliminary Observations.*

THIS fossil was found many years ago by Dr. Wavel, in a quarry near Barnstaple: Mr. Hatchett, who visited the place in 1796, described it as filling some of the cavities and veins in a rock of soft argillaceous schist. When first made known, it was considered as a zeolite; Mr. Hatchett, however, concluded, from its geological position, that it most probably did not belong to that class of stones; and Dr. Babington, from its physical characters, and from some experiments on its solution in acids, made at his request by Mr. Stockler, ascertained that it was a mineral body as yet not described, and that it contained a considerable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general investigation of its chemical nature; and that gentleman liberally supplied me with specimens for analysis.

II. *Sensible Characters of the Fossil.*

The most common appearance of the fossil is in small hemispherical groups of crystals, composed of a number of filaments radiating from a common centre, and inserted on the surface of the schist; but in some instances it exists as a collection of irregularly disposed prisms forming small veins in the stone: as yet, I believe, no insulated or distinct crystal has been found. Its colour is white, in a few cases with a tinge of gray or of green, and in some pieces (appa-

* From the *Transactions of the Royal Society* for 1805.

rently beginning to decompose) of yellow. Its lustre is silky; some of the specimens possess semi-transparency, but in general it is nearly opake. Its texture is loose, but small fragments possess great hardness, so as to scratch agate.

It produces no effect on the smell when breathed upon; has no taste; does not become electrical or phosphorescent by heat or friction; and does not adhere to the tongue till after it has been strongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loses its hardness, and becomes quite opake. In consequence of the minuteness of the portions in which it is found, few of them exceeding the size of a pea, it is very difficult to ascertain its specific gravity with any precision; but from several trials I am disposed to believe that it does not exceed 2·70, that of water being considered as 1·00.

III. *Chemical Characters of the Fossil.*

The perfectly white and semi-transparent specimens of the fossil are soluble both in the mineral acids and in fixed alkaline lixivia by heat, without sensibly effervescing, and without leaving any notable residuum; but a small part remains undissolved when coloured or opake specimens are exposed to the alkaline lixivia.

A small semi-transparent piece, acted on by the highest heat of an excellent forge, had its crystalline texture destroyed, and was rendered opake; but it did not enter into fusion. After the experiment it adhered strongly to the tongue, and was found to have lost more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the fossil. When it was acted on by a heat of from 212° to 600° Fahrenheit in a glass tube it gave out an elastic vapour, which, when condensed, appeared as a clear fluid possessing a slight empyreumatic smell, but no taste different from that of pure water.

The solution of the fossil in sulphuric acid, when evaporated sufficiently, deposited crystals which appeared in thin plates, and had all the properties of sulphate of alumine; and the solid matter, when redissolved and mixed with a little carbonate of potash, slowly deposited octaëdral crystals of alum. The solid matter precipitated from the solution of the white and semi-transparent fossil in muriatic acid, was in no manner acted upon by solution of carbonate of ammonia, and therefore it could not contain any glucine

glucine or ittria; and its perfect solubility without residuum in alkaline lixivia showed that it was alumine.

When the opake varieties of the fossil were fully exposed to the agency of alkaline lixivia, the residuum never amounted to more than 1-20th part of the weight of the whole. In the white opake variety it was merely calcareous earth; for, when dissolved in muriatic acid, not in excess, it gave a white precipitate when mixed with solution of oxalate of ammonia, and did not affect solution of prussiate of potash and iron.

In the green opake variety calcareous earth was indicated by solution of oxalate of ammonia: and it contained oxide of manganese; for it was not precipitated by solution of ammonia; but was rendered turbid, and of a gray colour, by solution of prussiate of potash and iron.

The residuum of the alkaline solution of the yellow variety, when dissolved in muriatic acid, produced a small quantity of white solid matter when mixed with the solution of the oxalate of ammonia, and gave a light yellow precipitate by exposure to ammonia; but after this, when neutralized, it did not affect prussiate of potash and iron, so that its colouring matter, as there is every reason to believe, was oxide of iron.

IV. *Analysis of the Fossil.*

Eighty grains of the fossil, consisting of the whitest and most transparent parts that could be obtained, were introduced into a small glass tube having a bulb of sufficient capacity to receive them with great ease. To the end of this tube a small glass globe, attached to another tube communicating with a pneumatic mercurial apparatus, was joined by fusion by means of the blow-pipe.

The bulb of the tube was exposed to the heat of an Argand lamp, and the globe was preserved cool by being placed in a vessel of cold water. In consequence of this arrangement, the fluid disengaged by the heat became condensed, and no elastic matter could be lost. The process was continued for half an hour, when the glass tube was quite red.

A very minute portion only of permanently elastic fluid passed into the pneumatic apparatus, and when examined it proved to be common air. The quantity of clear fluid collected, when poured into another vessel, weighed 19 grains; but, when the interior of the apparatus had been carefully wiped and dried, the whole loss indicated was 21 grains. The 19 grains of fluid had a faint smell, similar to that of burning peat; it was transparent, and tasted like

 distilled

distilled water; but it slightly reddened litmus paper. It produced no cloudiness in solutions of muriate of barytes, of acetite of lead, of nitrate of silver, or of sulphate of iron.

The 59 grains of solid matter were dissolved in diluted sulphuric acid, which left no residuum; and the solution was mixed with potash in sufficient quantity to cause the alumine at first precipitated again to dissolve. What remained undissolved by potash, after being collected and properly washed, was heated strongly and weighed; its quantity was a grain and quarter. It was white, caustic to the taste, and had all the properties of lime.

The solution was mixed with nitric acid till it became sour. Solution of carbonate of ammonia was then poured into it till the effect of decomposition ceased. The whole thrown into a filtrating apparatus left solid matter, which, when carefully washed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general results of the experiments, when calculated upon, indicated for 100 parts of this specimen,

Of alumine - - -	70
Of lime - - -	1·4
Of fluid - - -	26·2
Loss - - -	2·4

The loss I am inclined to attribute to some fluid remaining in the stone after the process of distillation; for I have found, from several experiments, that a red heat is not sufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a strong white heat.

Fifty grains of a very transparent part of the fossil, by being exposed in a red heat for 15 minutes, lost 13 grains; but when they were heated to whiteness, the deficiency amounted to 15 grains; and the case was similar in other trials.

Different specimens of the fossil were examined with great care, for the purpose of ascertaining whether any minute portion of fixed alkali existed in them; but no indications of this substance could be observed: the processes were conducted by means of solution of the unaltered fossil in nitric acid; the earths and oxides were precipitated from the solution by being boiled with carbonate of ammonia; and after their separation the fluid was evaporated to dryness, and the nitrate of ammonia decomposed by heat, when no residuum occurred.

A comparative analysis of 30 grains of a very pellucid specimen was made by solution in lixivium of potash. This

specimen

specimen lost eight grains by long continued ignition, after which it easily dissolved in the lixivium by heat, leaving a residuum of a quarter of a grain only, which was red oxide of iron. The precipitate from the solution of potash, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Several specimens were distilled in the manner above described, and in all cases the water collected had similar properties. The only test by which the presence of acid matter in it could be detected was litmus paper; and in some cases the effect upon this substance was barely perceptible.

V. *General Observations.*

I have made several experiments with the hope of ascertaining the nature of the acid matter in the water; but, from the impossibility of procuring any considerable quantity of the fossil, they have been wholly unsuccessful. It is, however, evident, from the experiments already detailed, that it is not one of the known mineral acids.

I am disposed to believe, from the minuteness of its proportion, and from the difference of this proportion in different cases, that it is not essential to the composition of the stone; and that, as well as the oxide of manganese, that of iron, and the lime, it is only an accidental ingredient; and on this idea the pure matter of the fossil must be considered as a chemical combination of about thirty parts of water and seventy of alumine.

The experiments of M. Theodore de Saussure on the precipitation* of alumine from its solutions, have demonstrated the affinity of this body for water; but as yet I believe no aluminous stone, except that which I have just described, has been found, containing so large a proportion of water as thirty parts in the hundred.

The diaspore, which has been examined by M. Vauquelin, and which loses sixteen or seventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is supposed by that excellent chemist to be a compound of alumine and water. Its physical and chemical characters differ, however, very much from those of the new fossil, and other researches are wanting to ascertain whether the part of it volatilized by heat is of the same kind.

I have examined a fossil from near St. Austle, in Cornwall, very similar to the fossil from Barnstaple in all its

* Journal de Physique, tom. lii p. 280.

general chemical characters; and I have been informed that an analysis of it, made by the Rev. William Gregor some months since, proves that it consists of similar ingredients.

Dr. Babington has proposed to call the fossil from Devonshire *Wavellite*, from Dr. Wavel, the gentleman who discovered it; but if a name founded upon its chemical composition be preferred, it may be denominated *hydrargillite*, from ὕδωρ water, and ἄργιλλος clay.

VI. *Experiments on Wootz.* *By Mr.* David Mushet*.

The following experiments were made at the request of sir Joseph Banks, on five cakes of wootz, with which he supplied me for that purpose. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the same quality, it will be proper first to describe the differences observable in their external form and appearance.

No. 1. was a dense solid cake, without any flaw or fungous appearance upon the flat, or, what I suppose to be, the upper side. The round or under surface was covered with small pits or hollows, two of which were of considerable depth; one through which the slit or cut had run, and another nearly as large towards the edge of the cake. These depressions, the effects, as I suppose, of a species of crystallization in cooling, were continued round the edges, and even approached a little way upon the upper surface of the wootz.

The cake was a quarter of an inch thicker at one extremity of the diameter than that at the other; from which I infer, that the pot or crucible in which this cake had been made had not occupied the furnace in a vertical position. Its convexity, compared to that of the other five, was second. Upon breaking the thin fin of steel, which connects the half cakes together, I found it to possess a very small dense white grain. This appearance never takes place but with steel of the best quality, and is less frequent in very high steel, though the quality be otherwise good.

Upon examining the break with attention, I perceived several laminæ and minute cells filled with rust, which in working are never expected to unite or shut together. The grain otherwise was uniformly regular in point of colour and size, and possessed a favourable appearance of steel.

* From the *Transactions of the Royal Society* for 1805.

No. 2.

No. 2. This cake had two very different aspects; one side was dense and regular, the other hollow, spongy, and protuberant. The under surface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one side, it became flatter. The grain exposed by breaking was larger, bluer in colour, and more sparkling than No. 1. In breaking, the fracture tore but slightly out, and displayed the same unconnected laminæ, with rusty surfaces, as were observed in No. 1. Beside these, two thin fins of malleable iron projected from the unsound side, and seemed incorporated with the mass of steel throughout. Towards the centre of the break, and near to the excrescence common to all the cakes, groups of malleable grains were distinctly visible. The same appearance, though in a slighter degree, manifested itself in various places throughout the break.

No. 3. The upper surface of this cake contained several deep pits, which seemed to result from the want of proper fluidity in fusion. They differed materially from those described upon the convex sides of No. 1 and 2, and were of that kind that would materially affect the steel in forging.

The under or convex side of this cake presented a few crystalline depressions, and those very small; the convexity was greater than that of No. 1 and 2, the fracture of the fin almost smooth, and only in one place exhibited a small degree of tenacity in the act of parting. In the middle of the break, about half an inch of soft steel was evident; and in different spots throughout numerous groups of malleable grains and thin laminæ of soft blue tough iron made their appearance.

No. 4. was a thick dense cake possessed of the greatest convexity; the depressions upon the under side were neither so large nor so numerous as those in No. 1 and 2, nor did they approach the upper surface of the cake further than the acute edge. This surface had the most evident marks of hammering to depress the feeder, or fungous part of the metal, which in the manufacturing seems the gate or orifice by which the metal descends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being solid. Towards the feeder it seemed loose and crumbly, and much oxidated. The grain divided itself into two distinct strata, one of a dense whitish colour, the other large and bluish, containing a number of small specks of great brilliancy. Several irregular lines of malleable iron pervaded the mass in various

 places,

places, which indicated a compound too heterogeneous for good steel.

5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet was smooth, and free from depressions, or honey-comb on both surfaces. The feeder, instead of being an excrescence, presented a deep concave beautifully crystallized.

In breaking, the fracture tore out considerably, but presented a very irregular quality of grain. That towards the under surface was small and uniform, but towards the flat or upper surface it increased in size, and in the blueness of its colour, till it passed into the state of malleable iron.

The break of this steel, though apparently soft, was the least homogeneous of the whole, and throughout it presented a very brilliant arrangement of crystal, which in other steel is always viewed with suspicion.

General Remark.

Uniformly the grain and density of the wootz are homogeneous, and free from malleable iron towards the under or round surface; but always the reverse towards the feeder or upper side.

Remarks in Forging.

No. 1. One-half of the cake was heated slowly, by an annealing heat, to a deep red, and put under a sharp broad-mouthed chisel with a small degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the slit or cut originally in the cake.

The heat in this trial was so moderate that I was afraid that the crack had arisen from a want of tenacity, occasioned by the heat being too low.

The other half was heated a few shades higher, and subjected to the same mode of cutting: before the chisel had half way reached the bottom, the piece parted in two in the direction of the depression made by the cutting instrument. The additional heat in this instance proved an injury, while the cracking of the steel in both cases, particularly the former, was a certain proof of the abundance, or rather of the excess of the steely principle.

The fractures of both half cakes, now obtained for a second time, were materially different from that obtained by the simple division of the cake. The grain was nearly uniform, distinctly marked, but of too gray a colour for serviceable steel. Two of the quarters being drawn into neat

bars

bars under hand-hammers at a low heat, one of them contained a number of cracks and fissures. The fracture was gray, tore out a little in breaking, but was otherwise yolky and excessively dense. A small bar, of penknife size, was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the steel was greater, and it stood firmly under the hammer at a bright red heat.

The other two quarters of this cake were squared a little, and successively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into small penknife size. One of the bars from an outside piece, always the most solid, was entirely free from cracks, and had only one small scale running upon one side.

These bars exhibited a tougher break than those drawn by hand; the colour was whiter, and the grain possessed a more regular and silky appearance.

Forging No. 2.

One-half of this cake was heated to a scarlet shade, and put under the cutting chisel; it was at first struck lightly, then reheated, and cut comparatively soft; but a small crack had over-run the progress of the chisel. Its softness in cutting was attributed to an evident want of solidity. The other half cake felt harder under the hammer, but proved afterwards spongy throughout the mass. In the act of cutting, a loose pulverized matter was disengaged from some of the cells, possessed of a shining appearance.

The fractures obtained in consequence of the division of the half cakes presented a flattish crystallized appearance, more resembling very white cast iron than steel capable of being extended under the hammer. One of the middle cuts was entirely cellular with crystallized interiors, and incapable of drawing; the corresponding cut of the other half cake was drawn into a straight bar three quarters of an inch in breadth and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one shade lighter than No. 1; it tore less out, was equally yolky, and possessed on the whole an aspect very unfavourable for good steel.

The other two outside quarters were also drawn into shape, one under the tilt hammer, and the other by hand. These were more solid in the fracture, possessed fewer surface-cracks, stood a higher degree of heat, tore out more, and exhibited

exhibited a silky glossy grain, at least two shades lighter in the colour than the centre pieces.

Forging 3d Cake.

One-half of this cake, first subjected to be cut, was found softer than any of the preceding, and exhibited no symptom of cracking. The other half was cut at three heats, but found loose and hollow in the extreme. A considerable portion of the same brilliant powder, formerly noticed, was here again disengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulverescent state, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumstance led me to examine every pore and cell throughout the whole fragments. On the upper surface of two of them I found small pits containing a portion of the ore, which had been slightly agglutinated in the fire, but still highly magnetic. The upper surface of the present cake, close by the gate or feeder, contained a large pit filled with a stratum of semi-fused ore, surmounted by a mass of vitrified matter, which bore evident marks of containing calcareous earth.

Those who have devoted sufficient attention to the affinities of iron and earths for carbon, will be surprised to find that, on this particular subject, the rude fabricators of steel in Hindostan have got the start of our more polished countrymen in the manufacture of steel.

Two bars of wootz were formed from this cake, and these, in point of quality, inferior to any of those formerly produced. The appearance of the metal was more varied, less homogeneous, and contained more distinct laminæ with rusty surfaces than either of the two former cakes.

It appeared highly probable, from the observations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was such as would have formed a quality of steel softer than No. 1 and 2; but as steel of such softness requires a greater heat to fuse it than when more fully saturated with carbonaceous matter, it is probable that the furnace had not been sufficiently powerful to occasion complete fusion of the whole mass, and generate a steel homogeneous in all its parts.

Forging 4th Cake.

Both halves of this cake cut pleasantly, and with a degree of tenacity and resistance, mixed at the same time with softness

ness beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The resulting bars were nearly perfect. A slight scale was observable upon the bar from that quarter which contained the figure. The fracture was solid, though not homogeneous as to quality and colour, and it appeared pretty evident that a considerable portion of one side through the whole bar was in the state of malleable iron, and of course not capable of being hardened. It was a subject of considerable regret that the cake the most perfect and the most tenacious of the whole, in the process of forging, should get an imperfection which rendered it useless for the perfect purposes of steel.

Forging 5th Cake.

The first half of this cake cut uncommonly soft for wootz, but by cracking before the chisel still exhibited a want of proper tenacity. The next half cut equally soft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, sometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and scale, although the fracture exhibited a fair break of a light blue colour, and the grain was distinctly marked, and free from yolks.

General Remarks.

The formation of wootz appears to me to be in consequence of the fusion of a peculiar ore, perhaps calcareous, or rendered highly so by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other vessel or crucible, is equally presumable, in which the separated metal is allowed to cool; hence the crystallization that occupies the pits and cells found in and upon the under or rounded surface of the wootz cakes.

The want of homogeneity and of real solidity in almost every cake of wootz, appears to me to be a direct consequence of the want of heat sufficiently powerful to effect a perfect reduction; what strengthens this supposition much is, that those cakes that are the hardest, *i. e.* that contain the greatest quantity of carbonaceous matter, and of course form the most fusible steel, are always the most solid and homogeneous. On the contrary, those cakes into which the cutting chisel most easily finds its way, are in general

cellular,

cellular, replete with laminæ, and abound in veins of malleable iron.

It is probable, had the native of Hindostan the means of rendering his cast steel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality and convenient for those purposes to which it is applied.

The hammering, which is evident around the feeder and upon the upper surface in general, may thus be accounted for:—When the cake is taken from the pot or crucible, the feeder will most probably be slightly elevated, and the top of the cake partially covered with small masses of ore and steel iron, which the paucity of the heat had left either imperfectly separated or unfused. These most probably, to make the product more marketable, are cut off at a second heating, and the whole surface hammered smooth.

I have observed the same facts and similar appearances in operations of a like nature, and can account satisfactorily for it as follows:

The first portions of metal that are separated in experiments of this nature, contain the largest share of the whole carbon introduced into the mixture. It follows, of course, that an inferior degree of heat will maintain this portion of metal in a state of fluidity, but that a much higher temperature is requisite to reduce the particles of metal, thus for a season robbed of their carbon, and bring them into contact with the portion first rendered fluid, to receive their proportion of the steely principle. Where the heat is languid, the descent of the last portions of iron is sluggish, the mass below begins to lose its fluidity, while its disposition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic masses of various diameters, rising up for half an inch or more into the glass that covers the metal; these are neatly welded and inserted into each other, and diminish in diameter as they go up. The length or even the existence of this feeder or excrescence depends upon the heat in general, and upon its temperature at different periods of the same process. If there has been sufficient heat, the surface will be convex, and uniformly crystalline; but if the heat has been urged, after the feeder has been formed and an affinity established between it and the steelified mass below, it will only partially disappear in the latter, and the head or part of the upper end of the feeder will be found suspended in the glass that covers the steel.

The

The same or similar phænomena take place in separating crude iron from its ores, when highly carbonated, and difficult, from an excess of carbon, of being fused.

The division of the wootz cake by the manufacturers of Hindostan, I apprehend, is merely to facilitate its subsequent application to the purposes of the artist; it may serve at the same time as a test of the quality of the steel.

To ascertain, by direct experiment, whether wootz owed its hardness to an extra quantity of carbon, the following experiments were performed with various portions of wootz, of common cast steel and of white crude iron, premising that, in operations with iron and its ores, I have always found the comparative measure of carbon best ascertained by the quantity of lead which was reduced from flint glass.

1st Cake.

Fragments of wootz - - -	65 grs.
Pounded flint glass three times the weight	195

This mixture was exposed to a heat of 160° of Wedgewood, and the wootz fused into a well crystallized spherule of steel. A thin crust of revived lead was found below the wootz, which weighed 9 grains, or $\frac{138}{1000}$ the weight of the wootz.

2d Cake.

Fragments of wootz - - -	80 grs.
Flint glass, same proportion as above -	240

The fusion of the mixture in this experiment was productive of a mass of lead weighing 10 grains, equal to 1-8th the weight of the wootz.

3d Cake.

Fragments of wootz - -	75 grs.
Flint glass - - -	225

The mass of lead precipitated beneath the steel in this experiment, amounted to 9 grains, or $\frac{12}{100}$ the weight of the wootz employed.

4th Cake.

Fragments of wootz - -	93 grs.
Flint glass - - -	279

Lead obtained, precipitated from the glass by means of the carbon of the wootz, 14½ grains; equal to $\frac{156}{1000}$ the weight of the wootz.

5th Cake.

Fragments of wootz - -	69 grs.
Flint glass - - -	207

The

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{102}{1000}$ the weight of the wootz.

6th. Cast Steel formed with 1-60th Part its weight of Carbon.

Fragments	-	-	90 grs.
Crystal glass	-	-	270

Lead revived $8\frac{1}{2}$ grains, equal to $\frac{94}{1000}$ the weight of the steel introduced.

7th. White cast Iron dropt while Fluid into Water.

Fragments	-	-	103 grs.
Crystal glass	-	-	309

The fusion of this precipitated $23\frac{1}{2}$ grains of lead, which is equal to $\frac{228}{1000}$ the weight of the cast iron.

Recapitulation of these Experiments.

1st Cake of wootz revived of lead				-	·139 grs.
2d	ditto	-	-	-	·125
3d	ditto	-	-	-	·120
4th	ditto	-	-	-	·156
5th	ditto	-	-	-	·102
Steel containing 1-60th of its weight of carbon					·094
Cast iron	-	-	-	-	·228

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appears to me to be quite sufficient to account for its refractory nature, and unhomogeneous texture.

Notwithstanding the many imperfections with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

VII. *An*

VII. *An Essay on Medical Entomology. By* F. Chaumeton, *Physician to the Army.*

[Concluded from p. 351.]

Formica—*The Ant.*—These insects, on the pedicle of their abdomen, have a small vertical scale, a large head, small eyes, broken antennæ, and strong mandibles. Each species consists of three kinds; males and females, provided with long wings, and neutrals or labourers, who are destitute of them. The two last kinds have sharp retractile stings.

Ants live like bees in large societies. The government of both is founded on injustice, ingratitude, and barbarity. There is no difference but in the choice of the victims; in a bee-hive the males are banished or cut in pieces after they have given birth to a numerous family; in an ant-hill the females are cruelly expelled as soon as their eggs have been deposited*.

The strong penetrating emanations which escape from an ant-hill, have given reason to suspect that the insects which inhabit it possess medicinal properties, and this conjecture has often been verified by experience. A cataplasm of bruised ants, with their nymphæ, commonly called eggs, and a portion of their habitation, has been sometimes applied with success to limbs attacked with rheumatic pains, œdema, or palsy. The same epithem has increased the energy of the organs of generation. Baths rendered stimulating by the expressed juice of a large quantity of ants, have been found very efficacious in similar cases. The desire, no doubt, of having at all seasons a medicine proved to be so useful, gave rise to the invention of oil of ants. In regard to Hoffman and Kunrath's water of magnanimity, its pompous title was never justified by experience. It may be readily conceived, that in such compositions the virtue of the ants is altered or destroyed, when it is considered that it resides essentially in the acid, of which these insects furnish, by mere lixiviation or distillation, a quantity equal to half their weight.

The formic acid, diluted with water, is agreeable to the palate, and with the addition of a little sugar forms excellent lemonade. Ardrisson and Œhrne have proposed to substitute this acid in the room of vinegar for domestic

* I am of opinion, however, that these animals die a natural death, like the drones.

purposes. Alcohol, with which it perfectly mixes, ought, as we may say, to give it wings; to multiply its virtues, and in particular, to increase that of rousing the palled organs.

The formic acid unites so easily with alkalis, that, if a piece of linen imbibed with these bases be presented to an ant-hill, you will obtain formiates of soda, potash, or ammonia. I have strong reasons for believing that these two salts are preferable to the acetites of potash and ammonia; and if the formic acid has a great analogy to the phosphoric, as Thouvenel thinks, the formiate of soda would furnish the healing art with a gentle purgative, much cheaper than the phosphate.

ORDER VII.

APTERA.

Scorpio—*The Scorpion.*—The scorpion is distinguished by characters so striking that it is impossible not to know it on the first view. It has a long articulated tail, terminating in a sharp moveable hook, which to this animal is both a defensive and offensive weapon. Under its mandibles there are enormous feelers, terminating in pincers, like those of crabs. These feelers, which Spielman calls cheliform antennæ, are much longer than the feet. The latter are eight in number, as well as the eyes. At the posterior part of the breast also there are two dentated scales in the form of combs, the use of which has not yet been discovered*.

Scorpio Europæus.—The European scorpion lives like the other species in the warm or very temperate climates. It is common in Spain, in Italy, and in the south of France. Its length varies from one to two inches: its pincers are oval and angular: its combs have sixteen or eighteen teeth.

If the sting of the European scorpion were mortal, as has been asserted, it would be a powerful cause of depopulation in certain towns, in which I have seen this insect inhabiting without molestation the greater part of the houses, and infesting even the beds. We must not, therefore, give implicit belief to the tales circulated on this subject. I was stung at Lodi by a brown scorpion an inch in length, and of the size of a goose quill. The result was only a slight pain, and a superficial phlogosis, which was soon dissi-

* It is supposed that they serve as organs of respiration, and that they have an analogy to the branchiæ of the crustacea, for scorpions in their form approach to that order of animals.

pated.

pated*. Several soldiers were also stung in their lodgings, and at the military hospital, which swarmed with these animals. Some of the patients came to me with the insects still adhering to the part of the body which they had wounded: none of them experienced any accident more serious than I did. G. Fabbroni of Florence, G. Vasi of Rome, and G. L. Targioni of Naples, assured me that the sting of the scorpion is scarcely ever accompanied with alarming symptoms in these cities, though the temperature there is much warmer than in Lombardy. These imaginary dangers, however, have given rise to the preparation of oil of scorpions, which, notwithstanding the present improved state of science, is classed in several new works among the alexipharmacs: we must not, however, reject with contempt the testimony of Monardes, who pretends that he found benefit in the plague from liniments of this oil, as the horrid effects of that terrible scourge have often been prevented by frictions with olive oil †.

The Arabs injected oil of scorpions into the bladder to dissolve the stone, and it is needless to add that their attempts were always fruitless. The moderns have severely reproached the Arabians for their stupid confidence; but they have not been less credulous or more successful: we have seen them extol saponaceous and alkaline preparations, lime water, the interior bark of the lime tree, and, in particular, the trailing arbutus, *arbutus uva ursi*. This shrub has in turns heated the imagination of Barbeyrac, Quer, Girardi, Murray, de Haen, &c. The last does not hesitate to propose it as a real lithontriptic. But no one is ignorant that this man, so haughty and so passionately fond of fame, has not always impressed his writings with the seal of truth. If there exists a solvent of the stone in the bladder, it must be discovered by chemistry, and the philanthropic labours of Fourcroy seem already to hold out a consoling prospect. But still our hopes are very feeble, and it is much to be apprehended that the resources of medicine will be always confined to one infallible lithontriptic, namely cystotomy.

CANCER—*The Crab.*—The numerous species which constitute this genus have forms exceedingly various. Their

* I caused myself to be stung at Florence, and with the same result, in the presence of F. Fontana, who had been employed in analysing chemically the European Scorpion, and who had found in the juices of that insect an acid completely formed, and a gummy viscid matter, analogous to the poison of the viper.

† Desgenette's Hist. Med. de l'Armée d'Orient.

internal and external organization seems to remove them from all other insects except the monoculi, to which they have a great affinity. On this account several naturalists have united these two genera under the name of *crustacea*. But was it necessary to make of them a separate class, and will naturalists never become sensible of the inconvenience of those endless divisions which render zoology an inexplicable chaos?

Crabs have a head and breast confounded in one piece, which bears five pair of legs, the first of which terminate generally in pincers. The tail is of greater or less size, and formed of different articulations: their eyes are compounded, and supported by a moveable pedicle: they have for the most part four antennæ formed of threads, sometimes double or treble: the branchiæ are very complex: they have a muscular heart, from which proceed a great many vessels; a stomach supported by an osseous structure, and containing three hard stony pieces, which pound their aliments. The organs of generation are double in each sex, and have their exit at the bottom of one of the pairs of legs.

All crabs are aquatic, and change their shell every year: at the same time also they throw up the stones from their stomach*.

The cray-fish (*Cancer astacus*) is commonly served up at our tables.

The great horse crab (*Cancer pagurus*), the lobster (*Cancer gammarus*), the prawn (*Cancer serratus*), the white shrimp (*Cancer squilla*), and the spiny lobster (*Cancer homarus*), have a more delicate taste. They all furnish an abundant quantity of gelatine, which renders broth nutritive and detergent. It is by the first of these properties that it acts in phthisis: it develops the second in cutaneous diseases, and Pinel recommends it in leprosy†. I am of opinion with Bichat, that, notwithstanding the sarcasms sometimes just thrown out against the humoral medicine, it rests on a real foundation; and that in a multitude of cases every thing ought to be referred to a vitiated state of the humours‡.

At the period when the cray-fish casts its shell, to assume a new one, there are found on the sides of its stomach, between the membranes of that viscus, two calcareous concretions, which are employed as absorbents,

* Cuvier Tab. Element. de l'Hist. Nat. des Anim. p. 456.
† Nosogr. Philos. deux. edit. n. 858.
‡ Anat. Gener. part I. p. 256.

under

under the ridiculous denomination of crab's eyes. A similar virtue is ascribed to the pincers of the larger species, *Cancer pagurus*, *gammarus*, &c. These inert and hurtful substances have, however, been long since proscribed by sound chemistry. Were it proved that a morbific affection was produced by the presence of an acid in the primary or secondary passages, pure and not carbonated magnesia would be the most proper remedy.

Oniscus—*The Wood-louse.*—The body of this insect is oval, and formed of articulated segments, the first seven of which have each a pair of legs; their eyes are compounded and fixed, and their antennæ setaceous.

The common wood-louse, *Oniscus asellus*, the tail of which is terminated by two filiform appendages, or the armadillo, which is indebted for its denomination to the property it has of folding itself double on the least danger, is the one chosen for medicinal purposes.

The trials made by Lister, Neumann, and Cartheuser, to determine the constituent principles of wood-lice, were only rude sketches. Thouvenel threw some light on the chemical analyses of these insects, and on the use of them in the art of healing. When distilled alone in a balneum mariæ, they gave a water sufficiently alkaline to render syrup of violets greenish. Being then treated with water and alcohol, they furnished a fourth part of their weight of extractive and cirous matter, which ether separated from each other, dissolving the latter without touching the former. The expressed juice of wood-lice contains muriates of lime and of potash, in which reside the dissolving and aperient qualities, which cannot be refused to these insects. Dioscorides and Entmuller extol the efficacy of them in obstructions of the abdominal viscera. Riviere has confirmed the utility of them in arthritic affections, foul ulcers, and tumors of the breasts of women. They are killed by the steam of alcohol, and are then dried and pulverised. This preparation is superfluous, and even prejudicial, when these insects are employed in disorders of the breast. I have observed the good effects of the juice of these insects in two chronic pulmonary catarrhs. The preparation I prescribed in spoonfuls was as follows:

℞. Infusion of creeping ivy five ounces.
Expressed juice of 150 wood-lice, bruised alive.
Refined sugar, pulverised, one ounce.
Syrup of poppies one ounce.
Ethereous sulphuric alcohol twelve drops.

VIII. *Short Account of Travels between the Tropics, by Messrs.* HUMBOLDT *and* BONPLAND, *in* 1799, 1800, 1801, 1802, 1803, *and* 1804. *By* J. C. DELAMETHERIE.

[Concluded from p. 362.]

DURING his residence at Quito M. Humboldt received a letter from the French National Institute, informing him that captain Baudin had set out for New Holland, pursuing an easterly course by the Cape of Good Hope. He found it necessary therefore to give up all idea of joining him, though our travellers had entertained this hope for thirteen months, by which means they lost the advantage of an easy passage from the Havannah to Mexico and the Philippines. It had made them travel by sea and by land more than a thousand leagues to the south, exposed to every extreme of temperature, from summits covered with perpetual snow to the bottom of those profound ravines where the thermometer stands night and day between 25° and 31° of Reaumur. But, accustomed to disappointments of every kind, they readily consoled themselves on account of their fate. They were once more sensible that man must depend only on what can be produced by his own energy; and Baudin's voyage, or rather the false intelligence of the direction he had taken, made them traverse immense countries towards which no naturalist perhaps would otherwise have turned his researches. M. Humboldt being then resolved to pursue his own expedition, proceeded from Quito towards the river Amazon and Lima, with a view of making the important observation of the transit of Mercury over the sun's disk.

Our travellers first visited the ruins of Lactacunga, Hambato, and Riobamba, a district convulsed by the dreadful earthquake of the year 1797. They passed through the snows of Assonay to Cuenca, and thence with great difficulty, on account of the carriage of their instruments and packages of plants, by the paramo of Saraguro to Loxa. It was here, in the forests of Gonzanama and Malacates, that they studied the valuable tree which first made known to man the febrifuge qualities of cinchona. The extent of the territory which their travels embraced, gave them an advantage never before enjoyed by any botanist, namely, that of comparing the different kinds of cinchona of Santa Fé, Popayan, Cuenca, Loxa, and Jaen, with the *cuspa* and *cuspare* of Cumana and Rio Carony, the latter of which, named improperly *Cortex angusturæ*, appears to belong

belong to a new genus of the *pentandria monogynia*, with alternate leaves.

From Loxa they entered Peru by Ayavaca and Gounca-bamba, traversing the high summit of the Andes, to proceed to the river Amazon. They had to pass thirty-five times in the course of two days the river Chamaya, sometimes on a raft, and sometimes by fording. They saw the superb remains of the causeway of Ynga, which may be compared to the most beautiful causeways in France and Spain, and which proceeds on the porphyritic ridge of the Andes, from Cusco to Assonay, and is furnished with *tambo* (inns) and public fountains. They then embarked on a raft of *ochroma*, at the small Indian village of Chamaya, and descended by the river of the same name, to that of the Amazons, determining by the culmination of several stars, and by the difference of time, the astronomical position of that confluence.

La Condamine, when he returned from Quito to Para and to France, embarked on the river Amazon only below Quebrada de Chucunga; he therefore observed the longitude only at the mouth of the Rio Napo. M. Humboldt endeavoured to supply this deficiency in the beautiful chart of the French astronomer, navigating the river Amazon as far as the cataracts of Rentema, and forming at Tomependa, the capital of the province of Jaen de Bracamorros, a detailed plan of that unknown part of the Upper Maranon, both from his own observations and the information obtained from Indian travellers. M. Bonpland in the mean time made an interesting excursion to the forests around the town of Jaen, where he discovered new species of cinchona; and after greatly suffering from the scorching heat of these solitary districts, and admiring a vegetation rich in new species of *Jacquinia*, *Godoya*, *Porteria*, *Bougainvillea*, *Colletia*, and *Pisonia*, our three travellers crossed for the fifth time the cordillera of the Andes by Montan, in order to return to Peru.

They fixed the point where Borda's compass indicated the zero of the magnetic inclination, though at 7 degrees of south latitude. They examined the mines of Hualguayoc, where native silver is found in large masses at the height of 2000 toises above the level of the sea, in mines, some metalliferous veins of which contain petrified shells, and which, with those of Huantajayo, are at present the richest of Peru. From Caxamarca, celebrated by its thermal waters, and by the ruins of the palace of Atahualpa, they descended to Truxillo, in the neighbourhood of which are

 found

found vestiges of the immense Peruvian city of Mansiche, ornamented with pyramids, in one of which was discovered, in the eighteenth century, hammered gold to the value of more than 150,000l. sterling.

On this western declivity of the Andes our travellers enjoyed, for the first time, the striking view of the Pacific Ocean; and from that long and narrow valley, the inhabitants of which are unacquainted with rain or thunder, and where, under a happy climate, the most absolute power, and that most dangerous to man, theocracy itself, seems to imitate the beneficence of nature.

From Truxillo they followed the dry coasts of the South Sea, formerly watered and rendered fertile by the canals of the Ynga; nothing of which remains but melancholy ruins. When they arrived, by Santa and Guarmey, at Lima, they remained some months in that interesting capital of Peru, the inhabitants of which are distinguished by the vivacity of their genius and the liberality of their sentiments. M. Humboldt had the happiness of observing, in a pretty complete manner, at the port of Callao at Lima, the end of the transit of Mercury: a circumstance the more fortunate, as the thick fog which prevails at that season often prevents the sun's disk from being seen for twenty days. He was astonished to find in Peru, at so immense a distance from Europe, the newest literary productions in chemistry, mathematics, and physiology; and he admired the great intellectual activity of a people whom the Europeans accuse of indolence and luxury.

In the month of January 1803 our travellers embarked in the king's corvette La Castora for Guyaquil; a passage which is performed, by the help of the winds and the currents, in three or four days, whereas the return from Guyaquil requires as many months. In the former port, situated on the banks of an immense river, the vegetation of which in palms, *plumeria*, *tabernæmontana*, and *scitamineæ*, is majestic beyond all description. They heard growling every moment the volcano of Catopaxi, which made a dreadful explosion on the 6th of January 1803.

They immediately set out that they might have a nearer view of its ravages, and to visit it a second time; but the unexpected news of the sudden departure of the Atlanta frigate, and the fear of not finding another opportunity for several months, obliged them to return, after being tormented for seven days by the mosquitoes of Babaoyo and Ugibar.

They had a favourable navigation of thirty days on the Pacific

Pacific Ocean to Acapulco, the western port of the kingdom of New Spain, celebrated by the beauty of its bason, which appears to have been cut out in the granite rocks by the violence of earthquakes; celebrated also by the wretchedness of its inhabitants, who see there millions of piastres embarked for the Philippines and China; and unfortunately celebrated by a climate as scorching as mortal.

M. Humboldt intended at first to stay only a few months in Mexico, and to hasten his return to Europe; his travels had already been too long; the instruments, and particularly the time-keepers, began to be gradually deranged; and all the efforts he had made to get new ones had proved fruitless. Besides, the progress of the sciences in Europe is so rapid, that in travels of more than four years a traveller may see certain phænomena under points of view which are no longer interesting when his labours are presented to the public.

M. Humboldt flattered himself with the hope of being in England in the months of August or September 1803; but the attraction of a country so beautiful and so variegated as the kingdom of New Spain, the great hospitality of its inhabitants, and the dread of the yellow fever at Vera Cruz, which cuts off almost all those who between the months of June and October come down from the mountains, induced him to defer his departure till the middle of winter. After having occupied his attention with plants, the state of the air, the hourly variations of the barometer, the phænomena of the magnet, and in particular the longitude of Acapulco, a port in which two able astronomers, Messrs. Espinosa and Galeano, had before made observations, our travellers set out for Mexico. They ascended gradually from the scorching valleys of Mescala and Papagayo, where the thermometer in the shade stood at 32° of Reaumur, and where they passed the river on the fruit of the *crescentia pinnata*, bound together by ropes of agave, to the high plateaux of Chilpantzingo, Tehuilotepec and Tasco.

At these heights of six or seven hundred toises above the level of the sea, in consequence of the mildness and coolness of the climate, the oak, cypress, fir, and fern, begin to be seen, together with the kinds of grain cultivated in Europe.

Having spent some time in the mines of Tasco, the oldest and formerly the richest in the kingdom, and having studied the nature of those silvery veins which pass from the hard calcareous rock to micaceous schist and inclose foliaceous gypsum, they ascended, by Cuernaraca and the cold regions of of Guchilaqua, to the capital of Mexico. This city, which has 150,000 inhabitants, and stands on the site of the old Tenochtitlan, between the lakes of Tezcuco and Xochimilo, which have decreased in size since the Spaniards, to lessen the danger of inundations, have opened the mountains of Sincoc, is intersected by broad straight streets. It stands in sight of two snowy mountains, one of which is named Popocatepec; and of a volcano still burning; and, at the height of 1160 toises, enjoys a temperate and agreeable climate: it is surrounded by canals, walks bordered with trees, a multitude of Indian hamlets, and without doubt may be compared to the finest cities of Europe. It is distinguished also by its large scientific establishments, which may vie with several of the old continent, and to which there are none similar in the new.

The botanical garden, directed by that excellent botanist M. Cervantes; the expedition of M. Sesse, who is accompanied by able draftsmen, and whose object is to acquire a knowledge of the plants of Mexico; the School of Mines, established by the liberality of the corps of miners and by the creative genius of M. d'Elhuyar; and the Academy of Painting, Engraving, and Sculpture; all tend to diffuse taste and knowledge in a country the riches of which seem to oppose intellectual culture.

With instruments taken from the excellent collection of the School of Mines, M. Humboldt determined the longitude of Mexico, in which there was an error of nearly two degrees, as has been confirmed by corresponding observations of the satellites made at the Havannah.

After a stay of some months in that capital, our travellers visited the celebrated mines of Moran and Real-del-Monte, where the vein of La Biscayna has given millions of piastres to the counts De Regla; they examined the obsidian stones of Oyamel, which form strata in the pearl stone and porphyry, and served as knives to the antient Mexicans. The whole of this country, filled with basaltes, amygdaloids, and calcareous and secondary formations, from the large cavern of Danto, traversed by a river to the porphyritic rocks of Actopan, presents phænomena interesting to the geologue, which have been already examined by M. del Rio, the pupil of Werner, and one of the most learned mineralogists of the present day.

On their return from their excursion to Moran in July 1803, they undertook another to the northern part of the kingdom. At first they directed their researches to Huehuetoca, where, at the expense of six millions of piastres, an an aperture has been formed in the mountain of Sincoc to drain off the waters from the valley of Mexico to the river Montezuma. They then passed Queretaro, by Salamanca and the fertile plains of Yrapuato, to Guanaxuato, a town which contains 50,000 inhabitants: it is situated in a narrow defile, and celebrated by its mines, which are of far greater consequence than those of Potosi.

The mine of count de Valenciana, which has given birth to a considerable town on a hill which thirty years ago scarcely afforded pasture to goats, is already 1840 feet in perpendicular depth. It is the deepest and richest in the world; the annual profit of the proprietors having never been less than three millions of livres, and it sometimes amounts to five or six.

After two months employed in measurements and geological researches, and after having examined the thermal waters of Comagillas, the temperature of which is 11° of Reaumur higher than those of the Philippine islands, which Sonnerat considers as the hottest in the world, our travellers proceeded through the valley of St. Jago, where they thought they saw in several lakes at the summits of the basaltic mountains so many craters of burnt-out volcanoes, to Valladolid, the capital of the antient kingdom of Michoacan. They thence descended, notwithstanding the continual autumnal rains, by Patzquaro, situated on the margin of a very extensive lake towards the coast of the Pacific Ocean, to the plains of Jorullo, where in the course of one night in 1759, during one of the greatest convulsions which the globe ever experienced, there issued from the earth a volcano 1494 feet in height, surrounded by more than 2000 mouths still emitting smoke. They descended into the burning crater of the great volcano to the perpendicular depth of 258 feet, jumping over fissures which exhaled flaming sulphurated hydrogen gas. After great danger, arising from the brittleness of the basaltic and sienitic lava, they reached nearly the bottom of the crater, and analysed the air in it, which was found to be surcharged in an extraordinary manner with carbonic acid.

From the kingdom of Michoacan, one of the most agreeable and most fertile countries in the Indies, they returned to Mexico by the high plateau of Tolucca, in which they measured the snowy mountain of the same name, ascending to its highest summit, the peak of Fraile, which rises 2364 toises above the level of the sea: they visited also at Tolucca the famous hand-tree the *cheiranthostæmon* of M. Cervantes, a genus which presents a phænomenon almost unique,

unique,—that of there being only one individual of it, which has existed since the remotest antiquity.

On their return to the capital of Mexico they remained there several months to arrange their herbals, abundant in gramineous plants, and their geological collections; to calculate their barometric and trigonometrical measurements performed in the course of that year; and in particular to make fair drawings of the geological atlas, which M. Humboldt proposes to publish.

Their return furnished them also with an opportunity of assisting at the erection of the colossal equestrian statue of the king, which one artist, M. Tolsa, overcoming difficulties of which a proper idea cannot be formed in Europe, modelled, cast, and erected on a very high pedestal: it is wrought in the simplest style, and would be an ornament to the finest capitals in Europe.

In January 1804 our travellers left Mexico to explore the eastern declivity of the cordillera of New Spain: they measured geometrically the two volcanoes of Puebla, Popocatepec and Itzaccihuatl. According to a fabulous tradition, Diego Ordaz entered the inaccessible crater of the former suspended by ropes, in order to collect sulphur, which may be found every where in the plains.

M. Humboldt discovered that the volcano of Popocatepec, on which M. Sonnenschmidt, a zealous mineralogist, had the courage to ascend 2557 toises, is higher than the peak of Orizaba, which has hitherto been considered the highest colossus of the country of Anahuac: he measured also the great pyramid of Cholula, a mysterious work constructed of unbaked brick by the Tultequas, and from the summit of which there is a most beautiful view over the snowy summits and smiling plains of Tlaxcala.

After these researches they descended by Perote to Xalapa, a town situated at the height of 674 toises above the level of the sea, at a mean height at which the inhabitants enjoy the fruits of all climates, and a temperature equally mild and beneficial to the health of man. It was here that, by the kindness of Mr. Thomas Murphy, a respectable individual, who to a large fortune adds a taste for the sciences, our travellers found every facility imaginable for performing their operations in the neighbouring mountains.

The level of the horrid road which leads from Xalapa to Perote, through almost impenetrable forests of oaks and firs, and which has begun to be converted into a magnificent causeway, was three times taken with the barometer. M. Humboldt, notwithstanding the quantity of snow which

 had

had fallen the evening before, ascended to the summit of the famous Cofre, which is 162 toises higher than the peak of Teneriffe, and fixed its position by direct observations. He measured also trigonometrically the peak of Orizava, which the Indians call Sitlaltepetl, because the luminous exhalations of its crater resemble at a distance a falling star, and respecting the longitude of which M. Ferrer published very exact operations.

After an interesting residence in these countries, where, under the shade of the *liquidambar* and *amyris*, are found growing the *epidendrum vanilla* and *convolvulus jalappa*, two productions equally valuable for exportation, our travellers descended towards the coast of Vera Cruz, situated between hills of shifting sand, the reverberation of which causes a suffocating heat; but happily escaped the yellow fever, which prevailed there at the time.

They proceeded in a Spanish frigate to the Havannah to get the collections and herbals left there in 1800, and, after a stay of two months, embarked for the United States: but they were exposed to great danger in the channel of the Bahamas from a hurricane which lasted seven days.

After a passage of thirty-two days they arrived at Philadelphia; remained in that city and in Washington two months; and returned to Europe in August 1804 by the way of Bourdeaux with a great number of drawings, thirty-five boxes of collections, and 6000 species of plants.

IX. *An Account of Sutton Spa, near Shrewsbury.* By *Dr.* Evans*.

Sutton Spa is situated within two miles south of Shrewsbury, on the slope of a gentle eminence, and close to a village of the same name, the property of the right honourable lord Berwick. The spring issues from a rocky stratum of ash-coloured clay, or argillaceous schistus, containing (as appears by its effervescence with nitrous acid) a small portion of lime; and, in its present unimproved state, yields but a scanty stream. In the neighbourhood are several beds of soft limestone and coal, the latter mineral accompanying nearly the whole course of Meole-brook. In the Sutton pits it is mixed with so large a proportion of pyrites, or sulphuret of iron, as to be used only for inferior purposes.

* This is a continuation of Mr. Plymley's Account of the Mineral Productions of Shropshire.—See our last Number, p. 304.

Fresh

Fresh from the spring, the Sutton water is clear and colourless, and exhales a slightly sulphureous smell; which is most perceptible in rainy weather*. It sparkles little when poured into a glass, having no uncombined carbonic acid in its composition. When first drawn, its strong salt taste is evidently mixed with a chalybeate flavour; but the latter is wholly lost on exposure for a few hours, bubbles of air separating slowly, and a reddish sediment lining the sides and bottom of the vessel.

Its temperature, the thermometer in the open air standing at about 70°, varied from 53° to 55° of Fahrenheit; but at another time, the thermometer in the air being at 55°, sunk as low as 48°.

The infusions of litmus and red cabbage were not reddened by it when fresh, nor greened after boiling, or long exposure; showing the absence of any disengaged acid or alkali, or of any material portion of earthy carbonate.

When fresh, it instantly struck a reddish purple with tincture of galls; but no change was produced when it had been boiled a few minutes, or exposed some time to the atmosphere. The former circumstance clearly proves the presence of iron, and the latter, that it was wholly held in solution by a fugacious acid.

Mixed with lime water it deposited a reddish sediment of the carbonate of lime and iron oxide; and with caustic ammonia, a reddish cloud, formed by the above metal, with probably a small portion of magnesia.

The saccharine or oxalic acid threw down a large and immediate precipitate of oxalited lime. With mild kali, a copious dirty sediment was instantly formed of earth and iron, separated from their acids by the superior affinity of the alkali.

The marine and nitrous acids produced no change, or rendered it more transparent; but a few drops of sulphuric acid produced instantly a copious deposition of selenite.

Muriated barytes did not show the presence of any sulphuric compound.

A solution of acetited lead caused an immediate milkiness in the water, which, however, became perfectly transparent on the addition of distilled vinegar. This also proves the absence of any sulphuric acid, which would have formed with the lead an insoluble compound. Characters traced

* The decomposition of pyrites, and consequent evolution of hepatic air, or sulphurated hydrogen gas, being in proportion to the quantity of water present.

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on paper with the above solution, and exposed to the vapour of the fresh water, became visible, of a light brown colour, by the action of hepatic air or sulphurated hydrogen gas. Some silver leaf exposed to its vapour became faintly yellow, and a globule of bright quicksilver was slightly tarnished. But a solution of nitrated silver produced merely a white precipitate, turning blue on exposure to light; the usual effect of the muriatic acid; the quantity, therefore, of hepatic air is probably very small.

A portion of the water, evaporated slowly, formed beautiful cubic crystals, which, with a drop of sulphuric acid, gave out the peculiar smell and gray fumes of the muriatic acid.

From a gallon of the water fresh from the well, were obtained eight ounces, or about 14.$\frac{440}{1000}$ cubic inches of volatile contents, of which about one-eighth part extinguished flame, precipitated lime water, and was evidently carbonic acid. On applying to the remainder (which was not soluble in water by repeated agitation) the flame of a candle, a slight combustion took place, and afterwards the flame burnt nearly as well as in common air. One measure of this air, mixed with an equal bulk of nitrous air in an accurate eudiometer, was diminished from 200 to 1.$\frac{350}{1000}$, while the common air of the room, added to nitrous air in the same proportion, was lowered to 1.$\frac{100}{1000}$, its purity being in a direct ratio to the degree of diminution. It was therefore atmospheric air, with a slight mixture of sulphurated hydrogen gas, and somewhat more than the usual portion of azote, as is always the case with the air of chalybeate waters.

A wine gallon of the fresh water being evaporated to dryness, there remained of residuum 2 ounces 3 drachms, or 1320 grains; this, mixed with 12 ounces of cold distilled water, left on the filter 12 grains of a reddish brown sediment.

Caustic ammonia added to the clear solution did not precipitate any magnesia worth collecting; but mild kali threw down a copious sediment of aërated lime, which, on exsiccation, weighed 206 grains; and must, according to Bergman and Kirwan, have been produced from 226 grains of muriated lime, the former containing $\frac{55}{100}$ parts of pure lime, and the latter $\frac{20}{100}$ of muriatic acid.

Subtracting this, and the twelve grains of brown residuum, from the whole, there remain in a gallon of the water 1082 grains of muriate of soda or common salt. The 12 grains of residuum, treated with diluted marine acid,

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and again precipitated by caustic ammonia, produced nearly half a grain of iron oxide, which, fused with charcoal by the blowpipe, was strongly attracted by the magnet, and assumed some degree of metallic lustre. The remaining 11½ grains, insoluble in the acid, evidently consisted of clay, mixed, as appeared in a magnifier, with several crystals of silex, which were probably merely suspended, and not in a state of solution, in the water.

	Grains.	
We have then, in a wine gallon of the Sutton water, of muriate of soda - -	1802	0
Muriate of lime, with an admixture of muriated magnesia - - - - -	226	0
Carbonate of iron - - -	0	5
Clay and silex - - -	11	5
Total of solid contents	1320	0

	Cubic Inches.	
Carbonic acid - - -	1	805
Common air contaminated with azote and sulphurated hydrogen gas - -	12	635
Total of volatile contents	14	440

This, in common with most mineral waters, varies not a little in the quantity and proportions of its ingredients at different seasons and in different states of the atmosphere. At one time, the caustic ammonia produced no effect on the water; at another, it deposited a very considerable portion of magnesia; and the hepatic smell is sometimes not in the least perceptible, particularly in dry weather*. But, however provoking these variations may be to the accurate chemist, they are luckily of little moment to the practical physician. A considerable latitude, in this respect, makes no material difference in the medicinal effects; much more depending on the quantity of the water, as a diluent and detergent, than on any other circumstance.

I am greatly indebted to my ingenious friend Mr. Dugard, house-surgeon to the Salop Infirmary, for his kind assistance in making the above analysis.

* In the present exposed state of the opening of the well, this must particularly happen, rainy weather weakening the saline impregnation, and a warm air exhaling the gases and precipitating the iron. The well known effect of salts in operating more powerfully as they are the more diluted, compensates for the diminished strength of the solution.

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The Sutton water has by many been compared with that of Cheltenham, and supposed to contain nearly the same ingredients. It bears, however, a much closer resemblance to sea water, as will be evident from the annexed table*.

We have accordingly found it most beneficial in those cases for which sea water is usually recommended. It yields the same salutary stimulus to the stomach in chronic weakness of that organ, and obviates, both mildly and effectually, the habitual costiveness of hypochondriac patients. Diluted in a large portion of liquid, the saline ingredients serve to wash out any acrid sordes collected in the first passages, which I believe to be one of the most important uses of mineral waters. On no other principle can we explain the uniform good effects produced on the digestive organs by waters so various in the nature and proportion of their contents, and in the degree of their impregnation.

Conveyed by the lacteals into the mass of circulating fluids, and thence through the different secretory organs, this water has proved highly serviceable in a great variety of glandular affections; and, being disposed to pass off with the finer parts of the blood, promotes the excretions of urine or perspiration, according to the attendant circumstances of clothing, temperature of the air, &c. Absorbed by the lymphatics, the acrid muriates stimulate the torpid vessels, and wash out any acrimony accumulated in consequence of that inirritability. We hence may readily explain their efficacy in those disorders to which the poorer classes

	Carbonate of Iron.	Selenite.	Muriate of Soda.	Muriate of Lime.	Muriate of Magnesia.	Sulphate of Lime and Magnesia.
* In a wine gallon of Cheltenham water, as analysed by Dr. Fothergill.	5	40	5	0	25	480
Sutton water.	½	—	1082	226	—	—
Sea water, taken up 60 fathoms deep, in latitude of the Canaries, and analysed by Bergman.	—	45	1928	—	524	—

It must here be observed, that the sea, being more strongly impregnated with salt in proportion to the warmth of the climate, contains on our coasts not more than 1-30th its weight, instead of 1-23d of saline contents.

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are particularly liable, chronic diseases of the skin, and scrophula. In the former, which have so improperly been termed *scorbutic* *, this water has been found a very valuable remedy, both externally and internally applied.

In the cure of scrophula, the superior merits of sea water, first introduced to public notice by Dr. Russel, have ever since been uniformly and universally acknowledged †. A similarity of ingredients would naturally lead us to expect similar effects from the Sutton water; and I am happy to bear testimony, that a twenty years' attendance at the Salop Infirmary, as well as in private practice, has furnished me with abundant proofs of its success in the treatment of scrophulous affections. Yet I will frankly own, that in this deceitful, and, I fear, increasing malady, the effects of medicine are frequently but too fallacious. At certain seasons of the year, and particular periods of life, the symptoms will subside spontaneously, and the credit due to Nature be given to the remedy last employed.

In addition to the above properties possessed by the Sutton spring in common with sea water, it enjoys one evident advantage, in containing iron. Though the minute portion of it, in this and many other mineral springs, may be thought inadequate to any useful purpose,—experience, our surest guide, has amply proved the contrary; and it is now well ascertained, that small and repeated doses of this valuable metal produce far more beneficial and permanent effects on the constitution than the much larger ones formerly prescribed.

It has been variously administered, either as a calx or oxide, or as already combined with an acid solvent; but in no form has it proved so uniformly efficacious as when prepared by Nature herself, and existing as a carbonate in chalybeate springs. Winged, as it were, by its aërial acid, it pervades the remotest vessels and minutest capillaries of the system, invigorating every fibre, and rendering evacuation by the saline ingredients both safe and salutary. Of the external use of this water as a cold or tepid bath, I can speak from theory alone, no such conveniences having been

* I say improperly, because the real scurvy is connected with a state of the system the most opposite of any to the diseases here mentioned.

† It is worthy of remark, that almost every popular remedy for this complaint contains the marine acid in its composition. At one time, the muriated barytes was in high celebrity, and now the muriated lime is rising into equal credit. I have often prescribed with success, the tinct. ferr. muriat. as combining the tonic powers of iron with the stimulus of the acid; but I think I have found the simple marine acid full as efficacious as any of its combinations.

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as yet provided. But analogy fully warrants us to suppose it nearly as powerful as sea water, and applicable to the same useful purposes. In cutaneous foulnesses, in scrophula, in chronic rheumatism, paralytic affections, and, above all, in the cachexies of young females, attended with uterine obstructions, we have every reason to expect the greatest advantages from its applications as a warm or tepid bath.

This being by no means intended as a medical communication, I have given merely a faint sketch of the virtues to be expected from the Sutton water. The outline may be readily filled up hereafter, whenever the improved state of the Spa shall require a more accurate description of its best modes of exhibition, and the diseases to which it is applicable.

Though its value has been long felt and acknowledged by the immediate neighbourhood, the spring still remains in a very rude and neglected state; an iron spout attached to a piece of wood stretched across the opening, forming the only channel for the water's exit. Entangled in its passage to the brook below, it has produced an artificial morass, its surface being abundantly covered with ochery scum, from the deposition of iron oxide. Some time since, it was held in contemplation by the noble proprietor to erect baths, &c. for the accommodation of invalids; but the military avocations of the day postponed the truly benevolent institution. With the return of the blessings of peace, it will, I trust, be resumed, and an inestimable benefit thereby conferred on an extensive and populous district. The morass might easily be drained by channels communicating with the brook; and baths erected on this site would have both a constant supply from the well, and a regular discharge of the refuse water.

While almost every fishing village on the coast is preparing conveniences for sea bathing, how desirable would it be to extend similar advantages to the interior parts of the island, where poverty or infirmity renders it impossible to visit the distant sea! In this county, abounding in minerals, whose subterraneous wealth is beyond all calculation, there is probably scarce a parish that would not supply a mineral water for the benefit of the neighbouring poor, were the springs properly examined.

The air of Sutton, as might be expected from its open elevated situation, is dry and wholesome. The site commands a rich and highly variegated prospect; bounded on one side by the magnificent group of Freyddin and

 Moel

Moel y Golfa, with a long range of Welsh mountains rising in full majesty beyond them; and on the other, by their no mean rivals, the Wrekin and Stretton hills. The view of Shrewsbury, betwixt the branches of the adjoining wood, particularly when the setting sun gilds every object with his mellowest light, is greatly and most deservedly admired. The walk from Shrewsbury is pleasant and picturesque; and the neighbourhood of a reasonable and abundant market can be considered as no trifling object, when compared with the extravagant prices and scanty accommodations of many of our remote watering places.

X. *On the Blight or Mildew of Wheat**.

But the most remarkable effect of the seasons of the present year (1800) is that of wheat being, in particular situations, injured by blight or mildew—in a dry summer. In this district (the Vale of Exeter) many fine looking crops were, in a manner, cut off by this malady: the straw becoming black as soot, and the grain shrivelled and light. In one instance which I particularly attended to, it was barely worth the labour of thrashing out; even at the present prices! owing, however, in some considerable degree, I apprehend, to the imprudence of the grower, who suffered it to stand to ripen after the blight had seized it; while a more judicious manager in this quarter of the county †, by cutting his wheat as soon as he perceived it to be struck with the disease, preserved it, he believes, from material injury. This precaution, however, it is very probable, ninety-nine growers in a hundred did not take: and the country may have lost, in the most alarming hour of scarcity, some hundred thousand quarters of wheat by this one defect in English agriculture ‡!

1804. A similar, but more universal effect took place this summer, which has likewise been characterized by dryness, at least in those parts of the island in which my observations have been made.

On my return from South Wales to London, early in this September, wheat crops evidently appeared, by the dark

* From Mr. Marshall's new edition of the *Rural Economy of the West of England.*

† Mr. Smith, of Axminster.

‡ See the Rural Economy of Gloucestershire, for remarks on this important point of management.

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hue of their straw, or their stubbles, to have been more or less blighted; excepting in a few instances in Gloucestershire, and others in Oxfordshire, in which instances only strong, yellow, healthy stubbles were observable.

The cause of the disease in the county in which I had the best opportunity of observing it (Caermarthenshire), appeared, very evidently, to proceed from some cold rains which fell about the middle of August. Before that time wheat crops in general looked healthy, and were beginning to change to a bright colour. But presently after a few cold wet days the malady became obvious to the naked eye. The straw lost its smooth varnished surface, being occupied by innumerable specks, which changed in a few days, in less than a week, to a dark or blackish colour, giving the straw a dusky appearance*.

A gentleman of Caermarthenshire, who is attentive to agricultural concerns, is of opinion that this destructive disease may be prevented by sowing old seed; namely, wheat of the preceding year's growth, instead of new wheat, agreeably to the practice of the Cotswold Hills of Gloucestershire. I am much inclined to think, that by sowing early, agreeably to that practice (see Gloucestershire, II. 51.), this fatal disease might frequently be avoided, early ripe crops being, from all the observations that I have hitherto made, the least subject to its baleful effect. Corn which ripens under the hot summer sun of July, is not so liable to cold chilling rains as that which remains unmatured until the sun begins to lose its power, and the nights to increase in length and coolness.

A certain preventive of this disaster would be a discovery worth millions to the country. Until this be made, let the grower of wheat not only endeavour to sow early, but let him look narrowly to his crop during the critical time of the filling of the grain; and whenever he may perceive it to be smitten with the disease, let him lose no time in cutting it, suffering it to lie on the stubble until the straw be firm and crisp enough to be set up in sheaves, without adhering in the binding places;—allowing it to remain in the field, until the grain shall have received the nutriment which the straw may be able to impart. Where wheat has been grown on "lammas land," and the ground

* Devonshire had its rains in the ripening season of 1800. A third instance of the blight of wheat succeeding rain, was observed in the same county, in 1794. And a fourth was equally obvious, in 1785, in the Midland Counties, as may be seen in the Rural Economy of that department, minute 74.

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obliged to be cleared by the first of August, crops have been known to be cut "as green as grass," and to be carried off and spread upon grass land to dry. Yet the grain has been found to mature, and always to afford a fine-skinned beautiful sample. Raygrass that is cut even while in blossom, is well known to mature its seeds with the sap that is lodged in the stems. Hence there is nothing to fear from cutting wheat or other corn before the straw be ripe.

1805. *April.* That the operation of this disease is carried on by the fungus tribe, evidently appears from the ingenious and persevering labours of botanists*. But fungi, it is equally evident, are an *effect,* not the *cause* of the disease. They are the vermin of the more perfect vegetables; and fasten on them, whether in a dead or in a diseased state; but seldom, I believe, while they are in full health and vigour. Their minute and volatile seeds may be said to be every where present—ready to produce their kind wherever they may find a genial matrix. Such at least appears to be the nature of the fungus, or fungi, of wheat; for it may be liable to the attack of more than one species. In a dry warm summer, which is well known to be favourable to the health, vigour, and productiveness of the wheat crop, the seeds of fungi are harmless, so long as the fine weather continues. On the contrary, in a cold wet season, which gives languor and weakness to the wheat plants, few crops escape entirely their destructive effects. A standing crop not unfrequently escapes, while plots that are lodged in the same field, especially in pits and hollow places, become liable to their attack. And by the facts above stated, we plainly see, that even strong healthy crops may, in a few days, or perhaps in a few hours, be rendered liable to be assailed—not progressively, as by an infectious disease, but at once, as by a blast or blight. In the state of the atmosphere we are to look for the cause of the disease in a standing crop; and nothing is so likely to bring on the fatal predisposition of the plants as a succession of cold rains while the grain is forming. The coolness necessarily gives a check to the rich saccharine juices which are then rising towards the ear; and the moisture may, at the same time, assist the seeds of the fungi to germinate and take root. Thus reason and facts concur in pointing out the cause and the operation of the disease†. The natural event is

* As they are set forth in a paper just published by Sir Joseph Banks.

† There appear to be reasons why corn which happens to be struck with this disease in a dry warm summer is exposed to excessive injury, as facts pretty

is too well known, and it is the business of art to endeavour to prevent it.

If by cutting down the crop, as soon as it is found to be diseased, the operation can be stopped—as experience, in different instances, has shown that it may—the remedy is easy *.

A probable means of prevention is that of inducing early ripeness (for reasons above offered), either by sowing early, or by forcing manures, or by selecting and establishing early varieties—of wheat most especially;—as early varieties of pease and other esculent plants are raised by gardeners: a work which only requires ordinary attention, and which it is hoped will, without delay, be set about and encouraged by every attentive grower of wheat, and every promoter of rural improvements in the united kingdom.

For the method of raising and improving varieties of wheat, see the Rural Economy of Yorkshire, vol. ii. p. 4.

XI. *On the Origin of Stones that have fallen from the Atmosphere.* By CHARLES HUTTON, *L.L.D.* and *F.R.S.*

THE following observations are copied from a note by Dr. Hutton on Dr. Halley's Paper on Extraordinary Me-

pretty evidently show that it is. The habits of the plants render them more susceptible of injury, their rich juices more liable to be checked, and the seeds of fungi, it is probable, are more widely, if not more plentifully, distributed, by such a state of the air, than they are by a cool moist atmosphere.

* It may be asked in what manner the remedy is thus effected. But to the practical farmer the fact is all that is required. To him it is equally as indifferent to know the operation of the remedy as the operation of the disease. Those who have profited by the remedy here recommended, believe that it "kills the mildew." (See Gloucestershire, vol. ii. p. 54.) And if it shall appear that the fungus of wheat requires a free supply of air to keep it alive, or in a state of health and vigour, the effect of cutting down the crop will be explained. It will perhaps be found, by experience, that the closer it is allowed to lie upon the ground, and the sooner it is bound up in sheaves (provided the natural ascent of the sap to the ear be not thereby interrupted), the more effectual and complete will be the remedy.

Further, it may be suggested, on the evidence of attentive observation, that if wheat which has been attacked by this disease be suffered to remain in the field with the ears exposed, until it may have received the ameliorating influence of dews or moderate rain (to soften, relax, and assist the natural rise of the sap), the more productive it will probably become. See Minutes of Agriculture, in Surrey, No. 4.

And it may be still further added, that grain which is cut while under ripe, is less liable to be injured in the field by moist weather, than that which has stood until it be fully or over ripe.

teors, given in the sixth volume of the Abridgment of the Philosophical Transactions, now publishing.

Dr. Halley takes it for granted that the luminous bodies sometimes seen in the atmosphere are merely unkindled vapours. Dr. Hutton observes, that "the difficulty, not to say impossibility of conceiving how any exhalations could be raised so high, ought to have hinted the idea of some other origin," and then proceeds as follows:

"Later observations have induced a belief that these luminous appearances are allied to, if not the same as, the stones which have frequently been known to fall from the atmosphere, at different times, and in all parts of the earth. Several of the phænomena are common to both. These luminous bodies are seen to move with very great velocities, in oblique directions descending; commonly with a loud hissing noise, resembling that of a mortar shell, or cannon ball, or rather that of an irregular hard mass projected violently through the air; surrounded by a blaze or flame, tapering off to a narrow stream in the hinder part of it; are heard to explode or burst, and seen to fly in pieces, the larger parts going foremost, and the smaller following in succession; are thus seen to fall on the earth, and strike it with great violence; that on examining the place of the fall, the parts are found scattered about, being still considerably warm, and most of them entered the earth several inches deep. After so many facts and concurring circumstances, it is difficult to refuse assent to the identity of the two phænomena: indeed it seems now not to be doubted, but generally acquiesced in. And hence it is concluded, that every such meteor-like appearance is attended by the fall of a stone, or of stones, though we do not always see the place of the fall, nor discover the stones.

"This conclusion, however, has contributed nothing towards discovering the origin of the phænomenon, at least as to its generation in the atmosphere: on the contrary, it seems still more difficult to account for the production of stones, than gaseous meteors, in the atmosphere, as well as to inflame and give them such violent motion. In fact, it seems concluded as a thing impossible to be done or conceived; and philosophers have given up the idea as hopeless. This circumstance has induced them to endeavour to discover some other cause or origin for these phænomena. But no idea that is probable, or even possible, has yet been started, excepting one, by the very celebrated mathematician Laplace, and that of so extraordinary a nature, as to astonish us with its novelty, and

and boldness of conception. This is no less than the conjecture that these stony masses are projected from the moon! a conjecture which none but an astronomer could have made, or at least have shown to be probable, or even possible. Any ordinary person might at random utter the vague expression of a thing coming from the moon; but no one, except the philosopher, could propose the conjecture seriously, and prove its possibility. This M. Laplace has been enabled to do by strict mathematical calculation. He has proved that a mass, if projected by a volcano from the moon, with a certain velocity, of about a mile and half per second (which is possible to be done), it will thence be thrown beyond the sphere of the moon's attraction, and into the confines of the earth's; the consequence of which is, that the mass must presently fall to the earth, and become a part of it.

" To prepare the way for a calculation, and a comparison of this supposed cause with the phænomena, it will be useful here to premise a short account of the late and best observed circumstances in the appearance of fireballs, and the fall of stony masses from the atmosphere, extracted from the last published accounts of some of the more remarkable cases.

[The greater part of the facts adduced by Dr. Hutton having already appeared in the Philosophical Magazine, we omit them here, and confine ourselves to the remarks he has subjoined to them.]

" Having now given a summary of the facts and evidence, as well with regard to the circumstances attending these singular bodies, as the ingredients they are composed of, and their outward appearance and structure, we are now to consider what inferences respecting their probable origin may be drawn from this mass of information. And indeed we may safely conclude, as it has been inferred from the whole, by the philosophers best qualified to judge of the circumstances, as follow, viz. that the bodies in question have fallen on the surface of the earth; but that they were not projected by any terrestrial volcanoes; and that we have no right, from the known laws of nature, to suppose that they were formed in the upper regions of the atmosphere. Such a negative conclusion has been thought all that we are, in the present state of our knowledge, entitled to draw.

" In this embarrassing predicament, the total want of any other possible way of accounting for the origin of those bodies, an idea has been started, perhaps at first merely at random,

random, that since there is no other possible manner of accounting for them, then they must have dropped from the moon. And, indeed, this singular thought has now advanced into a serious hypothesis, which it must be allowed is unincumbered with any of the foregoing difficulties; having at least possibility in its favour, which no other hypothesis yet proposed can claim.

"As the attraction of gravitation extends through the whole planetary system, a body placed at the surface of the moon is affected chiefly by two forces, one drawing it toward the centre of the earth, and another drawing it toward that of the moon. The latter of these forces however, near the moon's surface, is incomparably the greater. But as we recede from the moon, and approach toward the earth, this force decreases, while the other augments, till at length a point of station is found between the two planets, where these forces are exactly equal; so that a body placed there must remain at rest: but if it be removed still nearer to the earth, then this planet would have the superior attraction, and the body must fall towards it. If a body then be projected from the moon towards the earth, with a force sufficient to carry it beyond this point of equal attraction, it must necessarily fall on the earth. Such then is the idea of the manner in which the bodies must be made to pass from the moon to the earth, if that can be done, the *possibility* of which is now necessary to be considered.

"Now supposing a mass to be projected from the moon, in a direct line towards the earth, by a volcano, or by the production of steam by subterranean heat, and supposing for the present those two planets to remain at rest, then it has been demonstrated, on the Newtonian estimation of the moon's mass, that a force projecting the body with a velocity of 12,000 feet in a second, would be sufficient to carry it beyond the point of equal attraction. But this estimate of the moon's mass is now allowed to be much above the truth; and on M. Laplace's calculation it appears that a force of little more than half the above power would be sufficient to produce the effect, that is, a force capable of projecting a body with a velocity of less than a mile and a half per second. But we have known cannon balls projected by the force of gunpowder, with a velocity of 2500 feet per second, or upwards, that is, about half a mile. It follows therefore, that a projectile force, communicating a velocity about three times that of a cannon ball, would be sufficient to throw the body from the moon beyond the point of equal attraction, and cause it to reach the earth.

Now

Now there can be little doubt that a force equal to that is exerted by volcanoes on the earth, as well as by the production of steam from subterranean heat, when we consider the huge masses of rock, so many times larger than cannon balls, thrown on such occasions to heights also so much greater. We may easily imagine too such cause of motion to exist in the moon as well as in the earth, and that in a superior degree, if we may judge from the supposed symptoms of volcanoes recently observed in the moon, by the powerful tubes of Dr. Herschel; and still more, if we consider that all projections from the earth suffer an enormous resistance and diminution, by the dense atmosphere of this planet, while it has been rendered probable, from optical considerations, that the moon has little or no atmosphere at all, to give any such resistance to projectiles.

" Thus then we are fully authorised in concluding, that the case of *possibility* is completely made out; that a known power exists in nature, capable of producing the foregoing effect, of detaching a mass of matter from the moon, and transferring it to the earth, in the form of a flaming meteor or burning stone; at the same time we are utterly ignorant of any other process in nature by which the same phænomenon can be produced. Having thus discovered a way in which it is possible to produce those appearances, we shall now endeavour to show, from all the concomitant circumstances, that these accord exceedingly well with the natural effects of the supposed cause, and thence give it a very high degree of *probability*.

" This important desideratum will perhaps be best attained by examining the consequences of a substance supposed to be projected by a volcano from the moon, into the sphere of the earth's superior attraction; and then comparing those with the known and visible phænomena of the blazing meteors or burning stones that fall through the air on the earth. And if in this comparison a striking coincidence or resemblance shall always or mostly be found, it will be difficult for the human mind to resist the persuasion that the assumed cause involves a degree of probability but little short of certainty itself. Now the chief phænomena attending these blazing meteors, or burning stones, are these: 1. That they appear or blaze out suddenly. 2. That they move with a surprising rapid motion, nearly horizontal, but a little inclined downwards. 3. That they move in several different directions, with respect to the points of the compass. 4. That in their flight they yield a loud whizzing sound. 5. That they commonly burst with a violent explosion

plosion and report. 6. That they fall on the earth with great force in a sloping direction. 7. That they are very hot at first, remain hot a considerable time, and exhibit visible tokens of fusion on their surface. 8. That the fallen stony masses have all the same external appearance and contexture, as well as internally the same nature and composition. 9. That they are totally different from all our terrestrial bodies, both natural and artificial.

" Now these phænomena will naturally compare with the circumstances of a substance projected by a lunar volcano, and in the order in which they are here enumerated. And first with respect to the leading circumstance, that of a sudden blazing meteoric appearance, which is not that of a small bright spark, first seen at immense distance, and then gradually increasing with the diminution of its distance. And this circumstance appears very naturally to result from the assumed cause. For the body being projected from a lunar volcano, may well be supposed in an ignited state, like inflamed matter thrown up by our terrestrial volcanoes, which passing through the comparatively vacuum, in the space between the moon and the earth's sensible atmosphere, it will probably enter the superior parts of this atmosphere with but little diminution of its original heat; from which circumstance, united with that of its violent motion, this being 10 or 12 times that of a cannon ball, and through a part of the atmosphere probably consisting chiefly of the inflammable gas, rising from the earth to the top of the atmosphere, the body may well be supposed to become suddenly inflamed, as the natural effect of these circumstances; indeed it would be surprising if it did not. From whence it appears that the sudden inflammation of the body, on entering the earth's atmosphere, is exactly what might be expected to happen.

" 2. Secondly, to trace the body through the earth's atmosphere, we are to observe that it enters the top of it, with the great velocity acquired by descending from the point of equal attraction, which is such as would carry the body to the earth's surface in a very few additional seconds of time, if it met with no obstruction. But as it enters deeper in the atmosphere, it meets with still more and more resistance from the increasing density of the air; by which the great velocity of 6 miles per second must soon be greatly reduced to one that will be uniform, and only a small part of its former great velocity. This remaining part of its motion will be various in different bodies, being more or less as the body is larger or smaller, and as it is

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more or less specifically heavy; but, for a particular instance, if the body were a globe of 12 inches diameter, and of the same gravity as the atmospheric stones, the motion would decrease so, as to be little more than a quarter of a mile per second of perpendicular descent. Now while the body is thus descending, the earth itself is affected by a two-fold motion, both the diurnal and the annual one, with both of which the descent of the body is to be compounded. The earth's motion of rotation at the equator is about 17 miles in a minute, or $\frac{2}{7}$ of a mile in a second; but in the middle latitudes of Europe little more than the half of that, or little above half a quarter of a mile in a second: and if we compound this motion with that of the descending body, as in mechanics, this may cause the body to appear to descend obliquely, though but a little, the motion being nearer the perpendicular than the horizontal direction. But the other motion of the earth, or that in its annual course, is about 20 miles in a second, which is 80 times greater than the perpendicular descent in the instance above mentioned; so that, if this motion be compounded with the descending one of the body, it must necessarily give it the appearance of a very rapid motion, in a direction nearly parallel to the horizon, but a little declining downwards. A circumstance which exactly agrees with the usual appearances of these meteoric bodies, as stated in the 2d article of the enumerated phænomena.

" 3. Again, with regard to the apparent direction of the body, this will evidently be various, being that compounded of the body's descent and the direction of the earth's annual motion at the time of the fall, which is itself various in the different seasons of the year, according to the direction of the several points of the ecliptic to the earth's meridian or axis. Usually, however, from the great excess of the earth's motion above that of the falling body, the direction of this must appear to be nearly opposite to that of the former. And in fact this exactly agrees with a remark made by Dr. Halley, in his account of the meteors in his paper above given, where he says that the direction of the meteor's motion was exactly opposite to that of the earth in her orbit. And if this shall generally be found to be the case, it will prove a powerful confirmation of this theory of the lunar substances. Unfortunately, however, the observations on this point are very few and mostly inaccurate: the angle or direction of the fallen stones has not been recorded; and that of the flying meteor commonly mistaken, all the various observers giving it a different course, some even directly the reverse of others. In future, it will be very advisable that the observers of fallen stones observe and record the direction or bearing of the perforation made by the body in the earth, which will give us perhaps the course of the path nearer than any other observation.

"4. In the flight of these meteoric stones, it is commonly observed that they yield a loud whizzing sound. Indeed it would be surprising if they did not. For if the like sound be given by the smooth and regularly formed cannon ball, and heard at a considerable distance, how exceedingly great must be that of a body so much larger, which is of an irregular form and surface too, and striking the air with 50 or 100 times the velocity.

"5. That they commonly burst and fly in pieces in their rapid flight, is a circumstance exceeding likely to happen, both from the violent state of fusion on their surface, and from the extreme rapidity of their motion through the air. If a grinding stone, from its quick rotation, be sometimes burst and fly in pieces, and if the same thing happens to cannon balls, when made of stone, and discharged with considerable velocity, merely by the friction and resistance of the air, how much more is the same to be expected to happen to the atmospheric stones, moving with more than 50 times the velocity, and when their surface may well be supposed to be partly loosened or dissolved by the extremity of the heat there.

"6. That the stones strike the ground with a great force, and penetrate to a considerable depth, as is usually observed, is a circumstance only to be expected, from the extreme rapidity of their motion, and their great weight, when we consider that a cannon ball, or a mortar shell, will often bury itself many inches, or even some feet in the earth.

"7. That these stones, when soon sought after and found, are hot, and exhibit the marks of recent fusion, are also the natural consequences of the extreme degree of inflammation in which their surface had been put during their flight through the air.

"8. That these stony masses have all the same external appearance and contexture, as well as internally the same nature and composition, are circumstances that strongly point out an identity of origin, whatever may be the cause to which they owe so generally uniform a conformation. And when it is considered, 9thly, that in those respects they differ totally from all terrestrial compositions hitherto known or discovered, they lead the mind strongly to ascribe

ascribe them to some other origin than the earth we inhabit; and none so likely as coming from our neighbouring planet.

"Upon the whole then it appears highly probable, that the flaming meteors, and the burning stones that fall on the earth, are one and the same thing. It also appears impossible, or in the extremest degree improbable, to ascribe these, either to a formation in the superior parts of the atmosphere, or to the irruptions of terrestrial volcanoes, or to the generation by lightning striking the earth. But, on the other hand, that it is possible for such masses to be projected from the moon so as to reach the earth; and that all the phænomena of these meteors or falling stones, having a surprising conformity with the circumstances of masses that may be expelled from the moon by natural causes, unite in forming a body of strong evidence, that this is in all probability and actually the case."

XII. *Description of a Plough-ear which offers the least possible Resistance, and which may be easily constructed. By Mr.* Jefferson, *President of the United States of America*.*

The body of a plough ought not only to be the continuation of the wing of the sock, beginning at its posterior edge, but it must also be in the same plane. Its first function is to receive horizontally from the sock the earth, to raise it to the height proper for being turned over; to present in its passage the *least possible resistance*, and consequently to require only the *minimum* of moving power. Were its functions confined to this, the wedge would present, no doubt, the properest form for practice†; but the object is also to turn over the sod of earth. One of the edges of the ear ought then to have no elevation to avoid an useless wasting of force; the other edge ought on the contrary to

* From *Annales du Muséum National d'Histoire Naturelle, no.* 4. 1802.

† I am sensible that if the object were merely to raise the sod of earth to a given height by a determinate length of ear, without turning it over, the form which would give the least resistance would not be exactly that of a wedge with two plane faces; but the upper face ought to be curvilinear, according to the laws of the solid of least resistance described by mathematicians. But in this case the difference between the effect of the wedge with a curved face, and that of a wedge with a plane face, is so small, and it would be so difficult for workmen to construct the former, that the wedge with a plane face ought to be preferred in practice as the first element of our method of construction. (*Note of the* [illegible].)

go on ascending until it has passed the perpendicular, in order that the sod may be inverted by its own weight; and to obtain this effect with the least possible resistance, the inclination of the ear must increase gradually from the moment that it has received the sod.

In this second function the ear acts then like a wedge situated in an oblique direction or ascending, the point of which recedes horizontally on the earth, while the other end continues to rise till it passes the perpendicular. Or, to consider it under another point of view, let us place on the ground a wedge, the breadth of which is equal to that of the sock of the plough, and which in length is equal to the sock from the wing to the posterior extremity, and the height of the heel is equal to the thickness of the sock: draw a diagonal on the upper surface from the left angle of the point to the angle on the right of the upper part of the heel; slope the face by making it bevel from the diagonal to the right edge which touches the earth: this half will evidently be the properest form for discharging the required functions, namely, to remove and turn over gradually the sod, and with the least force possible. If the left of the diagonal be sloped in the same manner, that is to say, if we suppose a straight line, the length of which is equal at least to that of the wedge, applied on the face already sloped, and moving backwards on that face, parallel to itself, and to the two ends of the wedge, at the same time that its lower end keeps itself always along the lower end of the right face, the result will be a curved surface, the essential character of which is, that it will be a combination of the principle of the wedge, considered according to two directions, which cross each other, and will give what we require, a plough-ear presenting the least possible resistance.

This ear, besides, is attended with the valuable advantage that it can be made by any common workman by a process so exact that its form will not vary the thickness of a hair. One of the great faults of this essential part of the plough is the want of precision, because, workmen having no other guide than the eye, scarcely two of them are similar.

It is easier, indeed, to construct with precision the plough-ear in question when one has seen the method which furnishes the means once put in practice, than to describe the method by the aid of language, or to represent it by figures. I shall, however, try to give a description of it.

Let the proposed breadth and depth of the furrow, as well as the length of the head of the plough, from its junction with

with the wing to its posterior end, be given, for these data will determine the dimensions of the block from which the ear of the plough must be cut. Let us suppose the breadth of the furrow to be 9 inches, the depth 6, and the length of the head two feet: the block then (Plate I. fig. 1.) must be 9 inches in breadth at its base *b c*, and $13\frac{1}{2}$ inches at its summit *a d*; for, if it had at the top only the breadth *a e* equal to that of the base, the sod, raised in a perpendicular direction, would by its own elasticity fall back into the furrow. The experience which I have acquired in my own land, has proved to me that in a height of 12 inches the elevation of the ear ought to go beyond the perpendicular $4\frac{1}{2}$ inches, which gives an angle of about $20\frac{1}{2}°$, in order that the weight of the sod may in all cases overcome its elasticity. The block must be 12 inches in height, because, if the height of the ear were not equal to twice the depth of the furrow, when friable and sandy earth is tilled it would pass the ear, rising up like waves. It must be in length 3 feet, one of which will serve to form the tail that fixes the ear to the stilt of the plough.

The first operation consists in forming this tail by sawing the block (fig. 2.) across from *a* to *b* on its left side, and at the distance of 12 inches from the end *f g*: then continue the notch perpendicularly along *b c* till within an inch and a half of its right side; then taking *d i* and *e h*, each equal $1\frac{1}{2}$ inch, make a mark with the saw along the line *d e*, parallel to the right side. The piece *a b c d e f g* will fall of itself, and leave the tail *c d e h i k*, an inch and a half in thickness. It is of the anterior part *a b c k l m n* of the block that the ear must be formed.

By means of a square trace out on all the faces of the block lines at an inch distance from each other, of which there will necessarily be 23: then draw the diagonals *k m* (fig. 3.) on the upper face, and *k o* on that which is situated on the right; make the saw enter at the point *m*, directing it towards *k*, and making it descend along the line *m l* until it mark out a straight line between *k* and *l* (fig. 5.); then make the saw enter at the point *o*, and, preserving the direction *o k*, make it descend along the line *o l* until it meet with the central diagonal *k l*, which had been formed by the first cut: the pyramid *k m n o l* (fig. 4.) will fall of itself, and leave the block in the form represented by fig. 5.

It is here to be observed, that in the last operation, instead of stopping the saw at the central diagonal *k l*, if we had continued to notch the block, keeping on the same plane, the wedge *l m n o k b* (fig. 3.) would have been taken away,

away, and there would have remained another wedge *l o k b a r*, which, as I observed before, in speaking of the principle in regard to the construction of the ear, would exhibit the most perfect form, were the only object to raise the sod; but as it must also be turned over, the left half of the upper wedge has been preserved, in order to continue, on the same side, the bevel to be formed on the right half of the lower wedge.

Let us now proceed to the means of producing this bevel, in order to obtain which we had the precaution to trace out lines around the block before we removed the pyramid (fig. 4.). Care must be taken not to confound these lines, now that they are separated by the vacuity left by the suppression of that pyramid (fig. 5.). Make the saw enter in the two points of the first line, situated at the places where the latter is interrupted, and which are the two points where it is intersected by the external diagonals *o k* and *m k*, continuing the stroke on that first line till it reach on the one hand the central diagonal *k l*, and on the other the lower right edge *o h* of the block (fig. 5.): the posterior end of the saw will come out at some point situated on the upper trace in a straight line with the corresponding points of the edge and the central diagonal. Continue to do the same thing on all the points formed by the intersection of the exterior diagonals and lines traced out around the block, taking always the central diagonal, and the edge *o h* as the term, and the traces as directors: the result will be, that when you have formed several cuts with the saw, the end of that instrument, which came out before at the upper face of the block, will come out at the face situated on the left of the latter; and all these different cuts of the saw will have marked out as many straight lines, which extending from the lower edge *o h* of the block, will proceed to cut the central diagonal. Now by the help of any proper tool remove the sawn parts, taking care to leave visible the traces of the saw, and this face of the ear will be finished*. The traces

* The figures 9 and 10, which we have added here to those which accompany Mr. Jefferson's memoir, were drawn in perspective by M. Valencienne, assistant naturalist belonging to the museum, and may serve to give a better idea of the result of the operation here described. Let us suppose that the saw cuts the lines *m k*, *o k* (fig. 9.) in the points *x* and *t*, taken in the traces *x ζ* and *t s* situated in the same plane parallel to *b a r c*, and the prolongations of which on the triangles *m k l* and *o k l* are the lines *x z* and *t z*; the saw must then penetrate the block remaining in the plane in question until its edge has arrived at the point *s*, and at the same time touch the point *z* of the central diagonal *k l*. The same edge of the saw will come out at some point *y* of the face *m k l*, so that the three points *s*, *z*, *y*, will be in [illegible] the

traces will serve to show how the wedge which is at the right angle rises gradually on the direct or lower face of the wedge, the inclination of which is preserved in the central diagonal. One may easily conceive and render sensible the manner in which the sod is raised on the ear, which we have described, by tracing out on the ground a parallelogram two feet long and nine inches broad, as *abcd* (fig 6): then placing in the point *b* the end of a stick 27½ inches in length, and raising the other end 12 inches above the point *c*: (the line *de*, equal to 4½ inches, represents the quantity which the height of the ear exceeds the perpendicular). When this is done, take another stick 12 inches in length, and, placing it on *ab*, make it move backwards, and parallel to itself from *ab* to *cd*, taking care to keep one of its ends always on the line *ad*; while the other end moves along the stick *be*, which here represents the central diagonal. The motion of this stick of 12 inches in length will be that of our ascending wedge, and will show how each transverse line of the sod is carried from its first horizontal position until it be raised to a height which exceeds the perpendicular so much as to make it fall inverted by its own weight.

But to return to our operation:—it remains to construct the lower part of the ear. Invert the block and make the saw enter at the points where the line *al* (fig. 9.) meets with the traces, and continue your stroke along these traces until both ends of the saw approach within an inch, or any other convenient thickness, of the opposite face of the ear. When the cuts are finished, remove, as before, the sawn pieces, and the ear will be finished *.

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the same straight line. But if this operation be repeated in different places of the lines *mk*, *okb* from *k* to a certain height, the points of the face *mkb*, at which the saw comes out, will form a curve *kyn*. Beyond this height the saw, always directed in such a manner that at the end of its motion it shall touch at the same time the edge *oh* and the central diagonal *kl*, will come out at other points situated on the posterior face *abml*, and the series of these points will form a second curve *nl*, which will meet the first in the point *n*. These two curves being traced out, let us suppose straight lines drawn to the places where the saw stopped each time that it touched the diagonal *kl*, and of which one, as already said, passes through the points *s*, *z*, *y*; and let us conceive a surface touching all these straight lines, and whose limits, on the one hand, shall be the curves *kyn*, *nl*, and on the other the edge *oh*, this surface, which must be uncovered by sections made with a proper instrument, will form one of the faces of the ear. The latter is represented fig. 10, and the face in question is that which appears before, and which is indicated by *inlor*. It will be remarked that the angle situated towards *k* (fig. 9) on the part *kedick* of the block has also been cut off by a section made from *d* to *r*, agreeably to what will be said hereafter.—*Note of the French Editor.*

* We shall here add to this description an illustration similar to that given

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It is fixed to the plough by morticing the fore part *ol* (fig. 5 and 10.) into the posterior edge of the sock, which must be made double, like the case of a comb, that it may receive and secure this fore part of the ear. A screw-nail is then made to pass through the ear and the handle of the sock at the place of their contact, and two other screw-nails pass through the tail of the ear and the right handle of the plough. The part of the tail which passes beyond the handle must be cut diagonally, and the work will be finished.

In describing this operation I have followed the simplest course, that it may be more easily conceived; but I have been taught by practice, that it requires some useful modifications. Thus, instead of beginning to form the block as represented *abcd* (fig. 7.), where *ab* is 12 inches in length and the angle at *b* is a right one, I cut off towards the bottom, and along the whole length *bc* of the block, a wedge *bce*, the line *l* being equal to the thickness of the bar of the sock (which I suppose to be 1½ inch); for, as the face of the wing inclines from the bar to the ground, if the block were placed on the sock, without taking into the account this inclination, the side *ab* would lose its perpendicular direction, and the side *ad* would cease to be horizontal. Besides, instead of leaving at the top of the block a breadth of 13½ inches from *m* to *n* (fig. 8.), I remove from the right side a kind of wedge *nkicpn* of 1½ inch in thickness; because experience has shown me that the tail, which by these means has become more oblique, as *ci* instead of *ki*, fits more conveniently to the side of the handle. The diagonal

in regard to the anterior face of the ear. The thickness of the latter being determined by that of the part *kcdieh* (fig. 9.), or, what amounts to the same thing, by the length of the lines *ck*, *di*, *eh*, let us first conceive that there has been traced out, proceeding from the point *c*, the curve *cup* parallel to *kyn*, and then, proceeding from the point *p*, the curve *p*ϑ parallel to *ln*. Let us next suppose that the saw cuts the edge *al* of the face *abml* in the point δ, situated in the same plane as *x*ζ and *ts*, which plane has been taken for example in regard to the anterior face of the ear. The saw must be directed along the traces δζ and δ*s* in such a manner that its motion shall stop at the term where its edge on the one hand shall touch the curve *cp* in the point *u* situated on the trace *xz*, and on the other shall be situated parallel to the line *szy* at which the saw stopped on the other side of the ear. The edge of the saw will then cut the face *alo*τ in some point ε, so situated that the straight line drawn through that point and the point *u* shall be parallel to the straight line which passes through the point *s*, *z*, *y*. If you continue in the same manner cutting with the saw different points of the edge *al*, those by which it comes out will form on the face *alo*τ a curve εμϑ; and if through these points and those corresponding to them in the lines *cp*, *p*ϑ, there be drawn straight lines, such as that which passes through the points ε, *u*, and which we have taken as an example, the surface touching these straight lines, and uncovered by means of any sharp instrument, will form with the remainder *ehol*ϑμε of the plane *alo*τ, the posterior face of the ear, such as is represented fig. 10.—*Note of the French Editor.*

gonal of the upper face is consequently removed back from k to c; and we have mc instead of mk, as above. These modifications may be easily comprehended by those acquainted with the general principle.

In the different experiments to which ears have been subjected to determine the quantity by which the right upper side of the block passes beyond the perpendicular, and to fix the relation between the height and the depth of the furrow, they were made only of wood; but since my experiments have convinced me, that for a furrow 9 inches broad and 6 in depth, the dimensions I have given are the best, I propose in future to have these ears made of cast iron.

I am sensible that this description may appear already too long and too minute for a subject which has hitherto been considered as unworthy of furnishing matter of application to science; but, if the plough is really the implement most useful to man, the improvement of it can never be thought a vain speculation. However, the combination of a theory satisfactory to the learned, with a practice which falls within the reach of the most unlettered labourer, must meet with a favourable reception from two classes of men who render most service to society.

XIII. *Proceedings of Learned Societies.*

THE LONDON INSTITUTION.

At a very numerous and respectable meeting at the London Tavern, May 23, 1805, Sir F. Baring, M. P. in the chair, the following resolutions were unanimously adopted:

1. That it is expedient to establish an institution upon a liberal and extensive scale, in some central situation in the city of London, the object of which shall be to provide—
   1. A library, to contain works of intrinsic value.
   2. Lectures for the diffusion of useful knowledge.
   3. Reading-rooms for the daily papers, periodical publications, interesting pamphlets, and foreign journals.
2. That this institution shall consist of a limited number of proprietors, and of life and annual subscribers.
3. That the interest of the proprietors shall be equal, permanent, transferable, and hereditary, and shall extend to the absolute property of the whole establishment; they shall be entitled to such extraordinary privileges as may be

 consistent

consistent with general convenience, and upon them shall devolve the exclusive right of the management of the institution.

4. That the life and annual subscribers shall have the same use of, and access to, the institution as the proprietors.

5. That the qualification of a proprietor be fixed for the present at seventy-five guineas.

6. That the subscription for life be for the present twenty-five guineas.

7. That ladies shall be received as subscribers to the lectures, under such regulations and upon such terms as may hereafter be determined.

8. That as soon as one hundred persons have declared their intention to become proprietors, a general meeting of all such persons shall be convened, who shall proceed as they see occasion, to carry the plan into effect, to appoint a committee to draw up regulations for the institution, and to submit the same to a general meeting of the proprietors for their approbation.

9. That this institution be denominated the *London Institution, for the Advancement of Literature and the Diffusion of useful Knowledge.*

10. That the following persons be a committee to receive the names of such gentlemen as may desire to become proprietors or life subscribers, and to conduct the progress of the proposed establishment, until a general meeting of the proprietors can be held:

Sir F. Baring, bart. M. P.	John Smith, esq. M. P.
J. J. Angerstein, esq.	Robert Wigram jun. esq.
Richard Sharp, esq.	Samuel Woods, esq.
George Hibbert, esq.	

11. That one-third of the sums subscribed be paid on or before the 10th of June, one-third on or before the 1st of October next, and the remaining third on or before the 1st of January next.

Resolved unanimously,

That the thanks of this meeting be given to those gentlemen with whom this design originated.

That the thanks of this meeting be given to G. Hibbert, esq. and R. Sharp, esq. for moving and seconding the foregoing resolutions.

F. Baring, Chairman.

The chairman having left the chair,

Resolved unanimously,

That the thanks of this meeting be given to sir F. Baring, bart.

bart. for taking the chair, and for the ability and impartiality with which he has conducted the business of this day.

The subscription having proceeded with unexpected rapidity, a general meeting of the proprietors will be held on Tuesday next, at the London Tavern, when the chair will be taken at one o'clock precisely.

London Tavern, May 28, 1805.

At a meeting of the proprietors of the London Institution, Sir F. Baring, Bart. M. P. in the chair,

Resolved,

That the subscription for the names of proprietors be now closed.

That before any measures are taken for carrying the plan into execution, a petition be presented to his majesty, praying that he would be graciously pleased to grant a charter to the institution.

That an outline of the plan be laid before the right honourable the secretary of state for the home department.

That for these purposes it is expedient to elect a committee of managers to continue till a charter shall be obtained.

That the following proprietors be now elected as temporary managers of this institution:

Sir Francis Baring, bart. M. P. president
John Julius Angerstein, esq.
Thomas Baring, esq.
Thomas Bodley, esq.
Harvey Christian Combe, esq. M. P. alderman
Richard Clarke, F. R. S. chamberlain
George Hibbert, esq.
Benjamin Harrison, esq.
Henry Hoare, esq.
Sir Hugh Ingliss, bart. M. P.
Beeston Long, esq.
William Manning, esq. M.P.
William Heseltine Pepys, esq.
Sir Charles Price, bart. M.P. alderman
Job Matthew Raikes, esq.
John Rennie, esq.
Matthew Raine, D. D.
Richard Sharp, esq. F. A. S.
John Smith, esq. M. P.
Henry Thornton, esq. M. P.
Samuel Woods, esq.
Robert Wigram jun. esq.

That the plan, after it has received the approbation of a general meeting of proprietors, be laid before his majesty's secretary of state, for the purpose of soliciting a charter for the institution.

That the subsequent proceedings of the managers be laid before a general meeting of the proprietors for their approbation; and that, in the mean time, the committee of managers are hereby authorized to adopt such measures and to defray such expense as may be necessary for the establishment of this institution.

That sir William Curtis, bart. be appointed treasurer to this institution; and that those gentlemen who have declared their intention to become proprietors and life subscribers be requested to pay one-third of their subscriptions into the banking-house of Robarts, Curtis, and Co. on account of the said treasurer.

That any person neglecting to pay the first or succeeding instalments on his subscription within fourteen days after the date fixed by the eleventh resolution of the general meeting, shall forfeit his right to any share or privilege in this institution.

That the foregoing resolutions be printed, together with those adopted at the general meeting of the 23d instant, and transmitted to every proprietor.

That the thanks of this meeting be given to sir F. Baring, bart. M. P. for his conduct in the chair.

BRITISH INSTITUTION FOR PROMOTING THE FINE ARTS.

At a meeting of subscribers to the plan for a British Institution for promoting the Fine Arts in the united kingdom, held at the Thatched House Tavern, the 4th of June 1805, present,

The Earl of Dartmouth in the chair,

The earl of Aylesford
John Julius Angerstein, esq.
The duke of Bedford
Sir George Beaumont, bart.
Thomas Bernard, esq.
Rt. hon. Isaac Corry, M.P.
Rev. William Carr
James Christie, esq.
The bishop of Durham
Lord De Dunstanville
Charles Duncombe, esq. M. P.
Sir Wm. Elford, bart. M.P.
Sir Abraham Hume, bart.
Henry Hope, esq.
Thomas Hope, esq.
Lord viscount Lowther
Edward L. Loveden, esq.
Samuel Lysons, esq.
Philip Metcalfe, esq. M.P.
William Morland, esq. M.P.
Lord Northwick
Lord Henry Petty, M. P.
William Smith, esq. M. P.
Richard Troward, esq.
Samuel Whitbread, esq. M. P.
Caleb Whiteford, esq.

It was moved by lord viscount Lowther, and seconded by the duke of Bedford, and unanimously resolved,

That the British Institution for promoting the Fine Arts in the United Kingdom, under his majesty's most gracious patronage, do commence and take place this day, being his majesty's birth-day,

It

It was moved by the right honourable Isaac Corry, and seconded by John Julius Angerstein, esq. and resolved,

That the earl of Dartmouth, lord viscount Lowther, the right honourable Charles Long, sir George Beaumont, bart. sir Abraham Hume, bart. sir Francis Baring, bart. Thomas Hope, esq. William Smith, esq. and Thomas Bernard, esq. be a select committee to prepare a draft of regulations for the British Institution, to inquire after a local situation for it, and to make their report to an adjourned meeting of subscribers of fifty guineas or upwards, to be held at the Thatched House Tavern, on Tuesday next, at half past twelve o'clock: the chair to be taken at one o'clock precisely.

It was moved by the earl of Aylesford, and seconded by Henry Hope, esq. and resolved,

That subscribers of one guinea a year, or of ten guineas in one sum, have personal admission to the rooms of exhibition: that subscribers of three guineas a year, or of thirty guineas in one sum, have personal admission, and the right of introducing a friend each day: that subscribers of five guineas a year have the same personal admission, together with the right of introducing two friends each day: that subscribers of fifty guineas have the same privileges for life, and be governors of the institution: that subscribers of one hundred guineas or upwards have the same privileges in perpetuity, and be governors of the institution, their rights to be transmissible on death, subject to the regulations hereafter to be adopted; and that the institution be under the government of a committee of directors, consisting of the president, four vice-presidents, and twelve other persons, from time to time to be elected by and out of the governors.

DARTMOUTH, Chairman.

And the earl of Dartmouth having quitted the chair,

Resolved unanimously, That the thanks of the meeting be presented to his lordship for his great attention to the business of the day.

Persons disposed to promote the Institution are requested to address themselves, by letter, to any of the select committee; or to send their names to Mr. Hatchard's, No. 190, Piccadilly, where the books of subscriptions are left.

NEW INSTITUTIONS IN AMERICA.

A lettter from New York, dated April 1st, says:—"Among the numerous institutions which have been formed in this country in the course of 1804, there are three in particular in which the public take a warm interest.

 The

The first is a Society of Agriculture established at Washington under the special protection of government. The president of the United States, the chiefs of administration, the senators and deputies to congress, are members of it in right of their situation. The society have already acquired a convenient edifice, with a field of thirty acres; the commencement of a library, and that excellent collection of ploughs and other agricultural implements which formerly belonged to general Washington. The form of its administration, the capital which it can possess (specified in bushels of wheat), and its whole organization have been fixed by its charter of incorporation, which constitutes the society into a political body, and ensures the existence of it for ever. The answers to the numerous questions which it sent, soon after its formation, to the societies of different countries, form, it is said, an interesting work, which will be published.

The second institution is a Botanical Garden in the neighbourhood of New York, for which the subscribers have obtained also a charter. As soon as the large green-house is completed, the most curious productions of the southern provinces will be sent to it.

The third institution is an Academy of Fine Arts. The first idea of this establishment originated with Mr. Livingston, the American minister at Paris; and the public were so sensible of its importance, that long before the arrival of the plaster casts, which that gentleman presented to it, the subscribers, of twenty-five piastres each, amounted to 180. Mr. Vandeline, a native of America, who has resided several years at Paris, where he has become an eminent painter, has sent to the academy some fine paintings.

The president, by the support of the friends to this institution, has purchased for it that beautiful edifice which forms the centre of the circus lately built on Hudson's river, the large hall of which is lighted by a rotunda of cast iron entirely filled with panes of glass. It is here that Mr. Livingston's plaster casts, among which there is one of the celebrated Laocoon, have been deposited: and seventeen pupils are already employed in making drawings from these fine models.

SOCIETY OF THE SCIENCES AT FLUSHING.

In the meeting of November 2, last year, at the Museum in Middleburgh, the society proposed again the two following questions, announced in the year 1803, and to which no answers had been received.

I. What

I. What are the natural causes that the bottom of the harbours in Middleburgh and the Welzinge channel has been so perceptibly raised, during a series of years, by an accumulation of the mud? What are the simplest, most effectual, and least expensive means of remedying this evil? and is it possible to give a sufficient depth to these harbours and channel, and to maintain them in that state?

II. A history of the influx of the current of the sea according to fixed laws, and in a determinate line? What are these laws? Is the course of these currents prejudicial to our dykes and to the strand, and in what degree? What are the practicable means of giving to this prejudicial current another direction, and of conducting it to other places, so as to obviate its destructive effect?

The prize is a gold medal; and the answers must be sent in before the 1st of January 1806.

The two following questions also, announced last year, are again proposed for the same reason as the preceding: the first for a year, the second for an indefinite time.

I. As the utility of pouring out oil and other fat substances during storms at sea, is established by sufficient proofs; but as the objection, that this mean may be prejudicial to ships which follow, has not been entirely obviated, the society requires to know: What is the physical principle of calming the waves by pouring out fat substances? and, Can the above objection be entirely done away by an explanation of this point?

II. What was the geographical state of Zeeland in regard, in particular, to rivers and streams, from the earliest periods to the commencement of the government of Counts? What changes took place in it between the latter period and the end of the fourteenth century? Has it continued the same, or have evident alterations taken place? and what are these alterations?

The society has proposed also three new questions, the prize for which is a gold medal; the first to be answered before the 1st of August 1805, the other two before the 1st of January 1806.

I. As we have no general history of the sciences and fine arts in this country, which would be both agreeable and useful; and as such a history is not to be expected until histories of each branch be composed, the society has resolved to turn its attention to this object, and to announce one part annually as the subject of a prize question, with a view that materials may be collected for a general history, and that our countrymen may in the mean time enjoy the

benefit

benefit of particular histories. But as it appears necessary to the society that the compass of this fertile subject should be treated in methodical order, for the purpose of avoiding confusion and needless prolixity, and that not only a proper distinction should be made between the sciences and fine arts, but that a proper distribution of them should be previously established, according to which the society may propose its annual questions: and as it appears also that several men of letters who have written on the division of literature, science, and the fine arts, which, according to the opinion of the antients, are so intimately connected with each other, evidenrly differ, the society considers it necessary first to propose the following question:—Is there any connection between the sciences and the fine arts? Is it possible to separate them from each other, and to distribute and arrange both in a regular series? What is the best order, and at the same time the best adapted to make the literary history of the various branches of the sciences and fine arts serve as materials for prize questions?

II. As the Pythagorean philosopher, Apollonius of Tyana, has, by many of the pagan and other writers, been placed in the same rank with our blessed Saviour Jesus Christ, the society requires to know: "What real or probable information is to be obtained in regard to this man? And what proofs of the truth of the evangelical writings can be deduced from a comparison of the accounts given us of Apollonius by Philostratus and others, and of Jesus Christ by the evangelists; together with a comparison of the external relation of these writers?

III. As the bloody feuds known under the name of *Hoeksch* and *Kabeljaauwsch* form the principal part of the early history of this country, and as different opinions have been entertained in regard to various circumstances relating to them, the society wishes for a more satisfactory account of the origin of these two parties. Was it not earlier than the destructive quarrel between Margaret of Hennegau and her son William V? What gave occasion to the appellations *Hoekseh* and *Kabeljaauwseh?* Is the real etymology of these words established, and what was the principal object of these parties from their origin to the time when they became extinct?

The answers, written in the Dutch, Latin, or French languages, but in a legible hand, must be transmitted, sealed up in the usual manner, to A. Dryfhout, the secretary, at Middleburgh, before the periods above announced.

XIV. *In-*

XIV. *Intelligence and Miscellaneous Articles.*

PRIZE QUESTIONS.

The king of Prussia has proposed the following prize question in regard to the yellow fever:—"Are there sufficient grounds, founded on indubitable facts, for believing that the contagion of the yellow fever can convey its infection to substances destitute of life, without losing any of its force, and in such a manner that the contact of these substances can communicate the infection to sound persons, and by these means convey the fever to other countries?

The prize for the best answer is 200 ducats, and for the second best 100 ducats. The answers must be written in Latin, German, or French; and transmitted to the Superior College of Medicine before the 1st of January 1807.

NEW METAL.

Dr. Richter, of Berlin, has discovered in the cobalt ore of the Saxon mines a new metal which has properties common to cobalt and nickel, but which differs from both. He has given it the name of *niccolan*. A particular account of this metal has been published in Gehler's Journal of Chemistry.

ASTRONOMY.

A table of the right ascension and declination of Ceres and Pallas.

	Ceres.					Pallas.				
	AR.			Decl. N.		AR.			Decl. S.	
1805	h	m	s	°	′	h	m	s	°	′
July 28	5	24	8	21	2	3	48	52	2	20
31	5	29	0	21	11	3	53	24	2	42
Aug. 3	5	33	52	21	19	3	57	56	3	4
6	5	38	40	21	27	4	2	24	3	29
9	5	43	28	21	34	4	6	48	3	65
12	5	48	8	21	41	4	11	8	4	22
15	5	52	48	21	47	4	15	20	4	52
18	5	57	24	21	53	4	18	22	5	23
21	6	2	0	21	59	4	23	36	5	56
24	6	6	32	22	4	4	27	40	6	31
27	6	11	4	22	8	4	31	36	7	8
30	6	15	28	22	13	4	35	28	7	47

Juno is not yet visible.

THE

THE TIDES.

A correspondent remarks, that from the peculiar position of the two grand luminaries, the sun and the moon, on the 10th day of August next, a great increase of tide may be expected on the three following days; and that a very good opportunity will then offer to ascertain the moon's influence over the ocean, by observing the height of the tide at the principal maritime ports, particularly at Chepstow, the Bristol channel, and at London bridge.

DEATH.

On the 9th of March last, at the age of about 76, the celebrated Felix Fontana, director of the Royal Museum at Florence. "He died," says Fabbroni, "full of glory." Being attacked twenty-seven days before by an apoplexy, he was assisted during the fit by the duke De Bonelli, who was accidentally passing at the time. After this accident his mental faculties were so weakened that he was scarcely able to make the necessary disposition of his property in favour of his relations, friends, and domestics.

The physical sciences have lost in Fontana a man by whom they were cultivated with unremitting ardour. Italy, in particular, regrets in him one of its brightest ornaments. He possessed the rare talent of an observer. He had great boldness of conception, uncommon strength of judgment, and an obstinate perseverance in every thing he undertook. The numerous and laborious experiments he made on the poison of the viper are a proof of it, as well as those by which he threw great light on the animal œconomy. The cabinet of Florence is indebted to his persevering courage, thwarted by difficulties and obstacles, for the immense and valuable collection, to which there is nothing equal in Europe, of wax models of every kind executed under his assiduous and minute direction. It is also indebted to him for two wooden statues which can be taken to pieces: one of them could not be finished in his life-time, and perhaps will never be completed after his death. The reader will be astonished to learn that it consists of six thousand different pieces, and is destined to show in its decomposition the whole system, the bowels and membranes of the human body.

These labours, though assiduous, left him sufficient time to cultivate the other branches of the physical sciences, on which he has left works written both in Italian and in French. His style is perspicuous and elegant, valuable qualities,

qualities, in which he participated with his brother Gregory Fontana.

His obsequies were celebrated with great solemnity in his parish: his body was opened before the most celebrated professors, and the features of his face were taken off by a plaster cast. His remains were deposited in a leaden coffin, with the principal circumstances of his life written on parchment inclosed in a metal tube closely soldered. This coffin, put into another of fir, was interred three days after under the public chapel of the noviciate of the minor conventual brothers of Sainte-Croix, close to the ashes of Galileo and Viviani, Michael Angelo and Machiavel. One of his executors, M. Petter Ferroni, a celebrated mathematican, will make known to the republic of letters the valuable manuscripts left by this eminent philosopher, as well as those of Gregory Fontana, found among his philosophical collections.

LIST OF PATENTS FOR NEW INVENTIONS.

Richard Jubb, of Bridge-row, in the parish of St. George, Hanover-square, in the county of Middlesex, whitesmith; for improvements in making and tuning the musical instrument called the pedal harp, by which the half-quarter note is produced thereon with peculiar sweetness and harmony; and the further addition of an harmonic stop made thereto; and also certain improvements in tuning the violin and other stringed instruments.

Barrodall Robert Dodd, of Change-alley, in the city of London, civil engineer; for various improvements in the construction of fire-places, and adapting stoves and grates thereto.

Joseph Bramah, of Pimlico, in the county of Middlesex, engineer; for sundry improvements in the art of making paper.

Thomas Rowntree, of the parish of Christ Church, in the county of Surrey, engine-maker; for an axletree and box for carriages on an improved plan.

Charles Hobson, of Sheffield, in the county of York, plater, and Charles Silvester, of the same place, chemist; for a method of manufacturing the metal called zinc into wire, and into vessels and utensils for culinary and other purposes.

METEOR-

METEOROLOGICAL TABLE

By Mr. Carey, of the Strand,

For June 1805.

Days of the Month.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 27	52°	64°	49°	30·00	45°	Fair
28	50	60	45	·15	32	Fair
29	51	64	52	·18	72	Fair
30	56	70	50	·10	48	Fair
31	53	61	44	·23	49	Fair
June 1	46	56	44	·30	51	Fair
2	47	57	46	·28	44	Fair
3	50	69	55	·02	35	Fair
4	46	56	46	29·98	34	Cloudy
5	47	57	49	·92	30	Cloudy
6	51	68	50	·80	60	Fair
7	52	68	55	30·01	63	Fair
8	56	70	56	·01	58	Fair
9	56	75	60	29·82	50	Fair
10	61	66	55	·52	20	Rain
11	56	64	54	·48	19	Showery
12	55	59	52	·96	28	Showery
13	54	68	54	30·12	45	Showery
14	52	54	51	29·80	0	Rain
15	50	58	54	·88	35	Cloudy
16	54	64	56	·96	38	Cloudy
17	58	68	54	·92	54	Fair
18	56	63	55	·73	50	Fair
19	54	61	51	·96	38	Showery
20	50	51	50	·95	10	Showery
21	49	54	49	30·02	25	Cloudy
22	50	58	50	·10	35	Cloudy
23	54	68	56	29·90	43	Fair
24	60	73	60	·82	60	Fair
25	58	59	49	·60	15	Showery
26	51	66	..	·85	51	Cloudy

N. B. The barometer's height is taken at noon.

XV. *Letter to M.* LACEPEDE, *of Paris, on the Natural History of North America. By* BENJAMIN SMITH BARTON, *M. D. Professor of Materia Medica, Natural History, and Botany, in the University of Pennsylvania*.*

IT is a long time since I have received a letter from you. I have anxiously expected one, as I am very desirous to know what progress you have made in your work † on Fishes. I should, indeed, have been very glad if my leisure had permitted me to have transcribed for your use a very considerable number of facts relative to the fishes of North America, especially of the United States. These facts will, however, be published in two works in which I am engaged, viz. my Fragments of the Natural History of Pennsylvania, and my Travels through various Parts of the United States. I shall only observe at present, that many of our fishes are undescribed by the different writers whom I have had an opportunity of consulting; and I believe I may assert that much very interesting matter relative to the manners, the migrations, &c. of various American species is entirely unnoticed.

I exceedingly rejoice to find, by the French and other foreign publications, that all the branches of natural history are making so much progress in your country. On this side of the Atlantic we also are doing something; as much, perhaps, as could be expected from us. The museum, founded by our countryman Mr. C. W. Peale, is very respectable, both for the number and value of the articles which it contains. Within the last three or four years several new species of quadrupeds, or mammalia, have been discovered, and our knowledge concerning other species has been greatly extended. You are, doubtless, well informed that two pretty complete skeletons of the mammoth (as it has long been called) have been discovered. One of these has been sent to Europe, and it is probable that you will have an opportunity of seeing it at Paris ‡. I think you will have no hesitation in agreeing with me, that this monstrous animal must be referred to the genus *elephas*. As far as we are enabled to judge from the bony fabric of the animal, (and I take this to be an excellent foundation upon which to construct generic characters,) the American mam-

* Communicated by the Author. † This has been published.

‡ Mr. Peale junior, after exhibiting the mammoth here spoken of in London, returned with it for America without visiting the Continent of Europe.—EDIT.

moth was a true elephant. If in the form of his grinders, in the curvature of his defences or tusks, and in several other circumstances, he differed considerably from the living elephants that are now known to us, those differences do no more than assure us that the American animal constituted a species distinct from the (living) elephants of Asia and Africa. The American species is unquestionably lost; for nature, it would seem, is much less anxious to preserve the whole of her created species than some illustrious naturalists have supposed. The skeletons or bones of some other large animals, more or less allied to the family of elephants, have also been discovered in different parts of North America. Among these I recognize the grinders of a species which, if not the same as the elephant of Asia, must have been (as to the form of its grinders at least) more nearly allied to that species than is the mammoth. The bones of another large animal have been discovered. These appear to have belonged to a species of *trichechus*; perhaps to the *trichechus rosmarus*, or *morse*. We occasionally find the bones of some of the largest of the *cetacea* in situations very remote from those in which the living animals are at present to be seen. The scapula of a species of whale has been found at a considerable distance beneath the surface of the earth within the limits of the city of Philadelphia. Several years ago, the tooth of the monodon, or narwhal, was found at the distance of a few miles from the city. These last-mentioned facts, however, need not excite much surprise, since very extensive portions of the present dry country exhibit the most unequivocal proofs of an antient covering by the sea. I may add, that within the memory of our history whales were not uncommon in some of our bays and rivers, where they are no longer seen *.

You have, I suppose, heard of the large bones which have been found, in a nitrous cave, in the back parts of Virginia. Mr. Jefferson, the president of the United States, has given an interesting memoir on the subject of these bones in the fourth volume of the Transactions of the American Philosophical Society. He supposes them to have belonged to a large animal of the genus *felis*. But these remains must be referred to a very different family of animals; to some one of the genera in the order *tardigrada*: the *bruta* of Linnæus. I have little doubt that they and the bones found near the Plata, in South America, belong

* Since the above was written a whale (*balæna musculus*) about thirty-five feet in length was caught in the river Delaware, at the distance of several miles below Philadelphia.

to

to the same species: at all events, to an animal of the same genus; the *megatherium* of your countryman M. Cuvier. Many similar discoveries may be expected from the countries of the United States when it shall be our lot to possess men of more leisure than we do at present; or even when our labourers shall more generally know that subjects of this kind are interesting to philosophers both here and abroad.

In the fourth volume of the Transactions of the American Philosophical Society, I have given an account of a new species of dipus or jerboa, which I call *dipus Americanus*. I have discovered some other species of this genus, particularly one, which I call *dipus mellivorus*. It is very destructive to our bee-hives, eating the honey: hence the specific name. We are very rich in small animals of the order *glires* of Linnæus. There has lately been discovered a species of mus, somewhat larger than the common house-mouse, which has some of the singular habits of the opossum tribe. This animal is a native of Virginia and other parts of the United States. You have seen Dr. Shaw's account of the *mus bursarius*, or Canada rat. Either this species (which was discovered in Canada), or another very nearly allied to it, is common in the state of Georgia and other southern parts of the United States. In Georgia it is known by the ridiculous name of *salamander*. I take it to be the *tozan* or *tuza* of Clavigero. If the Canada and Mexican animal be the same species, its range through the continent is very great. But I have long since discovered that the quadrupeds of America have a very extended geographical range. I may say the same of the trees and other vegetables of this portion of the world.

I must now return to some of our large animals. The animal best known in the United States by the name of *elk* is essentially different from the *cervus alces*, or *moos*, and has not hitherto been described by any of your systematic naturalists. I call it *cervus wapiti* (*wapiti* being one of its Indian names), and shall give a pretty ample account of it in my Fragments, part ii., now in the press. This is not the only North American cervus with which the naturalists of Europe appear to be unacquainted. But what will you say, when I inform you that there has lately been discovered an American species of sheep! You know that some of the missionary jesuits, who visited California towards the end of the 17th century, inform us that they found in that country two sorts of deer, which they call sheep, from their resemblance, in make, to the sheep of Europe. The first sort is said to be as large as a calf of one or two years old; its head is much like that of a stag, and its horns like those of a ram. Both its tail and hair are speckled, and shorter than a stag's. Its hoof is large, round, and cleft like that of an ox. The flesh of this animal is said to be very tender and delicious. The second sort differs less from the sheep of Europe. Some of them are white, and others black. They are larger than the common sheep, have much more wool, which is very good, and easy to be spun and wrought *. In the History of California, by Venegas, there is a figure of one of these animals, which the Monqui Indians, inhabiting that country, call *tayé* †. Mr. Zimmermann seems to entertain no doubt that the taye (or tage, as he calls it,) is the same animal as the argali, or wild sheep, which inhabits the north-east parts of Asia and the country of Kamtschatka ‡. Mr. Pennant, though less positive, is of the same opinion §. This, however, appears to me to be a doubtful point. Venegas's figure rather forbids the idea that the Asiatic and American animal are the same. The horns of the former are less incurvated than those of the latter. The abbé Clavigero says the taye is "unquestionably the ibex of Pliny, described by count de Buffon under the name of *bouquetin* ‖." This cannot be; judging by the figure of the Californian animal, it appears to be most essentially different from the bouquetin, which is the *capra ibex* of Linnæus.

I have lately received some additional information concerning the existence of a large horned animal, probably the taye, in the country adjacent to the river Missouri, the great western branch of the Mississippi. This animal is a native of the Stony mountains, about the head waters of the Missouri. It is nearly of the size of an elk, and of the colour of a fallow deer. Its horns resemble those of a ram, but are turned, in a spiral form, like a trumpet, and are of an enormous size, some of them measuring eight (French) inches in diameter. The animal is said not to live longer than ten or twelve years, because its horns, advancing forward in proportion as the creature grows, finally pass the mouth in such a manner as to prevent it from eating grass, upon which alone it lives; and thus it falls a victim to its hunger. The Indians of the country make of the horns

* Philosophical Transactions abridged, &c. vol. v. part 2. p. 194.

† Noticia de la California, &c. tomo primero, p. 43, 44. Madrid 1757.

‡ Specimen Zoologiæ Geographicæ, &c. p. 632, 633.

§ Arctic Zoology, vol. i. p. 13, 14.

‖ The History of Mexico, &c. vol. xi. p. 324.

spoons

spoons and cups, some of the last of which are large enough to contain a sufficiency of food for the breakfast or dinner of four men.

I have been well assured that a small species of goat, spotted black and white, inhabits the country beyond the Mississippi, to the south of the Missouri. They are said to be numerous. They have also been seen, but less plentifully, about the mouth of the Arkansaw river, which empties itself into the Mississippi nearly in the latitude of 33° 50′. They are said to be much smaller than the common kind of goat, and extremely wild. This is possibly a variety of the common goat; but it is more probable that it is a distinct species, or perhaps a species of the genus antelope. Francis Ximenez, in his Account of New Spain, of which the country that is watered by the Arkansaw is a part, says there are in this country great numbers of rock goats, which the savages call *mazatl**. These rock goats may be the same animal as the small pied goat which I have mentioned. But this is conjecture. I must add that I have received the most undoubted information of the existence of great herds of a small horned animal in that part of New Spain which is watered by the Red river, a considerable western branch of the Mississippi †. From the description which has been communicated to me, I think there can be little doubt that the animal is either a species of goat or antelope, and very probably the *mazatl* of Ximenez. Clavigero's confident assertion, that the taye of the Monqui Indians is the bouquetin or capra ibex, renders it probable that this last-mentioned animal is actually a native of the western parts of North America. In that case, perhaps the mazatl and the Red river animal are no other than the ibex. Certain I am, that the taye cannot be the same as the ibex.

The *ursus maritimus*, or polar bear (*ours de mer* of Buffon), is said to be a common animal in the country adjacent to a river called the Plata, which empties itself into the Missouri about four hundred leagues above the junction of this latter river with the Mississippi. As the Missouri, from its source to its mouth, pursues a course nearly due east, we find that the white bear is common twelve hundred miles west of the Mississippi, nearly in the latitude of 40°; this being about the latitude of the mouth of the Missouri. To the north of this the animal is much more

* Francis Ximenez, as quoted by De Laet, in his Novus Orbis, p. 232.
† The mouth of this river is nearly in latitude 31°.

common. I have seen a number of the claws of one of these animals from the Plata, and they appeared to me to be the claws of the great polar bear.

If the animal which I have just mentioned should prove to be the ursus maritimus, we shall be obliged to assign to this species, in a geographical view of animals, a much more southern climate than it is supposed to exist in. And we should not forget that many facts conspire to render it probable that various species of quadrupeds were once more extensively diffused over the earth than they are at present. With respect to the very animal of which I am speaking, it would appear to have been formerly an inhabitant of several countries in which it is at present unknown. It is not certain, however, that the white bear of the river Plata is the ursus maritimus. Perhaps, it will prove to be a new species. The claws which I have seen lead me to suppose that the animal to which they belonged could not be inferior in size to the large white bear of the pole.

Permit me in this place to give you some account of the travels of an intelligent Indian who lately returned from a very long journey many hundred miles to the north-west of Detroit. This Indian had left his countrymen (the Mohawks) for the purpose of hunting, but was not inattentive to many of the objects of natural history about him. He reports, that the game of the country which he visited was the buffaloe (*bos Americanus*), black bear (*ursus Americanus*), white bear (*ursus maritimus?*), the latter much larger than the former, with a remarkably broad foot furnished with nails or claws as long as a man's finger: moos (*cervus alces*), elk (my *cervus wapiti*), "goats which climb up the rocks;" a kind of "sheep with a hairy back, much like a deer, but furnished with long wool over the belly, and with large horns," one of which he saw that weighed seven pounds; a kind of deer which the French in some parts of America call *cupree*; the fisher (I believe a species of mustela), the otter, the beaver, and a species of fox. He met with various kinds of birds which he had never seen before. The country which he passed through is covered with extensive plains, or praires (as they are frequently called in the United States), and has very few trees. Those which he saw were principally aspen (*populus tremuloides?*), birch (*betula*), and a species of *pinus*, or pine. During two winters that he resided in this remote part of our continent, it never rained once. The rains of the summer are very uncertain. Those which do fall are precipitated in heavy gusts.

gusts. Our philosophical Indian travelled in a canoe, but met with no less than seventy-six carrying-places in the course of his long journey.

[To be continued.]

XVI. *On the Action of Platina and Mercury upon each other.* *By* Richard Chenevix, *Esq. F.R.S. M.R.I.A. &c.*

[Concluded from p. 35.]

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other in such a manner as to disguise the properties of both. I shall therefore waive for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which does not happen when it is alone. I have tried to produce the same effect with other metals and platina, but I have not observed any thing similar. It is therefore fair to conclude, that when a solution of platina is precipitated in a metallic state by a solution of green sulphate of iron, either silver or mercury is present.

The precipitation of a mixed solution of platina and silver requires no further caution than to free the salt of platina as much as possible from muriatic acid; for, as I observed in my former paper, the effect of nitrate of silver poured into muriate of platina, is to produce a precipitate, not of muriate of silver, but of a triple muriate of platina and silver. It was by this experiment that I then proved the affinity of these two metals; for, when silver is not present,

 muriate

muriate of platina is among the most soluble salts. The best method of presenting the three solutions of platina, silver, and green sulphate of iron to each other, is first to pour the filtered solution of the last into the solution of platina, and then, after mixing them thoroughly together, to add the solution of silver by degrees, and to stir them constantly. In this, as in all similar operations, the presence of all acids, salts, &c. excepting those necessary for the operation, should be avoided; and if proper proportions have been used, and all circumstances attended to, the precipitation of these two metals will be very complete.

But the precipitation by a solution of mercury requires to be further considered, as the state of oxidizement of this metal, as well as the acid in which it is dissolved, produces a considerable modification in the result. In the first place, the oxide, at the minimum of oxidizement, dissolved in muriatic acid, is unfit for the experiment; and even the red oxide dissolved in the same acid, or corrosive sublimate, is not the most advantageous. When a warm solution of the latter is poured into a mixed solution of platina and green sulphate of iron also warm, as in the case of silver, these substances are brought into contact under the most favourable circumstances. Yet even thus the precipitation is slowly and imperfectly formed, often not till several hours have elapsed; and sometimes a very great deficiency of weight is observed between the quantities used and those recovered directly by this method. If a solution of nitrate of mercury be used, the effect is produced more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of silver, and the combination which ensues from it, suggested to me an experiment which I must state at length, as from the result of it consequences are deduced which modify some of the experiments of my former paper.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any substance but the solvents of these metals, might be accomplished as in the case of silver and platina. I therefore poured a solution of nitrate of mercury, which solution, being at the minimum of oxidizement, consequently formed an insoluble muriate with muriatic acid, into a solution of muriate of platina. The result was a triple salt of platina and mercury, which, when the mercury was completely and totally at the minimum of oxidizement, was nearly insoluble. To procure it in this state it is sufficient to put more metallic mercury into dilute nitric acid than the nitric acid can dissolve, and to boil them together. This

triple

triple salt of platina and mercury shall be presently examined. From this it is evident that to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid, the addition of green sulphate of iron is superfluous.

But if mercury be raised to its maximum of oxidizement in nitric acid the case is different, for no precipitation occurs till the green sulphate of iron is added. The most advantageous method for precipitating platina and mercury by green sulphate of iron is, I believe, the following. Mix a solution of platina with a solution of green sulphate of iron, both warm, and add to them a solution of nitrate of mercury at the maximum of oxidizement, also warm. It is necessary to avoid excess of acid, salt, &c. in this as in all such cases. With due care the precipitation of both metals will then be complete.

By comparing the experiments made with mercury and platina with those made with silver and platina, a striking resemblance will be found. This induced me to pursue the analogy, and to examine whether, independently of the action of platina, mercury had not the same property of being precipitated by green sulphate of iron as silver. Nitrate of silver is precipitated by green sulphate of iron, but muriate of silver is not sensibly acted upon by the same reagent. The insolubility of muriate of silver might be alleged as the cause of this, if I had not tried the experiment by pouring nitrate of silver into green muriate of iron, in which case all the substances were presented to each other in solution. The result was not reduction, but muriate of silver and nitrate of iron. This fact rests upon a much more extensive basis than mere mechanical circumstances; and, if pursued with the attention it deserves, it would lead us into the wide expanse of complicated affinities and their relations. From reasoning alone we should be disposed to think that an acid, so easily decomposed as the nitric, would be sufficient to prevent the reduction of a metal which it can dissolve. But on the one hand it can spend its oxygen upon a part of the oxide of the green sulphate of iron, while on the other its affinity for oxide of silver is not powerful enough to retain it, when there is another part of the oxide of iron present to deprive it of oxygen. But the affinity of muriatic acid for oxide of silver, one of the strongest at present known, is sufficient to counterbalance all the other forces. There are many other instances of the same kind.

If then a solution of green sulphate of iron be brought into contact with either soluble or insoluble muriate of mercury,

mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be dissolved in nitric acid, and green sulphate of iron be added, the mercury is precipitated in the metallic state.

These experiments are much stronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general consideration, but in their application to the present subject. They most materially modify and are indispensable to the accuracy of the results I formerly stated; but I was not aware of them at the time I first engaged in the investigation of this subject. I can also now explain a very material difference between some proportions observed by M. Richter and myself in an experiment which that chemist had made as a repetition of one of mine.

I had poured a solution of green sulphate of iron into a solution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate consisting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment; that is, he used 100 of gold and 300 of mercury, and obtained a precipitate weighing 102. He is surprised at the difference of weight between our results, which might be owing to his method of repeating the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury.

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury, which I believe to be in the same state; my reason for thinking so is, that I have often observed, that a glass vessel, in which I had sublimed some

some of it, was lined with a thin gray metallic coat. If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury, by green sulphate of iron, cannot be adduced as an argument to support the affinity of these metals, since the effect is the same whether they are separate or united.

These preliminary considerations were necessary, as well for the rectification of my former experiments, as for the pursuit of my present object: and now to return to platina.

Exp. 1. If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in solution. This effect makes it advantageous to use a greater quantity of the solution of mercury than is merely capable of drawing down the given quantity of platina along with itself in the form of a metallic precipitate. When this precipitate is washed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but those of the mercury and the green sulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer dissolves any part of it, there still remains more undissolved matter than the original weight of the platina used in the experiment. By exposure to heat, little more is left in general than the original platina; and sometimes even a diminution may be observed; for, as the experiment is not attended with uniform success, it does not always happen that the whole of the platina is precipitated, but a portion of it will sometimes resist the action of the green sulphate of iron, even when sufficient mercury has been used. Before the precipitate has been exposed to heat it is dissolved more easily than platina by nitro-muriatic acid; and the solution, when nearly in a neutral state, gives a copious metallic precipitate (yet not equal to the quantity employed) when boiled with a solution of green sulphate of iron.

Exp. 2. When a mixed solution of platina and mercury is precipitated by metallic iron, a quantity equal to the sum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate so formed, the original weight of platina, together with a considerable increase, remains behind, nor can nitric acid sensibly

sibly diminish it. It yields more easily than platina to the action of nitro-muriatic acid, and its solution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green sulphate of iron. If this precipitate be exposed to a strong heat after it has been boiled with nitric acid, it loses a great part of its weight, and the platina alone will generally be found to remain.

Exp. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of count Mussin Pushkin to form an amalgam, and, after being rubbed for a considerable time with mercury, is exposed in a crucible to a heat gradually increased till it becomes violent, a metallic powder remains in the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this paper.

Exp. 4. If sulphur be added to the ingredients recommended by count Mussin Pushkin, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the button which results from the operation is generally more considerable.

Exp. 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

Exp. 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

Exp. 7. If to a mixed solution of platina and mercury phosphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

Exp. 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured

poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes place is independent of every substance except the metals themselves.

Exp. 9. One of the most delicate tests that I have observed in chemistry is recent muriate of tin, which detects the presence of the smallest portion of mercury. When a single drop of a saturate solution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a saturate solution of recent muriate of tin are added, the liquor becomes a little turbid, and of a smoke-gray colour. If these 500 grains of liquid be diluted with ten times their weight of water, the effect is of course diminished, but still it is perceptible. I had on a former occasion observed the action of recent muriate of tin upon a solution of platina. If a solution of recent muriate of tin be poured into a mixed solution of platina and mercury, not too concentrated, it can hardly be distinguished from a simple solution of platina. But if too much mercury be present, the excess is acted upon as mercury; and the liquor assumes a darker colour than with platina alone.

From all these experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic state by green sulphate of iron. By Experiments 1 and 2, it is proved, 1st, That platina can protect a considerable quantity of mercury from the action of nitric acid: and, 2dly, That mercury can increase the action of nitro-muriatic acid upon platina. From Experiments 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in such a manner as not to be separated by the degree of heat necessary to fuse the compound, since after the fusion it retains that property, which is essentially characteristic of the presence of mercury in a solution of platina. The eighth Experiment proves that the action of mercury upon platina is not confined to the metallic state; but that these metals can combine and form an insoluble triple salt with an acid which produces a very soluble compound with platina alone. The ninth Experiment shows that platina can retain in solution a certain quantity of mercury, and prevent its reduction by a substance which acts most powerfully to that effect, when platina is not present. That part of the general position, therefore, which is the object of this paper is proved, if these experiments, upon being repeated by other chemists, shall be found to be accurate.

One or two of the above experiments seem to be in contradiction to some that I have stated in my paper upon palladium; for in the present examples platina protects mercury against the action of nitric acid; whereas in palladium the mercury is not only acted upon itself, but it conduces to the solution of platina in the same acid. I am well aware of this objection; but, confining myself to my present object, I shall waive all further discussion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemistry, that the strongest affinities are those which produce in any substance the greatest deviation from its usual properties.

When a button of the alloy of platina and mercury, as prepared by any of the above methods, is dissolved in nitro-muriatic acid, and afterwards precipitated by green sulphate of iron, the entire quantity of the alloy used is seldom obtained. A considerable portion of platina resists the action of green sulphate of iron, and remains in solution. This may be looked upon as the excess of platina, and can be recovered by a plate of iron. Hence it appears that less mercury is fixed than can determine the precipitation of the entire quantity of platina; yet in this state it can draw down a greater quantity of the latter than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also, that substances possessing different properties have resulted from my attempts to combine platina with mercury.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above.

But,

But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and not unlikely, that a portion of mercury may be retained in solution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean result, however, was that the precipitate by green sulphate of iron consisted of about 17 of mercury and 83 of platina, when the specific gravity was about 16.

With regard to palladium, lest it should be supposed that either my own observations or those of others have given me cause to alter my opinion, I will add, that I have as yet seen no arguments of sufficient weight to convince me, in opposition to experiment, that palladium is a simple substance. Repeated failure in the attempt to form it, I am too well accustomed to, not to believe that it may happen in well conducted operations; but four successful trials, which were not performed in secret, are in my mind a sufficient answer to that objection. By determining the present question we may overcome the prepossession conceived by many against the possibility of rendering mercury as fixed, at an elevated temperature, as other metals; we may be led to see no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path, between the visions of alchemy on the one hand, and the equally unphilosophical prejudices on the other, which they are likely to create. In the course of experiments just now related, I have seen nothing but what tends to confirm my former results; yet the only means which I can, after all, prescribe for succeeding, is perseverance.

To ascertain whether the opinion of Messrs. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium, had any just foundation, I observed the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

I might have added some more experiments to corroborate the evidence I have adduced to prove my assertion of the fixation of mercury by platina; but Messrs. Vauquelin and Fourcroy have promised the Institute of France a continuation of their researches, and M. Richter concludes his paper with saying that he will return to the subject. From the labours of such persons some great and important fact must issue, and I hope that the present subject will not be excluded from their consideration. The facts contained in

 this

this paper cannot be submitted to too severe a scrutiny; and no judge can be more rigid or more competent than the very person who was the first to doubt my former experiments. But it is necessary to be observed by whoever shall think them worth the trouble of verifying, that even these experiments are liable to fail, unless proper precautions are used; that I have never operated upon less than one hundred grains; and that the results which I have stated, however simple they may appear, have been the constant labour of some weeks.

POSTSCRIPT.

Since this paper was written, Dr. Wollaston has published some experiments upon platina. He has found that palladium is contained in very small quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. Wollaston. I have not yet seen a copy of his paper; but I shall merely observe here, that, whatever be the quantity of palladium found in a natural state, no conclusion can be drawn as to its being simple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events the amalgamation to which platina is submitted before it reaches Europe, is sufficient to account for a small portion of palladium.

XVII. *Observations on the polishing of Glass, and on the Amalgam used for silvering Mirrors.* *By* B. G. Sage *.

Having been consulted in regard to the bad effects of some calces or red oxides of iron, which alter the surface of glass by rendering it dull and yellowish, I analysed these calces of iron, and found out the cause on which this defect depends. Red calx or oxide of iron, called *colcothar*, is employed with water for giving the last polish to glass intended for mirrors.

Were not the oxide or calx of tin, commonly known by the name of *putty*, so dear, it would be far preferable to red calx or oxide of iron, obtained by the decomposition of martial vitriol, either by calcining it in a fire proper for disengaging the acid or decomposing the sulphate of iron by marine salt. In the latter case, the red oxide or calx of iron retains a little of that salt, which is of no hurt in the

* From the *Journal de Physique*, Thermidor, an 12.

polishing

polishing of glass: but the case is not the same if the colcothar or red oxide of iron retains martial vitriol. This salt, when dissolved in water, is decomposed, and the yellow ochre which results from it penetrates the glass, forms a crust on it, and renders it greasy, dull, and yellowish; a tint which is communicated to the image of the object presented to the mirror.

Glass when smoothed and polished does not acquire the property of reflecting objects till it has been *silvered* (as it is called), an operation effected by means of an amalgam. The tin leaf employed must be of the size of the glass, because, when pieces of that metal are united by means of mercury, they exhibit the appearance of lines. Tin is one of those metallic substances which become soonest oxidated by the means of mercury. If there remains a portion of that calx, of a blackish gray colour, on the leaf of tin, it produces a spot or stain in the mirror, and the part where it is cannot reflect objects presented to it: great care, therefore, is taken in silvering glass to remove the calx of tin from the surface of the amalgam.

The process is as follows:—The leaf of tin is laid on a very smooth stone table, and mercury being poured over the metal, it is extended over the surface of it by means of a rubber made of bits of cloth. At the same moment the surface of the leaf of tin becomes covered with blackish oxide, which is removed with the rubber. More mercury is then poured over the tin, where it remains at a level to the thickness of more than a line, without running off. The glass is applied in a horizontal direction to the table at one of its extremities, and being pushed forwards it drives before it the oxide of tin which is at the surface of the amalgam. A number of weights are then placed on the glass which floats on the amalgam, in order to press it down. Without this precaution the glass would exhibit the interstices of the crystals resulting from the amalgam. These crystals have the form of large square laminæ irregularly disposed.

To obtain leaves of tin, which are sometimes six or seven feet in length, with a proportionate breadth, they are not rolled but hammered. The prepared tin is first cast between two plates of polished iron, or between two smooth stones not of a porous nature, such as thunder stone. Twelve of these plates are placed over each other; and they are then beat on a stone mass with heavy hammers, one side of which is plain and the other rounded. The plates joined together are first beaten with the latter: when they become

 extended

extended the number of the plates is doubled, so that they amount sometimes to eighty or more. They are then smoothed with the flat side of the hammer, and are beat till they acquire the length of six or seven feet, and the breadth of four or five. The small block of tin from which they are formed is at first ten inches long, six in breadth, and a line and a quarter in thickness.

When the leaves are of less extent, and thin, from eighty to a hundred of them are smoothed together.

Tin extracted from the amalgam which has been employed for silvering glass, exhibits a remarkable peculiarity. When fused in an iron pan, its whole surface becomes covered with a multitude of tetraëdral prismatic crystals two or three lines in length and a quarter of a line in thickness. The interior of these pieces of tin, when cut with a chisel, have a grayer tint than pure tin, which is as white as silver. The latter crystallizes also by cooling; but it requires care. When it begins to be fixed, decant the part which is still in fusion, and there will remain at the bottom of the crucible beautiful crystals of a dull white colour, which appeared to me to be cubes or parallelopipedons.

The peculiar and constant crystallization of tin taken from the amalgam of mirrors, the leaden gray colour which the mass of this metal had, and the mystery made of the preparation of this tin, induced me to try whether I could not discover by analysis the substance mixed with it.

Having calcined this tin in a test, it was reduced to a powder of a delicate red colour, and increased in its weight 1-25th. The magnet attracted particles of iron, the result of the hammering. It appears that this metal concurs to produce the crystallization of the tin, and the singularity exhibited by the solution of its oxide in nitric acid. At first, nothing is manifested but a slight effervescence, which soon subsides; but four or five minutes after, the mixtures become very hot, and a stronger effervescence takes place, accompanied with a great deal of nitrous gas, which is disengaged with an explosion, and there remains in the glass a magma of a pale red colour.

The white oxide of tin, mixed also with nitric acid at 32°, exhibits neither effervescence nor disengagement of nitrous gas.

I fused this reddish calx of tin with three parts of black flux and a little charcoal powder, and extracted from it 18 pounds of tin per quintal. This metal was brittle, a property arising from the lead, which contributes also to attenuate the colour of the tin. If the lead is found there in

4 larger

larger quantity, it is because there are four-fifths of tin absorbed by the alkaline flux.

To determine the quantity of lead contained in the tin extracted from the amalgam of mirrors, I decomposed a hundred parts of it by four hundred parts of nitric acid at 32°. A great deal of nitrous gas was disengaged, and there remained at the bottom of the matrass a white magma. I washed it with distilled water, and evaporated the ley, which produced a twenty-fifth of nitrous ammoniacal salt mixed with nitrate of lead, which predominates, and forms nearly two-thirds of the saline residuum; a proportion which would indicate that the tin employed for silvering mirrors contains three pounds of lead per quintal.

I now return to the mercury extracted by distillation from this amalgam. It volatilizes a portion of tin, which remains there so intimately combined that it cannot be separated by a second distillation of the mercury. I was able to disengage from it the tin by shaking the mercury with nitric acid, which attacks and oxidates the tin. I washed the mercury and strained it through a piece of linen. In this state it may be employed for gilding, but when it contains the smallest quantity of tin it stains the articles.

What I have related in this memoir shows that red oxide of iron, known under the name of colcothar, is not proper for polishing glass when it contains vitriol; that the tin employed for silvering mirrors contains lead and iron; that when this tin is separated from the mercury by distillation this metal crystallizes with the greatest facility and without any precaution; and, in the last place, it is shown that a portion of tin is volatilized by the mercury during the distillation of the amalgam, and that it cannot be separated but by the nitric acid.

XVIII. *Extract of two Letters from Captain* Von Krusenstern, *Commander of the Russian Expedition to Japan. dated the Harbour of St. Peter and St. Paul, July* 19, *and August* 20, 1804.

[Concluded from p. 13.]

The Frenchman is now at Kamtchatka. I shall mention hereafter by what accident he remained on board the ship. This man is a singular phænomenon: he had forgotten his own name, those of his father and mother, and that of the place from which he came. He sung to us some patriotic

 songs,

songs, which he, however, mutilated very much. He at length recovered his French; and then remembered that he came from Bourdeaux, that his father was named John Cabrit or Joseph Cabrit; but instead of Cabrit he sometimes said Cadiche; and Roberts called him John. As he now saw that he could no more get back from the ship to his dear Nukahivah, he exhibited a wonderful mixture of melancholy and levity. Sometimes he would fall a-laughing, and afterwards say in a whining tone: *Moi beaucoup triste la madame, la mademoiselle.* He had a wife who had brought him a son or a daughter; and his father-in-law had given him a house with coco-nut and bread-fruit trees. It was curious to observe in what manner his ideas were expanded. One time, recollecting some of his joyful scenes at Bourdeaux, he suddenly exclaimed, as if he had seen a vision,—*Beaucoup de chandelles, beaucoup de violons, beaucoup de musique, les madames, les mademoiselles!* As may be readily supposed, we did not know what this meant; but we at length conceived that he might remember his having been at the play. He still thought of Nukahivah, and he has not yet given up the idea of returning thither. He soon recovered his French, and made use of expressions which he could not have learnt from us, as we were not acquainted with them, such as the names of the different sails, &c. He often afforded us subject of laughter. Having asked him in what the natives of Nukahivah showed acuteness, he replied,—*Beaucoup d'esprit, ill ne couchera pas avec sa soeur, un autre baisera sa soeur et il couchera avec un autre fille, beaucoup d'esprit!* I here quote his own words. From these expressions it is seen that certain degrees of consanguinity are in that island forbidden. I once gave him a good new shirt, but he immediately bartered it with one of the sailors for a red flannel jacket. When I told him that he had suffered himself to be cheated, he would not listen to me. As soon as he went on shore he put on the jacket, and with feathers on his head and a lance in his hand danced on an eminence, capering and jumping in a most extraordinary manner. Several of the natives then wished to accompany him, in order that they might go to war. He now showed them how they would creep along and conceal themselves behind bushes or among the grass, and in what manner, when they fell in with any of the enemy, they would beat them and carry them off. He seemed to be inspired with an enthusiastic spirit of warfare, and extolled what gives these people so much pleasure. When asked whether he would himself eat any of the flesh of of their enemies, he replied "No, I have never done so:" he added, that the sight of others eating it had made him sick: he had killed three enemies, but had exchanged them for swine. The latter circumstance was confirmed by the Englishman. From this it appears that war among these people is a kind of amusement, as hunting is among us. Ketenue is very averse to it, but is not able to prevent it. When I asked him why he did not prevent it, Joseph answered, When five or six seize an enemy and put him to death, they carry him into the woods and there devour him. How then can Ketenue prevent it? Having spoken so much of their enemies, I must now mention who they are. All the islands in the South Seas are exceedingly mountainous, as is the case with the Marquesas and Nukahivah: the fertile and inhabited valleys are separated by high, steep, and barren mountains. Tapeka Ketenue was the chief man at the Bay of Tayohoæ, where we lay at anchor; and other valleys are in the possession of other chiefs. With the inhabitants of many of these valleys the former are in a state of warfare. These are their enemies. The hostile parties are always separated by high mountains, and at each expedition these must be clambered up. They have few war canoes, for the purpose of undertaking expeditions by water. Ketenue's daughter was married to the chief of another bay, and, as she was conveyed thither by sea, it was agreed that during her life no war should be carried on at sea with the inhabitants of that bay. Should she come to Tayohoæ on a visit and die, her spirit would remain there, and no naval battle could be fought between the two bays. Hogs are killed on all their festivals and occasions of solemnity. When any one dies a banquet must be given; and therefore it is so difficult to procure any of these animals.

Their god Atua is the body of their deceased priest. The body is first besmeared with coco-nut oil, and toasted in the sun till it becomes hard and dry: it is then wrapped up in a piece of cloth, and being suspended in the priest's house, in the morai or wahitaabo, becomes their god. Sometimes it is consulted as an oracle; and Joseph is still convinced that the answers it gives are infallible. Roberts firmly believes that the natives here are well acquainted with the art of witchcraft. Joseph considers himself as an adept in it, and asserts that by means of certain knots he can make a sick person so ill that he must die. I had the courage to suffer him to try the experiment on myself; but unfortunately there is here no morai, where the string with the knots upon it must be buried. When the priest dies, war

 immediately

immediately ensues; and they then endeavour to find out some enemies, who are either eaten or merely hung up, I do not exactly know which, in honour of the deceased. When the priest dreams that he has eaten human flesh, he tells whether the person was tatooed or not, and gives nearly a correct description of him. A state of war now ensues; that is to say, some of them creep towards the enemy to endeavour to find a person answering this description, and, according to their opinion, the person of whom the priest dreams, will always fall into their hands: the person is always a *kikino*, that is, one who has broken *taaboo*. The Frenchman had been in several of the Marquesa islands; first in Santa Christina. The people, manners, and customs, are in all the same.

On the 18th of May we took advantage of a light breeze to get out of Anna Maria or Tayohoæ Bay. On this occasion the Frenchman remained on board. We had forgotten to send him on shore. When we were out in the open sea, the wind was so strong and so fresh, that, though an excellent swimmer, he would not venture to swim back to the shore. Our passage to the Sandwich islands, as is always the case between the tropics, was pleasant and agreeable, the weather being always fine.

On the 8th of June, in the afternoon, we were opposite to the south-east coast of the island of O-why-he, at the distance of about three or four Italian miles. The natives brought off to us a dish of sweet potatoes and a small hog; one of the women also came on board the ship. Towards night we stood off from the land, and next day were at the southern extremity at about the same distance as the preceding day. We purchased a small hog; a larger one they carried back with them because we could give them no cloth, of which they wanted a large quantity. A frock which we offered them was too small for them. On knives, axes, and mirrors, they set no value; and therefore we supposed that they must have been supplied with all these articles by the English and the Americans. Several of them spoke a little English: a woman who was among them held out her hand to us, and kept continually repeating,—"How do you do? Very well."

On the 10th we were opposite to the western coast. We here saw the large mountain Morno Roa, which is said to be as high as the Peak of Teneriffe. As the summit of it is very broad, it does not appear to have the same elevation. We were a great way from the shore, and no boats came off to us.

We

We now parted from the Neva and proceeded south-west, while the Neva determined to come to anchor in Karakakua Bay, where the celebrated Cook was killed. The natives of the Sandwich isles are more industrious than those of the Marquesas. Their canoes are exceedingly neat, and fully secured from being overset, by outriggers applied to them. The sea was very rough at the time when they visited us; but they suffered no damage. The cloth which they make of the bark of the paper-mulberry is very beautiful, and of bright colours. The broad girdle worn by the woman was of a lively red colour. The men are smaller than those of the Marquesas, and seem to be artful and deceitful people. In most of them the incisor teeth are wanting. These they are very careful to knock out. The women are larger and more elegantly made than those of the Marquesas. Both sexes have a darker colour than the natives of the Marquesas, though the climate in the latter is much warmer. Many of them also are tatooed, but not so strongly: some of them had several figures on their legs; and one had the figure of a musket and bayonet on the arm. This is nearly the whole of the intercourse we had with them. When we return from Kodjak we shall pay them another visit.

To be sure of the trade wind we again approached the equator, and proceeded west on the parallel of 17°. The weather was fine, and the wind as favourable as possible; so that we advanced more than two degrees every day. As we turned towards the north we began to be sensible of the effects of custom; the temperature of 18° of Reaumur was somewhat disagreeable to us. I asked Joseph whether it was ever so cold in Nukahivah. "*Jamais, jamais,*" replied he. In the latitude of 36° north, and longitude 191° west from Greenwich, we proceeded some degrees towards the west, but saw nothing of the Silver islands.

On the evening of July 13 we saw the high mountains of Kamtchatka. On the 15th we entered the harbour of St. Peter and St. Paul, and came to anchor. When we saw the fine prospect which here presented itself, we could not believe our own eyes: there was still a little snow to be seen on the summits of the highest mountains, but the lower ones were covered with beautiful verdure; and the smell of the birch-trees was perceived on board the ship. The only scorbutic patient in the ship was sent on shore, where he soon recovered. We were extremely fortunate, when it is considered that we were five months at sea, and for the last four months had lived entirely on salted provisions. Our sour crout had become spoilt at St. Catherine's, and was thrown overboard. A pthisicky German cook, who would not remain in Brazil though every kind of support was offered to him, died three days after we had crossed the line. The heat of Nukahivah had entirely exhausted him. He would certainly have died in Europe, but perhaps some months later.

The arrival of the Nadeshda was a very fortunate circumstance for this country. The beneficial effects of it are already felt by all Kamtchatka. Many necessary articles could no longer be obtained, and others had risen to a most exorbitant price. This evil has been remedied. Brandy, for example, at certain times had been sold for fifty rubles per can: from eighteen to twenty rubles was the common price; and at present it is six rubles: all other things were in the same proportion. The ambassador Resanof, and general Koschelef, a fine young man, settle the price, and superintend all public transactions.

In regard to the Kamtchatdales I have little to say; few of them now remain. All those in Paratunka have become extinct. Those left have, in a great measure, lost their originality of character: the only thing by which they are at present distinguished is, that they are excellent bear-hunters. There are here a great many bears, which feed on fish. I was told that above forty of them were seen round a whale lately cast on shore: this, perhaps, may be exaggeration, but it is nevertheless certain that they are very numerous.

I lately was present at an original Kamtchatdale dance: it represents the courtship of a bear. The bear, animated by the passion of love, gesticulates with great violence; emits wild tones, which die away almost in the throat: the female, by no means insensible to these strong indications of tenderness, answers with a kind of growling and snarling; her motions, however, were much more moderate. The whole was exceedingly disgusting, and a remain, no doubt, of the antient Schamans, as well as the dance of birds.

We lately received a visit on board ship from two of the natives. As we gave them a good reception they were highly pleased, and praised us much; telling us that we were very good men, just like the Kamtchatdales.

All the houses here, without exception, smell like stock-fish. The degree of the smell is determined by the greater or less cleanliness of the inhabitants. The people eat scarcely any thing but fish. The howling of the dogs, which

which are a kind of shepherds' dogs, is heard here almost every evening. They are very numerous; but tame, and never hurt any one. This harmlessness of disposition arises from their being fed by all the inhabitants; for every person who catches fish, as soon as the nets are drawn, throws a few to the dogs, which are accidentally present, and therefore they are fond of frequenting the sea shore. In winter they are chained up.

Captain Clark's grave is below an old birch tree. The epitaph La Perouse caused to be engraved on a plate of copper, and under it was inscribed,—"By the order of *count de la Perouse, chef d'escadre*," &c. The copper plate was fixed up with nails, but was nevertheless stolen. After this circumstance a voice was heard every night demanding it back; and it was at length restored. It is not nailed up at present, yet no person touches it.

The ship Slava Rossie, in which Billings performed his voyage, lies here sunk in the harbour.

Great complaint having been made here that there was no establishment for taking care of the sick, and that many perished for want of proper assistance, general Koschelef proposed a subscription, which was seconded by the ambassador; and each having subscribed 1000 rubles, the sum of 4000 was collected in the course of less than an hour. On its being remarked that this circumstance would give great satisfaction to the emperor, the enthusiasm became general. All this was done on board the ship, and it is not improbable that something has been collected on shore. I shall send you a further account of Kamtchatka when I return from Japan.

Our water continued sweet, and never became corrupted: for this we were indebted to the care of the captain, who always caused the casks to be charred. When at Copenhagen he had read in a journal, edited by Pfaff and Friedlander, that water would keep a long time uncorrupted in charred casks; and this we have found perfectly confirmed.

The following trait will serve to show Joseph's way of thinking, and what he considered allowable in Nukahivah. When the ship was unloaded here, it was found that a great deal of mischief had been done by the rats; they had emptied several pipes of wine, or at least gnawed the casks in such a manner that the wine had run out. As the whole almost of our provisions were on shore, they became very restless, and appeared in great numbers. I recollected that I had among my medicines *nux vomica*, some of which I mixed

mixed with dough. When Joseph heard that this would destroy rats, he asked if it would kill men also. Having answered in the affirmative, he requested me to give him some of it. On asking him what he meant to do with it, he replied, that when he returned to Nukahivah and ate with any of the natives, he would mix it privately with the food, so that the person should die. When I represented to him the atrocity of this idea, and told him that he might perhaps poison us also, he replied, that it would be highly criminal to poison us, but that in Nukahivah it was nothing; the natives, he said, bewitched each other in such a manner that they must die, and poisoning was not worse.

It is mentioned in Cook's Voyages that he frequently interchanged names with some of the kings or chiefs. Cook, in all probability, was taabooed by assuming the king's name; and the king took Cook's name in order to be taabooed against the English. But Cook, perhaps, laboured under a mistake; for Ketenue gave his name to captain Krusenstern, but assumed none in return. The same custom prevails in almost all the islands of the South Sea: it is to be supposed that Cook and other navigators must necessarily have fallen into many mistakes; for of this circumstance we should have been entirely ignorant, had it not been for the information given to us by Joseph and Roberts.

There have been English missionaries at Otaheite, Santa Christina, and even in Nukahivah. The one who was in Nukahivah was called Crook, and had the name of Ketenue's son: he was not able, however, to convert any of the natives, and soon quitted the island. These missionaries will have it in their power to communicate the most certain information respecting the natives of the islands in the South Sea, for some of the accounts given by others are contradictory.

I have had an opportunity of making a very droll observation. Wherever we touched where we did not understand the language, each person endeavoured to remedy that defect by the language of which he understood the least. One of our naturalists spoke Russian to the inhabitants of Nukahivah; the sailors spoke Portuguese: but in Brasil they had spoken English and Danish. A droll fellow, of the name of Kurganon, endeavoured to make his way with German, of which he understood only two words: *Wollen sie?* Will you?

The Kamtchatdales of Paratunka are said to have had the yellow fever. The under surgeon here called it *Febris Americana*

ricana flava. In inquiring the symptoms I committed a fault; for, instead of waiting for his answer, I said, "Were they not so and so?" And all his answers were in the affirmative. I need hardly remark, that on our arrival here we differed one day in our reckoning: to us on board the ship it was Sunday; on shore it was Monday.

XIX. *Extract from a Memoir of M.* PAYSSE, *principal Preparer of Medicines at the Camp of Utrecht, on the manufacturing, on a large Scale, of some Oxides of Mercury. By M.* PARMENTIER*.

M. PAYSSE, to whom we are indebted for the following information, in regard to the manner in which the Dutch prepare, on a large scale, red sulphurated oxide of mercury and red oxide of mercury by nitric acid, does not lose a moment to take advantage of his stay in Batavia. He seizes every opportunity of visiting the manufactories, so numerous in that country, as well as the cabinets of the curious, which may contribute towards his instruction. The difficulties of every kind which he encountered before he could get admission to these manufactories are a sufficient proof of his ardent zeal for the arts and sciences; but amidst the disappointments which the traveller experiences on his route while making researches, he often obtains an indemnity for the care he takes, and the sacrifices he makes, when he is so happy as to meet with any objects proper for seconding the desire he has to add to his knowledge, and to enrich his country with the discoveries of the industrious nations he is so fortunate as to visit. Such is the advantageous situation of M. Payssé, who has already procured us detached information in regard to many interesting objects. The reader will easily form an opinion of them from the following extract from the last memoir addressed to me by this chemist.

Red sulphurated Oxide of Mercury (Cinnabar).

This matter is prepared only in two manufactories at Amsterdam. The most considerable is that which belonged to the late M. Brand. M. Payssé assisted at an operation in which 800 pounds were prepared, divided into two portions. He observed all the details with every possible at-

* From the *Annales de Chimie,* No. 152.

tention;

tention; and, after comparing them with those published by M. Tuckert in the fourth volume of the *Annales de Chimie*, for the year 1790, he found only a very slight difference; so that the description of the chemist of Amsterdam may be considered as nearly exact. The following is what he has omitted. He does not speak of the duration of the flame, nor of its colour, which arises from the combustion of the union of the sulphur and mercury previously prepared and introduced into the apparatus. This flame, the disengagement of which is exceedingly rapid, exhibits the most various colours; first of a bright dazzling white, rising at least twelve decimetres above the dome of the furnace; then yellow and white orange yellow; blue and yellow, giving birth to the green and violet shades, and at last to blue and to green. Its disengagement is overcome towards the end by a sort of register of iron plate, when it no longer rises but to the height of some centimetres, and its colour becomes indigo or sky blue. The apparatus is then hermetically sealed, and luted on the outside with a mixture of clay and sand.

There is no doubt, from the loss experienced in the result of the operation, that the shades so various of this flame, the disengagement of which lasts about half an hour with 200 kilogrammes of matter, arise from the union of the sulphur with varied proportion of mercury at different degrees of oxygenation. The 400 kilogrammes, or about 800 pounds of red sulphurated oxide of mercury, were reduced to 379 or 322 kilogrammes, or between 738 and 745 pounds, which form a loss of from 27 to 31 kilogrammes.

In speaking of the vessels employed in this operation M. Tuckert forgot also to describe exactly their form. The principal vessel is not a jar, but a kind of crucible, round which the heat circulates, and which has over it an iron dome, through the summit of which the matter is introduced after the crucible has been brought to a red heat. The success of the operation depends in an essential manner on the management of the fire during the sublimation. The fuel employed is turf, and, according to M. Payssé's observation, none is better calculated for the purpose when a constant and moderate heat is required. This uniformity of temperature during the thirty hours of heat maintained in the furnace is, no doubt, one of the causes which contributes most to the success of the operation. Besides, experience, according to the acknowledgment of the workmen, speaks in favour of this opinion.

When red sulphurated oxide of mercury is prepared, a very

very large quantity of it is pulverized to be converted into vermilion. This preparation is still a secret among the Dutch. In every work of chemistry, however, the process for obtaining it is described. They merely say that the cinnabar is to be reduced to powder, then washed in water and dried. This method, which M. Paysse often employed, gave him always, indeed, for product a very beautiful red powder; but it must be allowed that it is inferior in beauty and the splendour of its colour to that manufactured in Holland.

China furnishes painting with a kind of red sulphurated oxide of mercury in powder (vermilion), much more esteemed than that of the Dutch. It is of a beautiful red colour, with a shade, the splendour of which nothing can equal. For some years, therefore, the Dutch have endeavoured to imitate it. M. Payssé saw some prepared in the manufactories of that country, the process of which is another mystery. This oxide rivals in beauty that of the Chinese. He is of opinion that in a little time it will attain to the same degree of perfection.

As he was not able to obtain any information in regard to the means employed to imitate this particular preparation, and suspecting that the splendour of the Chinese sulphurated oxide of mercury could arise only from the state of oxygenation, more or less advanced, in which the mercury may be in that combination, he made the following trial:—He took a hundred parts of Dutch red sulphurated oxide of mercury, and having pulverized them, put them into a glass capsule sheltered from the impression of the solar rays, and covered this powder with some cubic centimetres of pure water, taking care to stir the mixture for a month with a glass tube. At the end of seven or eight days he saw the oxide sensibly change, and assume a very agreeable shade. During about twenty-five days the splendour of the red increased gradually and acquired the utmost beauty. Having observed that the matter remained in the same state, and that it no longer underwent any apparent change, he decanted the water, and dried in the shade and in a gentle temperature the red sulphurated oxide of mercury. He compared it in this state, and when very dry, with that of the Chinese and that of the Dutch manufactories prepared by their secret process, but did not find any sensible difference in the splendour or beauty of the red; so that this very simple experiment puts us in possession of a process advantageous to the arts, and particularly that of painting, and which the Dutch will long keep a secret.

Being

Being desirous to ascertain whether the air and light alone might produce a similar effect on this sulphurated oxide, M. Payssé put a hundred parts of the same substance, in powder, into a vessel of the same kind, and exposed them to the action of a strong light for more than a month. He took care, as before, to renew the surfaces very frequently by stirring the matter, in order to multiply the points of contact of the sulphurated oxide with the air which served it as an atmosphere; but instead of acquiring a more agreeable red, it assumed a brick colour inclining to brown. After this experiment, there can be no doubt that light has a sensible influence on this substance, and tends to reduce the sulphurated oxide of mercury by taking from it a portion of oxygen, as it does from most matters of this kind exposed to immediate contact.

Red Oxide of Mercury by Nitric Acid (Red Precipitate).

This oxide, so generally known in commerce by its brilliancy and its beautiful colour, was also one of the preparations which M. Payssé had the curiosity to see made in Holland, in order that he might learn the means employed by the manufacturers to obtain always a brilliant red of a crystalline appearance, similar to that manufactured in Germany, and which comes to us in particular from Nuremberg, Franconia, Bâle in Swisserland, and from Trieste.

Every work on chemistry gives a process for preparing this oxide; but few of them agree in regard to the means of obtaining a beautiful, lively, and brilliant red. Some, after dissolving the mercury in nitric acid, evaporate the liquor and expose the matter to a pretty violent heat for some hours, in order to decompose the nitrate of mercury and convert it into red oxide: others pour a certain quantity of nitric acid over the first solution when dried and distilled. By repeating this operation several times M. Chaptal obtained red precipitate very beautiful and crystallized *. Some also, after having dried the nitrate of mercury, incorporate with it a new quantity of fluid mercury; and expose the mixture, when well formed, to a brisk heat of a dark red colour, between two crucibles luted together. Van Mons asserts, that in this manner he obtained, with great ease, red oxide of mercury perfectly crystallized †.

All these processes, though exactly described, are not

* Elemens de Chimie, art. *Mercure.*

† Journal de Physique et de Chimie de Van Mons; Memoires de M. Fischer et Lichtenberg, 15 Pluviose, an 10, p. 211; et 15 Brumaire, an 12, p. 178.

sufficient

sufficient to the chemist who is desirous of undertaking operations on a large scale, or who wishes to apply chemistry to the arts which depend on it. Unless one has acquired experience in extensive manufactories, and been habituated to the operations used in large laboratories, it will be impossible to succeed, even after many expensive and discouraging attempts.

M. Payssé has often employed the different methods here spoken of, and proceeded with all the care of one desirous to succeed; yet he never obtained results so satisfactory as he wished. He, however, adds, that the means proposed by M. Chaptal are those which were attended with the most constant success, though it was not complete. He varied his processes, employing nitric acid in different proportions, and of a different density and purity: but, notwithstanding the quantity of crystallized oxide which he obtained, it was not possible for him to account exactly for the phænomena which occasion so many variations in the results of this operation, which is apparently nothing, and which, however, is not performed without great difficulty. So much care is required in the application of the heat necessary to be employed, that in two operations where every thing is arranged in the same manner, and where all the circumstances appear to be absolutely similar, his results were almost always different. Sometimes one of the vessels contained a crystallized portion of oxide, while the other part was a red powder of a brick colour; sometimes the whole of the oxide in one of the vessels was converted into a beautiful crystalline precipitate (red oxide of mercury), and that in the next vessel, which had been treated the same, exhibited only a mass half yellow and red, without any appearance of crystallization; sometimes both vessels exhibited this oxide converted, in part, into a brilliant oxide, and the rest of a disagreeable red; and sometimes this oxide exhibited three very distinct colours, bright red, red inclining to brown, and orange red.

The second vessel exhibited only an oxide with two colours very distinct, orange and brick red without brilliancy. "Where (says M. Payssé) shall we seek for the cause of these anomalies but in the manner of evaporating the solution, and particularly in that of decomposing the metallic nitrate during the whole course of the operation?" We shall see, however, that the purity of the nitric acid contributes also very strongly to render difficult the conversion of the mercury into crystallized red oxide, and that the muriatic acid, with which the former is constantly mixed in the

the shops, presents a powerful obstacle when in too strong proportions.

By dissolving mercury in nitric acid mixed with muriatic acid there is almost always formed a white precipitate, which is the more abundant as the quantity of that acid is greater. Without attempting at present to determine the real nature of this precipitate, M. Payssé considers only the part which this combined substance performs in the oxidation proposed to be given to the mercury by decomposing the nitrate of that basis by the action of heat.

If crystallized nitrate of mercury, arising from a solution of that metal in nitric acid mixed with muriatic acid, be exposed to heat, this salt first begins to lose its water of crystallization; it is then decomposed, suffering to escape a portion of acid which it contained in excess, and which may be collected by means of a proper apparatus, because it has experienced no alteration. This disengagement is succeeded by that of gaseous nitrous acid, which is manifested by very elastic red vapours. This acid gas is almost always mixed with oxide of azote, or the latter follows the disengagement of the former. These two gases indicate the decomposition of the nitric acid, which gives a portion of its oxygen to the mercury, and is thereby brought to the nitrous state, or to that of oxide of azote. By continuing the heat, the last portions of the nitric acid abandon the mercury, and the latter is converted into an oxide more or less saturated with oxygen; indeed, its production seems to depend on the quantity of caloric which is accumulated on that substance during its passage to the state of red oxide.

When the vapours of the nitrous gas cease, the oxide changes its colour, passing successively from white to yellow, from yellow to orange, and from orange to red, more or less intense. It is generally when the red is very bright and beautiful that the vessel is taken from the fire, and the mass is then preserved as it is, or reduced to powder.

It would seem, on the first view, that an operation conducted as above ought to give, for constant product, an oxide well crystallized. This, indeed, is the case, 1st, When the nitric acid is free from muriatic acid, or when the latter is in very small quantity: 2d, When its density is equal to 34 or 38 degrees; 3d, When the desiccation and decomposition of the metallic nitrate have been effected slowly and in an uniform manner; 4th, And when the heat employed towards the end of the operation, and while the last portions of the acid disunite, has been graduated and main-

tained

tained nearly at the same degree: for if, as many chemists are used to do, and as sometimes happened to M. Payssé, the oxide be exposed to too sudden a heat at the moment when it acquires its beautiful colour as well as its brilliancy, it loses not only its crystalline appearance, but it assumes also a disagreeable shade of reddish brown: if the heat were carried further, it would even be partially or completely deoxygenated, and the mercury in this case would assume its primitive form, as the author, on several occasions, had an opportunity of observing.

If the quantity of muriatic acid with which the nitric acid may be mixed is too great, and if it rise to several hundredth parts of the dose employed, the particular combination resulting from its union with the oxide of mercury assumes not the characters of a simple hyper-oxygenated muriate, as might at first be presumed, but that of a new compound, which dissolves only in very small quantity in water, and the latter must even be boiling; which becomes sublimed alone in close vessels, taking or rather retaining its particular colour; which is brownish red, without an appearance of crystallization; and which M. Payssé considers as a muriate of mercury with excess of oxide, according to the results it gave when subjected to some experiments.

When this compound is found in too large proportion in the oxide of mercury which has been prepared, it always opposes the formation of the crystallized red oxide, as he several times remarked in his experiments. On the other hand, if the proportion be small it may be neglected, and it even insulates itself from the rest of the oxide in the vessel in which it is prepared: it occupies a line, and forms a separate stratum towards the upper part of the mercurial mass.

What M. Payssé has here mentioned in regard to the advantage there is in the preparation of the red oxide of mercury by nitric acid—that an acid, as free as possible from muriatic acid, should be employed—he had remarked in the experiments which he made every year in his course of chemical lectures; but not being able to form a very just opinion as to the results of some trials made on a small scale, and almost always uncertain, he was desirous, before he developed it, to observe with attention what takes place in the large operations performed in manufactories where considerable masses are used at one time. Now that all his doubts on this subject are removed, and since he knows the phænomena which take place when several hundreds of kilogrammes of mercurial oxide are treated in one opera-

 tion,

tion, what is the quality of the nitric acid employed, and the process to be followed in the application of heat during the whole time of the operation, the author can with certainty indicate a process for obtaining this oxide provided with all the qualities required in it, and which are sought for in manufactories.

Take mercury, free from every other metallic matter, 50 kilogrammes; nitric acid, deprived as much as possible of muriatic acid*, and of from 34 to 38 degrees, 70 kilogrammes; dissolve the metal in the acid, and assist their reciprocal action by a gentle heat in a sand-bath†; evaporate by distillation, and take the receiver from the retort when the vapours of the nitrous gas begin to manifest themselves, as they announce the decomposition of the mercurial nitrate. The point here is to employ a constant and moderate temperature, if you wish to ensure success to the operation‡: it is raised a little towards the end, that is to say, when the disengagement of the gaseous nitrous acid is no longer manifested, but in a manner not very sensible: the vessel must be exposed to this degree of heat till it is observed that the mass of red mercurial oxide is of a bright and brilliant red colour in all its parts. Eight hours of heat are in general sufficient for 200 kilogrammes of this substance.

* It may be tried by nitrate of silver, and if the quantity of muriatic acid appears weak it is neglected. It sometimes happens, however, and particularly when the nitric acid is too weak, or when the quantity is not sufficient, that the mercury is precipitated in a white oxide in proportion as it is formed, because it cannot be held in solution either by the water or by the remaining acid, and that the unoxidated mercury besides continually exercises a chemical attraction on one of the principles of the acid, and that the latter tends rather to be decomposed than to dissolve the mercury, already saturated with oxygen to a certain degree; so that care must be taken not to confound this property with that of the muriatic acid: moreover the precipitate, which is not the effect of the oxidation of the mercury, dissolves entirely in the heated nitric acid, while the other can dissolve only in very small quantity: this property alone would be sufficient to distinguish it. I might have dispensed with making this observation, since in my process I require a very pure and highly concentrated acid; but as it often happens that acid sufficiently strong cannot be procured, this deficiency may be supplied by quantity, and my remark in this case cannot be here misplaced.—*Note of M. Payssé.*

† This solution must be made in a glass retort, the bottom of which is broad. This vessel is preferable to the matrasses employed in Holland, because the only question here is to adapt to it a receiver, and to distil in order to collect the acid which is not decomposed on the metal. This object is by no means to be neglected in operations on a large scale.—*Note of M. Payssé.*

‡ The author wished to have been able to determine in a precise manner the degree of heat which ought to be applied to the oxide of mercury to give it the red colour as well as brilliancy; but this was impossible, because he was not provided with a pyrometer.

It

It has been already said that the turf employed in the manufactories of this country has some advantages over the other kinds of fuel; and this remark is true, for an equal heat may be easily and for a long time obtained, because this matter burns slowly and in a very uniform manner.

As charcoal, or the coals found in the bowels of the earth, are the two combustibles employed most commonly in France, none of the circumstances here mentioned must be neglected when they are used.

By strictly observing all the precautions here indicated, one will rarely fail to obtain oxide of mercury by nitric acid of a brilliant red colour and well crystallized.

If the mercurial mass, notwithstanding all the care taken, be not brilliant, or does not exhibit the crystalline aspect required, it must be reduced to coarse powder and again put into a new earthen vessel, at the bottom of which have been placed some cubic centimetres of nitric acid only, in order that the whole of the oxide which is not brilliant may be slightly impregnated by heating it in a sand-bath. One or two hours' exposure to heat under this apparatus will be sufficient to convert the oxide into crystallized precipitate.

Resuming what has been said on the method of preparing red oxide of mercury by nitric acid, we see that the conditions absolutely necessary for obtaining it constantly brilliant and crystallized are:

1st, To make choice of nitric acid without mixture of muriatic acid.

2d, To employ this acid at the degree of concentration already indicated.

3d, To evaporate the metallic solution in a moderate heat.

4th, To employ a vessel, the bottom of which is sufficiently broad to make the oxidated mass of mercury present a great deal of surface, and that it may be equally heated with the greater ease in every point at the same time, that the nitrate may be uniformly decomposed and may pass as speedily as possible to tne state of red oxide.

5th, That the heat may be gradually increased, and in proportion as the decomposition of the nitrate advances.

6th, That this temperature may be maintained the same during the whole time of the operation, that is to say, during the passage of the yellow to the red oxide required.

"I am very far," says M. Payssé, "according to my experiments and those I have seen performed on a large scale, from believing that crystallized red oxide of mercury is indebted for this brilliant appearance to a state of semi-

 vitrification,

vitrification, as M. Van Mons thinks*; for it would follow, if this oxide were really half vitrified by this operation, that a higher temperature would be capable and ought necessarily to convert it into glass: but nothing of the kind takes place; for I have exposed to a very strong heat, and at several times, four grammes of this oxide, well crystallized, in two crucibles luted together, and, instead of vitrifying, it lost not only its brilliancy but also its fine red colour, and acquired one of a disagreeable brick red. Exposing it to a still greater heat, a great part of it was reduced, and the rest of the oxide acquired a dark brown colour.

"I know, however, that there are bodies which are not susceptible of passing to complete vitrification, and which nevertheless experience, at a certain degree of heat, a paste-like fusion which is called semi-vitriform; but by heating the oxide of mercury in transparent vessels nothing similar is seen to take place, not the least vestige of a partial or general softening of the mass is observed. Besides, I have strong reasons for believing that the crystalline state of the mercury oxidated by nitric acid arises only from the degree of the oxygenation of that metal, and the uniform manner in which the oxidating principle combines with the mercury during the decomposition of the nitrate of that base, and its conversion into red oxide."

XX. *Second Extract from a Memoir of M.* Paysse, *principal Preparer of Medicines at the Camp of Utrecht, on the Method of manufacturing, on a large Scale, some Oxides of Mercury. By M.* Parmentier †.

Nothing remains, in order to make known the memoir of M. Payssé on the oxides of mercury, but to give a short view of the other experiments he made in regard to the red oxide of mercury by nitric acid (red precipitate): they form the complement of his process given in the preceding article. If this process be followed, it will remove all the uncertainty of manufacturers who hitherto have not been able to prepare this substance as is done in Holland. Every thing is easy in theory; but in the arts facts speak much better than the most brilliant reasoning.

To ascertain that the crystalline state of the red oxide of mercury

* Journal de Physique et de Chimie, années 10 et 12, pages 178 et 211.
† From the *Journal de Chimie,* No. 154.

mercury does not arise from a semi-vitrification, as M. Van Mons asserts, M. Payssé made a solution of mercury in nitric acid mixed with some hundredths of muriatic acid. Having evaporated the solution to dryness, he treated this matter with the same care and caution as the former; and when the operation was finished the mercurial oxide exhibited a red aspect, sufficiently beautiful in some parts and dull in others, but without any appearance of brilliancy or crystallization. This mass was pulverized, and again introduced into another glass vessel, into the bottom of which he took the precaution of pouring a small quantity of nitric acid, that the mercurial powder might be slightly impregnated. He then proceeded as before, exposing the vessel to a gentle and graduated heat. But, notwithstanding all the precautions he observed during the operation, the oxide which was the result of them had passed to red without exhibiting the least vestige of crystallization or brilliancy. This experiment was repeated three times, but with no better success. It is evident, however, that if the red oxide of mercury is indebted for its brilliancy only to the semi-vitrified state it experiences when heated, there is no reason why this matter should not be constantly in the same state every time it is prepared, since nothing is necessary, according to Van Mons, but to apply to it a violent heat between two crucibles luted together.

In preparing crystallized red oxide of mercury, M. Payssé made observations which gave him reason to suspect that the brilliant state of this substance is owing rather to the constant degree of the oxygenation in which the mercury is, than to any other cause, and that the presence of the muriatic acid in the nitric acid is an obstacle to the formation of that brilliant state by the new combinations to which it gives birth during the operation. What he supposed is now become certainty, as the following experiments will show.

He took a hundred parts of red oxide of mercury, brilliant, and prepared by nitric acid mixed with muriatic acid; and, having introduced them into a long-necked matrass furnished with a bent glass tube communicating with a pneumatic apparatus, they were gradually heated till the bottom of the vessel became red. He suffered to escape the whole of the atmospheric air contained in the apparatus, that he might obtain, free from mixture, the oxygen gas furnished by the oxide. Having exposed the matrass to heat for a sufficient time, and waited for the complete reduction of

 the

the mercurial oxide, he suffered the whole to cool, and diluted it. He then decanted the mercury, and collected the whole which adhered to the neck of the matrass: its total weight was 0·81. The receiver which had served to collect the oxygen gas was obscure, and indicated that a small quantity of mercury taken from the gaseous state was condensed on its interior sides. To force the oxygen gas to abandon all the metallic particles which it might contain, he surrounded the receiver with pounded ice; and when the receiver had cooled for an hour he conveyed the gas into another vessel, and collected with care the whole of the mercurial oxide which lined the inside of the vessel. Slight friction with the finger against the glass was sufficient to effect the reduction of it, and to collect it in globules in a brilliant state. Its weight was 0·02, which with the 0·81 found in the matrass gave 0·83. The neck of the latter vessel exhibited small white crystals, which he collected with care: their weight amounted to about 0·01; and their taste and other chemical properties convinced him that they were hyper-oxygenated muriate of mercury. A reddish powder inclining to brown lined also a part of the dome of the matrass, which it was necessary to break before it could be collected. When carefully examined he found in it the characters and properties of this new combination, already mentioned in the beginning of this memoir, and which he calls muriate of mercury with excess of oxide: its weight was 0·03. By these results it is seen that the quantity of oxygen was 0·13. The same experiments being repeated several times in succession, the results were always the same as the preceding.

A hundred parts of red oxide of mercury, very brilliant and well crystallized, and prepared with nitric acid free from muriatic acid, were treated in the same manner as in the preceding experiments: the reduction of the oxide was complete, and the products were exactly 0·82 of mercury and 0·18 of oxygen.

M. Payssé treated in the same manner red oxides of mercury prepared in the Dutch manufactories and those he obtained by his own trials: the proportions of the principles which constituted these oxides, all very brilliant, exhibited variations very little sensible. They amounted only to nearly a hundredth part; so that it may be considered as certain that crystallized red oxides of mercury are indebted for this state to a combination of oxygen with the mercury, the proportions of the former being always between 18 and 19, while

while those of the oxides which have not brilliancy contain at most from 13 to 14 of that principle.

Two incontestable advantages result, then, from the preparation of crystallized red oxide of mercury.

1st, An increase of the product of that oxide, the mean term of which is five per cent. more than when it is not brilliant.

2d, The impossibility or at least great difficulty which avarice may experience in adulterating this product of art by red oxide of lead.

XXI. *Facts relative to a nondescript Aquatic Animal. By Mr.* John Snart, *Optician.*

To Mr. Tilloch.

SIR,

Herewith I send you the exact drawing of a very singular aquatic creature I have lately discovered residing in ponds in which the frog, &c. is generated from the first rudiments or spawn of the parent and brought forth in the tadpole state; in which stage of being it becomes the prey of the said creature; without the persecution of which, I am persuaded frogs would infinitely more abound than at present.

The creature in question is of a most curious construction; having six legs, with the feet armed with talons, two palpi or feelers, and four antennæ with a bifurcated plumated tail. The body is divided into ten semi-crustaceous lobes somewhat like the armadillo (exclusive of the head and neck, which form two more), by means of the joints of which he is enabled to inflect himself into almost any posture. The head is flatted like the scollop, and broad; the mouth is of the whole width of the head; and proceeding from the sides of the superior mandible or upper jaw spring two tentacula-like forceps, which it opens or closes at pleasure; these are curved and pointed like those of the forficula or common earwig, and with these it seizes its prey, of which the tadpole seems to be the principal favourite. Though tadpoles are frequently found much larger than it, yet it pursues them with the greatest confidence. When it overtakes them it punctures their skin with its forceps, and, after lacerating them so as to fetch blood, drags them towards its mouth, into which it receives the effusion.

 So

So insatiably voracious is this little water dragon (if I may use an appropriate epithet for want of a name), that in the space of a few hours (and that in the night), out of about eighteen, it had killed, and withal much maimed, no less than eight or ten tadpoles, most of which were bigger than itself; and could it have made the same rapid progress in the water which tadpoles do, I have no doubt they would all have fallen a sacrifice to this little sanguinary tyrant, who (with short intervals of rest after a full meal) is incessantly roving in search of blood!

At first sight it appeared to be of the binocular class; but taking a good magnifier, I observed the eyes to be composed of two annular clusters, not reticulated, but each containing six distinct, roundish, bright, black orbs, at small distances from each other; the intermediate spaces, as well as more considerable ones, in the centre of each cluster, being of a piece with the colour and texture of the skin of the other parts of the body, which, with some little variegated exceptions on the back of the head, is of a colour resembling the mud of the Thames water, of the texture of that of a common shrimp, and like it (when alive) of a semi-transparent nature. Indeed the divisions of the whole body are more like this than any other creature I have yet seen. But its conformation in all other respects is quite as dissimilar as I have stated.

When it seizes its prey, if exceedingly vulnerable like the tadpole, it lacerates it so deep as to make the forceps meet and even cross in the punctures, when it amuses and gratifies itself by working them in and out until the blood flows from the wounds, at which time they are alternately withdrawn and applied to the mouth, as if to taste the gore with the one, while the captive is detained by the other; which if it approves, the poor struggling victim is drawn there too; but if otherwise, it contents itself by repeated lacerations until its imaginary enemy is dead, as was the case with a common earth-worm I threw into the water, and several flies, which were never drawn towards the mouth at all; while the tadpoles are exhausted of their blood until they become a mere skin with a small proportion of gelatinous matter left in it; for their adversary seems not to have convenient organs for entire deglutition, or he would no doubt quickly destroy the whole subject: but owing to the narrowness of the neck, and its crustaceous texture, the œsophagus is incapable of expansion to any considerable degree; yet this incapacity on his part is no

 security

security to the other, seeing they are almost cut in two by their being brought into so close contact with the mouth of their destroyer, and quite drained of their very vitals.

Thus nature, as if to counteract her wonted profuse fecundity in this diminutive scale of beings on the one hand, seems on the other to have made this formidable nondescript adversary to thin the race. So tenacious is he of his prey, that, having once fastened on, he will bear to be drawn quite out of the water, and held for some minutes suspended by the hold he has taken by the forceps ere he will let go his victim; and so determinately undaunted as to bear to be lashed with a small twig, which he has the hardiness to endure. The opening of his forceps seems to fascinate his victims; they become, as it were, transfixed by torpor, and riveted to the spot, though naturally capable of swimming much faster than their enemy.

One particular more which I observed may not be amiss to notice, which is, the ebbing and flowing of the blood, which does not appear to circulate through all the parts, but by a kind of undulatory motion, or rather pulsation, proceeds and recedes towards and from the head to about half way down the body in one entire mass. Though without doubt the whole frame is visited by this vital principle, yet it is in such small quantities as to elude the most minute inspection I could bestow upon it; and if the quantities were not, indeed, very minute, it could not but be visible through the semi-transparency of the body; for it is not limpid like water, but of so sanguine a tint as to give the middle part of the body a black appearance.

Notwithstanding these minute particulars which I have made myself acquainted with, I shall not venture to determine what class it ranks in, because it seems to participate of several, or at least to possess members and faculties in common with two or three; and as I could not gain any information on this head from my books, though there seems somewhat very characteristic in this creature, I thought it might not be impertinent or displeasing to some of your readers to have the best information I could extract from strict observation of its functions and amusements. And although it comes without the minuteness and accuracy of zoological classification, yet I believe I am perfectly correct in every particular stated, in which if I meet your concurrence it is very much at the service of your readers.

Description of the Drawing. (See Plate II.)

Fig. 1. View of the back parts, magnified.

Fig.

Fig. 2. The animal of its natural size, the belly upwards.

Fig. 3. Under part of the head as seen in a strong light, by means of which not only the outward but also the inward direction of its members is discernible: thus we see the roots of the antennæ join together and communicate with the two great canals *aa*, which carry the blood to all the parts; and I am persuaded the forceps join issue there too; but the parts are too glandular to allow of this being distinctly seen.

Besides, upon the creature eating some mackrel liver, a great quantity of blood was discharged from the under fissures near the tips of these forceps a great number of times. Meeting with a medium of nearly the same density as itself (*i. e.* the water), it diffuses itself in the form of a thick smoke issuing from a furnace chimney when fresh fuel is added; after which the forceps became so close that no magnifier I could use upon him would enable me to see them: yet this affords a presumption that they are each a kind of proboscis. Some blood was discharged from the mouth at the same time.

Fig. 4. The eye of its natural size.

Fig. 5. The same magnified.

London, 215, Tooley-street,
20th June 1805.

XXII. *On Elasticity.* *By* Alexander Tilloch. *An Essay read before the Askesian Society in the Session* 1802-3.

It is not my intention in the present paper to enter upon any inquiry respecting the laws by which elasticity acts, as they have often been investigated already, and are well known to every one acquainted with the first elements of mechanics. I mean merely to confine myself to a few thoughts on the physical cause of elasticity, or that property of bodies which enables them, after any external pressure, to restore themselves to their former figure.

The cause of elasticity has been proposed to be accounted for in various ways. The Cartesians held that it was a necessary consequence of their *materia subtilis*, or matter of the second element, making an effort to pass through pores too narrow for it. Thus, when a straight elastic body is bent by any force, the pores become contracted on the concave side, and, if they were before spherical, become for instance elliptical, or of some other form; and the materia subtilis,

subtilis, hindered in its attempt to pass, makes an effort to restore the body to its first state. In this theory there is something like the effort of a strong mind when first seizing upon a new truth, and endeavouring to reduce it to a dependence on and connection with known or admitted facts; but the defects in the reasoning ought to have convinced its supporters, that a more satisfactory way of accounting for the phænomenon was still a *desideratum*. It must be obvious to any person who will take the trouble to analyse the argument, that if the *materia subtilis* in a spherical cavity could by pressure (being hindered at the same time from passing off) accommodate itself to the same cavity rendered elliptical, that in doing so it has in fact become smaller in volume than it was before. The materia subtilis is thus assumed to be compressible; and, as it makes an effort to restore the body, or, in other words, its cavities, to the first form, it is assumed to be expansive. But what does this amount to? Merely that a body is elastic because it contains elastic matter.

Other philosophers, disliking the *materia subtilis* of the Cartesians, have adopted an *ethereal medium*. Their mode of reasoning, however, is so similar to that of the former, that they leave the mind as unsatisfied as before. Indeed, the mere change of a name can throw no new light upon the subject.

Some account for elasticity by supposing that when an elastic body is bent or compressed a number of little vacuities are formed in it, and that on removing the force the pressure of the atmosphere, endeavouring to destroy the vacuities thus formed, restores the body to its first figure. This doctrine, however plausible, is inadmissible, if for no other, for this one reason:—Many bodies require a greater force to bend them to any given degree than can be found by multiplying the number of square inches in their surface by 14 pounds, the force exerted by the atmosphere on a square inch; besides, the phænomena of elasticity manifest themselves in vacuo.

Others assume that all bodies contain air in their vacuities, and ascribe their elastic property to that of the air inclosed in them. This is little more than a substitution of air for the *materia subtilis*, and the *ether* assumed by others. But whence has air itself the property of elasticity? This is a part of the general inquiry, and as necessary to be solved as the source whence other bodies derive the same property.

Others account for elasticity from the law of attraction, applied

applied with so much success, since the time of Newton, to the solution of many other of the phænomena of nature; and we are inclined to think that the more this subject is investigated the more will it appear that it acts an important part in producing those effects ascribed to elasticity. According to this theory, when an elastic body is struck or bent so that the component parts, or portions of them, are moved a little from each other, but not beyond their spheres of attraction, they must, on the cessation of the applied force, spring back to their natural state.

Repulsion also has been held to be the cause of elasticity in the case of aëriform fluids, and this repulsion is ascribed to the presence of heat. In this case repulsion is not made use of as the last term of our knowledge, but merely as expressive of a certain state of action ascribed to another cause. Some, however, make use of the expression without so defining it, and, if they mean any thing at all, use it to express an abstract property of which they know not the cause. We may therefore observe, in passing, that this term should be used as seldom as possible in philosophical subjects, and never unless the author has defined the sense in which he employs it.

Another theory has been proposed, which has been admitted by many as sufficient to account for all the phænomena, not of the elasticity of bodies only, but of matter in general. This theory, which has the celebrated Boscovich for its author, supposes that the whole matter of the universe consists of a great but finite number of simple, indivisible, INEXTENDED atoms, endued with *repulsive* and *attractive* forces, which vary and change from the one to the other according to circumstances pointed out in his System of Natural Philosophy, of which a good account may be seen in the Supplement to the *Encyclopædia Britannica*, under *Boscovich*. The most singular part of the system is, that his atoms, in their least and innermost distances, repel each other, and this power of repulsion increases as the distances are diminished: in sensible distances they attract each other, and this power decreases as the squares of the distances increase, constituting universal gravity: between the innermost repulsive force and the outermost attractive one, in the insensible distances, many varieties occur; at a certain distance the repulsive force vanishes—increase that distance, and attraction begins, increases, lessens, and vanishes, till, at a certain increase of distance, the force becomes repulsive; and so on alternately, always changing from the one to the other with the increased distances; sometimes more slowly,

slowly, sometimes more rapidly, and sometimes one of the forces may come to nothing and then return back to the same kind without passing to the other. And *for all this*, it seems, *there is full room in the distances that are* INSENSIBLE *to us*, seeing the least part of space is divisible *in infinitum*.

Assuming all this, and exhibiting a curve and other necessary appendages to assist the mind in comprehending his theory, the author applies it to explain all the phænomena of the material universe, assuming also in his progress such forms and arrangements as are required to make the system apply to the properties possessed by matter in those modifications which distinguish and divide it into classes: thus solid bodies are formed of parallelopipides, fibres, and of irregular figures, occasioning a greater cohesion than in fluids, and preventing the motion of the parts round one another; so that when one part is moved the rest follow. Those bodies whose particles are placed in limits which have strong repulsive arches within them are harder; those are softer whose particles have those arches of repulsion weaker. When the particles are placed in limits that have weak arches of repulsion and attraction on each side, the body is flexible; and, if those arches are short, the particles may come to new limits of cohesion, and remain bent: but if the arches are longer the repulsion and attraction may act, and restore the body to its former position; nay, in doing this with an accelerated velocity, the parts will pass their former limits, and then vibrating backwards and forwards exhibit that effect which is called elasticity.

On this theory we shall only observe, that whatever conviction it may carry to minds habituated to profound mathematical investigations, it can convey but little information to a man who merely aims at a knowledge of the properties of matter, as consisting, not of *inextended* atoms, but of such moleculæ as occupy sensible space. What the wiser is such a man for being told that certain forces exist, and that some idea may be formed of their mode of operating by conceiving them to act in the directions of certain curves, and with powers varying according to circumstances? He may assent to this; but as his weak mind can conceive nothing of matter inextended, either in itself or in its atoms, he cannot consider his difficulties as solved by merely having them stated to him in a new form; for to him the whole of this system appears to be no more at best but a regular mathematical statement of those operations of matter, the causes of which he still wishes to explore.

C. Barruel

C. Barruel has proposed a theory different in some respects from any of those I have mentioned,—more in appearance, however, than in reality. He contends that caloric acts a great share in the phænomena of elasticity, and maintains that *it is itself elastic* in consequence of the property which the moleculæ of this fluid have of repelling each other: a property, he says, the more probable as it is observed in the electric fluid, with which caloric has so great an analogy. "In a word," says Barruel, "we may be satisfied with admitting its elasticity as a fact from which we may set out as from an incontestable principle." To enter at great length into this theory, an account of which may be seen in the sixth volume of the Philosophical Magazine, p. 52, would encroach too much on our present time. The author presents, in his memoir on this subject, some curious thoughts, and well worthy of a perusal. It is only necessary here, however, to state in few words the substance of his reasoning. 1st, Every body in nature is porous, and these pores are proportioned to the density of the substance: 2d, These pores are filled with different fluids, and principally with caloric. But caloric possesses a strong repulsive force; from which it follows, that, when an elastic body is compressed, the caloric in its pores drives back, by its repulsive power, the displaced parts, and brings them to their former state.

On this theory it may be observed, that however true it may be that caloric acts a distinguishing part in the phænomena of elasticity, the author seems to have made hardly any other use of the fact than to put that substance in the place of the *materia subtilis* and *ether* of the earlier philosophers. He assumes too that it necessarily possesses elasticity; but he ought either to have first proved it, or at least to have demonstrated, that if that property be not inherent in caloric, there could be no elasticity in other matter. In short, were it even proved that caloric is naturally and essentially elastic, and the cause of elasticity in other matter, still the main question would remain unsolved, which would then be—What is the physical cause of elasticity in caloric?

Libes makes elasticity to depend on caloric interposed either between the moleculæ of bodies or combined with them, and at the same time on the attractive force of these moleculæ. "This being premised," says he, "I say that the restoration of solid bodies after compression is a combined effect, which depends in part on the repulsive force which their integral moleculæ have received from caloric,

loric, and in part from the attractive force of these moleculæ."

I should have been glad to have been able to have given something more of Libes's theory than this short notice, especially as this little seems to approach nearer to my ideas of the true theory than any thing I have yet noticed; but I know not in what work it is given. This notice is from the *Journal de Physique*, vol. l. p. 10. an 8.

Thus have I given a short account of all the theories that have been advanced to account for the phænomena of elasticity; or, at least, of all I recollect worthy of notice. Some of them, we have seen, in passing, fall entirely short of the object they aim at, and are therefore unworthy of further notice. In others, however, the mode of argument is so well managed, that had their authors attended to a single fact or two, which they have overlooked, it is probable that the subject would, before this time, have received that full elucidation of which I believe it is capable.

Like those who have gone before me, I may fail in the task I have imposed upon myself; but I hope to avoid inconsistency or unfair assumptions. It does not appear necessary to admit even that elasticity belongs to matter considered simply. Indeed, I think the contrary is the fact. But that my meaning may not be mistaken, I shall explain in as few words as possible this part, which may be considered as fundamental, of the doctrine I mean to propose. By simple matter I mean the primitive moleculæ, or atoms, of which bodies are formed. However complex may be the state in which we find bodies, they consist of elementary principles, which principles themselves are formed perhaps of others, but ultimately of *inelastic atoms* of simple matter. Let us, for example, take some matter considered as simple and elementary, say caloric, I would affirm of it that it is not necessarily elastic; and so of any other simple matter.

But if elasticity be not essentially necessary to matter considered simply, whence do bodies derive that property? I answer, From the same source whence they derive almost every other quality that belongs to them—from their composition and internal arrangement.

I would have said ALL *their qualities*, instead of limiting the expression to *almost all of them*, but that they possess one property which may, and probably does, depend on some other cause, I mean that of attraction. Of the cause of this we know nothing; but its existence and the laws by which it acts being known, philosophers do not fail to avail

themselves

themselves of them in explaining those phænomena into which they enter.

In our present inquiry, then, I mean to derive from attraction the help it offers in explaining the cause of elasticity; and I hope to make it evident, by a due consideration of attraction as common to all matter, and of the laws by which *caloric* constantly endeavours to maintain an equilibrium, not only among systems of bodies, but throughout each individual mass, that the efforts of these two, to maintain their respective powers over matter, and, in doing so, acting according to known and invariable laws, produce all those phænomena to which the term elastic has been applied, whether in solid or in aëriform substances.

I would say, then, that attraction, which pervades all matter, and caloric, which also pervades all matter, by their presence, and by an action in which both participate, occasion elasticity wherever it exists. Not that elasticity must follow as a necessary consequence of their presence, for then every substance in nature would be elastic, which many are not in the common sense of the word. Certain other conditions are necessary to elasticity; but without these two it could not exist.

It is admitted by all that attraction is the cause of the aggregation of the moleculæ of bodies. When the state of aggregation is such, that on the application of a given degree of mechanical force to the body the attraction of the moleculæ is overcome (or the body broken), it is called brittle: if the attraction is only partially deranged, the body will be found to have changed its form, and is then called flexible: when the body springs back, or, after certain vibrations, recovers its form, it is called elastic.

Many metallic bodies which are flexible in the sense just mentioned, may however by hammering be rendered elastic; that is, by merely bringing their moleculæ into more intimate union, or by bringing them reciprocally more within the spheres of each other's attraction. This is a circumstance which ought not to be lost sight of. Let us for a moment then inquire what takes place in the process, besides bringing the particles of the body more nearly into contact, or more of them into actual contact than were in that state before? When a bar of metal is hammered thinner than it was before, a quantity of caloric equal in volume to the diminution of volume imposed upon the bar has been expelled or driven out of it. I need not, however, insist on its being exactly equal in volume, as my present argument only requires that a certain quantity of caloric should

be

be driven out of the mass by the operation; and I believe in the present state of our knowledge that few will be inclined to dispute it. As it is, however, of some importance to establish this point, I shall, before proceeding further with the main argument, briefly point out some of the circumstances which seem to prove the escape of caloric: The bar becomes heated by the operation, and where heat manifests itself, it must be either passing off from or into the body. If the capacity of the body be diminished at the same time, and the chemical properties of the body remain unaltered, which in the instance under consideration is the case, how can we mistake the direction in which the caloric moves?

I am aware of what has been advanced respecting friction by men whose names stand high in the philosophical world, and that hammering is a species of friction: but names and opinions should never be substituted for facts; for, if it be true, which I deny not, but maintain that hammering is a species of friction, it would not be difficult to show that it is equally true that friction is a species of hammering. But if by hammering such a quantity of heat may be driven from the interior to the surface of a bar as will produce effects similar to those of a combustible body in a state of ignition, the caloric in this as in every other case must be something else than mere motion. It is subject to certain laws of motion, like every other species of matter; and like every other species of matter too, when moved from its place by any force, putting other matter in the space or spaces before occupied by it, it must take up another residence. To produce an accumulation of heat by hammering, repeated and a long continued succession of strokes are not necessary: proportion the mass of metal to the impulse to be applied, and with one stroke you may produce such a heat as will make the part of the metal where it is accumulated visible in the dark—hot enough to set fire to a combustible body.

It may here be also observed that heat is propagated even through what is usually called a vacuum, that is, through spaces absolutely void of every other species of matter, which could not possibly take place if caloric were not substantial; for motion is a non-entity when we attempt to conceive of it as distinct from matter—it is an accident of matter, and when we speak of its existence the presence of matter is always implied. This is an argument in proof of the substantiality of caloric which no powers of argument can overturn; for, whatever semblance of truth may attach

 to

to reasonings on motion as connected with matter, it vanishes entirely when the continuity of matter is broken, as in the case we have stated. But if in the case of the transmission of caloric through a vacuum, *caloric* is proved to exist independently of *motion*, why in any other case should the two be confounded?

But to return.—A bar of metal by being hammered has a quantity of caloric expressed from it by mere mechanical means, without undergoing any chemical change. In other words, its capacity for holding caloric has been abridged, without its affinity for that substance being lessened; and the aggregation of the mass has been increased in a ratio bearing some proportion to the diminution of its capacity for caloric.

When the hammer is first applied to the metal, the latter is, comparatively speaking, plastic, and gives but little resistance; but as the parts are brought into a closer state of aggregation the resistance increases, and the hammer recoils in proportion to the force with which it is applied and the degree of aggregation the mass has acquired: in other words, the metal has acquired a degree of elasticity proportioned to the time it has been subjected to the mechanical process. It appears then that by diminishing the capacity of the metal for heat, while its natural affinity remains unaltered, it acquires the property of being elastic.

Let us attend a little to the case before us. When the metal has received a certain degree of compression from the hammer, it refuses to receive more, and the hammer recoils; that is, by mechanical means a certain degree of caloric may be expressed from the metal, but as its affinity for caloric cannot be destroyed, the last portions of it cannot be expelled by any such process; and even a portion of what may be expelled can only be momentarily separated, viz. only during the continuance of the impulse. It is this last circumstance that occasions in the instance under examination an exhibition of what is called elasticity. That I may be the better enabled to convey my ideas on this point, I shall here call in the assistance of a figure to illustrate my meaning.

Let ABCD (Plate II. fig. 6.) be a mass of metal that has received all the density of which it is susceptible by hammering, or let it be a mass (as an anvil) hardened by any other process, in such a manner that it can receive no more permanent compression from the action of a hammer. If a stroke of a hammer be applied on the surface ABC, a momentary depression of the surface will take place, proportioned

portioned to the force that has been applied. Say that the curved line AaC represents this depression, and the area A B Ca its quantity: a quantity of caloric equal to the space A B Ca is momentarily displaced by the blow.

But in this case an attempt is made to separate, by mechanical means, a portion of that caloric which the mass demands by its affinity; and this law, exerting itself to restore the equilibrium, takes back the quantity thus violently attempted to be taken away, and with such rapidity that the hammer is no sooner at a than it is instantly pushed out by the reimbibed caloric. Nor is this all: caloric, being matter, must, when put in motion, obey the same laws that other matter would in similar circumstances. The reservoir that furnishes the supply (viz. the surrounding atmosphere) being inexhaustible, instead of the caloric ceasing to operate when it has brought the surface again to coincide with A B C, it carries it to b, a distance as far above B, or nearly so, as a was below it; and it is not till after repeated vibrations between these points that the surface at last comes to rest in its first position. Any one may satisfy himself of this fact by letting a hammer fall upon an anvil while he holds the handle easily in his hand; it will not give one but several strokes, proportioned to the force employed.

When the recoil of the stroke, as it is called, has carried the surface to b, why does not the mass retain the caloric (represented by the space AbC B) which it has received by the effort thus made by its affinity for caloric?—Because the affinity of aggregation of the mass forbids it. The two affinities—that of the whole mass for caloric, and that of the moleculæ for each other—find their powers balanced when the surface comes to rest in the line A B C.

From what has been stated respecting the effect produced by the blow of a hammer on a hardened mass of metal, it will not be difficult to trace the effect that will follow if a soft bar of metal be interposed. We have seen that a quantity of caloric is momentarily expelled from a hardened mass of metal when struck by a hammer, and as quickly reimbibed with an increase of quantity. When such a bar is struck on an anvil with a hammer, there is a displacement of the caloric from the part struck; and at the same time an increase in the aggregation of the moleculæ in the same part, that is, an increase of its elasticity. The quick return of caloric into the part makes the hammer recoil; but the moleculæ having been brought closer together by the blow, the caloric finds less lodging-room than before, and of course an increase of temperature follows; for the affinity of the mass for caloric is lessened as its moleculæ are brought closer together. A second blow is followed by a like effect, which is in like manner increased by succeeding blows. It is necessary, however, that these be given in such quick succession that the bar may not have time between to give off its caloric to surrounding bodies. If this be attended to, an accumulation of caloric must take place in the bar; for by the second blow a larger quantity of heat is displaced than by the first, and consequently a larger quantity is imbibed to be affected by the third blow; the quantity displaced by each blow being proportioned to that which was before present, and the quantity newly imbibed being proportioned to what was displaced by the last blow. This fact, with some latitude, will always be found to hold true. Need we wonder then at seeing a dexterous artisan lighting his forge without any other heat than what he can furnish to himself by means of his hammer and an iron rod?

In the case which we have just examined the phænomenon appears to depend on the displacement of caloric in a body by an external force, while the affinity of aggregation endeavours to retain it. Will this be found to hold in other cases?—Yes, making allowance for the difference of circumstances.

Let AB (fig. 7.) represent a spring of steel or any other metal. By any external force let it be bent into the form CD. It is plain that one side of the spring has been elongated and the other shortened.

But the spring before flexure had its moleculæ respectively at those distances or in that arrangement in which they best balanced each other; that is, the mutual effort of all was to keep the spring in the state it exhibited before any external force was applied; and the quantity of caloric resident in the mass was distributed throughout in proportion to the affinity of all the parts.

Let the proportion of caloric proper to any part of the mass in any common temperature be represented by the space comprehended between the two parallel lines *a b* and *cd*.

When the bar is bent, the lines *ab* and *cd* are made to approach each other, as at *e* on the concave side, and to recede on the convex side, suppose to *f* and *g*, so that they no longer remain parallel. In other words, a quantity of caloric has been displaced from the one side, and has found lodging-room in the other side of the bar; and what is said of this is understood of every other part of the spring. But the affinity of all the parts for caloric having undergone no

 change,

change, it is plain that when the external force is removed it must resume its first position in the mass, and, by doing so, restore it to its former figure, in effecting which the affinity of aggregation co-operates; for we have been supposing such a force only applied as could change the form, while applied, without overcoming the aggregation of the mass.

The vibrations which follow, if the spring be left quite free after force has been applied, may be explained in a way perfectly similar to what we have already laid down when speaking of a bar struck by a hammer. The caloric being put in motion, a larger quantity of it runs from the plus side to the side that was minus than the affinity of the latter demands, and is therefore driven back again, and so alternately, till by little and little it ceases its motion as equilibrium comes to be established.

After the bar AB has by any applied force been brought into the form CD, the caloric, which in the natural state of the bar resided between *b* and *d*, having been forced to find lodging-room towards *a c* on the side now rendered convex, and the lines *ab* and *cd*, or the portions of metal which they represent, being brought into contact in the point *e*; or, if the possibility of the perfect contact of the moleculæ be denied, as nearly into contact as possible in the point *e*; then the said point *e* (and so of any other point of the concave surface) becomes a fulcrum over which the bar may be broken if an increased force be applied; for all the caloric that can be removed by mechanical means from the concave side having been transferred to some more convex part of the bar, it must follow, as a consequence, that any attempt to make the metallic matter enter spaces already occupied by metallic matter must be vain, and can only operate to draw the moleculæ on the convex side to such a distance from each other as to admit foreign matter between them, viz. the atmosphere or other surrounding medium, after which it will be impossible for them to coalesce again.

In this way would we account for that effect which has hitherto been ascribed to the moleculæ being removed to such a distance from each other as to place them beyond each others sphere of attraction. It is true they are brought into such a situation that their attractive affinity cannot again unite them as an aggregate; but we think their attraction is not annihilated, as the common mode of expression may suggest to those who do not properly examine the matter. There is only a new affinity brought into play, viz. that of the metallic moleculæ for the newly interposed body; for, could that new affinity be destroyed, or, in other words, could the interposed substance be entirely removed, it is probable the affinity of the moleculæ would again be exerted. Of this some idea may be formed by attending to what takes place when two spheres of lead, a little flattened, are pressed together. In proportion as the air has been excluded will be the adherence of the two balls.

We would not wish, however, to be understood to assert that in every case a disjoined mass would unite but for the newly interposed substance, for several conditions are requisite to this effect which can rarely exist. Among these may be mentioned, that there should be no new arrangement of portions of the broken surfaces by the metal having by its tenacity drawn itself out into fibrous ligaments and protrusions; for in that case the points of contact upon joining the masses are so limited that the very weight of either part, that is, its gravity for the common centre of attraction, will act as a sufficient force to destroy the affinity which exerts itself to keep them united. We may also here remark, that in the confused crystallization of melted masses of metal, some of the portions may always be conceived to be under some restraint, as it were, and this must hold also after the metal has been hammered. Therefore on breaking the mass some of these will always protrude, or in some way or other change their position a little, so as to produce an effect similar to that before described—reducing to a comparatively small number the points that can be brought into contact. Therefore what we mean to suggest is only this—that if every interposed substance could be entirely removed, and it were possible to bring the original number of points into contact, the affinity of aggregation would act to unite the parts of the mass.

I might apply the reasoning employed in the case of the spring AB to other cases of solid bodies; but, from what I have said, I think any person may apply my reasoning in the same way as I would myself, whether he be convinced of its truth or not; to enlarge further appears therefore unnecessary. It is proper, however, that I should endeavour to show how the same doctrine applies to aëriform fluids.

When air is compressed, on removing the force it regains its first volume. This, however, is conditional. If the compressed air be of a given temperature, say 80°, and if it be afterwards reduced to a lower temperature, say 32°, it may so happen that the diminution of volume by reduction of temperature may more than counterbalance the compressing

pressing force that was employed. In this case, then, a mere abstraction of caloric annihilates a certain quantity of elastic force which belongs to the air when the common temperature is higher.

Again: Inclose a given quantity of air, not compressed, in any proper vessel, when the common temperature is 30°, and it will be found when the temperature becomes high, say 80°, that it has acquired an elastic force which it had not before.

The cause is obvious. The air inclosed at 30° has as strong an affinity for caloric when the common temperatur comes to 80° as the air not inclosed, but is prevented by want of room from satisfying itself to the extent of its affinity. On opening the vessel, however, the caloric finds an easy admission; the volume of the mass becomes increased, and a quantity equal to this increase discharges itself.

But when by force we compress air which is to be again liberated without waiting for any remarkable change of temperature, we only accomplish by mechanical means what nature effects by a mere change of temperature. We express a certain quantity of caloric from the air, while its affinity for that substance remains undiminished. Remove the restraint, and, the affinity exerting itself, a sudden increase of volume takes place, exhibiting that phænomenon which is usually called elastic force.

Again: When a foot-ball is struck (and so of similar cases) there is a displacement of caloric proportioned to the force applied and the nature of the covering. But this is only momentary; for, the affinity of the air for caloric remaining unaltered, a quantity of the latter, more than equal to what was displaced by the blow, is, for the reason before pointed out, instantly taken in by the air, and with such rapidity as to cause the ball to recoil from the foot in the same manner as a hammer does when struck on an anvil, but in a much more remarkable degree; for the recoil will always be proportioned to the force employed, compared with the affinity of the bodies for caloric.

Before concluding, we may observe generally, that every body in nature may be considered as in some measure elastic, though many of them cannot manifest that property to the extent which those bodies usually called elastic can: that is, a certain force may be applied to all of them without destroying their form, and by the application of the force a certain quantity of caloric may be first accumulated in and then given off from them. In every case of this kind (and they include every species of friction) caloric is first expressed from, and then imbibed, with a surplus quantity, into the parts of the body which undergo the mechanical action, and it is this circumstance which has so much puzzled some philosophers in certain experiments on friction. But as we may possibly lay before the society at a future period a few thoughts confined principally to this object, we forbear entering further into it at present.

XXIII. *On the Production of Muriates by the Galvanic Decomposition of Water: with a second Letter on the Subject from Mr.* W. Peel, *of Cambridge.*

In our last volume, page 279, we laid before our readers a letter from Mr. Peel, of Cambridge, announcing the production of *muriate of soda* by the Galvanic decomposition of water. That communication we considered as extremely important, and we suggested that such experiments as Mr. Peel was engaged in, might possibly lead to some knowledge of the composition of soda and the base of the muriatic acid.

The letter alluded to was dated the 23d of April last, and published in our number for that month. We have been not a little gratified since in finding that our suspicion has been in some degree confirmed by M. Cuvier's report of the labours of the Class of the Mathematical and Physical Sciences of the French National Institute from the 20th of June 1804 to the 20th of June 1805, published on the 25th of the last-mentioned month *. One of the articles of this report states, that M. Pacchiani, of Pisa, has discovered the radical of the acid in question, which he states to be hydrogen. By taking from water, by means of the Galvanic pile, a portion of its oxygen, he asserts that the water was converted into oxymuriatic acid; and that, consequently, "muriatic acid is hydrogen at its minimum of oxidation †; the oxymuriatic acid, hydrogen in the middle state; and water, hydrogen at its maximum of oxidation."

The following letter was intended for our last number, but did not reach us in time. It will be found as interesting to our philosophical readers as Mr. Peel's former communication. The result of his new experiment, so far as

* Part of this report is given in our present Number.

† In our 6th vol. p. 153, we announced that Girtanner maintained hydrogen to be the radical of the muriatic acid, and that this acid contained less oxygen than water.

the

the muriatic acid is concerned, is the same as that before communicated; but in this instance he has obtained a different alkali,—a kind of proof that the alkalis, as has for some time been suspected, are not essentially very different from each other.

Cambridge, June 4, 1805.

"SIR,

"Acccording to my promise I send you another letter, which I hope will be as favourably received as my last. Permit me to say, I feel myself much indebted to you for your suggestions *, which have led me to the discovery I now send you.

"Having proceeded to the formation of water from its elements, with which to repeat my former experiment, I found, when the oxygen and hydrogen gases were quite pure and exactly in due proportion, that no residuum of air was left, and that the water formed was not in the slightest degree acidulous. When the process was not conducted with great accuracy, or any precaution to have it accurate was omitted, I then found the water acidulous, and the acid that caused this acidity to be the nitric acid.

"The acidulous water thus obtained I neutralized with lime, from which I distilled the water, and this water I decomposed by the Galvanic process, as in the experiment detailed in my former letter.

"I did not imagine the using water so obtained could make the least difference on the result of the experiment; but as you had expressed a wish to have the trial made, I again undertook that interesting but very tedious labour.

"When I came to examine the residuum, to my great astonishment I found that *not muriate of soda but muriate of potash was produced!*

"I must own I feel myself entirely at a loss how to account for this, nor shall I attempt it. All I can say is, that this, as well as my former experiment, was conducted with the greatest care and accuracy that I could bestow. Perhaps your, or some of your readers', superior sagacity may furnish some hint that may lead to a satisfactory explanation of the phænomena. I am, &c.

"*To Mr. Tilloch.* "W. PEEL."

Our readers, we are persuaded, will agree with Mr. Peel in thinking the result, indeed, very singular. Some may, perhaps, be inclined to believe there must have been some

* See Note, vol. xxi. p. 280.

mistake

mistake in the experiment detailed in this or in the former letter; but till some person possessed of as much ingenuity and patience as Mr. Peel shall prove his experiments to be erroneous, we shall not question their accuracy *. Indeed, we had a suspicion that the result might possibly be affected by using water obtained from different sources, or distilled from different substances, and it was this suspicion that induced us to recommend to Mr. Peel a repetition of the experiment under new circumstances.

We may remark here, that Guyton suspects potash to be composed of *lime* and *hydrogen*. In the present experiment *lime* was employed to neutralize the acid in the water made use of; and though the water was distilled from the lime, it does not appear to us impossible that a small portion of it might be carried over. Indeed, if Guyton's opinion be well founded, it is very probable this was the case. Hydrogen the water would furnish by its decomposition.

We have not been informed of the nature of the residuum left by distilling the water made use of in Mr. Peel's first experiment; that is, what substances were held in solution by it—if spring water. It would be a curious circumstance if it should prove to have been combined with a little *magnesia*, as it would go some length in proving the truth of another opinion of Guyton, that soda is composed of *magnesia* and *hydrogen*; for it would only be necessary to suppose that in the distillation of the water there was carried over some of the magnesia, a very minute portion of which, other circumstances coinciding, might be all that was wanted to determine the kind of alkali to be formed.

Could the result depend at all on the circumstance of nitric acid having been previously in mixture with the water? In the production of nitre (nitrate of potash) from the corruption of animal and vegetable substances, possibly the previous formation of the acid from its elements has some share in determining the formation of that alkali for which it has the greatest affinity.

If the acidulous water employed in the last experiment had been distilled *per se*, or from some other substance than lime, would the result have been different? From Mr. Peel's experiments it seems extremely probable that very small and seemingly inappreciable differences in the way of

* We wish Mr. Peel had mentioned the tests and methods he made use of to ascertain the nature of the products he obtained. We are certain such information, in addition to what he has already given, would prove acceptable to our philosophical readers.

conducting

conducting such labours may determine the production of very different substances.

In short, it seems probable that some of those substances which in the present state of our knowledge we are obliged to consider as the most simple elements, such as oxygen, hydrogen, and azote, are, in fact, compounds; and if so, the formation of one or more of these may take place under circumstances in which we should not expect them to be present, and may produce such results as those now under consideration.

This subject is extremely interesting, and we hope Mr. Peel and other philosophers will continue to give it that attention which its importance seems to demand.—A. T.

XXIV. *Memoir on some zoological Facts applicable to the Theory of the Earth. Read in the Physical and Mathematical Class of the French National Institute on the 22d of October* 1804. *By* M. Peron, *Naturalist to the Expedition for making Discoveries in Australasia**.

> ———— Colles exire videntur;
> Surget humus; crescunt loca, decrescentibus undis.
> *Ovid. Met.* lib. i. ver. 342.

If excursions confined to the countries of Europe can furnish matter for so many useful works and for so many valuable comparisons, and if slight differences in the physical constitution of the soil, in its temperature, and in its productions, could give rise in all ages to grand ideas and important theories, how fertile in the most valuable results of every kind must be distant navigations!

The traveller in voyages of this kind, transported, as we may say, on the wings of the wind, traverses in a few months the most different climates; distances vanish, and small differences disappear along with them. The large masses alone can strike him; and they are every where reproduced with an opposition, and contrasts so great and so numerous, that the coldest imagination cannot fail to be interested in such a spectacle. In one place, the summit of the Peak of Teneriffe, which has been rendered celebrated by the valuable researches of M. Humboldt, seems to unfold before him the history of the grand catastrophes of nature, and of their effects; while in another he sees rising at the

* From the *Journal de Physique*, Frimaire, an 13.

the extremities of the Austral world those bulwarks of granite which she seems desirous of opposing to the fury of the boundless ocean. He soon arrives on the barren coasts of the west and north-west side of New Holland, where the phænomenon of the acquisitions made by the land presents itself with all the interest of which it is susceptible. In vain does he pass along coasts of two or three hundred leagues in extent; he every where observes eternal downs of white sand, which extend into the country as far as one can penetrate. The numerous islands he meets with exhibit to him a similar constitution; and the banks of sand, so frequent in these dangerous seas, have no other. But the fertile mountains of Timor already begin to appear; an eternal vegetation every where covers them with its rich productions: they are continued in large gradations, which rise more and more towards the interior of the land. Every thing is new in its aspect: he no longer sees those lacerated forms, those blackened peaks, and those threatening craters of Teneriffe, and of the Isles of France and of Bourbon; those striking and majestic masses of South Cape, Cape Pelé, and Cape Frederick-Henderick in Van Diemen's Land; much less that monotonous and tiresome aspect of the sandy coasts of New Holland. None of these pictures are applicable to the mountains of Timor. Their forms, though large, are softened; their prolongations are regular; their summits are broad, and sink down gradually by slight undulations, which disappear on the sea shore: in a word, every thing announces here the tranquillity of the tropics, and the peaceful action of nature and of time.

Amidst objects so grand, with terms of comparison so prodigious, the study of nature then more striking, is also more easy: all the petty objects of detail, the modern effects of a multitude of secondary causes, disappear, as we may say, before the grand ensemble of nature, and cease to occupy in our annals the too important part which they have been so many times made to perform. But we may safely affirm, that we shall have no real theory of the earth till the glorious period when the sciences can reckon among their votaries men desirous of emulating Humboldt. What he has done in regard to America ought to be done in regard to many distant countries and so many archipelagos still unknown. At the head of the latter appears New Holland, an immense country, hitherto little explored, but worthy of the attention of the governments much more than the naturalists of Europe.

SECTION

Section I.

Zoological Observations which may excite Doubts in regard to the primitive Union of New Holland and Van Diemen's Land.

Of all the observations which may be made in proceeding from Van Diemen's Land to New Holland, the easiest, no doubt the most important, and perhaps also the most inexplicable, is, the absolute difference of the two races who inhabit these two lands. If we except, indeed, the meagreness of the extremities, which is observed equally among both people, they have scarcely any thing common in their manners and customs, in their rude arts, in their implements for hunting or fishing, in their habitations or piroguas, in their arms or language, in the whole of their physical constitution, in the form of the cranium, or in the proportions of the face. This absolute dissimilarity appears also in the colour: the inhabitants of Van Diemen's Land being browner than those of New Holland; it appears also in a character hitherto considered as exclusive, namely, the nature of the hair. That of the inhabitants of Van Diemen's Land is short, woolly, and curled; that of the New Hollanders straight, lank, and stiff.

Now how can it be conceived that an island of 50 leagues in extent at most, so near to an immense continent, situated also at the extremities of the Austral world, and separated from every other known land by the enormous distances of five, eight, twelve, and even fifteen hundred leagues, should have a race of men altogether different from that of the neighbouring continent? How can we conceive this exclusion of all relation, so contrary to our ideas in regard to the communication and transmigration of nations? How can we account for the darker colour, and curled woolly hair, in a country much colder? It appears to me difficult, I confess, to assign a satisfactory reason for these anomalies. All these curious facts, which will be detailed in the general account of our long voyage, will be new proofs of the imperfection of our theories, which are always suited to the state of the knowledge of the age which gave birth to them. At present I must be contented with deducing from this first part of my observations the important consequence, that the separation of Van Diemen's Land from New Holland is not one of the modern operations of nature; for it is probable that if these two countries had been formerly joined they would have had the same race for inhabitants, and it would no doubt have been that which occupies,

occupies, with its ferocious tribes, the whole of the immense coasts of New Holland, from Cape Wilson to the burning coasts of the land of Arnheim and the great gulf of Carpentaria.

Another zoological fact tends still further to confirm this distinction, if not primitive at least very old, between New Holland and Van Diemen's Land. The dog, that animal so valuable to man, the faithful companion of his misfortunes, his travels, and dangers, the indefatigable instrument of his distant hunting excursions, every where so common on the continent, and which we found on all its coasts with the different hordes we had an opportunity of seeing, does not exist in Van Diemen's Land; at least we could observe no traces of this animal. We never saw any of them with the inhabitants, notwithstanding our daily intercourse with them. The case was the same with M. Labillardiere during Dentrecasteaux's voyage; and it does not appear that any other traveller ever saw any. The English whale fishers, whom I consulted on the subject, confirmed this circumstance, that the dog is not found in Van Diemen's Land.

Section II.

Zoological Observations which seem to indicate that the Summits of the Mountains of Van Diemen's Land, New Holland, and Timor, were formerly covered by the Sea.

One of the noblest and at the same time most incontestable results of modern geological researches is, the certainty of the sea having once stood at very considerable elevations above its present level. In almost every point of the old continent the proofs of this fact are as numerous as they are evident. They appear with interest in different parts of the new world; and M. Humboldt has lately communicated to us a very curious circumstance of this kind. In this point of view, as well as in many others, New Holland and Van Diemen's Land remained to be examined; as they might have formed an exception of sufficient importance to induce a very rigorous philosopher to deny the universality of the antient domination of the ocean, however favourable reasoning and analogy might be to it. Fortunately this deficiency was one of those which, depending only on the existence of a fact, could be easily supplied: it appears to me that it is completely so at present. On Van Diemen's Land indeed, on several points of New Holland, and on the summits of the mountains of Timor, I every where met with those valuable remains, which may be considered as irrefragable testimonies of the revolutions of nature.

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In the rapid view which I am going to take of my results in this respect, I shall treat in succession of what relates to fossil shells and zoophites. One of the principal reasons of this distinction, the importance of which I shall soon have occasion to prove, is the almost absolute exclusion of every large kind of solid zoophites after the 34th degree of south latitude, beyond which I observed only the difficult and orbicole tribes of the sponges, the alcyons, flustres, and some millepores.

A. *Petrified Shells.*

It would be too tedious and useless to enter here into the details of all my observations on this subject: it will be sufficient for me to give an account only of the principal results.

At Van Diemen's Land, towards the bottom of the North River, I observed, at the height of six or seven hundred feet above the level of the sea, large masses of petrified shells, all belonging to the *lime* genus of Lamarck, and constituting a species to which I could find none living analogous in the same places.

On several points of the east coast of the island Maria there are seen regular horizontal strata, consisting of a kind of whitish shelly freestone resting on granitic rocks, at the height of four or five hundred feet above the level of the sea.

At Kangaroo Island, those of St. Peter and St. Francis, and that portion of the continent situated behind them, I made similar observations: I found always some remains of petrified shells, at a greater or less distance, in the interior of the country, and at heights more or less considerable.

Vancouver and Mainzies had before observed some in Port King George, and in that point also I myself collected several specimens.

During the interesting excursion which my friend M. Bailly made into the interior of New Holland, ascending Swan's River for about twenty leagues, he found every where, as he told me, the ground covered with quartzy sand mixed with the remains of shells.

At the Bay of Seals this phænomenon occurs with more decisive characters. The whole substance of the barren isles of Dorré and that of Dirk-Hartog consist of freestone, sometimes reddish and sometimes whitish, filled with shells of different kinds.

This composition becomes still more striking at Timor.

 On

On the summit of those mountains, already mentioned, there is found, at the height of more than 15 or 1800 feet above the level of the sea, a great number of shells incrusted in the middle of the madreporic masses which they form. The most of these shells are in the siliceous state: some of them, still in the calcareous state, are more or less altered and friable. There are some monstrous ones among them. I have seen several individuals, and every person belonging to the expedition might have seen them also, which were not less than four or five feet in length. All these large shells evidently belonged to the genera *hippope* and *tridacne* of Lamarck; and, what is more important, the fossil individuals have such a resemblance to those of the same genus found alive on the sea shore at the bottom of the mountains, that I have thought proper to consider them as the same in my General Topography of the Bay of Coupang. Even the gigantic proportions of the fossil *tridacnes* are found in the living ones. I myself saw a valve which served daily as a trough to five or six hogs. In the Dutch fort there is another in which the soldiers of the garrison wash their linen. The absolute want of colour, common to the fossil and living *tridacnes*, is another reason for their identity. The case was the same with several kinds of zoophites, which, existing still on the coasts, seem to be so identic with some of those forming the mountains of that part of the island, that I made no hesitation in considering them as such. Since my return to Europe, however, having had occasion, in examining the beautiful collection of M. Defrance, to remark how easy it is to be mistaken in this respect, I must freely confess that I can no longer venture to warrant this identity, however probable it may appear, as my observations were not made with that minute attention which the subject deserves, and as whole specimens are not to be found in our collections. While I regret that I suffered so valuable an observation to escape me, I must mark Timor as the place most proper for determining the delicate and interesting question in regard to analogous living individuals, at least in the last classes of the animal kingdom.

Before I terminate what relates to petrified shells, it seems to me indispensably necessary to say a word of incrusted shells, which are too often confounded with the former.

B. *Of*

B. *Of Incrustations of different Kinds, and particularly the incrusted Shells found in different Parts of New Holland.*

One of the particular advantages of extensive navigations and long voyages is, that the theatre of observation is so much varied, and objects so multiplied, that nothing is often wanting but a sound judgment to make the most difficult comparisons, and to deduce from them important consequences. What man, for example, can see, with indifference, around him that succession of beautiful incrustations so frequent on the shores of Kangaroo Island, on those of the Archipelago, of St. Peter and St. Francis, and on the shores of the immense Bay of Seals? In one place whole trunks of shrubs are entirely covered with a mixed stratum of freestone and calcareous matter, and in others are accumulated branches of trees, roots, shells, zoophites, the bones of animals, and excrements of quadrupeds, concealed under the same covering. "One might be tempted to believe," said the unfortunate Riche, "that a new Perseus carried the head of Medusa over these distant coasts."

On the sight of so many striking singularities, how can we forbear inquiring into the cause, and how is it possible that it should not be discovered in the particular nature of the sand on these shores? The numerous shells, indeed, produced in these seas being continually rolled by the action of the waves on the neighbouring shore, are thus reduced to very small fragments, which being afterwards mixed with the quartzy sand, soon form with it a calcareous cement of a superior quality. In carefully examining its materials one might be tempted to believe that Dr. Higgins, in his ingenious Essay on Calcareous Cements, had stolen the secret of nature. The proportions, indeed, which he indicates as susceptible of forming the most solid combination, that is, one part of lime and seven of quartzy sand, are those which nature seems to have adopted for her cement. But whatever this composition may be, it is the only agent of those remarkable incrustations of which I have spoken. On the shore it soon incrusts every body thrown upon it; testacea, zoophites, galets, are all agglutinated by it. The observer sees, as we say, formed before his eyes, the breches and puddingstones of which the neighbouring rocks are composed. Transported by the winds, this active matter deposits itself on the nearest shrubs. At first it is only a light kind of dust, which soon becomes solid around the stem which it embraces. From that mo-

 ment

ment the mode of the nutrition of the vegetable becomes changed; it soon languishes, and, though still alive, it seems to have undergone a kind of general petrification. I have brought home a great many fine specimens of this sort, and the difficulty of transporting them alone prevented me from bringing back a more considerable number.

What is most singular in this operation of nature is, the speed with which this kind of metamorphosis is effected. I have reason, indeed, to believe, from my own observations, that a shell, a month after its being cast on the shore, can no longer be distinguished. The force of the solar rays, the vivacity of the light reflected by the white sand of the coast, are sufficient in a few days, with the sea water, to deprive it of its colour, and to disorganize it in such a manner, that in the middle of the calcareous stratum which has already seized it, the most experienced eye might mistake it, and range it in the class of the oldest petrified shells. One may judge of these alterations by the different specimens—how easily this mistake may be committed, and how impossible it would be to assign to the most of these shells a character proper to distinguish them from real fossils.

C. *Of Zoophites observed at great Heights above the present Level of the Sea.*

I have now concluded what relates to petrified shells, or those merely incrusted: it is seen, that from the most southern extremity of the eastern hemisphere to the middle of the equatorial regions they are found in greater or less numbers, and at greater or less heights. The case is not the same with solid zoophites: as already said, I could not find large species beyond the 34th degree of south latitude; and it does not appear that any other traveller observed any considerable number of these animals beyond the same point, either in the northern or the southern hemisphere. Driven, as we may say, from the one extremity of the world to the other, it is in the bosom of the warmest seas that this innumerable family of animals seem to have fixed their habitation and their empire: it is the latter zone in particular which gives birth exclusively to those formidable reefs, those numerous islands, those vast archipelagos, prodigious monuments of their power. All the Society Isles, Maitea, Tongataboo, Eona, Anamooka, Turtle Island in the Pacific Ocean, New Caledonia, Chain Islands, Tethuroa, Tioukea, Palliser's Isles, Tupai, Moopehea, the Isle of Cocos and that of Pines, Norfolk Island, How's Island, Palmerston Isles, several of the New Hebrides, Mallicolo, the

the archipelago of the Low Friendly Isles, Bougainville's Island, several points of New Guinea, all the islands scattered on the eastern side of New Holland, and in particular the formidable labyrinth which had like to have proved so fatal to the vessels of Bougainville and captain Cook; in a word, almost all those innumerable islands dispersed throughout the great equinoctial ocean, seem some of them entirely, and others only in part, to be the work of these feeble animals. The accounts of all the navigators who have traversed these seas are filled with expressions of the terror inspired by their labours. All of them, almost, were exposed to the greatest dangers in the midst of the reefs which they raise up from the bottom of the ocean to its surface, and no doubt the unfortunate navigator, the loss of whom France as well as all Europe deplores, was one of their numerous victims. . . . .

"The danger they present," says M. Labillardiere with great reason, "is the more to be apprehended as they form rugged rocks covered by the waves, and which cannot be perceived but at very short distances. If a calm comes on, and the ship is driven towards them by a current, her loss is almost inevitable: in vain would the crew attempt to save her by dropping their anchor; it would not reach the bottom even quite close to these walls of coral, which rise in a perpendicular direction from the bottom of the waters. These polypiers, the continual increase of which obstructs more and more the bason of the seas, are capable of frightening navigators; and many shallows, which at present afford a passage, will soon form shoals exceedingly dangerous."

Though less common in the seas which we traversed, these animals furnished me nevertheless with subject of observations the more valuable, as the general consequences deduced from them may be applied with more interest and more evidence to the history of the revolutions of our planet.

Thus, as I have said, from the 44th to the 34th degree south, no large species of solid zoophytes are found. It is at Port King George, in Nuyts Land, that these animals appear, for the first time, with those grand characters which they affect in the midst of the equinoctial regions. My particular observations, indeed, are reduced in this point to mere fragments, found here and there in the interior parts of the earth. The case is not the same with those of Mainzies and Vancouver. The details, for which we are indebted to these navigators, are too valuable of themselves, and par-

 ticularly

ticularly on account of the consequences with which they will furnish us, that I cannot here forbear transcribing what Vancouver has said on the subject.

"The country," says he, "is formed chiefly of coral, and it seems that its elevation above the level of the sea is of modern date; for not only the shores and banks which extend along the coast are in general composed of coral, since our lead always brought up some of it, but it was found also on the highest hills we ascended, and in particular on the summit of Bald Head, which is at such a height above the level of the sea, that it is seen at the distance of twelve or fourteen leagues. The coral here was in its primitive state, and especially on a level field of about eight acres, which did not produce the least blade of grass amidst the white sand with which it was covered, but from which arose branches of coral exactly similar to those exhibited by beds of the same substance above the surface of the sea, with ramifications of different sizes, some half an inch at least, and others four or five inches in circumference. Many of these coral fields, if I may use that expression, are to be met with: a large quantity of sea shells, some perfect and still adhering to the coral, and others at different degrees of dissolution, are observed in them. The coral itself was more or less friable; the extremity of the branches, some of which rose more than four feet above the sand, was easily reduced to powder. In regard to the parts which were near or below the surface, a certain degree of force was necessary to detach them from the foundation of rock from which they seemed to arise. I have seen coral in many places at a considerable distance from the sea, but I never saw it any where else so high and so perfect *."

This, no doubt, is one of the most curious facts of this kind, as well as one of the most important to be verified and to be examined. Will it now be believed that the two vessels belonging to our expedition, the Geographe and the Casuarina, remained for nearly a month at anchor in Port King George, at the foot, as we may say, of this Bald Head, so valuable to be visited, without any of the three naturalists, who still remained on board these vessels, being permitted to go thither?

Fortunately the large island of Timor presented a field still wider and more striking for observations on zoophytes. There every thing attests their power, and the revolutions effected by them in nature. They are found on the summits

* Vancouver's Voyage, vol. i.

of

of the highest mountains of Coupang, and they are easily distinguished: in the deepest caverns, and the widest fissures, they present a tissue, the characters of which cannot escape notice. In the excursion, so painful and so laborious, undertaken by me and my friend Lesueur, to hunt crocodiles at Olinama, we every where observed the same composition; at Oba, Lassiana, Meniki, Noebaki, Oebello, and Olinama. At the last-mentioned point we found ourselves opposite to the grand chain of mountains of Amufoa and Fatelou, the back of which is uninhabitable on account of the prodigious number of crocodiles which live in the marshes of that part of the coast. This broad plateau, which commands all that portion of Timor, is entirely composed of madreporic matters. From Oëana to Pacoula the whole country, according to the inhabitants, is limestone; and this is unanimously confirmed by the Dutch.

It is not only in this state of death and inactivity that the zoophytes of Timor ought to excite admiration and interest: they encumber, in the living state, the bottom of the sea; every where in the Bay of Babao they raise up reefs and islands. Turtle Island (*Rea Poulou*), Birds Island (*Bourou Poulou*), and Monkey Island (*Codé Poulou*), are exclusively their work. Long narrow reefs, which proceed from Point Simao, confine more and more the entrance of the bay in that quarter. They render inaccessible the coasts of Fatoumé and Soulama, and promote the increase of the land gained from the sea in all these points. On the coast of Osapa one may already, at low water, advance to the distance of more than three-fourths of a league on the shore abandoned by the waves: it is there that, with a mixture of astonishment and admiration, one may enjoy at ease the wonderful spectacle of thousands of these animals incessantly employed in the formation of the rocks on which one advances. All the genera are assembled at the same time at the feet of the observer; they press around him; their singular and fantastical forms, the different modifications of their colours, and those of their organization and their structure, attract, in turn, his attention and meditations; and when, provided with a good magnifying glass, he contemplates these beings, so weak he can scarcely conceive how nature, by means so small in appearance, should be able to raise up from the bottom of the sea those vast ridges of mountains which are continued over the face of the island, and which seem to form almost its whole substance. At Timor it would be easy to make a long series of observations on these interesting animals: the profound calmness of the sea, its high temperature, the nature of the shore, on which one may advance at low water, as already mentioned, to a very great distance, having the water scarcely up to the knee; the great abundance of these animals, and their variety, are all favourable to researches of this kind: they may be observed, described, and drawn in their natural state, as the water does not rise above them to the height of more than a few centimetres, or sometimes only millimetres; they may be seen in their state of contraction or extreme development; one may observe, also, their progressive increase, and its boundaries: in a word, there can be no doubt that a labour of this kind, undertaken by one or more enlightened naturalists, would contribute, in the most effectual manner, to the advancement of this part of natural history so little known, and which deserves so much to be carefully examined.

[To be continued.]

XXV. *Short Account of the Life of the late Dr.* Priestley.

Joseph Priestley, LL.D. F.R.S. and member of many foreign literary societies, was born at Field Head, near Leeds, in Yorkshire, on March 13, old style, in the year 1733. His mother died when he was very young; and his father, who was engaged in the cloth manufactory, marrying again, and having a large family, Joseph, when eight years of age, was taken into the house of a near relation, a lady eminent for piety and benevolence, who adopted and educated him as her own son.

He acquired the rudiments of the Latin and Greek languages under the instruction of Mr. Hague, a respectable clergyman, master of a free grammar school in the neighbourhood, and during the vacations he applied to the study of the Hebrew, Chaldee, and other oriental languages. By the assistance of Mr. Haggerston, who had been a pupil of the celebrated Maclaurin, he made a considerable proficiency in geometry, both speculative and practical algebra, and natural philosophy. He acquired also some skill in modern languages, in order to qualify himself for a merchant's counting-house, the delicacy of his constitution rendering it at one time doubtful whether he would be able to pursue his studies for a learned profession.

In his nineteenth year he entered as a student of divinity at at the academy of Daventry, which was the successor of that kept by Dr. Doddridge, at Northampton, and was conducted by Dr. Ashworth, whose first pupil young Priestley is said to have been. When about the age of twenty-two he was chosen assistant minister to the independent congregation of Needham-market, in Suffolk; and after a stay of three years at that place, he accepted an invitation to be pastor of a small congregation at Namptwich, in Cheshire, where he opened a day school, in the management of which he exhibited that turn for ingenious research and that spirit of improvement which were to be his distinguishing characteristics. He enlarged the minds of his pupils by philosophical experiments, and drew up an English grammar on an improved plan, which was his earliest publication.

On the death of the reverend Dr. Taylor, the tutor in divinity at Warrington academy, Dr. Aikin was chosen to supply his place, and Mr. Priestley was invited to undertake the vacant department in the belles lettres. He accordingly removed to Warrington in the year 1761, and soon after married a daughter of Mr. Wilkinson, of Bersham foundry, near Wrexham, a lady of an excellent heart and a strong understanding, and the faithful partner of all the vicissitudes of his life.

At Warrington the literary career of this eminent person properly commenced, and a variety of publications soon announced to the public the extent and originality of his talents. One of the first was a chart of biography, in which he ingeniously contrived to present an ocular image of the proportional duration of existence, and of the chronological period and synchronism of all the most eminent persons of all ages and countries, in the great departments of science, art, and public life. The favourable reception which this work experienced suggested a second chart of history, which exhibited in the like manner the extent, time, and duration of the different states and empires.

Having long amused himself with an electrical machine, and taken an interest in the progress of discovery in that branch of physics, he was induced to undertake a History of Electricity, with an account of its present state. It appears from his preface, that while engaged in this design he had enjoyed the advantage of personal intercourse with some eminent philosophers, among whom he acknowledges as coadjutors Dr. Watson, Dr. Franklin, and Mr. Canton. The work was first published at Warrington in 1767, 4to; and so well was it received that it underwent a fifth edition in 4to in 1794. It is, indeed, an admirable model of scientific history; full without superfluity, clear, methodical, candid, and unaffected. The original experiments detailed in it are highly ingenious, and gave a foretaste of that fertility of contrivance and sagacity of observation by which the author was afterwards so much distinguished.

His connection with Warrington ceased in 1768, at which time he accepted an invitation to officiate as pastor to a large and respectable congregation of protestant dissenters at Leeds. The favourable reception his History of Electricity had experienced induced him to adopt the grand design of tracing out the rise and progress of the other sciences in a historical form, and much of his time at this place was employed in his second work of this kind, entitled "The History and present State of the Discoveries relating to Vision, Light, and Colours;" which appeared in two vols. 4to, 1772. This work, though possessed of considerable merit, did not attain to the same popularity as the History of Electricity, and proved to be the termination of his plan: but science was no loser by this circumstance, as the activity of his mind was turned from the consideration of the discoveries of others to the attempt of making discoveries of his own; and nothing could be more brilliant than his success. It appears that at this period he had begun those experiments upon air which have given the greatest celebrity to his name as a natural philosopher.

In 1770 Dr. Priestley quitted Leeds; and having been recommended by his friend Dr. Price to the late marquis of Lansdown, then earl of Shelburne, he lived with his lordship in the capacity of his librarian, or rather as his literary and philosophical companion. During this period his family resided at Calne, in Wiltshire, adjacent to the country-seat of lord Shelburne. Dr. Priestley frequently accompanied his noble patron to London, and mixed at his house with several of the eminent characters of the time, by whom he was treated with every respect due to his character and talents. He also attended his lordship on a visit to Paris, where he was introduced to most of the celebrated men of letters and science in that capital.

To give a detailed account of Dr. Priestley's philosophical labours would require far more room than can be allotted to such an important object in a miscellany of this kind: we must therefore content ourselves with the following short notice. In the Philosophical Transactions for 1773 he published a paper containing observations on different kinds of air, which obtained the honorary prize of Copley's medal.

 These

These were reprinted, with many important additions, in the first volume of his Experiments and Observations on different Kinds of Air, 8vo, 1774. A second volume of this work appeared in 1775, and a third in 1777. Some of the most striking of his discoveries were those of nitrous and dephlogisticated air, or oxygen gas; of the restoration of vitiated air by vegetation; of the influence of light on vegetables; and of the effects of respiration on the blood. By these publications Dr. Priestley's fame was spread throughout all the enlightened countries of Europe, and most of the scientific bodies of Europe were ambitious to rank him among the number of their members.

The term of his engagement with lord Shelburne having expired, Dr. Priestley, with a pension of 150l. per annum, was at liberty to choose a new situation. He gave the preference to the populous town of Birmingham, induced chiefly by the advantages it afforded from the nature of its manufactures to the pursuits of chemical knowledge. It was also the residence of several men of science, among whom the names of Watt, Withering, Bolton, and Keir, are well known to the public. With these he was soon upon terms of friendly intercourse; and their *Lunarian Club* presented a constellation of talent which would not easily have been collected even in the metropolis.

He had not resided long at Birmingham when he was invited to undertake the office of pastor to a congregation of dissenters near that town, upon which he entered towards the close of the year 1780. The disgraceful scenes which took place at Birmingham in 1791, and which compelled Dr. Priestley to leave this situation as a fugitive, are well known to the public, and it is not our intention to revive the remembrance of them by entering into particulars. Suffice it to say, that the doctor's house, library, manuscripts, and apparatus, became a prey to the flames; and, though he received an indemnity for this loss, it was far from being an adequate compensation. The result of many years' painful research and scientific labour perished by this shameful outrage, which every friend to good order and justice deplored. For some time after this event Dr. Priestley lived as a wanderer, till he was invited to succeed Dr. Price in a congregation at Hackney; but the persecution he had experienced from the infatuated rabble, added to some family reasons, induced him to leave his native country, and to embark for America in 1794. The place he fixed on for his residence in the new world was Northumberland, a town in Pennsylvania, where having collected, by indefatigable pains, a valuable

valuable apparatus and well chosen library, he returned to his former pursuits. By many new experiments on the constitution of airs he became more and more fixed in the belief of the phlogistic theory, and in his opposition to the new French system of chemistry, of which he lived to be the sole opponent of note. The results of several of his inquiries on these topics were given both in separate publications and in the American Philosophical Transactions; and it is but fair to add, that the new theory is indebted to this opposition for some of the strongest proofs on which it is founded.

Dr. Priestley declined the offer of the chemical professorship in the college of Philadelphia, which was made to him soon after his arrival in America; and likewise another offer, of succeeding the late Dr. Ewing as principal of the same college, in the spring of 1803; preferring a life of retirement and leisure, that he might devote himself entirely to philosophical and theological inquiries. While he lived at Northumberland he had the misfortune to lose an excellent wife, and a beloved and dutiful son. These afflictions, though severely felt, he bore with becoming fortitude and resignation. Till the year 1801 he had enjoyed uninterrupted good health, having scarcely ever known what sickness was; but at that period he was attacked at Philadelphia by a constant indigestion, and difficulty of swallowing any kind of solid food. From about the beginning of November 1803 to the middle of January 1804 his complaint grew more serious, and at one time he was incapable of swallowing any thing for thirty hours. In the last fortnight of January his legs swelled nearly to his knees, and his weakness increasing very much, he expired on the 9th of February following.

As theology is entirely foreign to the object of the Philosophical Magazine, we have not thought proper to say any thing in regard to Dr. Priestley's writings on that subject, which are very numerous, nor the theological disputes in which he was engaged. His religious opinions are well known to the public, and therefore it is the less necessary for us to enter into any observations on them. The principal part of his other works are:—The History and present state of Electricity, with original Experiments, 4to: a familiar Introduction to the Study of Electricity, 8vo: the History and present State of Discoveries relating to Vision, Light, and Colours; two vols. 4to, with many plates: Experiments and Observations on different Kinds of Air, and other Branches of Natural Philosophy connected with the

 Subject,

Subject, 3 vols.: Experiments relating to the Decomposition of dephlogisticated and inflammable Air, and on the Generation of Air from Water—a pamphlet: Heads of a Course of Lectures on Experimental Philosophy, including Chemistry: a familiar Introduction to the Theory and Practice of Perspective, with copper-plates: a new Chart of History, containing a View of the principal Revolutions of Empire that have taken place in the World; with a Book describing it, containing an Epitome of Universal History: a Chart of Biography, with a Book containing an Explanation of it, and a Catalogue of all the Names inserted in it: the Rudiments of English Grammar, adapted to the Use of Schools; the same Grammar for the Use of those who have made some Proficiency in the Language: Lectures on History and general Policy, to which is prefixed an Essay on a Course of liberal Education for civil and active Life, 4to: Observations relating to Education, more especially as it respects the Mind; to which is added an Essay on a Course of liberal Education for civil and active Life: a Course of Lectures on Oratory and Criticism, 4to.

The following were published after the doctor went to America:—Experiments and Observations relating to the Analysis of atmospherical Air and the Generation of Air from Water: the Doctrine of Phlogiston established, and that of the Composition of Water refuted. Reprinted with additions 1803.

XXVI. *Notices respecting New Publications.*

Dr. Barton, professor of materia medica, natural history and botany, in the university of Pennsylvania, has announced his intention to publish, in America, a new periodical work, to be entitled "The Philadelphia Medical and Physical Journal;" to be published every six months.

Dr. Young's Course of Lectures on Natural Philosophy and the Mechanical Arts, delivered two years ago in the Theatre of the Royal Institution, is now printing, with considerable additions and improvements. The work will consist of two volumes, quarto; the first containing the text of the lectures nearly as they were delivered, but with such alterations as are calculated to make them still more intelligible to the most uninformed readers. The lectures are followed by a copious series of plates illustrative of every department

department of mechanical and physical science. The second volume will contain, in the first place, the mathematical elements of natural philosophy deduced from first principles, and in many instances extended by new investigations: secondly, a methodical catalogue of works relating to natural philosophy and the arts, with about ten thousand references to particular memoirs and passages, and a number of useful tables, and of concise abstracts and remarks: and lastly, a collection of the author's miscellaneous papers, reprinted, with some alterations, principally from the Philosophical Transactions. The work is expected to be completed early in the next winter.

XXVII. *Proceedings of Learned Societies.*

FRENCH NATIONAL INSTITUTE.

An Account of the Labours of the Class of the Mathematical and Physical Sciences of the French National Institute from the 20th of June 1804 *to the same Day* 1805. *By M.* Cuvier, *perpetual Secretary.*

PHYSICAL PART.

Almost all the sciences which engage the attention of the society have this year made curious and important acquisitions; and, as is usual, chemistry has obtained the most considerable and most numerous.

Count Rumford has examined heat under a new point of view. He has endeavoured to determine the force of the solar rays to produce it. The degree to which it is carried when its rays are concentrated by means of a burning glass is well known: but is their real power thereby augmented? or does the effect arise from their acting in greater number on a smaller space? To ascertain this, count Rumford invented a reservoir of heat, which is nothing else than a metal vessel filled with water having a thermometer immersed in it: it receives the solar rays on one of its faces, which is blackened, and the water it contains acquires a certain degree of heat. Count Rumford suffers these rays to arrive sometimes in a parallel direction and sometimes concentrated by a magnifying glass; but bringing the latter nearer or making it recede in such a manner that the rays shall strike on a greater or less space of the surface of the vessel, though their quantity continues always the same. The

The water in the reservoir always acquires the same degree of heat nearly in the same time. Hence the power of the rays to produce heat is always proportional to their quantity whether they are concentrated or not; or, what amounts to the same thing, the heat produced is proportional to the light absorbed.

It has long been believed that the heat of the earth does not all come from the sun, but that it is indebted for a great part of it to some focus concealed in its interior part: this is the old hypothesis of Descartes, which Buffon afterwards made the basis of other systems. M. Peron, sent by the Institute, as naturalist, with captain Baudin during his voyage of discovery, has made an extensive series of researches to ascertain the truth of this fact. He examined with an ingenious apparatus the temperature of the sea at different depths, and he every where found that it is colder the greater the depth. This result, agreeable to that before obtained by English navigators in other seas than those traversed by M. Peron, seems to destroy entirely the idea of a central fire. It is even probable that the deepest abysses of the sea are always frozen, even under the equator, in the same manner as the summits of the highest mountains *.

M. Biot has made a curious experiment in regard to the heat forced from bodies by compression. Oxygen and hydrogen gas, when merely mixed at the ordinary degree of the pressure of the atmosphere, have need, in order to combine, of the action of the electric spark. When put together in a condensing machine they combined merely by the heat which was disengaged, and abandoned one so considerable at the time of their combination that the machine burst every time the experiment was repeated †.

Common air, the medium in which not only the greater part of the phænomena of chemistry but those also of organic life take place, cannot be studied too carefully by philosophers. Its degree of purity, that is to say, the proportion of oxygen it contains, is one of the most important points that can be examined. Messrs. Humboldt and Gay-Lussac have compared the different means hitherto invented for measuring this proportion, and have shown that the best of all is that of Volta, which consists in burning hydrogen gas. A hundred parts in volume of oxygen are ne-

* This reasoning is very inconclusive. For, if the earth contains heat that does not come from the sun, the water which it warms must ascend to the surface, being displaced by that which is colder, and consequently more dense.—EDIT.

† See Philosophical Magazine, vol. xxi. p. 362.

cessary

cessary to saturate two hundred of hydrogen, whatever be the pressure and temperature. In this manner one may discover the hydrogen contained in any air whatever, even if it form only a three-thousandth part.

Messrs. Humboldt and Gay-Lussac have ascertained that there does not exist a sensible portion of hydrogen in the lower part of the atmosphere; and the aërostatic excursion of Messrs. Biot and Gay-Lussac, and that of M. Gay-Lussac alone, during which he rose to a much greater height, have confirmed that there is no more at the greatest elevation to which it is possible to rise, and far above that where the clouds are formed. Thus all the systems in which the formation of rain and other meteors was ascribed to the combustion of hydrogen gas, fall of themselves.

There still remains some uncertainty in regard to the number of the new metals which are mixed with platina. Were any confidence to be placed in the results hitherto announced, there would be, besides iron, copper, chrome, and lead, the metal discovered last year by Messrs. Fourcroy and Vauquelin, as well as by M. Descotils; two others found in it by Mr. Tennant, and two discovered by Dr. Wollaston called *rhodium* and *palladium*.

Dr. Wollaston, indeed, according to letters from London, discovered *palladium*, of which mention was made in my two last reports, and kept the discovery secret, as if to entrap chemists. He pretends that they have fallen completely into the snare, by imagining that this metal was a compound of platina and mercury; and, indeed, not only have the attempts of M. de Morveau to imitate *palladium*, according to the process of Mr. Chenevix, been unsuccessful, but the case was the same with three German chemists, Messrs. Rose, Gehler, and Richter. This *palladium*, therefore, ought to be a real metal. Is the case the same with *rhodium*, *osmium*, and *iridium?* or do these substances enter into the composition of each other, or into those discovered by Messrs. Fourcroy, Vauquelin, and Descotils? This question can be determined only by time.

Chemistry, however, appears to have acquired a new metal named *cerium*, from the planet Ceres. It was the oxide of this metal which M. Klaproth considered as a new earth, and named *ochroite*. Two Swedes, Messrs. Hessinger and Bezelius, have supposed it to be a metallic substance; and M. Vauquelin, who repeated their experiments, is of the same opinion. Nevertheless, as he was not able to reduce it completely, some doubts still remain.

We must leave also to time the confirmation of a discovery

very said, in a letter from Florence, to have been made by M. Pacchiani, professor at Pisa, of the radical of the muriatic acid, one of the most important questions, without doubt, that still remain to be resolved in chemistry. M. Pacchiani asserts, that he transformed water into oxygenated muriatic acid by taking from it a part of its oxygen by means of the Galvanic pile. The muriatic acid then will be hydrogen at its minimum of oxidation; the oxygenated muriatic acid, hydrogen in the middle state; and water, hydrogen at its maximum of oxidation.

Of all the objects of chemistry animal matters are the most embarrassing to it, on account of the great complication of their elements, and because the simplest agent that can be applied to them produces in them a thousand movements and transformations, the play of which escapes us, and of which we judge only by the results. This is what takes place, for example, when these matters are treated with nitric acid,—a method first employed by Scheele and Bergman, and from which M. Berthollet obtained so interesting results. The most apparent phænomenon, then, is the development of a great quantity of azote. Those next observed are an alteration of the acid; the production of a great deal of ammonia, of carbonic, oxalic, and malic acid; and the transformation of a part of these matters into tallow, and of another into a yellow bitter substance. But these effects vary, according to the strength of the acid, the duration of its action, and the nature of the matters subjected to it.

Messrs. Fourcroy and Vauquelin, by directing their researches to these variations, and the circumstances which attend them, have found that nitric acid applied to the muscular fibre, that is to say, flesh, transforms it by a first impression into a yellow matter, little sapid, little soluble, and yet acid; by a longer continued action, into another matter, also yellow and acid, but very little soluble and exceedingly bitter; and, in the last place, into a third matter, soluble but inflammable, and, what is very curious, detonating, not only in heat, like common gunpowder, but also by percussion.

Indigo furnishes a similar matter, and still more abundantly than animal matters. Messrs. Haussman and Walther had observed it for some time. Messrs. Fourcroy and Vauquelin suppose it to be produced by the disappearance of azote, and by the combination of the hydrogen and carbon of the flesh with a superabundance of oxygen furnished by the acid. They suppose that the yellow matter which tinges the

the bile, and that which colours the skin and the urine during the jaundice, is produced also by some combination of oxygen with the fibrine matter of the muscles, or with that of the blood.

Messrs. Fourcroy and Vauquelin have employed themselves also on the analysis of milk; and their researches have greatly simplified the theory of it. They have discovered that the acid which is developed in it, and which was considered to be of a particular nature, is nothing but the acid of vinegar modified by some animal substances and some salts which it holds in solution. Milk, according to them, must be considered as a mixed liquor, consisting of a great deal of water and of two kinds of matters, some of them really dissolved, as sugar, mucilage, muriate and sulphate of potash, and acetic acid; others merely suspended, as the matter of cheese, that of butter, and the phosphates of iron, lime and magnesia.

Considering the infinite complication of this first aliment of young animals, these gentlemen give us new motives for admiring the providence of nature, which has deposited in it all the materials of speedy growth. The caseous substance is almost the same as that of the muscles; the phosphate of iron is one of the elements of the blood; and that of lime forms the earthy basis, and is the cause of the hardness of the bones.

These gentlemen also have made a remark which may be interesting to medicine: it is, that the whey does not contain phosphoric salts, but when it can dissolve them in an excess of acid, and that it contains none when it is sweet.

There are in chemistry some questions, which though on the first view they seem entirely particular, yet the solution of them may extend to so many different objects that it might produce a revolution in the whole system of our knowledge. Such, for example, are the deposits formed by organized bodies of substances which we consider as simple, and which, as appears, these bodies, under several circumstances, could not acquire from without, but must have produced by combination.

Do animals form lime, and vegetables argil and silex, as some naturalists assert? The generation of stones and that of mountains, and the whole history of our globe, depend in some measure on this problem. It is to it we may refer the analysis of the tabasheer, a kind of stony concretion which is formed in the bamboo.

Messrs. Fourcroy and Vauquelin have found, as was said some years ago, that it is almost pure silex. But how could silex

silex be dissolved, and absorbed by the plant? How could it circulate in the sap? For all this must have been necessary before it could be deposited in the knots of the stem. Messrs. Fourcroy and Vauquelin are of opinion that potash has served as the solvent, and that it has carried with it into the sap these particles of silex. According to them, therefore, *tabasheer* proves nothing in favour of those who believe that silex can be produced merely by the act of vegetation.

These indefatigable chemists have carried their researches to a phænomenon of disease in vegetation, interesting by its singularity, and long known by the damage it occasions. They have endeavoured, in consequence of a memoir presented to the Institute by M. Girod-Chantrans, to determine the nature of the *smut* in wheat. They have found in it an oil of a green colour and of the consistence of butter; phosphoric acid, in part combined with magnesia, and lime, and ammonia; charcoal, and a vegeto-animal substance perfectly similar to that produced by the decomposition of the gluten of wheat by putrefaction. They thence conclude that the *smut* is a residuum of farina decomposed by putrid fermentation, and suppose that it arises from a superabundance of animal manure, and a too hot and moist temperature at the time of sowing, or when the grain is in flower. If agriculturists should acknowledge that these circumstances determine in reality a greater quantity of smut, it might be possible to prevent, in some cases, this scourge.

Messrs. Fourcroy and Vauquelin have analysed likewise a mould found at the depth of more than fifty feet in some desert islands of the South Seas, and which is employed as manure on the coasts of Peru, where it is called *guano*. This analysis has so great a resemblance to that of pigeons' dung, that there is reason to believe, with Mr. Humboldt, who brought this *guana* to Europe, that it is nothing but the excrement of birds, which frequent these islands in immense numbers.

This substance, as may be seen, is an object of very limited utility; but chemistry has long endeavoured to procure one to agriculture which would be of more universal importance, namely, sugar extracted from indigenous plants. We gave an account, at the time, of the efforts of M. Achard, of Berlin, to obtain it from beet-root. M. Proust, a celebrated Spanish chemist, has extracted it from grapes; he has given a detail of the whole process in a memoir sent to us from Madrid: hitherto, however, his sugar has neither the whiteness nor the hardness, and has not entirely the taste of the sugar made from the sugar cane.

According to recent intelligence from Germany, the process of M. Achard has been there much simplified; and this problem, so interesting in the present state of society, and which can scarcely fail to change the state of nations, is not far from being solved.

M. de Cossigny, a correspondent, has endeavoured to get more directly to this result. He is of opinion that the sugar cane might be cultivated in the southern provinces of France. It has, indeed, been cultivated at Nice for some time without producing sugar; but he asserts that this was owing to the juice being extracted too late, and to its having already undergone fermentation when boiled. He made very good sugar from canes cultivated in the *Jardin des Plantes*, but in a hot-house.

We have already spoken several times of the labours of M. Seguin in regard to the chemical arts and medical chemistry. He has continued them this year, and treated three important branches.

He first employed himself on the analysis of opium, in order to determine which of its component principles it is that gives it its medical properties. This celebrated juice exhibited three very distinct substances: a little acetous acid; another acid, which may be only the acetous or malic modified; a crystalline matter which appears to be new; an extract soluble in water and in alcohol; another extract soluble only in alcohol, acids, and alkalies; a vegetable oil a little concrete, and a sort of starch. Nothing remains but to try separately each of these substances, and to determine their respective effects on the animal body. M. Seguin is employed on this at present, and he has promised that he will soon communicate to us the result of his observations.

[To be continued.]

XXVIII. *Intelligence and Miscellaneous Articles.*

FORMATION OF MURIATIC ACID BY GALVANISM.

After that part of our present Number which contains the article respecting Mr. Peel's experiments was at press, we received Number III. of the Edinburgh Medical and

Surgical Journal, published on the 1st of July, to which was subjoined the following

"POSTSCRIPT,

"Containing an Account of the Discovery of the Composition of Muriatic Acid. By Professor PACCHIONI, of Pisa.

"Since this number of our journal was completed, and indeed part of the impression sent to London, we have received a latter, dated 15th May 1805, from our valuable and eminent correspondent Fabbroni, of Florence, in which he says, "a brilliant discovery has been made by one of my friends. I have inclosed an account of it, which you will transmit to my respectable and dear friend Kirwan, after having communicated it to the philosophers of your country through the medium of your journal." We therefore gladly take this means, though somewhat irregular, of complying with his request, and of giving to our readers the earliest possible notice of the discovery alluded to.

"*Letter of Dr.* FRANCIS PACCHIONI, *Professor of Philosophy in the University of Pisa, to* LAURENCE PIGNOTTI, *Historiographer to the King.*

"To you, my much respected friend, both on account of the spontaneous impulse of innate kindness with which you deigned to take so much care of my talents, such as they are, as to receive me among the number of your pupils, and on account of your having paved the way for my obtaining that very chair which was filled by you for many years with so much applause and honour to our country, rather than to any other person, shall I give an account of a discovery which I have made and satisfactorily verified. But these are not the only reasons by which my conduct is influenced. I wish, at the same time, to show my gratitude towards you, and to give you a proof that I am endeavouring to render myself more worthy of your esteem and friendship.

"It is perfectly known to you that, since last year, on account of the premiums proposed by that excellent general and philosopher Bonaparte, emperor of the French, for the advancement of that new and fertile branch of experimental philosophy discovered by the celebrated professor of Bologna, Galvani, and afterwards wonderfully extended by the sublime genius of Volta, I have contrived a great number of experiments, which I have performed with much care, and almost completed. These experiments have revealed to me many facts, which I am collecting for a memoir to be presented to the *Societa Italiana*; and have led me to a knowledge of the constituent elements of an acid which has hitherto proved refractory to all the efforts of chemistry. I speak of the muriatic acid, hitherto tortured in vain* with the electric spark, caloric, and all the play of affinity. You are perfectly acquainted with the different and discordant opinions of the most recent and approved writers concerning the nature of this acid; some of them considering it as a simple combustible body, others as formed of an unknown base combined with oxygen; and, lastly, others as a simple substance naturally acid. But these opinions have not contributed to the advancement of science, and are justly esteemed as mere hypotheses destitute of proof.

"Having, however, neglected these hypotheses, and considered the means by which the discovery of the nature of this acid has been hitherto attempted, it appeared to me that one had not yet been tried, viz. the continued action of the pile of the celebrated Volta, and I suspected that it might assist in leading me to discoveries which had hitherto escaped the research of experimental philosophers. As far as I can judge, my endeavours have been crowned with success, and have furnished me with satisfactory evidence of the nature of the constituent principles of muriatic acid.

"The simplicity of the apparatus, and of the means adopted to attain my views, the care with which I endeavoured to avoid every source of error, have, I hope, sufficiently secured me against those illusions which frequently deceive young men ardent in the pursuit of science, and even those practised in the art of extorting from nature her secrets. Want of time prevents me from relating the series of experiments by which I arrived at the discovery I have mentioned; but you may see it by perusing the manuscript of my memoir, which will be immediately published, to submit my researches and their results to the judgment of the learned. For the present, I shall select from the experiments and facts therein described those which are decisive, and which establish, in an evident manner, the following truths:

"I. Muriatic acid is an oxide of hydrogen, and consequently composed of hydrogen and oxygen.

"II. In the oxygenated muriatic acid, and therefore, *a fortiori*, in muriatic acid, there is a much less proportion of oxygen than in water.

* Professor Pacchioni could not possibly know that his discoveries had been in some measure anticipated by Mr. Peel, of Cambridge.—*Note of the Editors of the Edinburgh Medical Journal.*

 "III. Hy-

"III. Hydrogen is susceptible of very many and different degrees of oxidation, contrary to what is universally believed by pneumatic chemists, who assert that hydrogen is susceptible only of one invariable degree of oxidation, that in which it forms water.

"Having at first examined the phænomenon of the decomposition of water by the Galvanic pile, and having, by accurate experiments, ascertained the true theory, I readily discovered a very simple and exact apparatus, in which I could distinctly perceive the changes which happen to water, which, from the continued action of the Galvanic pile, is continually losing its oxygen at the surface of a wire of very pure gold immersed in it.

"I therefore proceeded to examine these gradual changes of water thus losing its oxygen; and I at last observed a very singular fact, which unequivocally indicated the formation of an acid. In other antecedent experiments I had examined the nature of the air obtained before arriving at this remarkable point, and I always found, by means of the eudiometer of Giobert, that it was very pure oxygen *, as the residuum scarcely amounted to one-sixtieth.

"Having thus examined the nature of the air formed in various experiments from the first moment of decomposition, until there were evident indications of the formation of an acid, I began to endeavour to determine, in a more positive manner, the existence and nature of this acid.

"When the water, or, to speak more accurately, the residual fluid, occupied about half the capacity of the receiver, which at first contained the water, this residual fluid presented the following characters:

"Its colour was an orange yellow, more or less deep, according as the bulk of the residual liquor was greater or less, and it resembled in appearance a true solution of gold.

"From the inferior orifice of the vessel, which was closed with a piece of taffety, and then with double bladder, there escaped a smell which was easily recognised to be that of oxygenated muriatic acid.

"The gold wire had in part lost its metallic lustre, and its surface appeared as if corroded by a solvent.

"The bit of taffety which had been in contact with the coloured fluid, in consequence of its action, was easily torn,

* In all experiments we are acquainted with, hydrogen gas was always evolved; but as we have no information concerning the arrangement of professor Pacchioni's apparatus, we cannot adduce this fact as conclusive against the accuracy of his experiments.—*Note of the Editors of the Edinburgh Medical Journal.*

as is usual with similar bodies when half burnt (semi-combusto).

"Around the edges of the vessel, on the bladder, there was formed a deep purple ring, which surrounded a circular space rendered entirely colourless, or white.

"A drop of this fluid tinged the skin of the hand, after some hours, with a beautiful rose colour.

"Having obtained, in various successive experiments, the same liquid, possessing constantly the same properties, I chose that obtained in the last experiment to subject it to chemical examination. The very able chemist of this university, Sig. Giuseppe Branchi, had the goodness to enter zealously into my views; and in his laboratory we easily proved,

"1. The existence of a volatile acid by the white vapours which were formed by ammonia placed near it.

"2. That this acid was certainly oxygenated muriatic acid, since it formed in nitrate of silver a curdy precipitate, the luna cornea of the antients, or the muriate of silver of the moderns. From these facts we may draw the following positive and undeniable results:

"1. Muriatic acid is an oxide of hydrogen, and is therefore composed of hydrogen and oxygen.

"2. Oxygenated muriatic acid, and of course muriatic acid, contains less oxygen than water does.

"3. Hydrogen has not one degree of oxygenation, but many. One of these constitutes water, another below it oxygenated muriatic acid, and, below this, there is another which constitutes muriatic acid.

"I shall mention the other degrees in another memoir, which will be published immediately.

"These, my much esteemed friend, are the decisive facts and experiments, which exclude every doubt, and which confirm my fortunate conjectures. It is long since experimental philosophy may be said to have become a source of wonders. The transmutation of azote into nitrous acid, and of hydrogen into water, appears to me truly wonderful, and your genius will enable you readily to judge whether the same epithet may be applied to the metamorphosis of water into the true solvent of gold and platinum, into that volatile substance which attacks and neutralizes pestilential miasmata, and presents so many resources to philosophy and the arts.

"After having thus discovered the elements of this refractory substance, I am engaged in determining their proportions by experiment and calculation.

"To

"To me it appears, that, the origin and nature of muriatic acid being now known, there is no longer any mystery in its formation, nor in that of the muriatic salts in the vast extent of the ocean. But these and other deductions will be explained by me in another place. They will have already occurred to you, and I should exceed the limits of this letter, if I were to enter further into the subject. With the most profound esteem and sincere attachment, I have the honour of subscribing myself

"Your much obliged servant and friend,

Pisa, May 9, 1805. "FRANCIS PACCHIONI."

"Contrary as the results announced in this interesting communication are to analogy, there are some facts from which they receive at least such a degree of confirmation as to entitle them to the attention of every one zealous in the cultivation of science. In 1801 Mr. Cruickshank discovered that infusion of litmus was reddened by the one end of the pile, and infusion of Brazil wood rendered purple by the other; but he supposed these effects to be owing to the formation of nitrous acid and ammonia; and only a few days before professor Pacchioni's letter was published at Pisa, the formation of muriatic acid by the Galvanic action was announced in London *, in a letter from Mr. Peel, dated Cambridge, April 23, 1805."

ON THE SAME SUBJECT.

After the preceding was in the hands of the printer, we received the following interesting communication.

To the Editor of the Philosophical Magazine.

SIR,

The very important discovery, announced in your Magazine for April last, by Mr. Peel, of Cambridge, has been lately confirmed by the evidence of professor Pacchioni, of Pisa †, who, without any knowledge of the experiments made in this country, attained similar results by the use of precisely the same means. There is one considerable point of difference, however, between the English and the Italian chemist, viz. that by passing a continued current of the Galvanic fluid through water, Mr. Peel obtained muriate of soda; while professor Pacchioni, having employed an interrupted gold wire for the same purpose, produced mu-

* Philosophical Magazine for April 1805, p. 279.

† See a letter from Fabbroni, of Florence, to the editors of the Edinburgh Medical and Surgical Journal, No. 3, published July 1.—H.

riate of gold. These experiments cannot fail to have excited an ardent interest in the mind of every chemist in this country, and an anxious expectation of the issue of the process in the hands of other experimentalists. For this reason I communicate to you the following account, though not perfectly conclusive, with a request that you will suppress it if more satisfactory testimony should reach you from any other quarter.

"The apparatus which I employed was such as would occur to any person having the same object in view; viz. a glass tube $4\frac{1}{2}$ inches long and ·35 inches diameter, in which were secured, by means of corks, two slips of platina (cut from a piece which was given to me, long ago, by Mr. Tennant) with their extremities at a proper distance from each other. The water, at the outset, amounted to two drachms; and was reduced, by six days' exposure to the current, (in part probably by evaporation, though carefully covered with pasteboard,) to a quantity which left half an inch of the tube unfilled. It had been most attentively purified, first by simple distillation, and again by a second distillation, after the addition of nitrate of silver. At the close of the experiment it was found to become opalescent in a few seconds by the mixture of nitrate of silver, and afterwards to undergo, when exposed to the light, the usual change of colour, indicating the presence of muriatic acid. To ascertain whether muriate of platina were present, I added a solution of muriate of ammonia to one portion, and to another carbonate of soda; but no precipitation ensued. This, however, might possibly be owing to the very dilute state of the solution; and I was proceeding to reduce the remainder by evaporation, with the view to further experiments, when the whole was unfortunately lost by accident.

"The repetition of this process requires the careful observance of one precaution, which is extremely likely to be overlooked. The water employed must, on no account, come into contact with the fingers of the operator; for I have found that from the surface of the skin there is a constant and copious excretion of muriate of soda, with, perhaps, a little muriate of ammonia. Of this any one may be satisfied by observing the change effected by nitrate of silver on pure distilled water after being poured on the palm of the hand; and if a glass tube, containing distilled water, be frequently inverted in a cup of the same, by means of the thumb or a finger, the water will be found to be precipitated by nitrate of silver. Suspecting that the corks might have furnished

furnished some muriatic salt, I added nitrate of silver to portions of pure water in which corks had been kept immersed 24 hours; but instead of opalescence being produced, the colour of the liquid passed through successive shades to that of Port wine, and the tingeing matter remained in solution, instead of settling to the bottom like muriate of silver. In future experiments on the synthesis of muriatic acid it will therefore be expedient to employ an apparatus in which the water shall neither come into contact with the fingers nor with corks. For transmitting the metallic wires, perforated glass stoppers, one of which has an aperture large enough to allow the water to escape as the gases are generated, would answer the purpose sufficiently. It is desirable also that the water employed should be well freed from air, and that the atmosphere should be excluded; for, if muriatic acid be generated, it will otherwise remain to be proved that azote is not one of its components; and this presumption is even confirmed by the extreme minuteness of the portion of muriatic acid which seemed to be produced in my experiment. If water contain all the elements of that acid, and nothing more be required to effect its transmutation than a change of their proportion, we might expect a considerable and unequivocal production of muriatic acid by the process of Galvanism. Another circumstance suggesting the presence of azote in this acid is, that on examining the liquor obtained by detonating impure hydrogen and oxygen gases in close copper vessels, Mr. Keir found that a small quantity of muriatic acid accompanied the nitrous acid thus formed. —See Keir's Dictionary, p. 119.

" The precautions which I have suggested will not be thought trivial by any one who recollects that one of the most accurate and celebrated chemists of this or any other time* was misled to a belief that he had effected the synthesis of muriatic acid, by a circumstance which was neglected solely from its apparent insignificance. The source of fallacy, in the instance alluded to, shows how unaccountably that acid may find its way into the subjects of our experiments, and introduce uncertainty into their results.

I am, sir, your obedient humble servant,

Manchester, July 23, 1805. " WILLIAM HENRY."

* Berthollet. The error arose from the employment of iron filings contaminated with muriatic acid, from which it required repeated washing with distilled water to free them, and which was even present in them when fresh made for the purpose.—*Annales de Chimie*, xxxix. 15, 16.

Mr.

Mr. Henry's suggestions are of great importance, and we doubt not will receive due attention from experimentalists. From all that has yet occurred on this subject, a strong presumption is furnished that we are on the verge of perhaps more than one important discovery in chemistry. At such a crisis, in particular, every phænomenon that by even the most remote analogies may appear connected with this inquiry, should be communicated by chemists for the benefit of science. For ourselves, we can only say we shall faithfully discharge the duty which devolves on us, to lay them carefully and correctly before the public.

TRAVELS.

Dr. Bolschoi, who, in the quality of physician, accompanied a Russian and Bucharian caravan, which in the year 1803, on its way from Troizk to Bucharia, was plundered by the Truchmens and Karakalpaks, and who on that occasion was taken prisoner, returned to Petersburgh about the beginning of November 1804, after undergoing various vicissitudes and sufferings. The following particulars respecting the Kirgisian Cozaks, communicated by this gentleman, are worthy of particular notice, as they relate to a people interesting to the Russian trade, but who have hitherto been highly prejudicial to it.

When the Kirgisians had divided by lot the booty which they obtained from this rich caravan, Dr. Bolschoi was estimated at the value of a camel. These plunderers cut to pieces the mathematical instruments, watches, telescopes, and other things of the like kind, that each might have a share of them. They did the same in regard to the medicines. The roots, powders, pills, and mixtures, were all divided into equal parts. Each person then threw his portion together into a vessel; and this they considered as the most valuable part of the plunder. When the Kirgisians found that their prisoner was a physician, and consequently, according to their idea, a kind of sorcerer, they thronged in crowds around him, that he might feel their pulse, in order to tell them, from the nature of it, whether the horse they had lost, the cow that had strayed, or the camel that was missing, would be again found: nay, some of them even wished him to tell from the nature of their pulse whether their absent sick mother, wife, sister, &c. would recover. If his answer turned out to be true, the prophet was rewarded; but in the contrary case, he was often subjected to the discipline of the whip.

A violent storm having once taken place, the whole body began to murmur; and a general suspicion fell on the captive doctor, who was considered as an adept in the art of witchcraft. It was immediately resolved that the sorcerer should be put to death: and this would certainly have been the case had not the storm fortunately subsided; so that the supposed sorcerer escaped with a slight correction.

As Mr. Bolschoi was considered as a man of a higher order, he was not sold into Bucharia with the other captives, but kept in the steppe. During his captivity he served as a common domestic, exposed to cold and hunger, and obliged to perform all those menial services which are allotted to the slaves of the Kirgisians. As he did not understand the language of his tyrants, he was beaten till he was able to tell the names of the most necessary articles in Kirgisian. He, however, did not long remain with one master, but was transferred from one person to another till he came into the service of the kan. With him he remained three months, but was exposed to no less hardship than under his former masters. The kan, however, in the hope of obtaining for him a large ransom, carried him to the Orenburg lines; at the distance of ten versts from which he gave him a rich Kirgisian dress, and in that state he was ransomed.

As some incorrect accounts of the Russian embassy to Ohina have been published in various journals, the following particulars, which appear to be correct, are taken from an extract of a letter written by a naturalist who was destined some time ago for a journey to Thibet, but who now is to accompany the embassy:—" Count Potocki, well known by his historical works, has been appointed the chief of this important mission. The principal naturalist is Mr. Adams, who accompanied count Mussin-Puschkin on his tour to the Caucasian mountains; Redofsky, formerly botanical gardener to count Alexis Razumofsky, is appointed physician, botanist, and entomologist; Pansner, mineralogist and geologue; Schubert, astronomer; and Klaproth junior, philologue. The whole of the scientific men, with their assistants, draughtsmen, artists, and a detachment of fifty soldiers, were to set out on the 4th of May: the embassy was to set off somewhat later. The route of the former was to be through Mosco, Nishney-Novgorod, Rasan, Ekaterinenbourg, and south from Tobolsk to Omsk, Kolywan, Irkutsk, to Kiachta, the Russian staple on the frontiers of Chinese Tartary. Here they will wait for the embassy, and for the Chinese Ta-tschins who are

are to escort the embassy, which will now consist of more than a hundred persons, through the desert of Robi and Yellow Mongolia to the city of Pekin.

M. Bergman, who has lately communicated to the public an interesting account of his long residence among the Kalmucs, is now preparing for another journey in the little frequented districts of Asia, which he has been invited by government to undertake, with the most liberal allowance for his support. M. Bergman has been appointed an assessor of the colleges; and leave has been given him to choose a physician, naturalist, and draughtsman, to accompany him. The benevolent Alexander has made provision for the wives of the travellers, in case any of them should die in the course of their journey.

M. Herman, professor of natural history at Dorpat, who last year made a tour through a part of Russian Finland, is about to return to that country with a draughtsman. It is supposed that on his return he will publish a journal of these two tours.

M. Giesecke, a Prussian mineralogist, has been for some time at Copenhagen. It is believed that the government proposes to send him to Greenland, where he will spend some years in examining that country in a mineralogical and geological point of view. Hitherto the Moravian missionaries have been the only persons who have ventured to reside for several years in Greenland.

ANTIQUITIES.

One of the houses of the city of Pompeii, buried under the lava of Vesuvius in the 79th year of the Christian æra, has been discovered by clearing away the lava, and a great many antique vases, coins, musical instruments, and a brazen Hercules, with several excellent paintings in fresco, have been found in it.

Some further account of this discovery is contained in the following letter, dated Naples, May 1, 1804:—"During the course of a search by digging, begun about seven years ago, the workmen discovered the capital of a pilaster, which was supposed to be the lateral face of a large gate. Last winter, the labour being resumed, the corresponding pilaster was found. The brazen hinges of the gate were transported to the Museum at Portici. The house to which this gate conducts is large and commodious, and richly ornamented with paintings and mosaic. It is surrounded by a beautiful a beautiful wall of cut stone, the joinings of which are so close, and the cement so perfect, that it has the appearance of a solid mass. The alley which serves as an entry is twelve palms in length and ten in breadth; it leads to a court, the walls of which are covered with stucco of different colours. The capitals and cornices are in very fine preservation. I remarked on them a large rose, which is a master-piece of elegance and design. All the chambers are ornamented with beautiful paintings on a red, blue, or yellow ground. They exhibit small exceedingly delicate columns, with flowers, candelabra, and other ornaments in the best taste. On the left are two apartments which in all probability were those of the master and mistress of the house.

" The painter had given full scope to his imagination in the composition of all these pieces, which I beheld with inexpressible pleasure. Nothing can be more attracting than a dance of masked personages; nothing more elegant than a small bird pecking at a basket of figs. In the middle of the court is a cistern, or *impluvium* of the Romans. On a marble pedestal is a young Hercules seated on a small fawn of bronze. These two pieces, one of which may weigh about twenty and the other forty pounds, are of the finest workmanship. From the mouth of the fawn water fell into a beautiful conch of Grecian marble. Behind the pedestal was a table, the feet of which, of antique yellow, represent the claws of an eagle. These works have also been conveyed to the Museum. A lateral corridor, on the right hand, conducts to a second court, which was surrounded by a portico, as appears by octangular columns coated with stucco."

Near the town of Fiesole, not far from Florence, a beautiful amphitheatre has been also discovered. The earth has already been cleared away from the greater part of it, and it appears that it was capable of containing at least 30,000 spectators.

VACCINATION.

We hear that Dr. Jenner is engaged in collecting reports from the different states of Europe, and from many of the other quarters of the globe, respecting the effects of vaccine inoculation on the mortality occasioned by the small-pox. In several of the largest cities on the continent, we are informed that he has already received the pleasing intelligence of the small-pox being either nearly or totally subdued. Among them is Vienna. But how melancholy is the reflection, that while the great and populous city of Vienna, which for time immemorial had been subjected to the incessant

cessant ravages of the small-pox, exhibited two deaths only by that disease in the year 1801, the city of London should even at this moment have to deplore the untimely fate of near fifty persons weekly by this horrid pestilence! a pestilence which it is obvious, from this and other similar examples, might not only be speedily banished from the metropolis, but from every part of the British empire.

PALLADIUM.

This new metal, recently discovered by Dr. Wollaston, may now be bought at Messrs. Knights' warehouse for chemical apparatus, Foster-lane, Cheapside, London.

ASTRONOMY.

A Table of the right Ascension and Declination of Ceres and Pallas.

	Ceres.					Pallas.				
	A R.			Decl. N.		A R.			Decl. S.	
1805	h	m	s	°	′	h	m	s	°	′
Sept. 2	6	39	52	22	17	4	39	12	8	27
5	6	24	8	22	21	4	42	48	9	9
8	6	28	20	22	24	4	46	16	9	53
11	6	32	24	22	28	4	49	36	10	39
14	6	36	24	22	31	4	52	48	11	26
17	6	40	20	22	34	4	55	52	12	15
20	6	44	8	22	38	4	58	44	13	5
23	6	47	48	22	41	5	1	24	13	57
26	6	51	28	22	44	5	3	56	14	50
29	6	54	56	22	47	5	6	16	15	45

LIST OF PATENTS FOR NEW INVENTIONS.

John Slater, of Liverpool, in the county palatine of Lancaster, gentleman; for certain improvements upon sawing-mills, or machines for sawing all kinds of timber.

Marc Isambard Brunel, of Portsea, in the county of Hants, gentleman; for saws and machinery, upon an improved construction, for sawing timber in an easy and expeditious manner.

John Edwards, of the parish of St. Paul, Covent Garden, in the country of Middlesex, currier and harness-maker; for certain improvements on bridles.

Obadiah Elliot, of the parish of St. Mary, Lambeth, in the county of Surrey, coach-maker; for certain improvements in the construction of coaches, chariots, barouches, landaus, and various other four-wheel carriages.

John

John Edwards, of the parish of St. Paul, Covent Garden, in the county of Middlesex, currier and harness-maker; for a machine or apparatus upon an improved construction, for the purpose of preventing persons being drowned, which he denominates the *life buoy*.

William Horrocks, of Stockport, in the county of Chester, cotton manufacturer; for further improvements to a machine for the weaving of cotton and other goods by hand, steam, water, or other power.

Charles Hobson, of Sheffield, in the county of York, silver plater, Charles Sylvester, of the same place, chemist, and John Moorhouse, of Sheffield aforesaid, surgeon; for a method of sheathing ships, roofing houses, and lining water-spouts, with a material not heretofore used for those purposes.

Thomas Pidgeon, of the parish of St. Pancras, in the county of Middlesex, gentleman; for a saddle upon an improved construction.

Abraham Ogier Stransbury, of the city of New York, in the United States; for locks and keys upon an improved construction.

John Bevans, of Little Queen-street, Lincoln's-inn Fields, in the county of Middlesex, carpenter and joiner; for a window-frame and sashes upon a principle wholly new, applicable to frames and sashes already made as to new ones, which conceal the sash-lines and exclude the air.

John Blunt, of the borough of Warwick, in the county of Warwick, surgeon; for an improvement to stirrups now in use, which is to be fixed thereto, and by means of which, whenever the stirrup happens to be in a reversed direction, by a horseman falling from his horse, the stirrup will immediately fall from the leather, by which means the same is suspended.

Samuel Miller, of the parish of St. Pancras, in the county of Middlesex, engineer; for an improvement upon, and machinery to be attached to, coaches and various other carriages, for the better accommodation of passengers.

John Cox Stevens, of New York, North America, but now residing in New Bond-street, in the county of Middlesex, gentleman; for a new method of generating steam.

Alexander Brodie, of Carey-street, in the liberty of the rolls, and county of Middlesex, iron-master and founder; for an improved method of making steam-engine boilers and steam boilers, for various other purposes; and of constructing the flue for the conveying the heat to the same, whereby the consumption of fuel is considerably lessened.

METEOR-

METEOROLOGICAL TABLE

BY MR. CAREY, OF THE STRAND,

For July 1805.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	52°	65°	53°	29·80	17°	Showery
28	54	56	51	·80	0	Thunder and hail showers
29	52	61	54	30·01	42	Fair
30	58	70	57	·13	48	Fair
July 1	57	69	61	·21	62	Fair
2	62	68	60	29·98	32	Cloudy
3	63	68	61	·80	15	Rain
4	65	76	68	·63	60	Fair
5	65	68	60	·60	42	Cloudy
6	60	66	56	·71	45	Showery
7	60	67	58	·88	29	Showery
8	55	59	56	·90	10	Showery
9	57	68	58	30·00	37	Fair
10	58	71	56	29·90	58	Fair
11	55	58	53	30·02	12	Cloudy
12	55	64	55	·01	52	Fair
13	56	66	56	29·98	52	Fair
14	58	68	57	·98	15	Cloudy
15	56	61	56	·99	18	Cloudy
16	57	66	57	30·03	51	Fair
17	55	61	55	·08	29	Cloudy
18	55	64	53	·06	42	Fair
19	54	64	58	·02	36	Fair
20	58	74	62	29·90	26	Fair
21	64	69	58	·68	6	Showery
22	58	70	59	·73	42	Fair, rain at night
23	60	70	60	·28	27	Showery
24	61	66	59	·65	40	Fair
25	60	64	57	·96	43	Cloudy
26	58	70	58	30·01	46	Fair

N. B. The barometer's height is taken at noon.

XXIX. *Account of Experiments made on a Mineral called Cerite, and on the particular Substance which it contains, and which has been considered as a new Metal.* By M. Vauquelin*.

M. Klaproth wrote to me, about eight months ago, that he had discovered, in a mineral of Batsnaes, in Sweden, a new earth to which he had given the name of *ochroit*, on account of the red colour which it assumes by calcination. He even sent me in a letter a small specimen of this substance; and having discovered in it, by several trials, the presence of a considerable quantity of oxide of iron, I started some doubts, in a note which I read in the Institute, in regard to the colour of that earth. I observed also in the same note, that this substance had as many metallic properties as earthy characters; but that the small quantity of it which I had in my possession did not allow me to give any decisive opinion on this subject †.

Some time after, Messrs. Berzelius and Hisenger, having been informed, by their correspondents at Paris, of M. Klaproth's labour, wrote to me to claim a priority, stating that they had sent to M. Klaproth the specimens of that mineral which he had employed for his experiments, and that at the same time they had announced to him that they had found a new metal in it. I can give no opinion on this difference. I shall only observe, that the well known delicacy of M. Klaproth, and the high reputation he has justly acquired by his numerous and important discoveries, render it very improbable that he would appropriate to himself the discovery of another. M. Klaproth must, no doubt, have received from another quarter the mineral in question; and his labour was perhaps terminated before he acquired any information respecting that of the Swedish chemists. What seems to justify this opinion is, that they obtained results entirely different.

Every thing, therefore, seems to show that M. Klaproth of Berlin, and Messrs. Berzelius and Hisenger of Stockholm, made experiments at the same time on the same mineral without having any communication with each other; and that each may have had the honour of the discovery.

The Swedish chemists transmitted to Paris a memoir on this

* From *Annales du Muséum National d'Histoire Naturelle*, No. 30.

† See *Annales de Chimie*, No. 149, Floreal 30, an 12.

this subject written in Swedish. M. Limdbon undertook a translation of it, and caused it to be printed in the *Annales de Chimie* *. In this memoir they give a history of the mineral, and point out the places where it is found, and the substances which accompany it: they then give an account of the methods they employed to ascertain the nature of it: they give the characters of the new substance it contains, and which they consider as a peculiar metal, to which they give the name of *cerium*; a denomination taken from the planet Ceres, discovered by M. Piazzi: they have thence formed that of cerite, to denote the natural ore of that metal.

About the end of November, that year, I received, by the care of Messrs. Hisenger and Berzelius, specimens of this fossil; with an invitation to repeat their experiments, and to determine whether the substance in question ought to be classed with the earths or the metals. I charged myself the more readily with this labour, as it furnished me with an opportunity of subjecting to experiment a new substance, and of pronouncing in regard to the opinion of respectable philosophers, whose sole object is truth.

It will be seen by this memoir that the force of facts has obliged me to adopt the opinion of the Swedish chemists. I must not omit to mention that I have been seconded in my experiments by Messrs. Tassaert and Bergman, both of whom are well versed in practical chemistry.

Physical Properties of Cerite.

This mineral is of a slight rose colour: it is sufficiently hard to scratch glass: its specific gravity is 45·30, and its dust is grayish: it becomes reddish by calcination, and loses twelve per cent.

There are some varieties which contain martial pyrites, and which are traversed by veins of green actinote.

Preliminary Trials on Cerite.

This mineral, when pulverized in a mortar of silex, does not increase in weight, which indicates that its hardness is not very great: its dust is of a rust gray colour.

When exposed to heat in a retort there are obtained some drops of water, which are condensed in the neck of the vessel.

The dust of cerite is powerfully attacked by the nitric muriatic and nitro-muriatic acids; caloric is developed, and there

* No. 150, Prairial 30, an 12.

there is disengaged carbonic acid as well as nitrous gas when nitric acid is employed.

After ebullition of half an hour the action of the acids appears to be exhausted, and there remains at the bottom of the vessel a dust more or less coloured, which is the silex contained in the mineral.

When cerite is treated with eight or ten times its weight of acid it is entirely decomposed by one operation, and without the necessity of beginning a second time; yet it is impossible by these means to obtain silex perfectly pure: it always retains a certain quantity of metallic oxide. It is only by fusing this earth with an alkali, and then combining it with an acid, that it is possible to obtain it pure, and free from all colouring matter: it generally forms about seventeen hundredths of the mineral. When the solutions of this matter are evaporated to dryness and the residuum is dissolved in water, there is formed a slight white precipitate, which appears to be a little silex which the acid held in solution.

Solutions of cerium are of a yellowish red colour, like that of the oxide of iron at its maximum of oxygenation; but when cerium is little oxidated they are only of a rose colour, similar to those of manganese and cobalt.

These solutions, decomposed by ammonia, furnish a very voluminous precipitate, which has the appearance of alumine mixed with oxide of iron, but which greatly differs from it in its properties: when dried in a gentle heat this precipitate is reduced to a granulated powder of a pale yellow colour, which becomes of a brick red by calcination. The matter simply dried in the air redissolves readily in the nitric and muriatic acids; but the red oxide, that which has not been calcined, is scarcely attacked, and does not dissolve in muriatic acid without producing a very considerable quantity of oxygenated muriatic acid.

The nitric solution readily crystallizes: the salt which it furnishes is soluble in alcohol: in regard to the muriatic solution it is very difficult to obtain crystals: this salt when dried is deliquescent.

The nitric and muriatic solutions are decomposed by alkaline sulphates, phosphates, borates, oxalates, tartrites, and carbonates: with sulphates there are formed yellow precipitates too soluble in water to be subjected to analytical experiments: besides, a part of the iron oxidated to a maximum is precipitated at the same time. The precipitate formed by the borates is still more soluble in acids: that produced by oxalates is attended with the inconvenience of carrying with it a little iron, which gives it a slight tint of a rose colour: it is soluble in acids. The tartrites form a precipitate much less soluble, which does not contain iron; but it is entirely soluble in caustic alkalies as well as in an excess of its concentrated acid, from which it is afterwards separated by water. Phosphates occasion a precipitate which is not soluble in acids without the aid of heat: iron remains in intimate combination with it. Prussiates precipitate solutions of cerium white, even when they contain evident traces of iron. All the precipitates here spoken of are white, and retain that colour after desiccation, except the phosphate, which becomes grayish. Sulphurets and hydro-sulphurets precipitate solutions of cerium white: the precipitates when washed retain their white colour in drying, and dissolve in acids with effervescence: carbonic acid is disengaged, but not an atom of hydrogenated sulphuret; which proves that cerium does not unite with sulphurized hydrogen.

Zinc, tin, and iron, immersed in a solution of muriate of cerium, do not effect a reduction of it. They precipitate a black matter, which is in too small quantity to be analysed: there is deposited at the same time a white powder, which appears to be an oxide of the precipitating metal.

An alcoholic solution of gall-nuts produces in muriate of cerium a yellowish precipitate not very abundant. The addition of a few drops of ammonia determines a very voluminous one of a brown colour, which becomes black and brilliant by desiccation: by the action of heat it resumes a beautiful brick red colour.

When the silex extracted from cerite is fused with an alkali, it is observed that the mixture assumes a beautiful pale straw colour, which soon passes to brown: if the surfaces be often renewed the whole matter becomes brown, but by adding a little charcoal this colour vanishes entirely.

Having made these preliminary trials on cerite, and ascertained the principal properties of the particular substance which it contains, I undertook to analyse it, in regard to quantity, in the following manner:

A hundred parts of this mineral in fine powder were mixed with ten times their weight of nitro-muriatic acid, and subjected to ebullition for an hour: the mixture being diluted with water, and filtered, left on the filter a brown dust, which was dried, and fused with caustic potash. The mixture being diluted with water, and then dissolved in muriatic acid, evaporated to dryness, and redissolved in water, left a powder which when collected on a filter, washed,

washed, and calcined, weighed seventeen parts: it was silex, still slightly coloured yellow.

The nitro-muriatic solution being evaporated to dryness, and its residuum redissolved in water, left about one part of silex coloured by a little oxide of cerium.

The same solution freed from silex, and united to the washings of the silex, was decomposed by ammonia: the oxide of cerium and the oxide of iron precipitated by these means, were separated from the liquor by filtration. The oxalic acid added to this liquor formed a precipitate which by calcination gave two parts of lime.

The metallic oxides, united and calcined, weighed seventy parts: they had a beautiful reddish brown colour. To separate the iron of the cerium the whole was dissolved in muriatic acid: the solution being concentrated to evaporate the excess of acid, then diluted with water and decomposed by tartrite of potash, there was formed a very abundant white precipitate, which being washed till it contained no more foreign salts, then dried, and calcined, gave sixty-seven parts of oxide of cerium.

The water from the washing of the tartrite of the cerium, being united and mixed with hydro-sulphuret of potash, gave a precipitate which became black in the air. It was oxide of iron, the weight of which after calcination was two parts.

Thus 100 parts of cerite subjected to analysis furnished,

1st, Silex - -	17
2d, Lime - -	2
3d, Oxide of iron -	2
4th, Oxide of cerium -	67
5th, Water and carbonic acid	12
	100

Though the specific gravity of cerite, the varied colours assumed by the particular matter it contains, and the oxygen disengaged during its solution in muriatic acid, afforded great probabilities in regard to the metallic nature of this substance; yet, as it was possible that these properties and these phænomena might be owing to the presence of some known metal, to manganese for example, I endeavoured to discover it by all the means which appeared to me proper for accomplishing that end; but I did not find any sensible traces of it. It therefore appears to me altogether improbable that manganese should contribute any thing towards the properties exhibited by the matter of cerite. The case is not the same with iron. I must confess that when any traces of it remain in the cerium it communicates to it a darker red colour; but as this matter, when disengaged from iron, as far as chemical means will allow, assumes still a reddish colour by calcination, and as in this state it furnishes as much oxygenated muriatic acid as before, it is equally impossible to ascribe these phænomena to the iron, which, as is well known, produces no oxygenated muriatic acid.

Thus as cerium, in which the slightest sign of the presence of iron, or of any other foreign matter, could not be detected by any means whatever, always assumes a red colour by calcination, and then gives oxygenated muriatic acid during its solution, I am forced to consider it as a metallic oxide rather than an earth, as M. Klaproth has done. Hitherto, indeed, chemists were not acquainted with any earth weighing five times as much as water, which has a colour of its own, which absorbs oxygen, and which, dissolving in common muriatic acid, produces oxygenated muriatic acid.

I had great hope that the reduction of this matter to the metallic state by the action of a strong heat would confirm the above probabilities, and convert them into certain truths; but this operation was not attended with all the success I expected.

In the first attempt, in which I had put into a charcoal crucible oxalate of cerium reduced to a paste with oil, the whole was volatilized by the violence and duration of the heat: at the bottom of the crucible I found only a metallic grain scarcely so large as the head of a pin, and which was an alloy of iron and cerium. This experiment, if it furnished no metal, proves at least that oxide of cerium is volatile; and I do not know that an earthy substance was ever thus volatilized.

In the second operation I put into a luted porcelain retort a paste made with tartrite of cerium, a little lamp-black and oil, in order that I might collect the metal if it should be volatilized as before; but as the form of my apparatus did not permit me to give as much heat, the matter was not reduced: it remained in its natural state mixed with the charcoal.

There were seen, however, on the sides of the retort a great number of small globules which had metallic brilliancy, and the substance of which had been manifestly volatilized. Some of the largest of these globules having been detached and broken, exhibited in the inside a white colour

colour and a foliaceous texture. There was also in the neck of the retort a slight reddish covering, the taste of which was exceedingly acrid and metallic: the quantity of the matter which formed this covering was too small to be subjected to experiments capable of determining its nature.

Three of these small metallic globules, which weighed together scarcely a fourth part of a grain, being put successively into nitric and muriatic acid, were not sensibly attacked: to effect the solution of them the union of these two acids was necessary. The solution being evaporated, and its residuum dissolved in water, had no colour; its taste was sensibly saccharine; and by the oxalate of ammonia and the prussiate of potash it gave white and flaky precipitates. It appears, then, that these globules, which I suspected to be iron, are really cerium. What is certain is, that these globules are much more fragile, whiter, and less liable to be attacked by acids than cast iron.

These experiments prove that cerium is volatile at a high temperature, and that it is probably only at the moment of its volatilization that it is reduced, unless we suppose that it is rather volatilized in the state of oxide. This I propose to ascertain by new trials.

Recapitulating what has been said in the course of this notice, it is seen, 1st, That cerium, freed from the foreign matters which accompany it in the mineral, is a substance susceptible of uniting with two quantities of oxygen very distinct.

2d, That with the first quantity it forms a white substance, soluble in acids, without any disengagement of oxygen.

3d, That with the second portion it assumes a slight red colour, combines only with difficulty with acids, and constantly produces a considerable quantity of oxygenated muriatic acid by dissolving in common muriatic acid.

4th, That these oxides do not dissolve in alkalies; but that when boiled together they no longer become coloured by the contact of the air; and that those which are red become white by a slight heat, without, however, combining with the alkalis.

5th, That their combinations with the sulphuric, phosphoric, oxalic, tartareous, and prussic acids are white, and insoluble in water.

6th, That, on the other hand, those which they form with nitric, muriatic, and acetic acids are very soluble in water and in alcohol, and are even deliquescent.

 7th, That

7th, That all these salts have an astringent and highly saccharine taste.

8th, That a good process for separating the iron of cerium is, to precipitate the latter from its nitric or muriatic solution by oxalate of ammonia or tartrite of potash, putting into the liquor a slight excess of acid; or, what is better, is, to calcine the muriate of cerium, to redissolve its residuum in the muriatic acid, to calcine again, and to repeat this three times, in order to sublime entirely the muriate of iron; which succeeds very well.

9th, That cerium does not unite with sulphurated hydrogen, like the other metallic oxides.

10th, That it appears irreducible by those means which generally succeed with the most refractory oxides, but that it is volatile, and that it is probably at this moment that its reduction is effected.

11th, That if, contrary to every appearance, cerium is not a metal, it has, at any rate, much more analogy and relation with that class of bodies than with any other; and for these reasons we shall place it, with Messrs. Hisenger and Berzelius, in that class, till it has been proved that it is better fitted to any other kind of matters.

12th, In the last place, that by some lucky chance, or means better combined than those hitherto employed, we shall obtain it in the metallic state; and I do not despair myself of meeting with this success.

XXX. *Memoir on some zoological Facts applicable to the Theory of the Earth. Read in the Physical and Mathematical Class of the French National Institute on the 22d of October* 1804. *By M.* Peron, *Naturalist to the Expedition for making Discoveries in Australasia.*

[Concluded from p. 166.]

I have now terminated the general history of petrified and living zoophytes: we have seen them cantoned, as we say, in that zone of the globe comprehended between the 34th degree of north and south latitude, where they fill the sea with dangerous reefs, form new islands, enlarge the old ones, and every where increase the domain of the land at the expense of the ocean which nourishes them in its bosom: we have seen their antient labours rising over the surface of the waves, and appearing again at great heights above their present level. The last phænomenon deserves our attention

tention for a moment; and two questions here present themselves to be resolved. Have the madreporic mountains been formed in the bosom of the sea? and, supposing this to be the case, what revolutions were capable of effecting so prodigious a change, either in their antient state or in that of the waves?

There can be no doubt that the former of these questions may, and ought to be, answered in the affirmative. Observation,—indeed experience, reasoning, and analogy, all unite to prove that these pelagian animals, the vast remains of which cover our continents with an organization similar to that of families now existing, have had the same origin and the same country. No objection has yet been made against this general assent. But if any doubts of this kind had been formed in regard to the different banks of testacea, or even of zoophytes, disseminated throughout the large continents at considerable distances from the sea shore, the consequences could not be extended to those reefs, those islands, and those archipelagoes, several of which still declare their origin by the little elevation they have acquired above the place where they were formed. It may therefore be considered as an incontestable fact, that all the madreporic productions we have seen raised more or less above the present level of the sea, were formed in its bosom.

The second question, it appears, may be resolved with as little difficulty. I shall here observe, to make use of an expression of the Nestor of the French navy in regard to the enormous bones seen at the Malouines at a considerable distance in the interior,—*the land has either been raised up, or the sea has sunk down.* In the first supposition, we cannot conceive any other cause susceptible of raising up similar masses, but volcanic eruptions as frequent as energetic. But independently of a multitude of other reasons which tend to make us reject a cause of this kind, do we not know that these grand convulsions of nature always leave behind them indelible traces of the disorder and confusion by which they are exclusively characterized? But nothing of this kind is observed in the madreporic countries. I have already spoken of the regular forms and insensible gradations of the island of Timor, an image and production at the same time of the calmness of nature: I have already quoted the ingenious observations of captain Vancouver, which alone are sufficient to show how peaceable was the cause which left these madreporic eminences uncovered, whether its action was slow, rapid, or even instantaneous. Labillardiere made similar observations: the two Forsters have furnished

nished us with valuable facts on the same subject: and M. Fleurieu himself, having given the opinion of these two naturalists, expresses himself as follows:—"To which of our common systems can we refer the origin of that prodigious number of small spots, either scattered or formed into groupes, or united into archipelagoes, which, according to the most accurate researches, appear to be still in a state of increase? These islands are met with at the distance of 1500 leagues from the continent and from large islands, and in the middle of a sea the depth of which cannot be measured with the sounding-line. The attentive eye of the enlightened observer has discovered nothing in these low islands which indicates the former existence, remains, or traces of burnt-out volcanoes, or volcanoes swallowed up by the sea; nothing which can show that they were produced by any convulsion of the globe: every thing, on the contrary, announces that they have been the production of ages; that the work is not yet completed; and that a gradual increase in them must take place: but that a long succession of years is necessary to render it sensible." The unanimous opinion, therefore, of all observers, while it rejects every idea of volcanic origin, destroys thereby every other opinion which might suppose that the earth itself could rise above the surface of the waves. This immediate consequence then results, that the water must have sunk down below its antient level.

A very delicate but interesting question here occurs: What became of the water of the sea when it abandoned the summits of the mountains formed in its bosom? The solution of this question appears to me to have an immediate dependence on another of the same nature, and no less difficult:—Whence arises that enormous quantity of calcareous matter, which, as we see, performs so extensive a part in the revolutions of the earth? Here, also, an immense field is opened to imagination and hypothesis. Satisfied myself with having collected, compared, and arranged the most correct observations, that I might deduce from them the most general and most certain consequences, I shall give, in a few words, those which I conceive to result from the numerous facts here stated.

1st, From the absolute difference of the two races in New Holland and Van Diemen's Land, as well as from the absence of the dog in the latter, I think myself authorized to conclude that the separation of these two countries must have taken place at a period much more remote than may at first be supposed.

2d, The

2d, The exclusion of all relation between these two races; the darker colour of the inhabitants of Van Diemen's Land; their short, woolly, and curled hair, in a country much colder than New Holland, where the contrary is the case; appear to me to be new proofs of the imperfection of our systems in regard to the intercourse of nations, their transmigrations, and the influence of climate on man.

3d, From the petrified shells and zoophytes which I observed in different places and at different heights in Van Diemen's Land, New Holland, and Timor, I infer that the sea formerly covered that part of the Austral lands which reaches from the 44th degree of south latitude to the 9th, over an extent of 700 leagues from south to north: a result the more valuable, as this immense region was the only one which remained to be known under this point of view.

4th, Having given this explanation as simple as satisfactory in regard to the formation of those beautiful calcareous incrustations so frequent on the south-west and north-west coasts of New Holland, I have taken an opportunity of proving how difficult it is, in certain cases, to distinguish bodies altered in this manner from those which are really fossils.

5th, In my observations on solid zoophytes I have confirmed their almost absolute exclusion from the most Austral seas of the Antarctic hemisphere; I have proved that this important family of animals is banished by nature to the middle of the warmest seas, to the most peaceful equinoctial regions, and those which border on them.

6th, We saw them there, in the state of petrifactions, forming all the low islands of the great equinoctial ocean, and some, at least, of the highest in that sea and in the Indian Ocean.

7th, We found them there, in the living state, interspersing the sea with new dangers, multiplying the reefs, enlarging the islands and archipelagoes, encumbering the harbours and ports, and every where throwing up new calcareous mountains.

While man, therefore, who styles himself the *king of nature*, rears, with labour, on the surface of the earth those frail monuments of his pride, which the breath of time must soon destroy; feeble animals, which have so long escaped his observation, and which he still disdains to notice, multiply at the bottom of the ocean these immense testimonies of power which brave ages, and which our imagination itself can hardly conceive.

XXXI. *Letter*

XXXI. *Letter to M.* Lacepede, *of Paris, on the Natural History of North America. By* Benjamin Smith Barton, *M.D. Professor of Materia Medica, Natural History, and Botany, in the University of Pennsylvania.*

[Concluded from p. 103.]

Our country, as you know, is very rich in birds. A considerable number of these have been described, or mentioned, by Buffon, Pennant, and other writers. But the ornithology of the North American continent is very imperfectly understood. Many of our birds have never been correctly, if at all, described. Some of these undescribed species are large birds; such as a species of plotus, called in Carolina the snake-bird; not to mention others. But it is a matter of more consequence to study the manners of those birds that have already been discovered, than merely to detect new species. I flatter myself that I have made considerable progress in the study of the *mores* of our birds. I have been particularly attentive to their instincts as we call them, and have even digested my collection of facts on this subject into order. These facts will form a part of a large work on the Instinct of Animals. It is now completely ascertained that some of the American species of swallows remain among us in a torpid state. They retire into the creves of rocks, into the hollows of trees, and other similar situations, and rest, for some months, in a lethargic sleep more or less profound. The species concerning which I have the most correct information, as to what regards their torpidity, are the purple martin (*hirundo purpurea*), and the chimney-bird (*hirundo pelasgia*). Both of these species, I repeat it, do sometimes become torpid in the climate of Pennsylvania. It is probable that the other species do the same. But I still adhere to my former opinion: I contend that the great body of American swallows, of different species, are really migratory birds; that is, they leave the climates of the United States on the coming on of cold weather, and retire southward, to more favourable latitudes. These facts show us how much birds can accommodate themselves to different situations, and give weight to an opinion which I have long entertained, that all animals are capable of the torpid condition. Even the little humming-bird (*trochilus colubris*), which is unquestionably a (generally) migratory bird, is sometimes overtaken by the colds of our climate, and, on such occasions, has been known to fall into the torpid state.

I lately

I lately ascertained a fact in ornithology, at once singular and interesting. It is well known that the *anas sponsa* of Linnæus, called in the United States summer duck, breeds in the hollows of trees, on the banks of rivers, or in islands at a considerable height from the ground. The young ones, soon after they are hatched, descend the sides of the tree, and thus make their way into the water. I do not know of a similar fact in the history of birds.

I have read, with much pleasure, count Morozzo's letter to you respecting a parrot hatched at Rome*. I am sorry, however, to find so sensible a writer adhering to the paradox, that "the parrots both of the old and new continent never pass the tropics, and seem confined to a zone of 23° on each side of the equator; and that "in their wild state they never pass these limits, which nature seems to have prescribed to them." The parrots of America are, I assure you, much greater voyagers than count Morozzo supposes. They sail far beyond the tropics. They are frequently found in immense flocks upon the river Ohio as high as the latitude of 40°: nay, a very large flight of these birds has been seen, on the eastern side of the United States, as high as the latitude of 42° †.

From the birds I pass to the oviparous quadrupeds and other amphibia. The American animals of this class have been very imperfectly investigated. Notwithstanding your labours in your inestimable work on the *Quadrupedes Ovipares,* and the labours of Schoepf, we possess several species of testudo which have entirely escaped your attention. In the western parts of Pennsylvania, Virginia, &c. there is a species of this genus considerably allied to, but still different from, the one which you have called *la molle*. The new species is called by some of our Indians *pi-si-li-Tul-pe,* which literally signifies "the soft-shelled turtle." They are very prolific, and both their flesh and eggs are deemed excellent eating. Our western rivers, such as the Ohio and its branches, likewise possess a very remarkable undescribed species of lacerta, if, indeed, it be not entirely a new genus. It is sometimes seen near twenty inches in length. By the white people it is called alligator, though it is very different from the southern animal of this name. Its whole body, but in particular the head, contains a milk-like fluid. This animal lives upon fish, frogs, &c. It is often taken with the line and hook. The Indians say it is poisonous: but

* See Philosophical Magazine, vol. xvi. p. 318.

† See my Fragments of the Natural History of Pennsylvania, part i.

this

this is a doubtful point. Several of our species of rana are entirely undescribed by naturalists. I may, with great confidence, make the same observation concerning our serpents. The little black rattlesnake, which inhabits the marshy grounds, is, I think, a new species. This species is seldom more than a foot in length, and is deemed extremely venomous. I suspect the species described by Mr. de Beauvois (in the Transactions of our Society), and which he calls *crotalus adamantinus*, is the same as that which is found in South America. I think it is not quite certain that the *crotalus horridus*, the most common species of North American rattlesnake, extends to South America. But I do not wish to be understood as speaking positively on this subject.

The history of the rattlesnake is by no means complete. I have, within the last two years, devoted a great deal of attention to this curious subject. I have had a number of living rattlesnakes under my immediate care. I have made a considerable number of experiments to ascertain the effects of the venom of this reptile upon different animals. It is unquestionably a most powerful poison. It often kills in a very few minutes. The effects of the poison are very various, not only in different species of animals, but even in different individuals of the same species. It sometimes induces most violent pains, which, if we may judge from the cries of the bitten animal, continue nearly to the close of its life. At other times, the poison induces death without creating any or but very little pain. Hitherto my principal experiments have been made with warm-blooded animals, such as dogs, cats, and rabbits. I am inclined to think that the venom exerts very inconsiderable effects upon cold-blooded animals. Warm-blooded animals that have been most violently affected by the poison, sometimes struggle through the danger, and perfectly recover, although no remedy has been applied. This may serve to show how many inert vegetables have acquired the reputation of curing the bite of the rattlesnake. I have ventured to apply a portion of the undiluted venom of a rattlesnake, recently thrown from its fang, to my tongue. I made this experiment in the presence of several gentlemen. But I do not think I shall venture to repeat the experiment. I did not find the venom insipid, as the abbé Fontana and his servant did the venom of the viper. It had, on the contrary, a peculiarly pungent taste, and left, for a considerable time, a pretty strong sense of heat upon my tongue and fauces. My observations have extended to every thing that might tend to illustrate the history of this wonderful reptile. I have found that its powers of digestion are very strong. Even the bony fabric of the animals which it devours is completely digested, or reduced to the state of a fluid mortar. This I have several times observed. Great, however, as is the faculty of digestion in this reptile, it is capable of living a very long time without any food, unless, perhaps, a small quantity of water. One of my rattlesnakes lived, without having ate one grain of any solid food, from the 28th of April to about the 9th of March following. It then died; but upon examining it I found it very fat. I have now two of these reptiles in my possession. One of them has eaten nothing since the middle of October last. The rattlesnake sheds its skin at least once every year. During the two summers that I have been making observations upon these animals, the old ones have shed their skins only once each year, viz. about the end of July. A young one, however, shed its skin twice in the course of the year, and that within five weeks. A rattle, or bell, is formed with each casting of the skin. It appears, therefore, that the full grown reptile generally acquires one bell annually. But I have elsewhere shown * that we cannot, with confidence, calculate the age of the animal from the number of its bells. I have, after much inquiry, ascertained the period at which these reptiles copulate. The Indians are my authorities. They assert that it is when the Indian corn (*zea mays*) is in flower; that is, about the 10th or 15th of July, between the latitudes of 39° and 41°.

I have had a number of very fine drawings made to represent the anatomical structure of the crotalus. These drawings it is my intention to have engraved, by some of the best of your artists, for my Anatomy and Physiology of the Rattlesnake; a work which I hope to publish in three or four years. It will contain every thing I have observed, or have been able to collect, relative to the structure, the functions, the manners, &c. of the crotalus horridus; together with observations on some other species of the same genus, and on various species of coluber, &c. all natives of the United States.

Perhaps no country of equal extent is richer in insects than the United States. In no country is it an object of more importance to attend to the history of these animals; for among them are enlisted some of the greatest enemies to the labours and industry of man. The heats of our cli-

* Supplement to a Memoir, &c.

mates

mates are extremely favourable to the generation and increase of insects: our colds are not sufficient to destroy them. We do not cultivate one important vegetable that is not exposed to the devastations of some highly injurious species of insect. Our wheat, you know, has for many years greatly suffered by a species of tipula, called by us the Hessian fly. This, at present, is much less formidable in its ravages than it has been. The curculio, or weavil, which it is highly probable we received from Europe, is still very destructive in the southern parts of the United States. A species of cimex, not, I think, described, assists other insects in destroying the finest of all the *cerealia*; I mean the zea mays, or Indian corn. Partly owing to the ravages of insects, it is to be feared that, in the course of a very few years, there will be a great scarcity of that fine fruit the peach in our country*. I could mention a hundred other insects of the most pernicious kinds. It is a melancholy fact, that the industry of my countrymen has been much more exerted in destroying the insects which infest one of the vilest of plants, I mean the tobacco, than in endeavouring to stop the ravages of any of those species which lay waste the most useful of our crops!

You must not, however, suppose, from what I have said, that the study of entomology is entirely neglected in the United States. On the contrary, this very important branch of natural history (for such, when it is properly cultivated, it unquestionably is,) has several votaries, some of them ardent votaries, in our country. The principal of these is the reverend Mr. Valentine Melscheimer, a clergyman, who employs a portion of his time in the cultivation of this science, which has always been deemed favourable to religion. This gentleman has discovered several hundred new species of American insects, a catalogue and description of which may, perhaps, be published. I must still regret, however, that it is chiefly the nomenclature of the North American insects that is attended to. Other more important parts of their natural history are too much neglected. Some progress, however, has already been made in discovering remedies against the ills which result from these animals. Moreover the useful properties of several other species have been discovered. Various species of the genus lytta of Fabricius inhabit our country. The *lytta vittata* of this en-

* The unripe fruit of the peach is greatly injured by a species of curculio; but the insects most pernicious to this tree are two lepidopterous insects, of the genus *zygæna* of Fabricius. These, while in the larva state, destroy the bark of the root.

tomologist

tomologist is called potatoe-fly, because it abounds upon the stems and leaves of the *solanum tuberosum*, or potatoe. It inhabits various other species of vegetables; such as the flax (*linum*), the beet (*beta*), black snake-root (*actæa racemosa*), &c. This insect has been found to be a very efficacious substitute for the cantharides of the shops, and is now employed, with this view, by many of our physicians.

I shall conclude this part of my letter by observing, that I still continue my inquiries into the natural history of the North American tribes. On this subject I have read before our Philosophical Society a discourse, which it is my intention to publish in a much more enlarged and perfect shape. Since the publication of my New Views, I have made great progress in collecting and comparing the languages of the American tribes with one another, and with the languages of Europe and Asia. Every step I take only serves to confirm me in my former opinion, that the (known) radical languages of North America are very few in number, and that all the Americans are of Asiatic origin. I have lately received from Mexico an inestimable treasure,—a collection of a number of vocabularies of the old Mexican dialects. Between these and the dialects of Asia I find great affinities, sufficient to convince me that the Mexican nations (contrary to the paradox of Camper) are not of European origin; much more than sufficient to excite in me the greatest surprise that the learned abbé Clarigero, after leisurely comparing the Mexican languages with those of the old world, should have found no affinity between them. The affinities, I repeat it, are very great.

Of all the branches of natural history, none, I think, is so little cultivated in the United States as mineralogy. This is the more remarkable, not merely by reason of the great utility of this branch of the science, but because its sister science, I mean chemistry, is ardently cultivated in different parts of the country, particularly in Philadelphia. Mineralogy, however, is not entirely neglected in the United States. It possesses, among other votaries, an ingenious cultivator in Dr. Adam Seybert, to whom we are indebted for the discovery of a mineral which is supposed to be corundum or adamantine spar. This is found to be abundant in the neighbourhood of Philadelphia. Mineralogy and chemistry, and I may add the other branches of natural history, have lately sustained a loss, not perhaps easily to be repaired, in the death of Mr. Thomas P. Smith, a young man of the most capacious mind, and of the warmest enthusiasm for the attainment and promotion of science.

 Mines

Mines of various kinds are very abundant in the United States. We are extremely rich in iron, copper, and lead. The iron mines of Pennsylvania are, perhaps, inexhaustible. Prodigious quantities of copper, nearly in a native state, have been discovered near Lake Superior. A bed of cinnabar has been discovered in Virginia; and several veins of plumbago have been detected in Pennsylvania and other parts of the Union. As to gold and silver, these, as yet, are principally to be found in the fields that are cultivated by the virtuous hand of agriculture. Our country is extremely rich in coal. This useful article has hitherto been found in the greatest abundance in the western parts of the United States, beyond the Alleghaney mountains. It is in this same part of the continent that the principal salt springs, and mines of sal-gem or rock-salt, are found. In a few years we shall, in all probability, be able to do without the salt of Europe. Hitherto very little sal-gem has been detected within the limits of the United States. I am not, indeed, certain that any has been detected. It must, however, abound at no great distance from the immense salt springs, which are now found in so many parts of our country. Of sal-gem, however, prodigious quantities have been discovered in the country that is watered by some of the branches of the Missouri, west of the Mississippi *.

I have lately returned from a three months tour (which had been principally undertaken for the recovery of my health) through the western parts of Virginia. I have visited many of the most interesting natural objects in that part of the United States, and have brought home a very considerable collection of vegetables. In travelling over some of the principal ranges of our mountains, particularly the Blue Ridge and the North Mountain, I have not observed that any of them are purely granitical. The stone composing these mountains is, indeed, various; but I think the predominant species is a petro-silex, of different degrees of hardness. Veins of schistus are sometimes found upon some parts of these mountains; but such veins are principally abundant about their bases. I have paid particular attention to the declivities of the mountains. These I find to be much less regular than Mr. Kirwan's ingenious observations (concerning mountains in general) would lead us

* Since writing the above, I have learned that very large quantities of sulphate of magnesia, or Epsom salt, have been discovered in some of the calcareous caves in the western parts of Virginia. It is also said that the borate of soda, or common borax, has been found in others of these caves. But this last report requires further confirmation.

to suppose. The general range of these American mountains is north-east and south-west. In some places the north-west and in others the south-east sides are the steepest. The valley which is comprehended between the North Mountains and the Blue Ridge is principally calcareous. Very generally the veins of limestone run parallel with the mountains, that is, north-east and south-west. When the nearest mountain varies from this direction, I observed that the adjacent strata in the valley do the same. These strata are sometimes perpendicular in their position, but never horizontal. It is remarkable, however, that most of the strata in the country west of the great Alleghaney mountains (which are to the west of the North Mountains) are arranged horizontally. This observation applies to the strata of limestone, schistus, freestone, and even to the stone coal and iron ore. This difference in the disposition of the strata east and west of the Alleghaney mountains is well entitled to the attention of naturalists. I have made it the subject of an express memoir.

It has long been conjectured that the calcareous valley which I have mentioned was once an arm of the ocean. This opinion has been maintained by Mr. Jefferson in his Notes on the State of Virginia. I entertain no doubt as to the antient covering of this valley by the sea, especially since my late researches have convinced me that the calcareous strata abound with marine exuviæ of various kinds. It is a circumstance very remarkable, that marine vestiges are much less abundant in the calcareous strata that are nearest to the present ocean, than in those which are at a much greater distance from it. Thus, although immense quarries of marble have been opened in the neighbourhood of Philadelphia, I have never yet been able to discover, though I have for several years been in search of them, the most distant semblance of a shell, or any thing of the kind, in hundreds of masses of this marble that I have examined. I do not, however, assert that such vestiges do not exist in the eastern Pennsylvania marble.

This very long letter was written principally for the amusement of my learned friend, who, following the footsteps of the immortal French Pliny, cannot but be interested in hearing any thing that relates to the progress of the great science of natural history. The letter is at your free disposal. I am, with very great respect, my dear sir,

Your friend and humble servant, &c.

To M. Lacepede. BENJAMIN SMITH BARTON.

Philadelphia, October 31, 1802.

XXXII. *On feeding Cattle with green Food; together with other ingenious and valuable Observations in Agriculture.* By Mr. EDWARD POWYS*.

I CONCEIVE the principal object respecting agriculture in the present state of this country, is to procure the greatest possible supply of the necessaries of life within the kingdom itself, and one principal means of doing this is, to raise the greatest produce from a given quantity of land.

To effect this, every encouragement should be given by land-owners to the cultivation of grain and turnips; because I look upon the produce of an acre of grain to be, to the produce of an acre of grass, in the proportion of at least fifteen to two, in furnishing the necessaries of life. I suppose the grain made into bread, and the grass digested by a feeding beast, and changed into an increase of weight.

One great means of increasing the growth of grain and turnips, I think, would be to encourage the farmer to make as much manure as possible. This would be effected by allowing him to sell all his wheat and rye straw, with the restriction of laying out the whole price in manure; and by gentlemen, who have land in their hands, trying the experiment of keeping their cattle and horses in the house upon green food great part of the summer.

For these last six years I have sold all the wheat straw I did not want for thatching and the beds of certain kinds of horses, and can assure you that the same farm has produced for some years back one-third more grain, and keeps double the live stock it did six years ago.

As a proof that what I say of keeping cattle in the house in summer upon green food is not matter of theory only, but of practice, I shall mention my own experience.

For these last five years I have kept eight or ten waggon horses in the stable upon clover, cut and carried for them once a day; the small waste that they made was thrown into a low cratch (or receptacle, with staves on each side) for my pigs, which have generally been from 25 to 40. My horses and pigs, thus fed, have eaten, between the beginning of May and corn harvest, from $2\frac{1}{4}$ to $3\frac{1}{2}$ acres, according to the goodness of the clover. My horses have been, by this means, in much better condition than if turned into a field; there has been a saving of at least eight or ten acres of clover for other stock; a great deal of the richest

* From *General View of the Agriculture of Shropshire.*

manure

manure has been made (much more, and richer, than in the same time in winter), and the additional daily expense has been, one man less than half his time, in cutting, raking, and carrying with a horse and cart, one load each day.

Experiment.

Dr.	£. s. d.	*Cr.*	£. s. d.
One man half a day for 13 weeks, at 4s. per week	2 12 0	Eight acres of clover saved by this experiment, at 50s. per acre -	20 0 0
One horse 13 weeks, at 6s.	3 18 0	Manure, at least, -	10 10 0
	6 10 0		
Profit - - - -	24 0 0		
	30 10 0		30 10 0

The first year I tried the experiment the manure made was estimated by a good farmer at 20l.; but I wish to make allowance for the value of the straw, and the manure that would have been made by the horses standing in the stable the usual hours in summer.

I must endeavour to remove an objection that may perhaps be made to this experiment, by observing, that I cannot think land injured any more by the green food being cut by the scythe, than by cattle or horses; and as to the dung that is dropped in summer, it breeds flies, and does more harm than good. I have ever thought land exhausted infinitely more by its produce being suffered to ripen and seed, than by its being cut in a green state. The advantage I had derived from this experiment, induced me last summer to try whether cattle might not be treated in the same way.

I began with putting into stalls 19; I afterwards increased my stock fed in this manner to 50, consisting of horses, feeding cattle, milking cows and colts, besides a large quantity of pigs.

The horses, as usual, answered well.

The feeding cattle came on much faster than I ever saw them in summer. The milking cows fed very much, and milked very well. The colts did well, and lived chiefly upon the refuse of the cattle. The pigs, as usual, ate the refuse of the horses.

The quantity of land run over with the scythe for this purpose was:

Fourteen acres of trefoil, very moderate, on account of the clover root having died in winter.

Two acres of vetches, very moderate, on account of the severe winter.

Five acres of very good grass.

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The cattle were turned out late at night for about six or seven hours.

The trefoil caused some trouble, on account of the cattle sometimes swelling, but brought them on very well, though they throve best upon the winter vetch or tares, and upon the grass. The daily expense was one old man of more than 70, to feed and clean them, another young man to cut, rake, and carry the food with a single horse cart.

If this stock had been turned out I should suppose they would have run over at least 60 acres, if the crop had been good, and much more, if the indifferent trefoil is considered.

Experiment.

Dr.	£. s. d.	Cr.	£. s. d.
Two men 13 weeks, at 14s.	9 2 0	Twenty-nine acres saved,	
One horse ditto - 7s.	4 11 0	at 50s. - -	97 10 0
	13 13 0		
Profit - - -	83 17 0		
	97 10 0		97 10 0

Any person that intends to practise this method should begin to cut his green food so early in spring that he may be able again to mow the same ground from hay to corn harvest.

I have before observed, that I never saw cattle in summer come on so fast. I speak this, not only from my own observation, but from that also of several farmers and butchers, who came through curiosity or business frequently to visit them. The most feeding green food is winter vetches; and the most advantageous mode of cultivating them, I think, is to plough up a clean stubble (that is intended for turnips), manure it, and sow it with vetches soon after corn harvest. When the vetches are all cut in May and June, or rather in the latter month, the field may be ploughed and sown with turnips for a winter crop.

From corn harvest till September 22, my cattle were all out in the fields at grass. I then took up thirty into stalls, and fed them with turnips which had been sown early in May, and which had arrived at a very good size. My first field of turnips has been carried off, ploughed and sown with wheat, which has been above the ground some time, and looks very promising.

I have practised this scheme of sowing turnips in May, carrying them off before the beginning or end of the following November, and then sowing the piece with wheat, for these last three years. And I have found this wheat much

much more productive than any sown after any other crop or fallow. I am speaking of dry sown land.

One year I got up all the turnips of a field, topped and butted them (throwing the tops and butts in heaps by themselves), carried the tops immediately as they were cut to a bare stubble for my cattle and sheep, and laid the butts up in large heaps either under cover or in my stack-yard, with straw over them. Where there was no straw in layers between them, they kept for two or three months; some that had layers of straw every foot or half yard perpendicular, soon began to decay near the straw, which was made to heat by the moisture from the turnips.

From these experiments upon turnips, and from observing that dry land of my own, though it produced crops of grain or turnips for many years together, with the change of clover (mown twice in the same year) only once in five years, did not lose any of its power, I have conceived that much more grain might be produced upon well cultivated farms. Wet land that is well cultivated might bear, in regular succession, crops of turnips, wheat, and barley or oats. Dry sound land may also bear the same succession when an early crop of turnips is wanted; and when turnips are wanted to stand the winter, a succession of turnips, barley, and wheat.

I think it is much more advantageous to carry all the turnips to cattle in stalls (except a very few left for sheep) than to eat them on the land, because they furnish much more food and manure. I am aware that many gentlemen of landed property will object to this constant tillage: in answer to which I shall only observe, that it has been my opinion and practice never to have any grass land that is not worth 40s. an acre; never to plough my grass land, but to till the rest constantly, with the intermission now and then of turnips and clover, the latter only for one year.

The farm I have above alluded to is about 240 acres, of which I have in grass land about 90 acres; in tillage for grain and turnips about 120 acres. The rest is generally clover, unless I have a single fallow for wheat upon a field of wet land.

I repeat it once more, that the interests of the public, of the landlord, and tenant, (for I know of no distinction when many years are taken into consideration) are united in the greatest produce of the necessaries of life; and that if arable land is kept clean and full of manure, it receives no injury from producing the greatest quantity of grain. The increased produce of land benefits the public in too obvious a manner to enlarge upon. It benefits the landlord, by his being able, at the expiration of certain fair intervals, to raise the rent of his farm; and the tenant or occupier, by getting more profit from a given quantity of land, and with nearly a given capital.

I have recommended turnips once in three years, because I think land requires cleaning once in that time, and because it is thus effected without losing the benefit of a crop in any year.

Much has been lately said about the superior advantage of cattle over horses in farmers' teams. I think some horses must be kept for the farmer to take his grain to market, and to carry his coal and lime. If he is so near a large town that he can draw at least two load of dung in a day, he will also want them for that purpose. Other team-work may very well be done by cattle. But I think cows are much more useful and beneficial than oxen, and that it would be an advantage to the kingdom if few or no oxen were reared. The uses of cattle are to work, milk, and feed. I have seen barren cows work as well as oxen; they require less keep, and walk faster. Oxen are of no use to the dairy, and they will not feed so fast as cows.

When first I commenced farmer I followed the example of my predecessor, in feeding chiefly oxen; but I soon found that cows fed much faster and on less meat, and for some years past have carefully avoided having any oxen in my stalls.

Meadows.

It should be considered as a great object of every landlord, or his steward, to procure watered or flooded meadows.

The best means of doing this is, to place the farm-yard on such an eminence of the farm that a stream can be procured to run through it, and afterwards over the greatest quantity of meadow land.

Common meadows ought to be well manured once in three years, and will then produce one ton and a half of hay per acre, and a pasture from the middle of September to Christmas.

Good watered meadows will bear to be grazed from the beginning or middle of August till May following, and will, between that time and hay harvest, produce one-fourth of hay more than the other.

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The difference of the profit of watered meadows over common, I think, is annually as underneath:

One-third of 5l. (the expense of manuring an acre of land) - - -	1	13	4
One-half ton of hay additional -	1	5	0
Difference of the value of grazing -	0	11	8
	£. 3	10	0

But besides the produce and profit, there are two other very great advantages in watered meadows. The one, saving manure for arable land, the other keeping the pastures free from stock the beginning of spring.

I have hitherto only mentioned a stream that runs through a farm-yard, but I have frequently observed very great advantages derived from nothing but clear spring water being turned over grass land.

If a farmer has a greater command of water than he wants for his meadows and pasture land, he may occasionally till some of them for two or three years, and they will produce great crops without manure. I saw this practised with great success, this last summer, by the late Mr. Bakewell, of Dishley, Leicestershire.

Size of Farms.

Much has been lately said upon the size of farms, from the high price of grain being supposed to arise from the opulence of the farmer, and his being able to keep back his grain from the market. I might combat this assertion by the well known fact, that at the harvest of 1794 there was not a fortnight's consumption of wheat in the kingdom, and yet the price was moderate. I might also add, that there never was so much wheat brought to market before Christmas as has been for these last two years, and that it has only been when wheat was plentiful that any of the stock remained in stacks at harvest; but I think the high price is known by sensible thinking people to arise from other causes. I shall therefore proceed to observe, that farms of from 200l. to 800l. per annum, and upwards, are much more beneficial to the public, the landlord, and tenant, than farms of from 50l. to 100l. per annum. The public are benefited by fewer people and horses being kept upon one farm of 300l. per annum, than upon six of 50l. each to do the same work, and therefore by a greater produce being left, after the supply of the families, for the consumption of the kingdom at large.

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The landlord is benefited by having fewer buildings to erect and repair, and by having more opulent tenants. The benefit of a large farm to the occupier I need not enlarge upon. The misery of the small farmer, under 50l. per annum, is extreme. He has not constant employment for himself and family (if at all large) upon his farm; he is in general above working day labour, is unable to exert himself and improve his small tract of land, and sits by the fire-side with his family great part of the winter, lamenting that his farm and his capital are not larger, and brooding nothing but discontent and indolence.

But while I am making these observations upon the advantage of large over small farms, let me notice the great benefit and comfort that the common workman, in any line, derives from sufficient grass land being attached to his dwelling to keep a cow in summer and winter. The landlord will also receive benefit, as well as self-satisfaction, from being the cause of the plenty that the produce of a cow makes in a large and poor family.

I can from experience assert, that the cottager can afford to give his landlord one-third, if not one-half, more for that small quantity of land than a farmer. The value of the cow is generally more than one year's rent, and the addition of a small cow-house is a trifling expense.

I cannot help recommending this the more strongly, because I know well, from experience, the astonishing comfort and advantage that a poor family receives from the produce of its cow, and that it is also for the interest as well as inward satisfaction of the landlord.

XXXIII. *New Process for decomposing Sulphate of Barytes in order to prepare the Muriate of that Earth; with a Method of preparing the Muriate. By M.* Goettling*.

Muriate of barytes is now so generally used, that every improvement in the mode of preparing it must be favourably received. M. Goettling's new method is as follows:

The decomposition of sulphate of barytes by means of charcoal requires a strong fire continued a long time, and never succeeds completely. This is owing, on the one hand, to the strongly oxygenated quality of the acidifying principle in the sulphuric acid, so that in its translation to the

* From *Taschen-buch fur Scheidkunstler.*

the charcoal it gives out but little caloric; and on the other hand, to the difficulty of imparting a certain degree of heat to a mixture into which a large quantity of a body that is so bad a conductor of heat as charcoal enters. To remedy the first of these defects, I had already proposed to increase the proportion of charcoal a little, and to incorporate with the mixture of charcoal and sulphate of barytes a twentieth of nitrate of potash. To remedy the second, Mr. Goettling advises to add muriate of soda to the mixture, which serves at the same time as a conductor of heat and a flux. The following is his method:

Four parts of native sulphate of barytes in fine powder are to be mixed with one part of muriate of soda and half a part of charcoal powder. This mixture is to be pressed hard into a Hessian crucible, and exposed for an hour and half to a red heat in a good wind-furnace. After it has grown cold, the mass is to be reduced to a coarse powder, and boiled for a moment with sixteen parts of water. The liquor is then to be filtered, and kept in well stopped bottles.

The time of exposure to heat may be shortened to one half, if the quantity of muriate of soda be doubled, and the matter occasionally stirred. In this case, too, double the quantity of water should be used to lixiviate the mass.

To prepare muriate of barytes with this lixivium of sulphuret of barytes, which at the same time holds in solution muriate of soda, muriatic acid is to be added in separate portions till sulphurated hydrogen gas is no longer extricated. The liquor is then to be filtered, a little hot water is to be poured on the residuum, and the liquor is to be evaporated to a pellicle. The lixivium being then filtered afresh, is to be set to crystallize; the muriate of soda, which is much more soluble in water than the muriate of barytes, and not more soluble with heat than without, is not deposited by cooling, and the muriate of barytes crystallizes alone.

The remaining lixivium is to be evaporated and set to crystallize again, and this is to be repeated till no more crystals of muriate of barytes are formed.

The barytic salt thus obtained, if care be taken not to employ an excess of muriatic acid, is perfectly white, on account of the hydro-sulphuret, by which the iron and other metallic substances are precipitated. To be more certain that it contains no muriate of soda, the different products of the crystallization should be mixed together, dissolved, and re-crystallized.

XXXIV. *Re-*

XXXIV. *Report on the Means of measuring the initial Velocity of Projectiles thrown from Cannon, both in an inclined and a horizontal Direction. Read in the Physical and Mathematical Class of the French National Institute in the Month of December* 1804 *.

The class charged Messrs. Bossut, Monge, and myself, to give in a report on the means for measuring the initial velocity of projectiles thrown from cannon, proposed by colonel Grobert, who constructed an apparatus of such dimensions that we could employ it for our preliminary experiments. This apparatus was as follows:

A horizontal revolving axis, of about 34 decimetres in length, has at each of its extremities a disk or circle of pasteboard placed perpendicular to the axis, the centres of which are in the same axis, to which it is fastened in such a manner that the whole system can turn rapidly without the respective positions of its different parts being deranged.

The rotary motion is communicated to the axis and to the disks by means of a weight suspended at the extremity of a rope, which, after passing over a pulley raised ten or twelve yards above the ground, rolls itself round the arbor of a wheel and axle fixed at the same level as the disks. An endless chain, which passes round on the one side the wheel of the axle, and on the other a pulley fixed on the axis of the disks, transmit to that axis the motion which the weight communicates to the wheel and axle during its fall.

This apparatus, as is seen, has the merit of being simple; and without entering into further details it may be readily conceived how it can be employed for measuring horizontal velocities. Let us suppose that the two disks are at rest, and that a ball traverses them in a direction parallel to the axis or the line passing through their centres: it is manifest that this axis will be in the same plane with the holes made in the disks; but if the disks turn around their axis while the ball passes from the one to the other, the plane containing the axis of rotation and the first hole will not coincide with the second hole; and if a second plane be made to pass through the second hole and the axis, the angle formed by these two planes will be the measure of the arc described by any point of the disks, while the ball or bullet passes over the interval by which they are separated.

* From the *Journal des Mines*, Floreal, an 12, no. 92.

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The question then is, to measure the velocity of the bullet.

1st, To impress an angular, uniform, and known velocity to the system of the axis and the two disks.

2d, To measure the arc comprehended between the two planes passing through the axis, and each of the holes or passages which the bullet has opened through the disks.

In the experiments which were made, the motion became sensibly uniform when the weight had arrived nearly at the half of the vertical space which it traversed: this was ascertained by measuring at two periods the time elapsed during the third and fourth quarters of the fall, and then comparing these times with the corresponding spaces passed over. For these measures we employed two excellent time-pieces that beat seconds; one by Louis Berthoud, and the other by Breguet.

In almost the whole of the experiments we substituted for the measure of the vertical space passed over by the weight, that of the number of turns and fractions of turns made by the arbor of the wheel and axle during a given number of seconds, which in every respect was much more precise and convenient.

Then, to measure the arc passed over by the disks while the ball went from the one to the other, we placed before each of these disks a screen or fixed piece of pasteboard, which was at a very small distance from it; so that the ball during its passage first traversed the first screen, then the first disk; then the second screen, and afterwards the second disk. When the piece was discharged the hole of the first disk was brought opposite to that of the first screen, and these two holes were in the same straight line with that made in the second screen; a wire, directed horizontally through the centre of the latter hole, pierced the second disk; and the arc, having its centre in the axis of rotation comprehended between the extremity of that wire and the centre of the hole made by the ball in the second disk, gave the measure of the angle described by the system of the two disks, while the ball had passed over the length of the axis.

It may be readily seen that the fixed screens, which give the absolute direction of the ball in space, furnish the means of making an allowance for the want of parallelism, if there be any between that direction and the axis of rotation of the disks.

The cannon employed for throwing the projectile was placed horizontally and parallel to the arbor of the disks, at a sufficient a sufficient distance from the first disk, that the motion given to the air by the explosion of the powder should not hurt the motion of that disk.

An apparatus exactly similar to the above was established by colonel Grobert in a place belonging to the School of Bridges and Causeways, where, with the commissioners, he made, a few years ago, a great number of experiments, at which were present several officers of the engineers and artillery, among whom were general Marescot and the senator La Martilliere.

This apparatus was far from having those dimensions and that perfection of which it was susceptible, and which the author proposes to give it. The object of the commissioners, therefore, was not so much to furnish results useful to the artillery, as to ascertain what advantage might be derived from it when constructed as it ought to be.

It is proper, before we speak of our experiments, that we should resolve a difficulty which naturally occurs to all men who are in the least acquainted with this subject, and which arises from the enormous difference supposed to exist between the velocity of a projectile thrown from a cannon and the angular velocity that may be given to the disks. It is concluded, indeed, from the experiments already known in regard to artillery, that the time employed by the ball or bullet in passing over the distance of three or four metres between the disks must be smaller than the hundredth part of a second, and it can hardly be conceived that during so short a time the disks can describe a sensible arc.

The solution of this difficulty is as follows:—When the motion has become uniform, the wheel and axis make generally 0·833 turns per second, and the axis of the disks 7·875 turns, which thus made 6·56 turns per second, correspond to each turn of that wheel: therefore a point placed on one disk at the distance of a metre from the axis passed over about 41 metres per second, which gives for 1-100dth second 41 centimetres, a length more than sufficient to furnish very exact measures.

The experiments were made with a common infantry musket and a horse musketoon, the barrels of which were resspectively 1·137 metre and 0·765 of interior length. They were charged with cartridges furnished from the arsenal. The first series of experiments having been more regular than was presumed, they were encouraged to employ more precision in the charges and more care in the proofs. The balls were weighed exactly: their mean weight was

24·7 grammes,

24·7 grammes, and each of them was projected with half its weight of powder.

The following is the formula employed to calculate the velocity of the balls.

The semi-circumference, which has unity for radius - - - - - $= \pi = 3 \cdot 141$

The ratio between the respective numbers of the turns made at the same time by the wheel of the axle and the pulley of the axis of the disks - - - $= k$

The time employed by the wheel of the axle to make n number of turns - $= t$

The distance of the hole made by the ball in the second disk from the axis of the disks $= r$

The arc passed over by that hole, while the disk goes from the one to the other - $= a$

The distance between the two disks - $= b$

Velocity of the ball between the disks $= V$

We have then the following equation:

$$V = \frac{2 \pi n}{k t} \cdot \frac{r}{a} b.$$

As it may be of some use to add to this formula a table of some experiments, we shall give the six following, made with the musketoon.

Number of the Experiments	n	t	a	V
		Seconds.	Metres.	Metres.
1	8	10	0·3510	402·3
2	8	10	0·3800	371·7
3	8	10	0·368	362·5
4	15	22	0·296	384·1
5	15	22	0·264	430·7
6	10	18	0·268	345·7
7	15	16	0·392	398·8
8	15	16	0·392	398.8
9	15	16	0·416	375·8
10	15	16	0·360	434·3

Mean velocity = 390·45.

Constant value of $k = \frac{1}{7 \cdot 875}$.

All the values of a are referred to that of r = 1 metre.

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The mean velocity deduced from the ten preceding experiments is 390·47 metres, nearly the same which results from the whole of the experiments. The mean value found for the velocity per second of balls thrown from the infantry musket was 428 metres, the ratio of which to the preceding is as 11 to 10. These experiments seem to indicate that the infantry musket might be shortened without much lessening its range; but besides that the commissioners had no intention of deducing from these first trials any conclusions applicable to artillery, it is proper to observe, that some military considerations, besides those of range, are in favour of length in an infantry musket.

If we wished to select from the accurate experiments hitherto published in regard to the projectiles of artillery some proper for being nearly an object of comparison with those mentioned above, we might take from the work of doctor Hutton those which he made with a cannon of the smallest dimensions, and which he marks No. 1, the bore of it being 7 decimetres in length and about 51 millimetres in diameter. The general results consigned to a table formed from the whole of the experiments, give for the case, when the weight of the charge of powder is, as above, one-half that of the bullet, an initial velocity of 435 metres per second, which differs very little from the velocity found with the infantry musket. Dr. Hutton's pieces numbered 2, 3, and 4, and which were longer, gave mean velocities more considerable.

The commissioners made some trials with half charges, that is to say, expelled the ball with the fourth part of its weight of powder: the mean value of the velocity of the ball thus projected was found for the infantry musket to be 254 metres, and for the musketoon 252. These two velocities are sensibly equal, and exceed the halves 214 and 195 of those given by whole charges.

There is reason to presume that these circumstances depend chiefly on the complete inflammation of the powder which takes place when there is only half a charge.

In the last place, the commissioners, to multiply their trials on the application of colonel Grobert's apparatus to horizontal firing, wished to obtain some data in regard to the resistance offered by the air to the motion of the ball, the diameter of which was 15 or 16 millimetres. The mouth of the barrel, which was first at the distance of 2·35 metres from the first fixed screen, was removed to that of 18·44 metres, by means of which its distance from the first fixed screen was 20·79 metres. In this position the velocity

 with

with which the ball thrown from the infantry musket passed over the interval between the one disk and the other, was found at a mean value to be 345 metres per second instead of 428. The diminution is in the ratio of 42 to 34. The experiments of the last kind are few in number, and we shall deduce from them no conclusion: we shall not say any thing either of some trials which were made to determine the loss of velocity which the ball experiences in traversing the two first leaves of the pasteboard; as the principal object of our trials was not, as already said, to employ this first apparatus for the advancement of the science of artillery, but to acquire some idea of the advantage which this science might derive from it when constructed with that perfection of which it is susceptible.

One of the most important changes which the author proposes is, to increase the diameter of the disks and the length of their axis in such a manner as to render them proper for determining the initial velocities of cannon balls of different calibres. It would be difficult to assign previously, and without preliminary trials, the term of this augmentation compatible with the possibility and exactness of experiments; but there is no doubt that the apparatus we used might be made of much greater dimensions, and such as might render it fit to be employed for trials with cannon.

Colonel Grobert proposes another change, which derives its principal utility from that already mentioned. The object of it is to afford the means of traversing the disks by throwing balls in different directions, from the horizontal to that which forms half a right angle with the vertical line. To accomplish this end he has invented the following mechanism, which is simple and easily constructed:—He does not make the disks revolve around a common axis, but gives to each of them a particular horizontal axis, to which a pulley is affixed. The axis of the windlass has two equal wheels corresponding to two pulleys, and two endless chains, each of which passes round a wheel and a pulley. The rotary motion which the windlass receives from the descending weight is thus communicated to the disks, and the dimensions of the wheels and pulleys must be well regulated to make the disks turn together and to perform exactly the same number of revolutions in the same period. This condition being fulfilled, the supporter of one of the disks (that which is furthest from the cannon) is disposed in such a manner that it can rise vertically and fix itself at different heights, for each of which there are added some links to the chain corresponding to that disk, in order to

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give it sufficient length; and by lowering the cannon it is thus possible to traverse the disks in different directions inclined to the horizon. It may be remarked that the projection of the surface of the disks on the plane perpendicular to the line of firing decreases more and more in proportion as that line inclines; but this is a very small inconvenience, as the greatest decrease which takes place in the ratio of about 7 to 5, leaves still a sufficient field for pointing with all the precision requisite.

It will not, perhaps, be so easy as might at first be believed to adjust the wheels, the pulleys, and the engagement, in such a manner as that the two disks may turn exactly together; we are, however, of opinion that there is nothing in this part of the apparatus which may not be made by any careful ingenious workman. Besides, if the machine be strong, and the parts well executed, there are means of avoiding the errors resulting from the want of coincidence which might exist in the motion of the disks. These means consist in counting the turns of the wheels made from the moment when the piece is fired till that when the machine stops, and causing these wheels to make the same number of turns in a contrary direction, and in such a manner that the wheels shall be brought to the same respective positions in which they were when the piece was fired.

We shall suppress several details relating both to the inclined range and to different pieces of mechanism invented by colonel Grobert for supplying the attention and hand of man in the experiments. By means of these pieces of mechanism the moving power, when it arrives at that point of its course where its motion becomes uniform, rests on two triggers, one of which causes a pendulum that swings seconds to oscillate in order to count the time, while the other establishes a communication between the motion of the windlass and that of a system of toothed wheels and pinions, furnished with an index to count the revolutions of the wheels. The weight, when it arrives at the lower extremity of its course, presses on other triggers which serve to set fire to the cannon, and to stop the counter of time and that of the number of turns. These different means may be useful, but it is sometimes attended with inconvenience to add too many parts to a piece of mechanism, and to make it so complex that it becomes liable to be deranged.

As the above explanation will be sufficient to give an idea of the utility that may be derived from the apparatus of co-

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lonel Grobert, we shall now add a short account of the methods hitherto employed in researches of the same kind.

It is only about sixty years ago that mathematicians began to apply to the theory of projectiles. Benjamin Robins appears to us to have opened the way, or at least to have published the first experiments worthy the attention of philosophers, and to have employed, for determining the initial velocity of musket bullets, a pendulum, against which he threw his projectiles, and the sought-for velocity was deduced from the amplitude of the oscillation: the same mathematician, when about to make particular experiments on gunpowder, deduced his results from the recoil of a small cannon attached to the lower part of the same pendulum *.

The chevalier d'Arcy, of the Academy of Sciences, about eight or ten years after Robins's work appeared, published, in the Memoirs of the Academy for 1751, a paper on the theory of artillery, containing a series of experiments made with great skill and care, and for which he employed, nearly in the same circumstances, two pendulums; against one of these he fired the ball, while the other, from which the small cannon was suspended, served to measure the recoil. In this manner he made those important experiments related in *Essais d'une Théorie de l'Artillerie,* published by the same author in 1760.

Fifteen years after, Dr. Hutton, of the Royal Academy, Woolwich, made new experiments with the pendulum and projectiles, much heavier than those employed by Robins. An account of them may be seen in the Philosophical Transactions for 1778.

About the same year count Rumford resumed and improved the method of making such experiments by means of a pendulum. He discovered a very simple way of suspending the cannon, in such a manner that the recoil took place without the axis ceasing to be horizontal. Dr. Hutton bestows great praise on these experiments, which are detailed in the Philosophical Transactions for 1781, and which have since been printed with considerable additions in a collection entitled Philosophical Papers, &c. London 1802.

In the last place, Dr. Hutton was employed during the years 1783, 1784, 1785, and 1786, on a numerous series of experiments, made with great care and expense, in regard to both kinds of pendulum. The collection of his memoirs, inserted in the Philosophical Transactions, and lately trans-

* See his Principles of Gunnery.

lated into French by colonel Villantroys, may be considered as the most complete and most instructive treatise on balistic experiments that ever appeared.

The apparatus proposed by colonel Grobert, as may be seen, is very different from those employed by the authors above mentioned; and whatever merit may be due to experiments made with pendulums, it must no doubt be allowed that it may be of some utility to make new ones by a very ingenious method, in which simplicity and œconomy are united, and which leads to the proposed end in the most direct manner, as the velocities sought for are deduced merely from the observation of the time employed by a moving body to make a certain number of revolutions around a fixed axis: an observation free from those dynamic calculations which are required by the method of Robins.

We have said nothing of the labours of Antoni in regard to the object of this report, and yet we cannot omit mentioning a machine which he speaks of in his *Essai sur la Poudre,* a French translation of which was published by M. de Flavigni in 1773. This machine, which Antoni says was invented by a mechanician named Mathey, consists of a horizontal circle supported by its centre on the upper extremity of a vertical axis, and serving as a base to a hollow cylinder of paper. A rotary motion is given to this cylinder by means of a weight affixed to a cord which passes over a pulley, and the projectile, thrown in a horizontal direction, when the angular velocity has become constant in the vertical plane of that axis, traverses the paper cylinder in two points. The distance of the second point from the diameter passing through the first, serves to measure the arc described by the system during the passage of the projectile in the interior of the hollow cylinder.

It is incontestable, after this description, that the fundamental idea of the process, by which a circular is compared with a rectilinear motion, belongs to Mathey; but without entering into any detail on the inconveniences of this machine, which no doubt have been the cause of its being little used, we shall content ourselves with observing that the apparatus of colonel Grobert differs from it essentially.

1st, By the horizontal position of its axis of rotation. The result of this difference is, that the axis can never be met by the projectile; which makes it easy to ascertain the solidity and regularity of the position and motion of the disks.

2d, The projectile does not traverse a cylindric surface, but two

two vertical planes, the extent and distance of which may be considerable, and which therefore may give very accurate measures.

3d, By the advantage it affords, not to be found in any other kind of apparatus yet known, of measuring the velocity of bullets of different sizes thrown in directions inclined to the horizon.

It remains for us to give an account of some experiments made to ascertain that the ball underwent no sensible deviation in traversing the disks. It is manifest from the first principles of dynamics, that the moment when the ball is in the plane of one disk in motion, it receives perpendicularly to its direction an impulse which, according to certain mathematical hypotheses, would give it parallel to the plane of the disk a velocity almost equal to that of the point where it meets (the mass of the ball being very small in regard to those of the different moving parts of the machine); and then the velocity of the ball, calculated according to the formula before given, would be infinite. The effective phænomena differ considerably from those deduced from similar hypotheses, considering the compressibility of the disks, their little hardness, and the prodigious rapidity with which they are penetrated* (the duration of the passage of the semi-diameter through the disk not being the 40,000dth part of a second); but it is no less important to determine exactly the influence which they have on the results. One of the commissioners is employed in analysing a dynamic problem, from which this determination may be concluded *a priori*; but as such a conclusion would not rest on physical data sufficiently certain, he preferred verifying by experiment whether the deviation was appreciable or not. For this purpose he placed three fixed screens at equal distances from each other; the second and third being placed respectively before the first and the second moveable disks.

* It is here proper to give the reasoning on which these assertions are founded. Every body in nature being more or less compressible, the state of final motion, resulting from the action of two bodies on each other, is not acquired even at the moment of contact, but after a fixed term, which is very short; and bodies during their contact pass through all the intermediate states of motion between initial and final. From these incontestable facts, if one of the bodies escapes the action of the other before the moment when by the natural consequence of the shock they would have ceased to press each other mutually, the state of motion at which that body will really arrive in that case will differ so much less from its initial state, and so much more from that in which it would have been had the shock been *consummated*, as the contact has been of less duration, and this duration may be so short that the initial state is not sensibly modified. This is the case of the experiments mentioned in the text.

 It

It may be readily conceived that in the hypothesis of deviation the hole made by the ball in the third fixed screen ought not to be in the same vertical plane with those made in the first and second screens; and thus this deviation becomes easy to be ascertained and measured.

Several balls were fired, the apparatus being arranged as here described. Each time a plumb-line was placed before the centre of the hole made in the first screen; and taking in a line this thread and the centre of the hole made in the second screen, it was very easy to see whether the centre of the third hole was in the vertical plane containing the other two centres. These observations were made with care and precision, and yet it was not possible to perceive the deviation estimable in the direction of the line passing through the centres of the three holes: thus the motion of the ball through the moveable disks is sensibly the same as if these disks were at rest. We are of opinion, however, that it will be useful to employ always three screens, disposed as above, in the experiments made in future on this subject: by these means we may either ascertain whether there be any deviation, or discover whether it be sensible; and there can be no doubt that it will be so, either in the case when small velocities are communicated to the projectile, or, in general, in those where the ratio between that velocity and the resistance which the bullet experiences in traversing the disks shall exceed certain limits.

It is proper to add, that the distance of the cannon from the furthest distant screen was about twelve metres, and that there was no reason to apprehend the inflections observed by Robins in distances of about a hundred metres, which according to him render rigorously the trajectory a curve of a double curvature.

What we have said on the insensible effect which the action of the disk has upon the ball to make it deviate, proves that the re-action of the ball on the disk cannot diminish its velocity by a sensible quantity: this conclusion might be deduced, besides, from other facts, relative both to the mechanism of the apparatus and to the data furnished by the experiments, and particularly by the measure of time before and after the discharge of the piece:—but it is needless to enter into further details.

To conclude: we are of opinion that the means to measure the initial velocity of projectiles thrown by fire-arms in directions both horizontal and inclined, proposed by colonel Grobert, and conformable to the summary description found in the above report, merit the approbation of the class.

class. We shall add, that a series of experiments made with an apparatus of larger dimensions, and executed with more care than that employed by the commissioners, might furnish results useful to artillery.

XXXV. *On the Buds and Ramifications of Plants; the Birth of these Organs, and the organic Relation between the Trunk and the Branches: in a Letter from* G. L. Koeler, *M. D. Professor of Botany and the Materia Medica in the Provisional School of Medicine at Mentz, to M.* Ventenat, *Member of the French National Institute* *.

MY DEAR FRIEND,

Nature seems to have thrown an impenetrable veil over the development of vegetable parts. In vain have botanists made efforts to surprise her; in vain do they watch her; she eludes their efforts, and laughs at their indefatigable patience. If it has been impossible, however, to accomplish by observations this end, which was the object of so much care and labour, their researches have still conducted them to a multitude of other valuable discoveries, which have assisted us in the study of the structure and interior œconomy of plants, and have shown to us the causes of several phænomena before considered as inexplicable.

The opinions of those who have endeavoured to discover in what manner nature develops one vegetable organ from another may be ranged into two classes.

Some have imagined that it is the *pith*, which makes its way through even the hardest wood to produce the ramifications of plants, and that it lengthens itself still to form the most essential parts of the vegetable body.

Others, and those the most recent, rejecting this opinion, have ascribed to the bark and *cortical strata* what their predecessors gave as the product of the *pith*. They have thought also that the increase in length and thickness depends on the same organs.

Placed between these two opinions, and though each of them was supported by great names, yet being unwilling *jurare in verba magistri*, I resolved to examine both without prejudice, and to give an opinion supported by my own observations. I did so; and discovered that Linnæus and Hales, who had maintained the former opinion, were

* From the *Journal de Physique*, Floreal, an 13.

 not

not far from the truth ; that their error was very excusable, for they might easily take for pith the *reproducing organ*, which will be mentioned in the course of this letter, and the herbaceous substance of which has a resemblance to that of this spongy organ. It may be said further in their favour, that they had not before them the experiments of Desfontaines, Coulomb, Hedwig, Mirbel, and Medicus, and so many other philosophers who have thrown light on a great number of points hitherto inexplicable in regard to the interior œconomy of vegetables.

The observations which this study gave me an opportunity of making, have conducted me to results so unexpected, that I did not think proper at first to trust to my own eyes. Convinced without being persuaded, and distrusting my own senses, especially in an experiment the results of which were in open contradiction to what we are taught by the greatest masters, I resolved to submit to you these observations, and to give them at the same time publicity, in order to call the attention of the most enlightened botanists to the new phænomenon which I think I have perceived. I must, however, say, that it is the force of conviction, the desire of instruction, and not a vain spirit of controversy so unworthy of a real naturalist, which induce me to think differently from several celebrated men superior to envy, and to whom I readily pay the tribute of my gratitude for the benefit I have received from their works.

Before I give my observations on the origin of buds, I shall first rectify some ideas generally adopted in regard to these organs.

" The name of *bud* is given in botany to small bodies more or less round or ovoid, and covered with scales hollowed out like a spoon, or with a down more or less thick. These organs are formed gradually in the spring season in the eyes of the leaves of most trees, shrubs, and plants of the dicotyledons, especially in climates where the winters are pretty severe. They contain and conceal the rudiments destined to be developed the following year into branches, leaves, and flowers. They have received from nature the faculty of resisting cold and humidity: several of them may be preserved, like a great many seeds, during one or more years, by remaining in a state of torpor until the development of their parts is excited by favourable circumstances."

These are the principles commonly received in regard to buds *in general*; but it appears to me that they are suited properly but to *one species*, that of most trees and shrubs in countries where the winter is pretty severe.

I shall now explain what, in my opinion, ought to be comprehended under the denomination of the *buds of phanegoram plants*. I give this name *to all the organs of these plants which contain the rudiments or germs of stems, branches, leaves, flowers, and even roots: each of these parts separately, or several of them united; or, in a word, the whole together.* This name belongs to them whatever be their size; their number; that of the different parts of which they are composed; the time of their appearance; their property of being preserved; the species which produces them, and the place of their insertion: modifications and peculiarities which depend on the different structure of the plants as well as the circumstances under which they are placed.

The definition I have given seems to me to unite several advantages: it embraces not only the whole of the organs together, even in regard to their origin; it dispenses also with the inconvenience of admitting a great number of exceptions in the uniform progress of nature; exceptions which no doubt prove that they have not been sufficiently examined. It is pretended that shrubs are distinguished from bushes or trees by their ramifications not being the result of buds. This distinction is merely arbitrary, since these plants really produce them; but their buds are very small, exceedingly thin, destitute of dry scales, and sometimes almost entirely concealed till the spring under the bark of the branches.

Admitting the definition I have proposed, it will be observed that buds differ from each other by a great number of characters in the various species of plants. I shall here mention only those traits which are indispensably necessary to support my definition.

In the *ligneous monocotyledon plants* the whole course of life is confined to the development of the first bud, arising from the neck of the root or place where it is joined to the stem. This bud contains the germs of all the organs which appear above the earth, and the end of its development is the term also of the life of the same plant. These plants, however, generally live one, and even several, centuries.

The propagation of plants by scions, by bulbs, and by other similar organs, which is very common to the herbaceous monocotyledons, is not essentially different from that by buds, as has been completely proved by M. Mirbel and several other botanists. Scions and bulbs are real buds; they are organs which contain the principles of new stems, branches, leaves, flowers, and roots. The part of the plant which

which produced them, and some modifications in their structure, make them, however, be considered as species different from every other.

In the *dicotyledon plants* and the *herbaceous monocotyledons* the summit of the root bears during the early age of the plant a single bud, named then *plumula*. This bud differs from that of the monocotyledon trees, as its development is ended before the death of the individual, and is followed by other buds, in consequence of the development of which the plant ramifies. The monocotyledon trees produce also secondary buds, but with this difference, that these buds are pushed out by the primitive bud itself, and that they fade much sooner than that from which they proceeded.

In *annual plants* the development of all the buds of the individual is completed in the space of a year. In the *biennial plants* it is terminated at the end of two years at most. *Plants with a vivacious root but annual stem* push out every year another bud from the neck of the root: from the developed parts of this principal bud other buds then issue, which make the ramifications of the vegetable appear.

The buds of the three sorts of plants here mentioned are not all, or very rarely are, covered with other organs to protect the germs against the intemperance of the weather. Here nature, in general, may dispense with them, because these buds develop themselves in the course of a very short time, and almost always in the best seasons; and the plants, or at least the stems, to which they are indebted for their birth, perish in the winter, or even sooner.

In most of these plants, as well as in bushes and some shrubs, the buds are small, thin, and pointed, as in the *viburnum*, the *rhamnus* (buckthorn), the *heliotrope*, the *cornel tree*, the gramineous plants, the *artemisia*, &c.

The ligneous dicotyledon plants in general push forth their buds only in spring. Their buds in winter remain in a state of inaction, and do not open till the return of spring. From this rule, however, are excluded all trees and bushes called *evergreens*, a great number of which perpetually send forth buds, so that the development of the leaves, branches, and even sometimes the flowers, never ceases during the life of these plants.

In almost all the dicotyledon trees and shrubs of the cold and temperate climates, the buds are formed in the eyes of the leaves, and always before the approach of winter. Botanists consider the most exterior folioles of these buds as *aborted folioles*, because they are dry and even sometimes sonorous

sonorous under the fingers. They are called *scales*, on account of their usual form. These organs are almost always covered with a resinous matter, which cements them very closely to each other, and which contributes not a little to defend the tender germs contained in the buds from the cold and moisture of the severe seasons.

In a great number of bushes and shrubs these *scales* are entirely wanting; but their place is supplied in several species by a *down*, which sometimes is pretty thick, and which affords sufficient protection to the buds against the rigour of the winter, as in the *viburnum* and the *ptelea*. In other buds of similar plants the parts still herbaceous and tender have neither down, nor scales, nor resinous matter, nor any other kind of covering, and they nevertheless withstand the intemperance of the seasons, unless it be excessive. Among the latter there are some the folioles of which cover each other firmly, as in the common lilac, the hazle, &c.; and there are some also in which the exterior folioles are neither so thick nor so firmly applied to each other as to be able to oppose the entrance of humidity, as is the case in the cornel tree.

The buds of several ligneous dicotyledon plants remain, in regard to their base and a great part of their body, concealed during winter under the bark, and their summit does not entirely open a passage for itself till the following spring. Here the bark serves as bandages to the tender elements of the new ramifications, as we see in a great number of shrubs.

Nature incloses the buds in several shrubs, as in the *berberis vulgaris*, with petioles very close to each other; it covers them also on one side by the branch from which they have proceeded, and on the other by the flat base of the prickles.

In the last place, there are some *ligneous dicotyledon vegetables* the buds of which are concealed and sheltered from the pernicious influence of the weather in a manner as singular as wonderful. The buds, unprovided with scales, but covered with a fine and thick down, form themselves under the concave base of the *supporters* of the leaves. During winter these *supporters* remain in their place in several plants; but when the sap ascends, the eye, becoming larger, rejects the tutor, of which it has no longer need. Sometimes shocks, and other accidents, make them fall earlier: in this case, the bud, being still secured by its natural pelisse, escapes generally the severe cold of winter.

I have observed that the supporter in question differs according

cording to its nature. In several plants it is a *real petiole*, as in the *seringa*, &c.; in others it is, as it were, *an intermediate organ between the branches and the petioles*, because it is not hollowed out in grooves at its upper surface, but entirely round; because its substance is more ligneous than herbaceous, though it falls annually; and because it forms internally a canal containing real pith, and closed towards the base of the petiole, so that the pith does not communicate with that of the part to which this supporter is attached. Similar *supporters* are found in the *rhus coriaria*, the *acer negundo*, the *robinia*, &c.

About three years ago I made this observation on the shumac; and I have reason to believe that no one ever made it before me, since I in vain consulted on this subject all my books on botany. I proposed to make a series of researches on this subject; and, indeed, they convinced me that nature covers in this manner the buds of several ligneous dicotyledons considered hitherto as not bearing any. It was my intention not to publish my observations until I had sufficiently repeated them, which could not be done till I had examined a much greater number of vegetables. But a little while ago I found in the *Mémoires pour servir à l'Anatomie et à la Physiologie Végétales, par M. Medicus* *, that this celebrated botanist had made the same observation on plane-trees. I immediately had recourse to other works, such as your excellent *Tableau du Regne Végétal*; and, though I found nothing in the first volume under the articles relating to that subject, I found in the third † that this peculiarity of the plane-tree had not escaped you. I then consulted the *Traité des Arbres et Arbustes*, by Duhamel, where, under the article *Platanus*, this great observer makes mention of the same phænomenon, without pointing out, however, whether it was known before; but as it has been known since that time, how comes it that no botanist, in treating specially of buds, has rectified his definition according to this observation?

Shumac furnished me also with an opportunity of observing several phænomena in regard to the *birth* and *development of buds*. I shall give you an account of them as briefly as possible.

Almost all botanists have adopted the opinion, that the leaves in the eyes of which the buds shoot forth approach, towards the end of their life, to the ligneous state by the

* *Beytrage zur Pflanzen anatomie und Pflanzen physiologie*, haft i. p. 24.
† Page 572.

influence

influence of the rays of the sun and of the air, and that they then no longer afford a free passage to the ascending sap. It is supposed that the fluids stop, and are accumulated at the base of these leaves, and that they thereby give birth to *cambium*, which occasions the ring or swelling there observed. It is pretended also that this *cambium* produces new vessels, which, obeying the impulse they daily receive from the ascending sap, become lengthened towards the surface of the bark, and force themselves to pierce it; after which they give birth to the bud.

This explanation, though ingenious, appears to me to be only a bold hypothesis, supported by facts which have not been examined with sufficient care. Instead of beginning by an explanation, it would have been better to discover first all the facts and all the circumstances by which this phænomenon is accompanied. And still we shall succeed as little in penetrating into this operation as we have done in regard to all the rest, the secret of which nature seems to have reserved to herself: the formation of the individuals and of their organs will be always inexplicable to us. Nature, however, far from precluding us from researches, excites us rather to watch her; and it is then that, by attending with assiduity and without prejudice, we often discover facts which form one step towards the truth. It may be objected to this explanation, that there are, indeed, buds which do not pierce the bark in the eyes of the leaves, and that the eye shows itself already at the time when the leaves have lost none of their vigour.

I shall now return to my own observations. In examining with attention the ligneous dicotyledon plants at the different seasons, it will be observed that *a body almost always soft, herbaceous, and green,* when it issues from the wood, enters into the bud destined to develop flowers or a branch, and that this body forms in reality the centre or axis of the bud. The same observation will be made when the wood of the branch which throws out the bud is already formed of several annual zones. It will be perceived also that this *herbaceous body* is rarely alone; that it has, for the most part, on each side, and at a very small distance from it, another *body*, and sometimes two, but of the same substance as itself: these *lateral herbaceous bodies* are smaller than it, and never penetrate into the substance of a bud which produces branches or flowers: they enter into those only of the leaves; that is to say, into those a petiole or leaf of which is destined to develop itself, which we observe at the base of a twig in several shrubs and bushes. There is is sometimes also at the base, and very near the largest herbaceous body, a similar body, which penetrates with it into the same bud. *All these herbaceous bodies* are produced each separately *in a medullary sheath,* and never in the bark, the liber, or the alburnum, nor in the annual zones of the wood. They are indeed *the prolongations of the bundles of tubes of the medullary sheath**.

The herbaceous bodies of which I here speak, in passing into the wood are not always in a horizontal direction: I mean, that they do not form in all the plants which I have examined a right angle with the medullary sheath: in several trees and shrubs they leave it much lower than the place where the bud issues from the bark, as is the case in the shumac, plane-tree, willows, &c.

When the bark, with the bud, has been removed, *these herbaceous prolongations* present themselves under different forms, according to the species to which they belong, the age of the branch, the place even where they are, and the angle at which they have traversed the wood. This is the reason why the figure of their section in the exterior zone of the wood varies from the form of a line or stroke to that of a point entirely round; sometimes there are several of them united, so that altogether they form scarcely a single line or oblong point. At the place where these herbaceous bodies traverse the wood, the ligneous fibres are separated from each other; and though they press them more or less, they still leave them a sufficient passage. This separation of the tubes and fibres of the wood is more apparent in the zones near to the medullary case, than in those which are more distant, and consequently nearer to the bark.

The thickness of these bodies differs also in the different species of plants. In those the wood of which is hard and compact they are smaller, and at the same time less cylindric than in those the wood of which is light. Those destined to push forth buds for twigs and flowers have more volume than those which produce leaf-buds.

They do not always remain green: they lose that colour as they change into wood. But they are found herbaceous, and filled with green substance, at least, until the bud has expanded into a flower-bud; and I have observed that they are still green and herbaceous, even in the twigs and young branches, if the wood of the species from which they have proceeded be white and light.

* These vessels are porous tubes, large simple tubes, tracheæ, and false tracheæ. See *Traité d'Anatomie et de Physiologie végétales, par Brisseau Mirbel,* vol. i. p. 186, et seq.; and the same in *Dict. des Sciences Naturelles,* vol. ii. p. 369.

The

The *same prolongations are found in herbaceous plants with a ramified stem:* for example, in the adult stems of the common cabbage, &c., where one may be convinced that the branches have no other origin there than that of the medullary sheath, and that they issue from real buds.

It seems to result, from what I have here said, that the reproducing organ of buds in the dicotyledon plants, is effectively and exclusively the *case or sheath which contains the pith:* it is even in the herbaceous monocotyledon plants, the spaces between the knots of which form empty tubes: nature, to produce branches, has formed in these plants solid knots or articulations, the structure of which is almost the same as that of the stems of plants the canal of which contains pith.

The opinion that the medullary case is the only organ which gives rise to buds, and consequently to branches, is confirmed by the observations which I made on the insertion of old branches even in trunks entirely dry. For this purpose I carefully examined the branches of the pine fir and *prunus spinosa* (sloe-tree), shumac, oak, apple-tree, &c., and every where I observed the same result. It is true, that in the old branches the prolongations are more herbaceous, and on that account are difficult to be distinguished, because the one which has penetrated into the principal substance of the bud, from which the branch has proceeded, has become ligneous: the small lateral ones are not even dried, and are still entirely covered and concealed both by the base of the branch, and by the annual strata of the wood, which had been formed after these prolongations had thrown out petioles or leaves. The perpendicular section of a branched trunk always shows, whatever be the number of the concentric strata of the ligneous body, that the medullary sheath of the branch proceeds from the medullary sheath of the trunk; that the branch never forms in the trunk an *inverted cone,* the summit of which is concealed by the strata of that part; and that the branch of a tree can never be compared, as has been done, to *a plant the roots of which are in a ligneous soil.* The different annual strata of a branch never cover each other, in any case, at their respective bases; and they are never separated from those of the trunk. These strata are in immediate communication with those of the trunk, in such a manner that the strata of the branch seem to have arisen from a prolongation of those of the trunk. It is very difficult to distinguish whether the medullary sheath takes its origin from

from that of the trunk, when the medullary canal of that part is filled with ligneous strata, which is the case in several bushes and shrubs where an interior liber is formed annually of the pith, until the canal which contains it has altogether disappeared, as in the ash, the oak, shumac, &c.; but it is principally in the latter that we may be convinced that it is never by the medullary sheath, nor by the interior cambium, that these internal zones are produced.

The *increase in length* of buds and twigs is ascribed to an erection of the tubes of the liber. If the bark of a young branch be separated as far as the terminal bud in a young poplar, for example, and particularly before winter, or in the following spring, it will be found that the inner bark has never become lengthened. The upper part of a branch and a flower-bud is formed only by the pith, the medullary sheath, and the bark. I think I may conclude from this observation, that the increase of the stems or trunks, and the ramifications, depends merely on the elongation of the vessels of the medullary sheath. The *alburnum*, the tubes of which have a direction perfectly straight, presents itself at the upper part of a branch under the form of separate fibres, which lose themselves at the surface of the medullary sheath. This observation explains to us also why the zones of the upper part of a branch are in number inferior to those of the base. It confirms also what I have said in regard to the birth of buds; for, if we examine the bud of a small branch in any tree whatever, we shall be convinced that it is the medullary sheath without exception that composes alone the interior of that organ.

Explanation of the figures belonging to the above article. See Plate III.

Fig. 1, A vertical section of a piece of a branch of shumac.

A, The bark.
B, Ligneous zone of one year.
C, The medullary sheath.
D, A bud.
E, The swelled-up part of the bark at the base of the bud.
F, Herbaceous prolongation of the medullary sheath, which has traversed the wood and given birth to the bud.
G, The pith.

Fig. 2,

Fig. 2, A similar, cut obliquely at the place where the prolongation passes through the wood.

A, A bud.
B, The bark.
C, Ligneous zone of the year.
D, Herbaceous prolongation of the medullary sheath passing through the wood and entering the bud.
E, The swelled part of the bark at the sides of the bud.
F, The pith.
G, The medullary sheath.

Fig. 3, The same piece seen in profile, to show the angle of the section.

Fig. 4, Vertical section of a piece of shumac of two years old, bearing a bud.

A, The bud produced from an herbaceous prolongation of the medullary sheath, which prolongation has passed through the two ligneous strata.

Fig. 5, Vertical section of a branched piece of shumac.

A, The bark.
B, Ligneous stratum of the second year.
C, Ligneous stratum of the first year.
D, Stratum arising from the lignification of the exterior part of the pith.
E, Medullary sheath of a branch.
F, Medullary sheath of a twig.
G, Pith.
H, Prolongation of the medullary sheath, become ligneous, and confounded with the wood produced by the pith.

Fig. 6, Vertical section of a branched piece of the *platanus occidentalis.*

A, Pith.
B, Medullary sheaths.
C, Ligneous stratum of the first year.
D, Ligneous stratum of the second year.
E, Lignified prolongations of the medullary sheath.

Fig. 7, Vertical section of a piece of hazle, bearing a twig with false wood, which has only one ligneous zone, and which has arisen from a prolongation of the medullary sheath, which prolongation, in order to produce the bud from which the twig resulted, has passed through seven zones of wood, and at length become ligneous.

[To be concluded in our next.]

 XXXVI. *Method*

XXXVI. *Method of obviating the Necessity of lifting Ships. By Mr.* Robert Seppings, *of Chatham Yard**.

The following is a description of an invention by Mr. Robert Seppings, late master shipwright assistant in his majesty's yard at Plymouth (now master shipwright at his majesty's yard, Chatham), for suspending, instead of lifting, ships, for the purpose of clearing them from their blocks, by which a very great saving will accrue to the public, and also two-thirds of the time formerly used in this operation.

From the saving of time another very important advantage is derived, namely, that of enabling large ships to be docked, suspended, and undocked, the same spring tides. Without enumerating the inconveniencies arising, and, perhaps, injuries, which ships are liable to sustain, from the former practice of lifting them, and which are removed by the present plan; that which relates to manual labour deserves particular attention; twenty men being sufficient to suspend a first-rate, whereas it would require upwards of 500 to lift her. The situation which Mr. Seppings held in Plymouth-yard, attached to him, in a great degree, the shoring and lifting of ships, as well as the other practical part of the profession of a shipwright. Here he had an opportunity of observing, and indeed it was a subject of general regret, how much time, expense, and labour, were required in lifting a ship, particularly ships of the line. This induced him to consider whether some contrivance could not be adopted to obviate these evils. And it occurred to him, that if he could so construct the blocks on which the ship rests, that the weight of the ship might be applied to assist in the operation, he should accomplish this very desirable end. In September 1800, the shoring and lifting the San Josef, a large Spanish first-rate, then in dock at Plymouth, was committed to his directions; to perform which, the assistance of the principal part of the artificers of the yard was requisite. In conducting this business, the plan, which will be hereafter described, occurred to his mind; and from that time, he, by various experiments, proved his theory to be correct: the blocks, constructed by him, upon which the ship rests, being so contrived, that the facility in removing them, is proportionate to the quantity of pressure; and this circumstance is always absolutely

* From the *Transactions of the Society of Arts*, who voted him the gold medal, 1804.

under command, by increasing or diminishing the angle of three wedges, which constitute one of the blocks; two of which are horizontal, and one vertical. By enlarging the angle of the horizontal wedges, the vertical wedge becomes of consequence more acute; and its power may be so increased, that it shall have a great tendency to displace the horizontal wedges, as was proved by a model, which accompanied the statement to the society; where the power of the screw is used as a substitute for the pressure of the ship.

Mr. Seppings caused three blocks to be made of hard wood agreeable to his invention, and the wedges of various angles. The horizontal wedges of the first block were nine degrees; of the second, seven; and of the third, five; of course, the angle of the vertical wedge of the first block was 162 degrees; of the second, 166; and of the third, 170. These blocks, or wedges, were well executed, and rubbed over with soft soap for the purpose of experiment. They were then placed in a dock, in his majesty's yard, at Plymouth, in which a sloop of war was to be docked: on examining them after the vessel was in, and the water gone, they were all found to have kept their situations, as placed before the ship rested upon them. Shores in their wake were then erected to sustain the ship, prior to the said blocks being taken from under the keel. The process of clearing them was, by applying the power of battering-rams to the sides of the outer ends of the horizontal wedges; alternate blows being given fore and aft; by which means they immediately receded, and the vertical wedges were disengaged. It was observed, even in this small ship, that the block which was formed of horizontal wedges of nine degrees, came away much easier than those of seven, and the one of seven than that of five. In removing the aforesaid blocks by the power of the battering-rams, which were suspended in the hands of the men employed, by their holding ropes passed through holes for that purpose, it was remarked by Mr. Seppings, that the operation was very laborious to the people; they having to support the weight of the battering-rams, as well as to set them in motion. He then conceived an idea of affixing wheels near the extremity of that part of the rams which strikes the wedges. This was done before the blocks were again placed; and it has since been found fully to answer the purpose intended, particularly in returning the horizontal wedges to their original situations, when the work is performed for which they were displaced; the wheels also giving a great increase of

 power

power to the rams, and decrease of labour to the artificers; besides which, the blows are given with much more exactness. The same blocks were again laid in another dock, in which a two-decked ship of the line was docked. On examination they were found to be very severely pressed, but were removed with great ease. They were again placed in another dock in which a three-decked ship of the line was docked. This ship having in her foremast and bowsprit, the blocks were put quite forward, that being the part which presses them with the greatest force. As soon as the water was out of the dock, it was observed that the horizontal wedges of nine and seven degrees had receded some feet from their original situations. This afforded Mr. Seppings a satisfactory proof, which experience has since demonstrated (though many persons before would not admit of, and others could not understand, the principle), that the facility of removing the blocks or wedges, was proportionate to the quantity of pressure upon them. The block of five degrees kept its place, but was immediately cleared, by applying the power of the battering-rams to the sides of the outer ends of the horizontal wedges. The above experiments being communicated to the Navy Board, Mr. Seppings was directed to attend them, and explain the principle of his invention; which explanation, further corroborated by the testimonials of his then superior officers, was so satisfactory, that a dock was ordered to be fitted at Plymouth under his immediate directions. The horizontal wedges in this, and in the other docks, that were afterwards fitted by him, are of cast iron, with an angle of about five degrees and a half, which, from repeated trials, are found equal to any pressure, having in no instance receded, and, when required, were easily removed. The vertical wedge is of wood, lined with a plate of wrought iron, half an inch thick. On the bottom of the dock, in the wake of each block, is a plate of iron of three quarters of an inch thick, so that iron at all times acts in contact with iron.

The placing the sustaining shores, the form and sizes of the wedges, and battering-rams, &c.; also the process of taking away, and again replacing, the wedges of which the block is composed, are also exemplified by a model.

The dock being prepared at Plymouth, in August 1801 the Canopus, a large French 80-gun ship, was taken in, and rested upon the blocks; and the complete success of the experiment was such, that other docks were ordered to be fitted at Sheerness and Portsmouth dock-yards, under Mr. Seppings's directions. At the former place a frigate, and

and at the latter a three-decked ship, were suspended in like manner. This happened in December 1802, and January 1803; and the reports were so favourable, as to cause directions to be given for the general adoption of these blocks in his majesty's yards. This invention being thought of national consequence, with respect to ships, but particularly those of the navy, government has been pleased to notice and reward Mr. Seppings for it.

The time required to disengage each block is from one to three minutes after the shores are placed: and a first-rate sits on about fifty blocks. Various are the causes for which a ship may be required to be cleared from her blocks, viz. to shift the main keel; to add additional fasle keel; to repair defects; to caulk the garboard seams, scarples of the keel, &c. Imperfections in the false keel, which are so very injurious to the cables, can, in the largest ship, be remedied in a few hours by this invention, without adding an additional shore, by taking away blocks forward, amidships, and abaft, at the same time; and, when the keel is repaired in the wake of those blocks, by returning them into their places, and then by taking out the next, and so on in succession. The blocks can be replaced in their original situations, by the application of the wheel battering-rams to the wedges, the power of which is so very great, that the weight of the ship can be taken from the shores that were placed to sustain her. There were one hundred and six ships of different classes, lifted at Plymouth dock-yard, from the 1st of January 1798 to the 31st of December 1800; and, had the operation of lifting taken less time, the number would have been very considerably increased; for the saving of a day is very frequently the cause of saving a spring tide, which makes the difference of a fortnight. The importance of this expedition, in time of war, cannot be sufficiently estimated.

This invention may be applied with great advantage, whenever it is necessary to erect shores, to support any great weights, as, for instance, to prop up a building during the repair of its foundation, &c. Captain Wells, of his majesty's ship Glory, of 98 guns, used wedges of Mr. Seppings's invention for a fid of a top-gallant mast of that ship. In 1803, the top-gallant masts of the Defence, of 74 guns, were fitted on this principle by Mr. Seppings: and, from repeated trials, since she has been cruizing in the North Sea, the wedge fids have been found in every respect to answer.

But it is Mr. Seppings's wish that it should be understood, that the idea of applying this invention to the fid of a top-gallant mast originated with captain Wells, who well understood the principle, and had received from him a model of the invention.

When it is required to strike a top-gallant mast, the top ropes are hove tight, and the pin which keeps the horizontal wedges in their place is taken out, by one man going aloft for that purpose; the other horizontal wedge is worked in the fid, as shown in the drawing and model that accompany this statement. The upper part of the fid hole is cut to form the vertical wedge. The advantage derived from fidding top-gallant masts in this way is, that they can be struck at the shortest notice, and without slacking the rigging, which is frequently the cause of springing and carrying them away, particularly those with long pole heads. The angle of the horizontal wedges for the fids of masts should be about twenty degrees.

The above account was accompanied with certificates from sir John Henslow, surveyor of the navy; Mr. M. Didram, master-shipwright of Portsmouth-yard; and Mr. John Carpenter, foreman of Sheerness dock-yard, confirming Mr. Seppings's statement.

Reference to the Engraving of Mr. Seppings's method of obviating the necessity of lifting ships. Plate IV.

This plan and section of a seventy-four gun ship describes the method of obviating the necessity of lifting ships, when there may be occasion to put additional false keels to them, or to make good the imperfections of those already on; also, when it may be necessary, to caulk the garboard seams, scarples, the keel, &c.; by which means a very considerable part of the expense will be saved, and much time gained. The blocks are cleared, and again returned by the following process. A sufficient number of shores are placed under the ship to sustain her weight, and set taught, stationed as near the keel as the working of the battering-rams fore and aft will admit. Avoid placing any opposite the blocks, as they would in that case hinder the return of the wedges with the battering-rams. A blow must then be given forward on the outer end of the iron wedges with the battering-rams in a fore and aft direction, which will cause them to slide aft, as shown in the plan. The battering rams abaft then return the blow, and the wedges again come forward; by the repetition of this operation, the wedges will be with great ease cleared, and the angular

block

block on the top will drop down. When the work is performed, the block must be replaced under the keel, and the wedges driven back bw working the rams athwart-ships, as described in the section.

N. B. In returning the iron wedges, to avoid straining the angular blocks, it is proposed to leave a few of them out forward and aft, and stop the ship up, by laying one iron wedge on the other, as shown at Fig. 1, Plate IV.

To facilitate the business, blocks may be cleared forward and aft at the same time, sufficient to get in place one length of false keel. If the false keel should want repairing, it may be done without any additional shores, by clearing one block at a time; and when the keel is repaired in the wake of that block, return the wedges, as above directed, and clear the next, &c.

Section and Plan, Plate IV. Fig. 2.

A, Keelson.
B, Ceiling.
C, Floor timber.
D, Dead or rising wood.
E, Plank of the bottom.
F, Keel and false keel.
G, Angular blocks with a half-inch iron-plate bolted to them.
H, Cast-iron wedges.
I, Iron-plate of three-fourths of an inch thick on the bottom of the dock.
K, Battering-rams, with wheels, and ropes for the hands.
L, Cast-iron wedges, having received a blow from forward.
M, Shores under the ship to sustain her weight.

Fig. 3. represents part of a top-gallant mast fitted with a wedge fid.

a, Top-gallant mast.
b, Fid, with one horizontal wedge worked on it.
c, Moveable wedge, with the iron strap and pin over it, to keep it in its situation.
d, Trussel trees.

XXXVII. *On the Variations of the Terrestrial Magnetism in different Latitudes.* *By Messieurs* HUMBOLDT *and* BIOT. *Read by M.* BIOT, *in the Mathematical and Physical Class of the French National Institute 26th Frimaire, An* 13*. (17*th December* 1804.)

AN inquiry into the laws of terrestrial magnetism is no doubt one of the most important questions that philosophers can propose. The observations already made on this subject have discovered phænomena so curious, that one cannot help endeavouring to solve the difficulties they present; but notwithstanding the efforts hitherto employed, it must be confessed that we are absolutely unacquainted with the causes of them.

It was difficult to obtain on this point any precise knowledge at a time when the construction of the compass was still imperfect; and so little time has elapsed since the discoveries of M. Coulomb have taught us to render them completely exact, it needs excite no astonishment that so few facts in the observations of travellers have been found worthy of confidence.

The expedition which M. Humboldt has terminated has procured for this part of philosophy a collection no less valuable than those with which he has enriched the other branches of human knowledge. Furnished with an excellent dipping-needle, constructed by Le Noir on the principles of Borda, M. Humboldt has made more than three hundred observations on the inclination of the magnet, and on the intensity of the magnetic force in that part of America which he traversed. By adding to these results those which he had already obtained in Europe before his departure, we shall have for the first time a series of correct facts on the variation of the magnetic forces in the northern part of the globe, and in some points of its southern part.

The friendship which M. Humboldt has testified for me since his return having given me an opportunity of communicating to him some experiments on this subject, which I made this year in the Alps, he immediately offered to unite his to mine in a memoir. But if friendship and a desire of making known new facts induced me to accept this offer of M. Humboldt, justice forbids me to take advantage of it to his prejudice; and I must here declare, that a very small part of it belongs to me.

* From the *Journal de Physique*, Frimaire, An 13.

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To place in order the facts and consequences which may be deduced from them, it is necessary to consider the action of terrestrial magnetism under different points of view, corresponding to the different classes of the phænomena which it produces.

If we consider it first in general, we find that it acts on the whole surface of the globe, and that it extends beyond it. This last fact, which was doubted, has been lately proved by one of us, and particularly by our friend M. Gay-Lussac, during his two aërostatic voyages. And if these observations, made with all the care possible, have not shown the least sensible diminution in the intensity of the magnetic force, at the greatest height to which man can attain, we have a right to conclude that this force extends to an indefinite distance from the earth, where it decreases, perhaps, in a very rapid manner, but which at present is unknown to us.

If we now consider magnetism at the surface even of the earth, we shall find three grand classes of phænomena, which it is necessary to study separately, in order to have a complete knowledge of its mode of action. These phænomena are; the declination of the magnetic needle, its inclination, and the intensity of the magnetic force, considered either comparatively in different places or in themselves, paying attention to the variations which they experience. It is thus that, after having discovered the action of gravity as a central force, its variation, resulting from the figure of the earth, was afterwards ascertained in different latitudes.

The declination of the magnetic needle appears to be that phænomenon which hitherto has more particularly fixed the attention of philosophers, on account, no doubt, of the assistance which they hoped to derive from it in determining the longitude; but when it was known that the declination changes in the same place, in the course of time, when its diurnal variations were remarked, and its irregular traversing, occasioned by different meteors; in a word, the difficulty of observing it at sea, within one degree nearly, it was necessary to abandon that hope, and to consider the cause of these phænomena as much more complex and abstruse than had been at first imagined.

In regard to the intensity of the magnetic force in different parts of the earth, it has never yet been measured in a comparative manner. The observations of M. Humboldt on this subject have discovered a very remarkable phænomenon; it is the variation of the intensity in different latitudes.

tudes, and its increase proceeding from the equator to the poles.

The compass, indeed, which at the departure of M. Humboldt gave at Paris 245 oscillations in 10 minutes, gave no more in Peru than 211, and it constantly varied in the same direction; that is to say, the number of the oscillations always decreased in approaching the equator, and always increased in advancing towards the north.

These differences cannot be ascribed to a diminution of force in the magnetism of the compass, nor can we suppose that it is weakened by the effect of time and of heat; for, after three years' residence in the warmest countries of the earth, the same compass gave again in Mexico oscillations as rapid as at Paris.

There is no reason, either, to doubt the justness of M. Humboldt's observations, for he often observed the oscillations in the vertical plane perpendicular to that meridian; but by decomposing the magnetic force in the latter plane, and comparing it with its total action, which is exercised in the former, we may from these data calculate its direction, and consequently the direction of the needle*. This inclination, thus calculated, is found always conformable to that which M. Humboldt observed directly. When he made his experiments, however, he could not foresee that they would be subjected to this proof by which M. La Place verified them.

As the justness of these observations cannot be contested, we must allow also the truth of the result which they indi-

* Let H O C (plate V. fig. 1.) be the plane of the magnetic meridian passing through the vertical O C; let O L be the direction of the needle situated in that plane, and O H a horizontal. The angle L O H will be the inclination of the needle, which we shall denote by I. If F represent the total magnetic force which acts in the direction O L, the part of this force, which acts according to O C, will be F sine of I: but the magnetic forces which determine the oscillations of the needle in any plane, are to each other as the squares of the oscillations made in the same time. If we denote then by M, the number of the oscillations made in 10′ of time in the magnetic meridian, and by P, the number of oscillations made also in 10′, in the perpendicular plane, we shall have the following proportion.

$$\frac{F \sin. I}{F} = \frac{P^2}{M^2}$$

from which we deduce

$$\text{Sin. } I = \frac{P^2}{M^2}$$

The inclination then may be calculated by this formula, when we have oscillations made in the two planes.

In like manner, by making a needle oscillate successively in several vertical planes, we might determine the direction of the magnetic meridian.

cate,

cate, and which is the increase of the magnetic force proceeding from the equator to the poles.

To follow these results with more facility it will be proper to set out from a fixed term, and it appears natural to make choice for that purpose of the points where the inclination of the magnetic needle is null, because they seem to indicate the places where the opposite action of the two terrestrial hemispheres is equal. The series of these points forms on the surface of the earth a curved line which differs very sensibly from the terrestrial equator, from which it deviates to the south in the Atlantic Ocean and to the north in the South Sea. This curve has been called the *magnetic equator*, from its analogy to the terrestrial equator, though it is not yet known whether it forms exactly a great circle of the earth. We shall examine this question hereafter; at present it will be sufficient to say, that M. Humboldt found this equator in Peru about 7·7963° (7° 1′) of south latitude, which places it, for that part of the earth, nearly in the spot where Wilke and Lemonnier had fixed it.

The places situated to the north of that point may be divided into four zones; the three first of which, being nearer the equator, are about 4·5° (4°) of breadth in latitude; while the latter, more extensive and more variable, is 16° (14°). So that the system of these zones extends in America from the magnetic equator to 25·5556° (23°) of north latitude, and comprehends in longitude an interval of about 56° (50°).

The first zone extends from 7·7963° (7° 1″) of south latitude to 3·22° (2° 54′). The mean number of the oscillations of the needle in the magnetic meridian in 10′ of time is there, 211·9: no observation gives less than 211, or more than 214. From M. Humboldt's observations one might form a similar zone on the south side of the magnetic equator, which would give the same results.

The second zone extends from 2·4630° (2° 13′) of south latitude to 3·61° (3° 15′) of north latitude. The mean term of the oscillations is there, 217·9: they are never below 220, or above 226.

The fourth zone, broader than the other two, extends from 10·2778° (9° 15′) to 25·7037° (23° 8′) of north latitude. Its mean term is 237: it never presents any observation below 229, or above 240.

We are unacquainted, in regard to this part of the earth, with the intensity of the magnetic force beyond the latitude of 26° (23°) north; and on the other hand, in Europe, where we have observations made in high latitudes, we have

have none in the neighbourhood of the equator: but we will not venture to compare these two classes of observations, which may belong to different systems of forces, as will be mentioned hereafter.

However, the only comparison of results, collected in America by M. Humboldt, appears to us to establish with certainty the increase of the magnetic force from the equator to the poles; and, without wishing to connect them too closely with the experiments made in Europe, we must remark that the latter accord so far also with the preceding as to indicate the phænomenon.

If we have thus divided the observations into zones parallel to the equator, it is in order that we may more easily show the truth of the fact which results from them, and in particular to render the demonstration independent of those small anomalies which are inevitably mixed with these results.

Though these anomalies are very trifling, they are, however, so sensible, and so frequently occur, that they cannot be ascribed entirely to errors in the observations. It appears more natural to ascribe them to the influence of local circumstances, and the particular attractions exercised by collections of ferrugineous matters, chains of mountains, or by the large masses of the continents.

One of us, indeed, having this summer carried to the Alps the magnetic needle employed in one of his late aërial excursions, he found that its tendency to return to the magnetic meridian was constantly stronger in these mountains than it was at Paris before his departure, and than it has been found since his return. This needle, which made at Paris 83·9° in 10 minutes of time, has varied in the following manner in the different places to which it was carried:

Places of observation.	Number of oscillations in ten minutes of time.
Paris before his departure -	83·9
Turin - - -	87·2
On Mount Genêvre -	88·2
Grenoble - - -	87·4
Lyons - - -	87·3
Geneva - - -	86·5
Dijon - - -	84·5
Paris, on his return -	83·9

These experiments were made with the greatest care, conjointly with excellent observers, and always employing the same watch verified by small pendulums, and taking the mean

mean terms between several serieses of observations, which always differed very little from each other. It appears thence to result that the action of the Alps has a sensible influence on the intensity of the magnetic force. M. Humboldt observed analogous effects at the bottom of the Pyrenees; for example, at Perpignan. It is not improbable that they arose from the mass of these mountains, or the ferrugineous matters contained in them; but whatever may be the cause, it is seen by these examples that the general action of terrestrial magnetism is sensibly modified by local circumstances, the differences of which may be perceived in places very little distant from each other. This truth will be further confirmed by the rest of this memoir.

It is to causes of this kind, no doubt, that we must ascribe the diminution of the magnetic forces observed in some mountains; a diminution which, on the first view, might appear contrary to the results obtained during the last aërial voyages. This conjecture is supported by several observations of M. Humboldt. By making his needle to oscillate on the mountain of Guadaloupe, which rises 676 metres (338 toises) above Santa-Fé, he found it in 10 minutes of time give two oscillations less than in the plain. At Silla, near Caracas, at the height of 2632 metres (1316 toises) above the coast, the diminution went so far as five oscillations; and, on the other hand, on the volcano of Antisana, at the height of 4934 metres (2467 toises), the number of oscillations in 10 minutes was 230; though at Quito it was only 218: which indicates an increase of intensity. I observed, indeed, a similar effect on the summit of Mount Genèvre, at the height of 1600 or 1800 metres (8 or 900 toises), as may be seen by the numbers which I have already given; and it was on this mountain that I found the greatest intensity of the magnetic force. I saw on the hill of La Superga, in the neighbourhood of Turin, an example of these variations equally striking. Observing, with Vassali, on this hill, at the elevation of about 600 metres (300 toises), we found 87 oscillations in 10 minutes of time. On the side of the hill we had 88·8 oscillations; and at the bottom, on the bank of the Po, we obtained 87·3. Though these results approach very near to each other, their difference is, however, sensible, and fully shows that their small variations must be considered as slight anomalies produced by local circumstances.

This examination leads us to consider the intensity of magnetism on the different points of the surface of the globe, as subject to two sorts of differences. One kind are general:

general: they depend merely on the situation of the places in regard to the magnetic equator, and belong to a general phænomenon, which is the increase of the intensity of the magnetic forces in proportion as we remove from the equator: the other kind of variations, which are much smaller and altogether irregular, seem to depend entirely on local circumstances, and modify either more or less the general results.

If we consider terrestrial magnetism as the effect of an attractive force inherent in all the material particles of the globe, or only in some of these particles, which we are far from determining, the general law will be, the total result of the system of attraction of all the particles, and the small anomalies will be produced by the particular attractions of the partial systems of the magnetic moleculæ diffused irregularly around each point; attractions rendered more sensible by the diminution of distance.

It now remains to consider the inclination of the magnetic needle in regard to the horizontal plane. It has long been known that this inclination is not every where the same: in the northern hemisphere the needle inclines towards the north; in the southern towards the south; the places where it becomes horizontal form the magnetic equator; and those where the inclination is equal, but not null, form on each side of that equator curved lines, to which the name of magnetic parallels has been given from their analogy to the terrestrial parallels. One may see in several works, and particularly in that of Lemonnier, entitled *Lois du Magnetism*, the figure of these parallels and their disposition on the face of the earth.

It evidently results from this disposition that the inclination increases in proportion as we recede from the magnetic equator; but the law which it follows in its increase has not yet, as far as appears to us, been given. To ascertain this law, however, would be of great utility; for the inclination seems to be the most constant of all the magnetic phænomena, and it exhibits much fewer anomalies than the intensity. Besides, if any rule, well confirmed, could be discovered on this subject, it might be employed with advantage at sea to determine the latitude when the weather does not admit an observation of the sun; which is the case in various places during the greater part of the year. We have some reason to expect this application when we see the delicacy of that indication in the observations of M. Humboldt, where we find 0·65° (35′ 6″) of difference between two towns so near each other as Nismes and Montpellier. These motives have induced us to study with great

interest

interest the series of observations made by M. Humboldt in regard to the inclination; and it appears to us that they may be represented very exactly by a mathematical hypothesis; to which we are far from attaching any reality in itself, but which we offer merely as a commodious and sure mode of connecting the results.

To discover this law, we must first exactly determine the position of the magnetic equator, which is as an intermediate line between the northern and the southern inclinations. For this purpose we have the advantage of being able to compare two direct observations; one of Lapeyrouse, and the other of M. Humboldt. The former found the magnetic equator on the coasts of Brasil at 12·1666° (10° 57′) of south latitude, and 28·2107° (25° 25′) of west longitude, counted from the meridian of Paris. The latter found the same equator in Peru at 7·7963° (7° 1′) of south latitude, and 89·6481° (80° 41′) of west longitude, also reckoned from the same meridian. These data are sufficient to calculate the position of the magnetic equator, supposing it to be a great circle of the terrestrial sphere; an hypothesis which appears to be conformable to observations. The inclination of this plane to the terrestrial equator is thus found to be equal to 11·0247° (10° 58′ 56″), and its occidental node on that equator is at 133·3719° (120° 2′ 5″) west from Paris, which places it a little beyond the continent of America, near the Gallipagos, in the South Sea; the other node is at 66·6281° (59° 57′ 55″) to the east of Paris, which places it in the Indian Seas*.

We

* To calculate this position let N E E′ (Plate V. fig. 2.) be the terrestrial equator; N H L the magnetic equator, supposed also to be a great circle; and H L the two points of that equator, observed by Messrs. Humboldt and Lapeyrouse. The latitudes HE, LE′, and the arc E E′, which is the difference of longitude of these two points, is known: consequently, if we suppose $HE = l$, $LE' = l'$, $EE' = v$, $EN = x$, and the angle $ENH = y$, we shall have two spherical triangles N E H, N E′L, which will give the two following equations:

$$\text{Sin.}\, x = \frac{\text{tang.}\, l \cot y}{R} \quad \text{sin.}\,(x + v) = \frac{\text{tang.}\, l' \cot.\, y}{R}$$

from which we deduce

$$\text{Sin.}\, \frac{(x + v)}{\text{sin.}\, x} = \frac{\text{tang.}\, l'}{\text{tang.}\, l}$$

and developing

$$\cot.\, x = \frac{\text{tang.}\, l'}{\text{tang.}\, l \;\text{sin.}\, v} - \frac{\cos.\, v}{\text{sin.}\, v}$$

Let us now take an auxiliary angle ϕ, so that we may have

$$\text{tang.}\, \phi = \frac{\text{tang.}\, l \;\text{sin.}\, v}{\text{tang.}\, l'}$$

and

We do not give this determination as rigorously exact: some corrections might no doubt be made to it, had we a greater number of observations equally precise; but we are of opinion that these corrections would be very small; and it will be seen hereafter that, independently of the confidence which the two observations we have employed deserve, we have other reasons for entertaining this opinion *.

It is very remarkable that this determination of the magnetic equator agrees almost perfectly with that given long ago by Wilke and Lemonnier. The latter in particular, who for want of direct observations had discussed a great number of corresponding observations, indicates the magnetic equator in Peru towards 7°$\frac{1}{3}$ of south latitude; and M. Humboldt found it in the same place at 7·7963° (7° 1′); besides, Lemonnier's chart, as well as that of M. Wilke, indicates for the inclination of the magnetic meridian 12·22° (about 11°), and they place the node about 155° 56′ (140°) of west longitude, reckoned from the meridian of Paris.

Can it be by chance, then, that these elements, found more than 40 years ago, should accord so well with ours founded on recent observations? or does the inclination of the magnetic equator experience only very small variations, while all the other symptoms of terrestrial magnetism change so rapidly? We should not be far from admitting the latter opinion, when we consider that the inclination of the magnetic needle has changed at Paris 3° during 60 years since it has been observed; and that at London, according to the observations of Mr. Graham, it has not changed 2° in 200 years; while the declination has varied more than 20° in the same interval, and has passed from east to west: but, on the other hand, the observation of the inclination is so difficult to be made with exactness, and it is so short a time since the art of measuring it with precision was known,

and we shall have

$$\text{tang. } x = \frac{\sin. v \sin. \phi}{\sin. (v - \phi)}.$$

By these equations we may find x, and then y, by any of the first two.

* Since this memoir was read, we have collected new information which confirms these first results. Lapeyrouse, after having doubled Cape Horn, fell in a second time with the magnetic equator in 18′ north lat. and 119° 7′ of longitude west from Paris. He was therefore very near the node of the magnetic equator, such as we have deduced it from observations. This fact establishes in a positive manner two important consequences: first, that the preceding determinations require only very slight corrections; and the second, that the magnetic equator is really a great circle of the earth, if not exactly at least very nearly.—*Note of the Authors.*

that

that it is perhaps more prudent to abstain from any premature opinion on phænomena the cause of which is totally unknown to us.

To employ the other observations of M. Humboldt in regard to the inclination, I first reduced the terrestrial latitudes and longitudes reckoned from the magnetic equator. The latter, being reckoned from the node of that equator in the South Sea, I could first perceive by these calculations that the position of that plane determined by our preceding researches was pretty exact; for some of the places, such as Santa-Fé and Javita, where M. Humboldt observed inclinations almost equal, were found nearly on the magnetic parallel, though distant from each other more than 6·6666° (6°) in longitude*.

When these reductions were made, I endeavoured to represent the signs of the inclinations observed, and to leave as little to chance as possible. I first tried a mathematical hypothesis conformable enough to the idea which has hitherto been entertained in regard to terrestrial magnetism.

I have supposed in the axis of the magnetic equator, and at an equal distance from the centre of the earth, two centres of attractive forces, the one austral and the other boreal, in such a manner as to represent the two opposite magnetic poles of the earth: I then calculated the effect which ought to result from the action of these centres in any point of the surface of the earth, making their attractive force reciprocally vary as the square of the distance; and in this manner I obtained the direction of the result of their forces, which ought to be that also of the magnetic needle in that latitude.

[To be continued.]

XXXVIII. *Account of a Case of Hydrophobia successfully treated by copious Bleeding and Mercury. In two Letters from Dr.* Robert Burton, *of Bent, in the State of Virginia, to Dr.* Benjamin Rush, *of Philadelphia*†.

SIR,

Believing that you are always disposed to encourage any thing which may throw light upon the treatment of diseases, I take the liberty of addressing to you the follow-

* This confirms what we have already said, that the magnetic equator is sensibly a great circle of the earth.—*Note of the Authors.*

† From the *American Medical Repository.*

ing case of hydrophobia, requesting a line or two, if you think it deserving your attention.

On July 4, 1803, at nine o'clock in the evening, I was desired to visit Thomas Brothers, aged 28 years. I was informed by the person who came for me, that he had been bitten by a dog, which his friends suspected to be mad. I found him in the hands of four young men, who were endeavouring to confine him, and thereby prevent him from injuring himself or friends. He recognised me, and requested me to give him my hand, which he made a violent effort to draw within his mouth. Conscious of his inclination to bite, he advised his friends to keep at a distance, mentioning that a mad dog had bitten him.

His symptoms were as follow: viz. a dull pain in his head, watery eyes, dull aspect, stricture and heaviness at the breast, and a high fever.

Believing, as you do, that there is but one fever, I determined to treat this case as an inflammatory fever. I therefore drew twenty ounces of blood; and, as he refused to take any thing aqueous, I had him drenched with a large dose of calomel and jalap.

July 5th, four *a. m.* Finding the symptoms worse, I took away sixteen ounces of blood, and applied two large epispastic plasters to his legs, hoping thereby to relieve the oppression of the præcordia and other symptoms.

Twelve *m.* Was informed that one of his friends had permitted him to take a stick in his mouth, which he bit so as to loosen several of his teeth. As he craved something to bite, I desired his friend to give him a piece of lead, which he bit until he almost exhausted his strength.

One *p. m.* Finding but little alteration, I drew eighteen ounces of blood, and had him drenched with the antimonial powders.

Two *p. m.* He slept until half after three, when he awoke, with the disposition to bite, oppression, &c., but not so violent.

July 6th, eight *a. m.* Found him biting the bed-clothes; his countenance maniacal, his pulse synocha, with a stricture of the breast, difficult deglutition, laborious breathing, and a discharge of saliva. I took away twenty-four ounces of blood, gave him a dose of calomel and jalap, and continued the powders.

Twelve *m.* Drew sixteen ounces of blood, and gave him laudanum.

Five

Five *p. m.* Found him in a slumber; his skin moist, and his fever and other symptoms much abated.

July 7th, eight *a. m.* Was informed that he had only two paroxysms during my absence, and that he had lost sixteen ounces of blood agreeably to directions. Notwithstanding the favourable aspect which the disease wore, I resolved to bleed him twice more, and then to induce an artificial fever by mercury, which would predominate over the hydrophobic. I therefore drew ten ounces of blood, and requested his friend to take eighteen ounces at night; to rub in a small quantity of mercurial ointment, and to give a mercurial pill every four hours.

July 8th, nine *a. m.* Found him convalescent, but continued the mercurial unction and pills.

July 9th, ten *a. m.* Found his gums sore, and discontinued the mercury.

July 15th, one *p. m.* Found him well, but with a considerable degree of debility.

It would be doing injustice to you not to mention that I was indebted to your lectures for the successful treatment of this disease.

August 21, 1803.

To Dr. Burton.

DEAR SIR,

Accept of my congratulations upon your rare triumph over a case of hydrophobia. I give you great credit for the boldness of your practice. You have deserved well of the profession of medicine.

In order to render your communication more satisfactory, permit me to request your answer to the following questions:

1. On what part of the body of your patient was the wound inflicted; and how long was the interval between the time of his being bitten and the attack of his fever?

2. Did he discover any aversion from the sight of water? and did he refuse to swallow liquids of all kinds?

3. What were the appearances of the blood drawn? Did it differ in the different stages of the disease?

Your answer to the above questions will much oblige your sincere friend,

BENJAMIN RUSH.

Philadelphia,
August 29, 1803.

To Dr. Benjamin Rush.

SIR,

I regret that business of an indispensable nature prevented me from being more particular in my communication. I drew it up in a hurry, intending to transcribe it, and insert such other notes as would throw light on the case; but being called out a few hours before the post set out from this place, I was obliged to forward the communication in the manner in which you received it.

The part of the body of my patient on which the wound was inflicted was a little above the union of the solæus and gastrocnemius muscles, which form the tendo-achillis. The interval between the time of his being bitten and the attack of the fever was twenty-four days.

He was, I was told, dull and solitary a few days previous to the attack. A few minutes before it, his friends found him two hundred yards from the house, apparently in a deep study. He has informed me, since his recovery, that he had a slight pain in the wound, attended with itching, and an uneasiness in the inguinal gland, several days before the fever.

He refused to swallow liquids, and the sight of water threw him into a convulsive agitation.

With regard to the appearances of the blood drawn, I am sorry to inform you, that after it became cold I did not examine it.

I am, sir, yours, &c.

ROBERT BURTON.

Bent Creek, Virginia,
September 18, 1803.

XXXIX. *Hints respecting a speedy Decomposition of Water by Means of Galvanism.* By Mr. WILLIAM WILSON.

To Mr. Tilloch.

SIR, London, August 22, 1805.

AT a time like the present, when there is every appearance of some important discoveries in chemistry being made by the help of Galvanism, any experiment connected with this subject that is not generally known (and especially such as relate to the decomposition of water, and that in a more rapid manner than is usually done by Galvanism) must be acceptable to persons engaged in this branch of science. I therefore take the liberty of troubling you with the following

ing account of some experiments I made, about a year and a half ago, with a Galvanic trough containing fifty pair of plates four inches square. If you think it worthy of a place in your Philosophical Magazine, you will insert it therein.

Being desirous of ascertaining whether water would be decomposed or no, if the wires, which were connected with the ends of the trough, were at a considerable distance from one another, I inserted two short silver wires through corks into the ends of a glass tube 36 inches long, and which was filled with water: the ends of the wires were about 34 inches asunder, which distance was too great for any visible decomposition of the water to take place; yet that wire which was connected with the zinc end of the trough, gave a very faint whitish cloud which descended *. With a shorter tube the decomposition of the water commenced when the ends of the wires were at the distance of 18 inches. It then struck me, that if a wire was interposed between the two end wires of the long tube a decomposition might possibly be effected at two places in the tube at the same time, and that the quantity of gas evolved would be greater than if it was evolved at only one place. I therefore introduced a piece of iron wire between the end wires, in such a manner that its ends came within an inch of them. When a communication was made with the trough there was a very copious evolution of gas at both ends of the interposed wire, and at that end wire that was connected with the copper end of the trough; and a red oxide of iron was formed at one end of the iron wire, while a black oxide was formed at the other.

To try if any increased effect would take place if there was a greater surface of the wires opposed to one another, I pushed the end wires further into the tube till their ends passed about an inch beyond the ends of the interposed wire: when a communication was made between them and the ends of the trough, a very rapid evolution of gas took place throughout the whole extent of the parts of the wires that were opposed.

Finding the quantity of gas much increased by this management, I introduced a wire into each end of a tube about 16 inches long. Each of these wires passed nearly the whole length of the tube without touching one another, so that the length of the opposed parts was 1[illegible] inches. When these were connected with the trough, there was a very co-

* With a battery of troughs containing 400 pair of plates 4 inches square, the decomposition took place when the wires were withdrawn to the ends of the tube.

pious evolution of gas through the whole length of the tube. The wires used in this were iron, and the red and black oxides were formed in considerable quantity.

Seeing the quantity of gas evolved with a given power is in proportion to the quantity of surface of the wires opposed, many contrivances might be used to increase the effect to a considerable degree. If thin plates of metal were used instead of wires, a greater surface would be opposed, and in all probability the effect would be increased. Several wires or plates might be arranged in the same tube, and alternately connected with the ends of the trough; or, if wire cloth was used instead of plates, probably the effect would be still further increased.

I am, sir, your obedient humble servant,
WILLIAM WILSON.

XL. *New Observations on Volcanoes and their Lava.* By G. A. DELUC*.

VOLCANOES have been so numerous on the surface of our continents, when they were under the waters of the antient sea; and as this class of mountains, raised by subterranean fires, manifest themselves still on the shores of the present sea, and in the middle of its waters, it is of importance to geology and the philosophy of the earth to obtain as just ideas of them as possible.

I have attended a great deal to this subject from my own observations; and I have shown, at different times, the errors into which several geologists and naturalists, in treating of it, have fallen.

This class of mountains, in particular, requires that we should see them, that we should behold them during their eruptions, that we should have traced the progress of their lava, and have observed closely their explosions; that we should have made a numerous collection of the matters which they throw up under their different circumstances, that we might afterwards be able to study them in the cabinet, and to judge of their composition according to the phænomena which have been observed on the spot.

This study is highly necessary when we apply to geology and the philosophy of the earth, in order that we may avoid falling into those mistakes which make us ascribe to subterranean fires what does not belong to them, or which leads us to refuse them what really belongs to them.

* From *Journal de Mines*, Thermidor, An. xii. No. 95.

We

We read in the *Journal de Physique* for January, 1804, under the title, *On the cause of Volcanoes,* the following assertions:

"What is the nature of the matters which maintain these subterranean fires? We have seen that Chimboraço, all these enormous volcanoes of Peru, and the Peak of Teneriffe, are composed of porphyry.

"The Puy-de-Dôme is also composed of porphyry, as well as the Mont-d'Or and the Cantal.

"Ætna, Solfatara, and Vesuvius, are also of the porphyry kind.

"These facts prove that the most considerable volcanoes with which we are acquainted are of porphyry."

This opinion, that the fires of volcanoes have their centres in such or such a rock, and that their lavas are produced from these rocks, has always appeared to me not to be founded on any certain data. Opinions also on this subject have varied; some having placed the origin of lava in horn rock, others in granite or schist, and at present it is assigned to porphyry.

I have always been of opinion that nothing certain could be determined in regard to this point. It ever remains uncertain whether the seat of the matters of which lava is formed be in compact rocks, or in strata in the state of softness, pulverulent, and muddy.

Those who see lava issue from a volcano in its state of fusion and incandescence, and in its cooling, are convinced that the nature of every thing is changed, that it exhibits a paste in which nothing can be known, except the substances which the volcanic fires have not reduced to fusion.

But these substances contained in the paste of lava, and those which are the most numerous, show us, that the strata from which they proceed cannot be similar to those exposed to the view, nor even to the most profound strata to which we can penetrate.

The schorl of volcanoes, which was named *augite,* and then *pyroxene,* an octaëdral prism with two biëdral pyramids, is not found in the strata with which we are acquainted; and the case is the same with the leucite or white garnet, a crystallization of a round form, with twenty-four trapezoidal faces. And these crystals, which are observed perfectly insulated in lava, are found there also, united in groups, which are likewise insulated, having no marks of former adhesion.

Here then we have two species of crystals exceedingly numerous in several kinds of lava. Those of Ætna are

 filled

filled with schorl; and those of Vesuvius, particularly the antient, contain schorl and leucites in great numbers*.

I shall make no mention of other substances, such as chrysolites and olivins, because their form is not sufficiently determined to enable us to decide whether they are found or not in the exterior strata.

It is not the lava of Vesuvius and Ætna alone which contains one or other of these crystals, or both of them together. Most of the lavas of the antient volcanoes in the neighbourhood of Rome are filled with myriads of leucites. Several of the lavas of the Brisgau contain schorls in great quantity. The gravel of the volcanic lake of Andernach is filled with them. They are found in the basaltes of the circle of Lewtomeritz in Bohemia, and in the scoriæ of the crater of Puy-de-la-Vache in Auvergne. I mention only the lavas of which I possess specimens, most of them collected by myself on the spot, or which were sent to me by my brother, who collected them in his excursions to the old volcanoes of Germany.

Are these two crystals so numerous in lava, the schorls of volcanoes, and leucites, found in any porphyry, granite, or horn rock? They are not found there: the question then is decided; lavas do not derive their origin from porphyry, nor from the two other rocks.

What, in all probability, has led to the contrary opinion, is the appearance of several kinds of lava, which, by the insulated substances they contain, have a porphyroid appearance, though they are not porphyritic.

Leucite is said to have been found:—Is this crystal, of a round form, with twenty-four trapezoidal faces, really that substance? If it is, in what kind of rock was it found? Is it found there by myriads, as in lava? Were this the case, must it not have been long since known? And if it be found only rarely, it is only an exception of very little consequence, compared with the grand fact presented by lava.

I have said that it is uncertain whether lava proceeds from solid rocks, or strata still in the state of softness, pulverulent,

* The biëdral pyramids of schorl are subject to several varieties, but never to that of the prism, which has always eight faces: these faces vary in their size like those of rock crystal. Some are frequently seen which have two opposite faces broader than the rest; a variety which is observed also in the prisms of rock crystal. These perhaps are modifications which have made these prisms be considered by Dolomieu and Spallanzani as hexaëdra: they are certainly as much octaëdral as rock crystal is hexaëdral, and the rose feld-spar of Baveno tetraëdral.

I have in my possession a leucite which exhibits a very singular accident. It is united to a schorl, one part of the length of which it embraces. This union has produced an elongation of the leucite to embrace the schorl.

lent, and muddy. When we reflect, indeed, that these crystals, the schorls of volcanoes, and leucites, are found in such great number in their paste, all insulated, and without bearing any marks of adhesion to any rock; when we consider also, that these schorls are found insulated one by one in myriads, mixed with the small scoriæ, thrown up by the mouths which vomited forth the enormous lava of Ætna in 1669; that this lava itself is filled with it,—it is not easy to conceive how they could all be contained in a solid rock. It is still more difficult to conceive that fires capable of fusing granite, horn rock, and porphyry, should spare schorls, leucites, and some other substances, which are fused and reduced to glass in our furnaces.

The volcanic mountain of Viterbo exhibits lavas where the leucites are so near each other that they occupy between them more space than the paste of the lava which contains them.

The lava of Ætna contains, besides schorls and some olivins, a multitude of crystalline laminæ, whitish, and semi-transparent. They are named without hesitation *feld-spar*, which appears to me not so certain as is supposed.

These laminæ are two or three lines in breadth, and about half a line in thickness. They are found also separated from each other, mixed with the schorls and the small scoriæ of Mount Rosso, or the crater of 1669. In the bed of a rivulet which runs down from Mount Ætna I found rolled fragments of old very black lava, which contained some of these laminæ in as great quantity as any marble can contain fragments of shells. It would be very extraordinary if these laminæ proceeded from feld-spars, such as those with which we are acquainted, and that they should not be found mixed with any fragment larger or better determined, which might indicate in a certain manner that origin.

Admitting the hypothesis, that the strata from which the lavas proceed are in a pulverulent and muddy state, containing elements of all these small crystals, one may conceive how they are formed there, insulated, grouped, or solitary, and are found then in the lava in that state of insulation.

The fragments of natural rocks thrown up by Vesuvius are not of the same kind as the matters of which the lava is composed. Most of these fragments are micaceous rocks, with laminæ of greater or less size, and of a kind of granite called *sienite*. I have found some composed of white

white quartzy rock; it is found sometimes of calcareous rock.

The most probable idea that can be formed in regard to the origin of these fragments is, that they have been carried from the borders of the strata through which the lava, that comes from great depths, has opened for itself a passage. These fragments are carried to the surface of the lava as far as the bottom of the chimney of the crater, whence they have been thrown out by explosions, mixed with fragments separated, or rather torn, from the lava; for it is not by the lava that they have been brought forth to view, but by explosions.

Some of these fragments of natural rocks have not been attacked by the fire; others have more or less; which depends, no doubt, on the place which they occupied in the volcano, and on the time which they remained in it. The most of the latter have retained at their surface a crust of lava, and this crust contains substances which are not the same as that of the fragment it covers.

On Vesuvius the strata pierced by eruptions are lower than the surface of the soil; in Auvergne and several places of Germany they are above; for this reason there are seen there in their place schists or granites, which the eruptions have broken to form for themselves a passage.

No volcano rests on natural strata; they sometimes show themselves on the exterior; but they have been opened by eruptions, and their edges have remained in their place.

The focus of no volcano exists or has existed in the cone which appears above the surface of the ground. They have been raised by eruptions, which, proceeding from great depths, have thrown them up through the upper strata. When it is said, therefore, that the volcanic mountains of Auvergne rest on granite, this is a mistake, and an incorrect expression has been used by those who have not formed a just idea of the phænomenon. Lava may have flowed upon granite or any other rock, and rested upon it; but this is never the case with the volcano itself: its bases are below all the rocks visible.

It is from the bosom even of the lava, when in a state of fusion in the interior of the volcano, that all the explosions proceed. In that state of fusion they contain all the matters which produce fermentations, and the disengagement of expansible fluids.

I have been enabled to ascertain this on Vesuvius as far as was possible. The continual noise which was heard through the two interior mouths of the crater which I had before

before my eyes, was that of an ebullition, accompanied with inflammable vapours, and the gerbes of burning matters which they threw up at intervals were separated pieces of the lava in its state of fusion. I saw several of them in the air change their form, and sometimes become flat on the bodies which they struck or embraced in falling. And among the most apparent of these fragments there are always a multitude of small ones of the size of peas and nuts, and still smaller ones, which show at their surface, by their asperities, all the characters of laceration.

The name of *scoriæ* has been given to these fragments to distinguish them from compact lava, though their composition be the same as that of the hardest lava; and it is for want of reflecting properly on this point that it has been said that it is the compact part only that we must observe, in order to judge of their nature. The pieces which I took from the flowing lava with an iron hook, have at their surface the same lacerations and the same asperities as the fragments thrown up by explosions, and both contain the same substances.

This separation, by tearing off the parcels of the lava effected by fermentations and explosions which proceed from their bosom, serves to explain those columns, sometimes prodigious, of volcanic sand which rise from the principal crater. When seen with a magnifying glass, this sand exhibits nothing but lava reduced very small, the particles of which, rough with inequalities, have the bright black colour and the varnish of recent lava.

Parcels of substances which exist in our strata, such as fragments of quartz, scales of mica, and crystals of feldspar, are found sometimes in lava. Similar matters must no doubt be disseminated in the composition of our globe, without there being reason to conclude that the strata from which they proceed are the same as the exterior strata. It is neither in the granites, the porphyries, nor the horn rock, and still less in the schists and calcareous rocks that the schorls of volcanoes, the leucites, and perhaps olivins, will be found. These small crystals are brought to view by the lava, otherwise they would be unknown to us.

These lavas contain a great deal of iron, which they acquire neither from the granite nor porphyries. Might not one see in the ferruginous sand which is found in abundance on the borders of the sea near Naples, and in the environs of Rome, specimens of that kind of pulverulent strata from which lava proceeds?

I have here offered enough to prove that it cannot be determined that lava proceeds from strata similar to those with which we are acquainted. The operations of volcanoes, those vast laboratories of nature, will always remain unknown to us, and on this subject our conjectures will always be very uncertain.

What is the nature of that mixture which gives birth to these eruptions, that produce lava and throw up mountains? What we observe as *certain* is, that the introduction of the water of the sea is necessary to excite these fermentations, as containing marine acid and other salts, which, united to the sulphuric acid, the bases of which are contained in abundance in the subterranean strata, determine these fermentations, which produce the disengagement of fire and other fluids, and all the grand effects that are the consequence.

Several naturalists have believed, and still believe, that fresh or rain water is sufficient for this purpose; but they are mistaken: this opinion is contradicted by every fact known. To be convinced of this, nothing is necessary but to take a short view of them. I have done it several times, as it is necessary to consider them often. I shall here enumerate the principal ones:—No burning mountain exists in the interior part of the earth; and all those which still burn are, without exception, in the neighbourhood of the sea, or surrounded by its waters. Among the deliquescent salts deposited by the smoke of volcanoes, we distinguish chiefly the marine salt, united to different bases. Several of the volcanoes of Iceland, and Heckla itself, sometimes throw up eruptions of water, which deposit marine salt in abundance. No extent of fresh water, however vast, gives birth to a volcano. These facts are sufficient to prove that the concurrence of sea-water is absolutely necessary to excite those fermentations which produce volcanoes.

I shall here repeat the distinction I have already made between burnt out volcanoes and the antient volcanoes, that I may range them in two separate classes.

When we simply give the name of *burnt out* or *extinguished* volcanoes to volcanic mountains which are in the middle of the continents, it is to represent them as having burnt while the land was dry, and inhabited as it is at present; which is not a just idea. These volcanoes have burnt when the land on which they are raised was under the waters of the antient sea, and none of them have burnt since our continents became dry. It is even very apparent that most of them were extinct before the retreat of the sea, as we find by numerous examples in the present sea.

Those

Those which I denominate extinct volcanoes are such as no longer burn, though surrounded by the sea, or placed on the borders of it. They would still burn, were not the inflammable matters by which they were raised, really exhausted and consumed. Of this kind is the volcano of Agde, in Languedoc. Of this kind also are many of the volcanic islands which have not thrown up fire since time immemorial.

M. Humboldt, in his letters written from Peru, speaks of the volcanoes which he visited, but what he says is not sufficiently precise to enable us to form a just idea of them. He represents Chimboraço as being composed of porphyry from its bottom to its summit, and adds, that the porphyry is 1900 toises in thickness; afterwards, he remarks, that it is almost improbable that Chimboraço, as well as Picchincha and Antisana, should be of a volcanic nature: "The place by which we ascended," says he, "is composed of burnt and scorified rock, mixed with pumice-stone, which resembles all the currents of lava in this country."

Here are two characters very different. If Chimboraço be porphyry from the top to the bottom, it is not composed of burnt and scorified rocks, mixed with pumice-stone; and if it be composed of burnt rocks, it cannot be porphyry. This expression, *burnt and scorified rocks*, is not even exact, because it excites the idea of natural rocks, altered in their place by fire, and they are certainly lava which have been thrown up by the volcano. But the truth must be, that Chimboraço, and all the other volcanoes of Peru, are composed of volcanic matters, from their base at the level of the sea to the summit.

I have just read in the *Annales du Muséum d'Histoire Naturelle**, a letter of the same traveller, written from Mexico, on his return from Peru, where, speaking of the volcanoes of Popayan, Pasto, Quito, and the other parts of the Andes, he says, "Great masses of this fossil (*obsidian*) have issued from the craters; and the sides of these gulphs, which we closely examined, consist of porphyry, the base of which holds a mean between obsidian and pitch-stone (*pechstein*)." M. Humboldt therefore considers obsidian, or black compact glass, as a natural fossil or rock, and not as volcanic glass.

Father de la Torre, who resided at Naples, and has written on Vesuvius, believed also that the interior of its mouth was composed of natural rocks and strata like every other

* No. 17.

mountain:

mountain: he calls them *strati naturali, sassi naturali*, though every thing there be the work of fire.

If M. Humboldt had been a witness to the birth and formation of the craters of which he speaks, he would soon have given them up entirely to the volcanic empire. The violence of the fire; the explosions and burning lava with which that empire would have reclaimed them, would soon have silenced all Neptunian pretension, and confirmed that these masses, which he calls *porphyry*, and their bases, holding a medium between obsidian and pitch-stone, are lava and vitrifications belonging to Vulcan. M. Humboldt derives his objection against the opinion that obsidian is volcanic glass, from its swelling up and becoming spongy and fibrous by the least degree of heat of a furnace, whence he concludes that it cannot be the production of fire.

An attentive examination of volcanic productions shows that their state and appearance depend on the nature of the matters which have been subjected to the action of the fires, on the degree of heat, the time and place where it has been exercised. Therefore a degree of heat which has been able to reduce any substance to compact glass, would not be sufficient to put it into a state of ebullition, and at that moment could not be carried to a degree capable of producing that effect: to this the want of free air may contribute. But there are some circumstances, even pretty frequent, of volcanic fires giving fibrous and puffed-up glass. I possess a vitrification from Lipari, the centre of which is compact glass, and the inside in laminæ, bubbles, and threads, like pumice-stone. I have in my possession another, part of which is glass nearly compact, and part glass very much puffed up. I found on the sea-shore, near Messina, two pieces of four or five inches in diameter, formed merely of vitreous laminæ, elongated, undulated, and full of puffed up places. I have two fragments of obsidian, or black compact glass of Ischia, one of the entire faces of which evidently shows by the circular undulations of the one, and the rounded inequalities of the other, that they have been in a state of fusion. I saw at Vulcano a vitreous mass, from which I broke a large fragment, the glass of which is compact in some parts, and full of puffed up places, some of them large and others small. Of this kind is the black compact glass of the volcanoes of Iceland.

Another objection of M. Humboldt is, that obsidian is found in such large masses that it may be compared to a quarry. But why should this be an objection? Vitreous lava does not differ from any other lava, but by more perfect

vitrification;

vitrification; and in regard to the size of the masses, it may be said that it has no bounds, since Ætna, a volcano much less considerable than those of Peru, throws up lava several leagues in extent, and of a very great thickness.

Obsidian, therefore, or the black glass of the volcanoes of Peru, is as certainly a production of their fires as the lava which is seen to issue from the bottom of every crater.

[To be continued.]

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

In the sitting of 25th April last there was read an interesting paper on an artificial substance possessing the principal characteristic properties of tannin, by Charles Hatchett, Esq. a member of the society.

The author, after mentioning the labours of Mr. Deyeux and Mr. Seguin, the former of whom first separated this substance from galls, the characteristic property of which, to precipitate gelatin from water, was ascertained by the latter; and after mentioning the experiments of Mr. Biggin, Mr. Proust, and Mr. Davy, remarks that no one had hitherto supposed that it could be produced by art, unless the fact mentioned by Mr. Chenevix, that "a decoction of coffee berries did not precipitate gelatine, unless they had been previously roasted," might be considered as an indication of it. Recent experiments have, however, shown him that tannin may be formed by very simple means, not only from vegetable, but from mineral and animal substances.

In the course of his experiments on lac and resins, he observed the powerful effects of nitric acid on these substances, and has since observed that by long digestion almost every species of resin is dissolved, and so completely altered that water does not cause any precipitation, and that by evaporation a deep yellow viscid substance is obtained, equally soluble in water and in alcohol. In his experiments afterwards, on the bitumens, he observed a material difference between their solutions and those of resins. With bitumens, nitric acid, by long digestion, formed a dark brown solution; a deep yellow coloured mass was separated, which, by subsequent digestion in another portion of nitric acid, was completely dissolved, and, by evaporation,

tion, was converted into a yellow viscid substance similar to that obtained from the resins. Mr. Hatchett therefore concluded that the dark brown solution was formed by the action of the nitric acid on the uncombined carbon of the bitumens; that the deep yellow portion constituted the essential part of the bitumens; and therefore that the dark solution was in fact dissolved coal. He accordingly tried pit coal, and, by a similar treatment, obtained the same dark brown solution in great abundance, but not when he used coals which contained little or no bitumen.

Having by means of nitric acid obtained solutions from asphaltum, from jet, pit coals, and charcoal, he evaporated each to dryness, very slowly, to expel the remaining acid without burning the residuum, which in each was a glossy brown substance, exhibiting a resinous fracture, soluble in water and in alcohol, and highly astringent. Exposed to heat, they smoked but little, swelled, and yielded a bulky coal. Their solutions reddened litmus paper, and precipitated various metallic and earthy salts, and also glue or isinglass, yielding a precipitate insoluble in water, either hot or cold—and consequently possessing all the properties of tannin, uncontaminated with gallic acid.

Mr. Hatchett then reduced some animal substances to the state of charcoal, and by a treatment similar to the above obtained from them tannin.

Some kinds of coal, which in their natural state yielded little or no tannin, on being brought to a red heat in a close vessel, and then digested with nitric acid, were almost wholly converted into that substance. The result was the same with various kinds of wood;—when charred they yielded a great quantity of tannin, though before undergoing that process they would yield none.

This ingenious paper contains other interesting details on matters connected with this subject, all tending to show that different substances yielded tannin in proportion to the quantity of their original carbon, and that substances reduced into coal in the humid way (as by the action of sulphuric acid) in like maner yield the tanning substance by nitric acid; but we shall not enter more into this detail till the paper itself be published.

On the 4th of July was read a paper by W. Hyde Wollaston, M. D. on the discovery of palladium; with observations on other substances found with platina.

The author, having purified a great quantity of platina by precipitation, had an opportunity of examining the various impurities usually mixed with the ore. This

This led him to the discovery of the new metal which he named *rhodium*, and also to the discovery which forms the principal subject of this paper. He mentions also having found blended with platina the ore of another metal, which has hitherto passed unobserved from its great resemblance to the grains of that metal. These grains he considers as the *ore of iridium*, the new metal discovered by Mr. Tennant. They are insoluble in nitro-muriatic acid, are harder than platina, brittle under the hammer, and break with a laminated fracture. Mr. Tennant has undertaken the analysis of a portion of this ore.

The author mentions having separated from the ore of platina, by a current of water, some very minute red crystals, the quantity of which was too small to admit of analysis; but from such an examination as he could give them he concludes them to be hyacinths.

Having separated these and other impurities from the ore of platina, as far as practicable by mechanical means, dissolved the ore, and obtained, in the form of a yellow triple salt, all the platina that could be precipitated by sal-ammoniac, clean bars of iron were used to separate the remaining platina. This precipitate, consisting of various metals, was subjected again to exactly the same treatment, when the precipitate obtained by sal-ammoniac was found to be not of so pale a yellow as before: bars of iron were also again used to precipitate what remained suspended. A repetition of the same process on this second precipitate led to the discovery of palladium; for Dr. Wollaston found that a portion of it resisted the action of the nitro-muriatic acid, though this powder had been twice completely dissolved before. The solution was very dark in colour, yielded by sal-ammoniac only a small quantity of precipitate, and, instead of becoming pale by the precipitation of the platina, retained the dark colour which it had acquired from the other metals held in solution. The second metallic precipitate, therefore, became the subject of investigation. Lead, iron, and copper, were detected in it by muriatic acid. Dilute nitrous acid separated a further portion of copper, forming, as usual, a blue solution; but when a stronger acid was used for the purpose of separating the remaining copper, the dark brown colour of the solution gave evidence that some other metal had been dissolved. A small portion of the solution was put on a surface of platina; and on applying a clean plate of copper a black precipitate was obtained, soluble in nitric acid, and consequently neither gold nor platina. The solution in that acid was red, therefore the metal was neither silver nor mercury; and having been

 precipitated

precipitated by copper, it was none of the other known metals. Mercury, agitated in a warm nitrous solution of this metal, acquired the consistence of an amalgam, which when exposed to a red heat left a white metal—palladium; which gave a red solution, as before, with nitrous acid, could not be precipitated by sal-ammoniac or by nitre, but yielded a yellow precipitate with prussiate of potash, and in the order of its affinities was precipitated by mercury, but not by silver. The author, however, adopted afterwards another process for obtaining palladium, depending on one of its most distinguishing properties, by means of which it may be obtained with the utmost facility. To a solution of crude platina, whether neutralized by evaporation of the redundant acid, or saturated by any of the alkalies, by lime or by magnesia, by mercury, copper, or iron, and also whether the platina has or has not been precipitated from it by sal-ammoniac, it is only necessary for the separation of the palladium that prussiate of mercury be added to the solution. In a short time it becomes turbid, and a flocculent precipitate is gradually formed of a pale yellowish white colour. The prussiate of palladium thus obtained, when heated, yields that metal in a pure state.

FRENCH NATIONAL INSTITUTE.

On the 23d of June the first class of the French National Institute, that of the Mathematical and Physical Sciences, held a public sitting, when the following papers were read:

1st, Chaptal's report on the prize question respecting the winter sleep of animals.

2d, Delambre's eulogy of P. F. A. Mechain.

3d, Memoir of Pinel on the treatment of lunatics in a large hospital, and on the result of three years' experience at the Salpetriere.

4th, Memoir on the terrestrial magnetism, by Biot.

5th, Jussieu's account respecting the last voyage of discovery.

6th, Cuvier's eulogy on Dr. Priestley.

The prizes also were adjudged for the papers on the last prize questions.

An Account of the Labours of the Class of the Mathematical and Physical Sciences of the French National Institute from the 20th of June 1804 *to the same Day* 1805. *By M.* Cuvier, *perpetual Secretary.*

[Continued from p. 178.]

We have in commerce three kinds of strong glue, those of England, Flanders, and Paris. The first is the best, and

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the third the worst. M. Seguin, having accurately compared their degrees of goodness, that is to say, of tenacity; has examined the difference of their chemical principles. He has always found in the glue of Paris an insoluble mixture of gelatine and calcareous soap, which is deposited when the glue is dissolved: in that of Flanders, a coagulated albumen, which deposits itself also: that of England alone is free from this mixture and deposit. Nothing remained but to discover a sure method of making glue similar to that of England.

M. Seguin first saw, that of all the animal parts capable of giving glue, skins furnish the best, and particularly the skins of adult animals killed by the butchers. He then saw that every thing depends on the method of freeing them from the hair. The worst glue is produced by skins freed from the hair by lime; that of skins freed from the hair by alkalies is a little better; but the most tenacious is obtained from skins freed from the hair by gallin, and particularly by the successive action of gallin and diluted sulphuric acid. But gallin is rare, and too dear for such an application. M. Seguin, therefore, did not obtain a complete solution of the problem which he proposed, but by finding out a substitute for gallin, which is moistened malt.

The name of *degras* is given to a matter employed for currying leather, and which is obtained in the preparation of shammy leather. There are two kinds of it; that of the *country* and that of *Niort*, which is better and dearer than the former. The *degras* of the country, according to Seguin, is composed of oxygenated oil, soap, and gelatin in particular states. The two latter principles hurt its effect. The *degras* of Niort contains none of these substances, and is only oil in a certain state. M. Seguin imitates it, the colour excepted, by treating oils with nitric acid; and the product he obtains proves a substitute, at less expense, for the *degras* of Niort.

M. Sage has shown us some singular products of the chemical art in foreign countries. The Chinese make furnaces which are as light as pasteboard, and which are incombustible, because they are made of amianthus. The same nation employ zinc for coin, a semi-metal which did not seem proper for such a purpose. The same chemist continues, with indefatigable ardour, to describe those objects interesting to geology which are contained in his collection. He has shown us this year several curious fossils belonging, for the most part, to the class of shells, such as terebratulæ, orthoceratites, nummularia, &c.

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The voyage of M. Peron, among the infinite number of interesting objects it has procured, has furnished us with two proper for throwing light on the history of these fossils. The shell called by naturalists *nautilus spirula* was among those still found alive the nearest to the *cornua ammonis*, the spiral camerines, and nummularia. M. Peron brought home the animal, and we have seen that it is not contained in the shell, but, on the contrary, that it contains the shell, as the cuttle-fish contains its bone. This animal belongs, therefore, to the genus of the cuttle-fish. It gives us reason to believe that the *cornua ammonis* and nummularia belong to it also, and he explains every thing that remained embarrassing on this subject. The same traveller has brought back also an animal near a-kin to the medusæ, which contains in its inside a cartilaginous disk entirely analogous in its structure to the concentric nummularia. M. Sage observed in a piece of coal the impression of a disk, which must have resembled that of this medusa still more than these nummularia themselves.

M. Cuvier, who has made known to the class these two results of M. Peron's collections, presented to it also two facts interesting to geology, discovered by himself.

The first is, that among the numerous animals of unknown genera with the remains of which the plaster quarries in the neighbourhood of Paris are filled, there is found a kind of opossum, a genus still existing, but only in the new continent: the other is, that the remains of a hyæna, very similar to that of the Cape of Good Hope, are scattered throughout the earth in different parts of France and Germany.

M. Desmarets has contributed also to extend this curious part of the natural history of animals known only by their remains. He has presented to us two sorts of fossil shells of Angoumois hitherto unknown to naturalists: he has read to us also a treatise on the different sorts of vegetable earth, their characters, and their origin. We have had likewise in mineralogy a Description of Guadaloupe by M. l'Escalier. This island is in part volcanic and in part madreporic.

M. Humboldt has given us a view of the geologic composition of the heights of the Cordilleras. M. Ramond has added new observations to those which he before made on the Pyrenees.

M. Lelievre has taught us that the kind of mineral called *pinite* has been discovered in France by Cordier, who found it in minerals collected in the environs of Clermont in Au-

vergne,

vergne, by M. Lecoq, commissioner of gunpowder. Hitherto it has been found only in Saxony.

Botany continues to be enriched with an increasing number of new species. The superb work on the *Jardin de Malmaison*, by M. Ventenat; the *Flora of the Oware of Benin*, by M. de Beauvois; that of the *Isles de France et de Bourbon*, by M. du Petit Thouars; that of New Holland, by M. de Bellardiere, are prosecuted with success. Messrs. de Humboldt and Bonpland have published the first number of that of *South America*.

M. Desfontaines has published a catalogue of all the vegetables in the *Jardin des Plantes*; a valuable work, not only for those who frequent that celebrated school, but also for all botanists. M. Broussonet has also given that of the *Jardin de Montpellier*.

Botany for a long time has been accustomed to honour those who cultivate and patronize it, by giving their names to the new genera it discovers; and experience has proved that such monuments are the most durable of all.

No person deserved this honour more than the empress, who takes so much pleasure in that agreeable science, and who promotes its progress so much. The Spanish botanists, Messrs. Ruiz and Pavon, had already paid her this honour by giving the name of her family to a beautiful plant of South America.

M. Ventenat, charged by her majesty with making known to the public all the new species of the garden of Malmaison, has consecrated to her a second, the Josephina, originally from New Holland, and near a-kin to the *digitalia* and the *pedalia*. The elevation of its stem and the beauty of its flowers will make it be cultivated in pleasure-gardens.

M. de Beauvois has dedicated to the emperor Napoleon a tree of the country of Oware, in Africa, distinguished by its splendour, and the size and singularity of its flower.

M. de Humboldt during his travels enriched the natural history of plants with general and very new considerations: he has traced out a sort of geography of them, in which he determines the limits of each species in latitude and in vertical height: it is the temperature which stops them in both directions; but as the degrees which suit each are different, they extend more in breadth, or rise higher, on the mountains, according to this difference; which may serve as a sure guide to agriculture in the choice of the plants which it destines to each position.

This indefatigable traveller has enriched no less the history of animals. He has described several new species, among which we have to remark in particular one of the fish thrown up sometimes by the volcanoes in South America. Do they live in subterranean lakes which have a communication with the sea?

M. Peron has communicated to us two observations exceedingly valuable in regard to the natural history of man. The first relates to the celebrated apron of the Hottentot women; denied by some, and differently described by others. M. Peron proves that it is a natural excrescence, which forms one of the characters of a particular race known under the name of the Boschmen. The other observation relates to the strength of the savages. A number of experiments, made by Regnier's dynamometer, has shown that they are sensibly weaker, *cæteris paribus*, than the people of civilized nations.

We long ago announced the celebrated work of Berthollet entitled *Statique Chimique*. M. Pinel has written another, the title of which is *Statique Anatomique*. We have another of the same kind written in the century before last by the celebrated Borelli; but mechanics and anatomy have both since that period made so much progress, that Borelli's work, excellent for its time, is at present superannuated.

A particular object of anatomy, namely, the teeth, has been long studied by M. Tenon. This profound anatomist has made so many discoveries on this subject, that it is more indebted to him than to any person who preceded him. He has lately read to us a memoir on a substance peculiar to the teeth of certain herbivorous animals, such as the horse and elephant, which envelops the enamel.

The same physician has communicated to us a great work on diseases of the eyes, which he will soon publish, and another on the diseases peculiar to hatters. The latter arise chiefly from the use which hatters make of mercury to render common hair fit to be manufactured into felt, since we were deprived of beaver skins by the loss of Canada. These diseases cannot be prevented but by weakening as much as possible the mercurial liquor employed, or by endeavouring to discover some other kind of hair which may be converted into felt without that liquor.

M. Tenon has read to us also several memoirs on surgery, concerning the different methods hitherto used to prevent or correct those accidents which are inseparable from our nature. Of this kind are instruments proper for extirpating polypes of the nose; and a method, by compression, of stopping hæmorrhages of the mouth.

[To be continued.]

MEDICAL AND CHIRURGICAL SOCIETY OF LONDON.

An institution has been lately established in London for the purpose of promoting a liberal and useful intercourse among the different branches of the medical profession, and of affording a centre for the reception of communications, and for the formation of a select and extensive professional library. It is called the *Medical and Chirurgical Society of London*, and it comprises in it a considerable number of professional men of the first character. The meetings (which will commence in October) will be held at the Society's apartments, Verulam Buildings, Gray's Inn, where any communications, or donations of books, are requested to be sent, directed to the Secretaries.

The following is a list of the officers and council for the present year:

President—Wm. Saunders, M.D. F.R.S.

John Abernethy, esq. F.R.S. vice-pres.
Charles Rochemont Aikin, esq. sec.
Wm. Babington, M.D. F.R.S. vice-pres.
Mattthew Baillie, M.D. F.R.S.
Thos. Bateman, M.D. F.L.S.
Gilbert Blane, M.D. F.R.S.
Sir Wm. Blizard, F.R.S. vice-pres.
John Cooke, M.D. F.A.S. vice-pres.
Astley Cooper, esq. F.R.S. treas.
James Curry, M.D. F.A.S.
Sir Walter Farquhar, bart. M.D.
Thompson Forster, esq.
Algernon Frampton, M.D.
John Heaviside, esq. F.R.S.
Alex. Marcet, M.D. foreign secretary.
Dav. Pitcairne, M.D. F.R.S.
Hen. Revell Reynolds, M.D. F.R.S.
H. Leigh Thomas, esq.
James Wilson, esq. F.R.S.
John Yelloly, M.D. secretary.

XLII. *Intelligence and Miscellaneous Articles.*

VACCINE INOCULATION.

The *Allgemeine Literatur-Zeitung*, of 24th of July, remarks that "it has been shown in some of the German publications on the cow-pock, that this method of inoculation was known in Germany before it was recommended by Dr. Jenner; and the case appears to be the same in France. M. Audouard of Castres, now at Paris, secretary to the society of the practice of medicine at Montpellier, has discovered that the method of vaccine inoculation was practised

practised in some of the departments of France long before the attempts of Dr. Jenner. M. Audouard will publish a work on this subject, in which it is expected he will give proofs that this method belongs originally to France." On this we may observe, that we have no doubt of the fact, for it was also practised in England by solitary individuals; but this does not lessen at all the merit due to Dr Jenner, who was the first to point out the proper use that might be made of the fact; nor of Drs. Pearson, Woodville, and others, who seconded his views, and have laboured with such incessant zeal to promote its general adoption, not only in this country, but in every part of the world—and with so much success, that at no very distant period we may expect to see the small-pox, the most dreadful scourge with which the human race were ever afflicted, banished from the face of the earth.

Ragusa, June 30th.

The vaccine has at length been adopted at this place, owing to the exertions of that indefatigable physician Stulli, and the repeated instigations of Dr. de Carro of Vienna, to whom we are indebted for this benefit. Dr. de Carro's *Catechism on the Vaccine*, translated into the Illyric language, and distributed in the town, and by the country clergymen, has contributed greatly to dispose the inhabitants for receiving it. The vaccine matter of Vienna has succeeded very well: in the course of a few weeks Stulli has vaccinated a hundred children; a great many for this country, which in some late years, and particularly in 1802, lost a great many children by the natural small-pox. This happy discovery is making great progress among the Dalmatians and Turks.

Manheim, July 28th.

According to the last accounts received by Dr. de Carro, at Vienna, respecting the progress of vaccination in the East Indies, the governor, by a public notice, dated January 19, 1803, requested the Europeans and natives belonging to the presidency of Fort St. George to take advantage of this salutary discovery. It results from the official reports of the board of medicine, that from the 1st of September 1802 to the 30th of April 1804, there were inoculated with success 145,840 persons, namely 165 Europeans, 4,141 Bramins, 41,806 Malabarese, 40,022 Gentoos, 10,926 Mahometans, 444 of mixed casts, 1,092 Portuguese, 35,975 Parias, 440 Mahrattas, 10,367 Canaputes, 462 Rajaputes. The

The raja of Tanjore encourages vaccination as much as possible, and the dewar of Travancore has himself submitted to it.

PROPOSAL FOR ENCOURAGING THE PRACTICE OF VACCINATION.

The great number of persons who have taken the small-pox in the present year shows plainly that so many have not been vaccinated as hath been represented. Indeed, we have long more than suspected that accounts given by many practitioners to the public of the number inoculated by each of them was not exact. A practitioner is too apt to assume consequence from the long list he produces, and this it is makes him apt to plume himself upon the credit given to him for the number rather than for the accuracy and novelty of his observations; or it induces him to strive to swell his list of number, rather than to bestow the labour of observation. Hence too it has happened that many persons have taken the small-pox subsequently to the practice of such inexact inoculators for the cow-pock. It is decisive that the numbers given to the public are exaggerated, not only for the reason just set forth, but because the total united sum from the different lists exceeds the sum on any reasonable calculation which the population affords to be inoculated. If we remove a cypher from the figures containing the numbers asserted, the sum remaining will be less remote from the most accurate calculation of the real number vaccinated: for instance, in some statements, instead of 10,000 say 1,000; for 5,000 say 500, and so in proportion.

The late prevalence of the small-pox has not only led to the above remarks, but to the consideration of the means of rendering vaccination effectual for extinguishing the small-pox. Supposing that when the cow-pock is duly excited, a person has as great a chance of security as after due inoculation for the small-pox, it was announced at a public meeting—the annual one of the original Vaccine Pock Institution, Broad Street, on the 7th February 1803,—that a plan would be brought forward to show the necessity of laws for the inoculation of every subject within a certain period after birth, as well as for the immediate prohibition of the inoculation of the small-pox. It was contended that the prohibition of the small-pox inoculation alone would be inadequate to the purpose of extinguishing the small-pox; and it was maintained that it was not more an infringement of the liberty of the subject to render the cow-pock cow-pock inoculation universal, than to prohibit, as already proposed, the small-pox inoculation*. The same gentleman (Dr. Pearson) who made these observations, has lately explained to the Vaccine Institution that he had been discouraged and induced to lay aside his plan, from the opinion of a great number of friends, who almost universally disapproved it as impracticable in the execution, and imprudent for the author. He has accordingly lately proposed two things, by way of rendering vaccination less liable to failures, and more extensively practised.

1. That each person Inoculated at the Institution shall have a ticket signed by three members of the medical establishment, attesting that the vaccination has been duly undergone; and that if the person so attested shall subsequently take the small-pox, such person shall be entitled to ten guineas from the Institution.

2. It is proposed that the medical establishment† shall grant certificates, which may have the effect of diplomas to qualify pupils, by attending the practice and lectures at the Institution.

It is remarkable, and indeed singular, that notwithstanding the Institution has been established near six years, not a single authenticated case of small-pox subsequently to the cow-pock has occurred in the practice.

Of the inoculation of persons who have undergone the cow-pock 30 *to* 50 *years ago; with some anecdotes of Farmer Jesty, the Vaccinator of his family in* 1774.

Mr. Benjamin Jesty, farmer, of Downshay, accompanied by his son Mr. Robert Jesty, lately visited the Vaccine Institution in Broad Street, where he proved, by authentic evidence of various kinds—

1. That he took the cow-pock from his own cows about 50 years ago, and although he had been often in the way of the small-pox, he had remained unsusceptible. He has a scar on one hand from the cow-pock.

2. That knowing many instances besides himself of persons never taking the small-pox who had taken the cow-pock in dairy farms; and that it was a harmless complaint; also being of opinion that he should avoid ingrafting diseases of human subjects, such as evil, lues, madness,

* See Philosophical Magazine, vol. xv, No. 57, p. 81.

† The medical establishment consists of—Doctors Pearson, Nihell, and Nelson; Surgeons extraordinary, Thomas Keate, Thomas Payne, and Thompson Foster, esquires; Surgeons in ordinary, John Gunning, J. C. Carpue, and J. Doratt, esquires; Visiting apothecaries, Francis Rivers, Augustus Brande, and P. de Bruyn, esquires.

ness, &c., by inoculating from the cow, he determined to prefer vaccination in his own family. Accordingly, when the small-pox prevailed in the town and neighbourhood of Yetminster, in 1774, where he then lived, he inoculated his wife, Mrs. Jesty, and his two sons Robert and Benjamin, with matter from his cows. Benjamin showed a large cicatrix, on the middle of the upper arm, left by this inoculation 31 years ago. The two sons were inoculated for the small-pox, without effect, 15 years ago; and they, as well as Mr. Jesty, have been often in contact with persons in the small-pox in the course of 31 years. To give further satisfaction, Mr. Robert Jesty, without hesitation, agreed to the proposal of being again inoculated while in town for the small-pox. Accordingly variolous limpid matter was very carefully inserted by four punctured places in the left arm, immediately from a child in the 6th day of the eruption. Red pimples appeared in the punctured parts the day after inoculation, which continued for two or three days, and then died away without any attending pain of the arm or arm-pit, or any constitutional disorder.

Mr. Jesty's aversion to the small-pox "humour," as he called it, occasioned him to prefer being *tested* with vaccine matter. Accordingly he was inoculated in four places in one arm with matter immediately from a subject in the 9th day of vaccination.

The farmer described how much he was censured by his neighbours for inoculating his family from "a *beest*—a brute creature without any soul;" and he was called "a hard-hearted man:" but he answered that the brutes were free from many disorders of men; and he saw that "there were many Christians who were greater brutes than the cows."

It is worthy of notice, all the four parties in the cow-pock inoculation have enjoyed an uncommonly good state of health, and are all athletic subjects. Mr. Jesty, who is 70 years old, is a fresh-looking man, and has the usual appearance of a man of five-and-fifty, or at the most of sixty. The indistinctness of his speech is from the loss of all his teeth.

To gratify the public, and to preserve for posterity this interesting part of the history of cow-pock inoculation, the Vaccine Institution have had a whole length picture of farmer Jesty painted by Mr. Sharp of Suffolk-street. It has been executed in a capitally successful manner; but it must be owned that the manly figure and fine countenance of the subject were in favour of the ingenious artist.

BOTANY.

BOTANY.

A private individual in the government of Astracan, has sent to the Russian minister of the interior the roots and leaves of a plant which grows there in great abundance, together with meal and bread prepared from these roots. The accompanying memoir states that these roots have been long used by the Calmucs as food; that the bread made from them is wholesome and well tasted; that in case of a scarcity, occasioned by a bad crop, it might be employed as a good substitute for common bread, and that the plant is easily propagated by seed.

Further examination has shown that this plant is nothing else than the *butomus umbellatus*, Linn.; in English the *flowering rush* or *water gladiole*; in French *butome à ombelle jonce fleuri*; in Tartaric *sussatok*; in Ostiak *russ*; in Russian *sussac, sotschnoj koren*. It grows in every part of Europe, in Siberia, and in the neighbourhood of Petersburgh, and particularly in marshes and rivers. The Calmucs roast the roots or dry them, and use them in that manner as food. According to the elder Gmelin, they are used also for the same purpose by the Ostiaks and neighbouring people. In former times a healing power was ascribed to this plant.

Meal has been prepared at Petersburgh from the roots, and bread baked of it. We are informed that the meal in kneading has all the properties of common meal. The dough rises very easily when leaven is added to it; and the bread is very little inferior in colour, taste, or smell, to wheaten bread; the only difference is, that it is not so tough, and readily breaks, in consequence of the fine fibres of the roots which remain in it, and it has also a little bitterness.

From all these facts it appears that this plant may become a substitute for corn; and if the bread be as wholesome as is asserted, this discovery deserves the utmost attention, and particularly in places which do not produce corn, and where this vegetable production can be cultivated.

The minister of the interior has announced this discovery to the emperor, who was greatly pleased with it, and ordered a present to be given to the person who transmitted the roots to Petersburgh.

Mr. Andrews's Work on Roses.—It is not unworthy of remark, that the Rose, though it has ever been celebrated as the queen of flowers, has been very little an object

 of

of the attention of botanists. It is hoped that this unaccountable defect, which has frequently been objected to the science of botany, will be in some degree removed by the new work lately announced by Mr. H. C. Andrews of Knightsbridge, the merits of whose Botanist's Repository, and Engravings of Heaths, are well known. It is to be a complete Monograph of the Genus *Rosa*, and will contain coloured figures of all the known species of Roses, and their numerous and beautiful varieties, drawn, engraved, described, and coloured, from the living plants, by Mr. Andrews.

LECTURES.

At the Theatre of Anatomy, Blenheim-street, Great Marlborough-street, the Autumnal Course of Lectures on Anatomy, Physiology, and Surgery, will commence on Tuesday, the first of October, at two o'clock in the afternoon, by Mr. Brookes.

In these Lectures the structure of the human body will be demonstrated on recent subjects, and further illustrated by preparations, and the functions of the different organs will be explained.

The surgical operations are performed, and every part of surgery so elucidated, as may best tend to complete the operating surgeon.

The art of injecting, and of making anatomical preparations, will be taught practically.

Gentlemen zealous in the pursuit of zoology will meet with uncommon opportunities of prosecuting their researches in comparative anatomy.

Surgeons in the army and navy may be assisted in renewing their anatomical knowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical Converzationes will be held weekly, when the different subjects treated of will be discussed familiarly, and the students' views forwarded.—To these none but pupils can be admitted.

Spacious apartments, thoroughly ventilated, and replete with every convenience, will be open in the morning for the purposes of dissecting and injecting; where Mr. Brookes attends to direct the students, and demonstrate the various parts as they appear on dissection.

An extensive Museum, containing preparations illustrative of every part of the human body, and its diseases, appertains to the Theatre, to which students will have occasional admittance.—Gentlemen inclined to support this

school

school by contributing preternatural or morbid parts, subjects in natural history, &c. (individually of little value to the possessors,) may have the pleasure of seeing them preserved, arranged, and registered, with the names of the donors.

The inconveniences usually attending anatomical investigations are counteracted by an antiseptic process, the result of experiments made by Mr. Brookes on human subjects at Paris in the year 1782; the account of which was delivered to the Royal Society, and read on the 17th of June 1784. This method has since been so far improved, that the florid colour of the muscles is preserved, and even heightened.—Pupils may be accommodated in the house.—Gentlemen established in practice, desirous of renewing their anatomical knowledge, may be accommodated with an apartment to dissect in privately.

The first Monday in October next will commence a Course of Lectures on Physic and Chemistry at the Laboratory in Whitcomb-street, at the usual morning hours, viz. on Therapeutics at a quarter before eight, on the Practice of Physic at half after eight, and on Chemistry at a quarter after nine o'clock.

These Lectures are delivered every morning, except on Saturdays, when at nine o'clock a Clinical Lecture is given on the cases of Dr. Pearson's patients in St. George's Hospital.

By George Pearson, M.D. F.R.S. of the College of Physicians, and Senior Physician to St. George's Hospital, &c. &c.

N. B. Proposals may be had at St. George's Hospital, and at No. 52, Leicester-square.

The following Courses of Lectures will be delivered at the Medical Theatre, St. Bartholomew's Hospital, during the ensuing winter:

On the Theory and Practice of Medicine, by Dr. Roberts and Dr. Powell.

On Anatomy and Physiology, by Mr. Abernethy.

On the Theory and Practice of Surgery, by Mr. Abernethy.

On Comparative Anatomy and Physiology, by Mr. Macartney.

On Chemistry, by Dr. Edwards.

On the Materia Medica, by Dr. Powell.

On Midwifery and the Diseases of Women and Children, by Dr. Thynne.

Anatomical

Anatomical Demonstrations and Practical Anatomy, by Mr. Lawrence.

The Anatomical Lectures will begin on Tuesday, October the first, at two o'clock, and the other Lectures on the succeeding days of the same week.

Further particulars may be learned by applying to Mr. Nicholson, at the Apothecary's Shop, St. Bartholomew's Hospital.

LIST OF PATENTS FOR NEW INVENTIONS.

Malcolm Cowan, of Gloucester-place, Portman-square, in the county of Middlesex, commander in the royal navy; for improvements in the construction of sails for ships and vessels of all descriptions.

Robert Barber, of Billborough, in the county of Nottingham, gentleman; for new and improved modes of making and shaping stockings and pieces, and also some new and improved kinds of stocking-stitch and warp-work.

Thomas James Plucknett, of Butt-lane, Deptford, in the county of Kent, gentleman; for a method of mowing corn, grass, and other things, by means of a machine moving on wheels, which may be worked either by men or horses.

William Collins, lieutenant in the royal navy; for a ventilator, upon a new or improved construction, for the purpose of ventilating tents and marquees of every description.

TRAVELS IN AFRICA.

By letters received, we learn that the celebrated traveller Mungo Parke, with his companions Messrs. Anderson and Scott, who sailed from Portsmouth in the Crescent transport, about six months ago, having touched at the islands of St. Jago and Goree, arrived at Kayay, on the river Gambia, on the 14th of April, whence they were to proceed in a few days into the interior of Africa, to effect the business on which they were dispatched, and which we believe to be of a very important and extensive nature. The heat was at that time so excessive, that the thermometer was constantly at 100 degrees and upwards in the shade, and for two or three hours after sunset continued at from 82 to 92 degrees. We are happy, however, to hear that notwithstanding this excess of heat the whole party had enjoyed perfect health: they had only lost one of the fifty men they had received from the African corps at Goree, though they had been above fourteen days in the river; and this man had been unwell before they left the island.

Mr. Seetzen, another traveller, arrived on the 4th of March at Aleppo; but intended in fourteen days to set out for Damascus, whence he was to proceed through Egypt to the interior parts of Africa.

METEORO-

METEOROLOGICAL TABLE

BY MR. CAREY, OF THE STRAND,

For August 1805.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	58°	72°	59°	29·68	54°	Fair
28	60	69	58	·68	26	Showery
29	59	68	59	·68	35	Showery
30	63	67	60	·78	39	Fair
31	62	66	56	·68	25	Showery
Aug. 1	60	69	57	·58	20	Showery
2	61	71	58	·44	38	Stormy
3	62	66	59	·77	21	Showery
4	62	69	61	·90	33	Cloudy
5	60	71	59	·64	63	Fair, with strong wind
6	62	71	57	·71	74	Fair
7	61	69	56	·86	49	Fair
8	60	73	57	·87	51	Fair
9	64	74	64	·89	30	Cloudy
10	64	72	62	30·02	53	Fair
11	63	76	64	·00	52	Fair
12	65	75	60	29·78	52	Fair
13	60	70	61	·90	64	Fair
14	60	67	55	·95	57	Showery
15	58	70	60	30·00	65	Fair
16	61	70	61	29·90	44	Fair
17	60	69	63	·99	52	Fair
18	64	69	60	·86	35	Cloudy, and heavy rain at night
19	60	59	54	·52	0	Showery
20	60	66	57	·62	25	Showery
21	59	60	56	·95	15	Cloudy
22	57	66	62	30·12	35	Fair
23	63	70	64	·18	39	Fair
24	64	71	63	·15	51	Fair
25	62	70	62	02	35	Fair
26	63	72	58	29·95	24	Cloudy

N. B. The barometer's height is taken at noon.

XLIII. *Extract from a Memoir entitled "Considerations on Colours, and several of their singular Appearances." Read in the Mathematical and Physical Class of the French National Institute, Ventose* 13, *An* 13. *By* C. A. Prieur*.

The author of this memoir endeavours to account for several phænomena which appear to him not to have been before properly explained: or, rather, his object is to give a general theory, by the help of which all the cases of coloured appearances, and even the most singular, may be referred to certain principles.

He sets out from known opinions in regard to the different kinds of luminous rays; on the mixture resulting from several of these rays taken at different places of the solar spectrum, and among others on that very remarkable case when the rays are so chosen that their union produces on the organ of sight the sensation of whiteness, even if two kinds of rays only be employed.

For these ideas we are indebted to the discoveries of the immortal Newton; and they flow immediately from the method which he proposed for determining what colour will be obtained from the mixture of any given quantities of other colours.

If we are desirous of comprehending fully what takes place in the vision of colours, it is first indispensably necessary that we should be familiarised with knowing the shades composed of different simple rays, and with forming correct ideas of *black* and *white*, and the complication which they produce in coloured appearances; and in particular to make ourselves acquainted with the correspondence of colours, which, taken two and two in a certain order, are susceptible of forming, by their union, *white* or any other complex shade at pleasure.

Two colours which have this kind of relation are called *complementary colours*; one of them being given, the determination of the other may be made, with more or less precision, by experience, calculation, or mere reasoning; and the consideration of them may be applied with much utility to a great number of cases, as will be seen hereafter.

Several details are here given, which those versed in optics, or habitually acquainted with the mixture of colours, may readily supply: besides, the rest of the memoir,

* From the *Journal de Chimie*, No. 160.

moir, of which we have undertaken to give an account, will afford us an opportunity of mentioning what will be most necessary for understanding the subject.

These preliminary observations are followed by some remarks on *contrasts*. The author employs this word to characterize the effect of the simultaneous vision of two substances of different colours when brought together under certain circumstances. The contrast here, then, is a comparison, from which there results a sensation of some difference, great or small. It is very generally known, and painters know it well, that a coloured matter which occupies a pretty extensive space, and brought near to, or surrounded by, some other colour, has not the same appearance as when it has near it other colours. But whence does this difference arise?

Before we answer this question, let us make an essential distinction. It either relates to homogeneous colours, that is to say, formed of one kind of rays, or to complex colours, arising from a mixture of heterogeneous rays, that is to say, composed of different sorts.

In regard to the first case, it must be confessed that we are ignorant whether the bringing together different simple colours would produce any alteration in their respective appearance. As one can rarely enjoy the sight of such colorations, and as it is not easy to dispose of them to our wish, no experiments have yet been made on their contrasts. This subject, however, deserves to be studied.

In regard to the cases of compound colours, (which is that of almost all the natural or artificial bodies, as the author shows in the sequel of his memoir,) the new colours manifested by the contrast are always conformable to the shade which would be obtained by suppressing from the colour proper to one of the bodies, the rays analogous to the colour of the other body.

Thus, if we place on red paper a small slip of paper painted of an orange colour, it will appear almost yellow. Removed then to yellow paper, the same orange slip will become almost red. After this, if it be put upon violet paper, it will resume a yellowish shade, but different from the preceding; and, in the last place, applied to green paper it will assume a new degree of a red colour.

The explanation of these examples according to the proposed rule is easy, if we suppose that the orange colour of the small slip observed arises (as is commonly the case) from an union of all the kinds of rays except the blues.

A multitude of combinations of colours placed thus above

above each other produce the colour of contrast indicated by the rule here given; but there are several circumstances which render the effect of it more striking, or modify the result.

It sometimes depends on the degree of the brightness with which the observed bodies are affected: they may be uniformly illuminated, or one of them more than the rest. The quantity of light which has entered simultaneously into the eye by the whole field of vision has also an influence. If the bodies consist of several rows, like a series of decreasing circles placed one within the other, the colours of each will re-act respectively on each other. At each junction there will be on both sides a border coloured by the contrast of the neighbouring body: these borders will extend more or less according to the splendour of the objects. The effect of one may become dull, or extinguish all the rest.

The colours of contrast show themselves also with more vivacity after some moments of observation, or if the objects have been agitated a little, as if to make them move slowly over the retina. It would appear that a certain fatigue of the eye, either instantaneous in regard to the intensity of the light, or more slowly by prolonged vision, concurs to produce the appearances in question. But excessive fatigue of that organ would occasion a degeneration of the colours belonging to another mode.

We ought not, then, to refer to contrasts those impressions mentioned by Epinus, which are propagated in the eye with a certain duration and a particular period of shades, when one has looked with intensity at a very brilliant light, such as that of the sun.

But the colours called by Buffon *accidental*, and respecting which Scherfer has given an interesting memoir, belong to the class of contrasts, or at least constantly follow the same law.

Coloured shadows are also a phænomenon of the same kind. Count Rumford has placed this truth beyond all doubt in two memoirs, in which he has treated this subject in an interesting manner*.

The author of that which we here analyse is of opinion, that we must ascribe also to contrasts those appearances of the solar light received through a hole in a coloured curtain, which general Meusnier remarked in consequence of their singularity. He assimilates to this also several cases of colours exhibited by opals, or more generally by bodies

* Philosophical Essays, vol. i. p. 319 et seq. 1802, London edit.

containing sensibly opake parts disseminated throughout a pellucid substance. By the same reasons he explains the colours under which the grayish dust collected by age on old paper or coloured stuffs shows itself, and deduces the same consequences in regard to the bluecish appearance of the veins of the human body.

He proposes, likewise, a new method of rendering very sensible the colours of contrasts more lively even than by the known process of *accidental colours*, and yet without occasioning extraordinary fatigue to the eye. The latter condition is of importance, for it is known that it is dangerous to expose to forced exercise an organ so delicate as the eye.

This method consists simply (when one is in an apartment and in the open light) in placing before the window the painted pieces of paper on which you intend to observe the contrasts, as in the example before mentioned. As the coloured paper, then, which serves as a field has a semi-transparency, and by these means is more illuminated, while the small band of another colour placed over it is, on account of the double thickness, more opake and in the shade; the colour arising from the contrast becomes thus more striking.

It is this disposition which produces the singularly striking effect of the contrast of a small piece of white card applied successively to paper, glass, or stuff of any colour whatever. When the transparent body is red, the opake white appears of a blueish green; it is then seen decidedly blue if the ground be orange; then of a sort of violet on a yellow ground, or green on crimson, &c.; always according to the exact correspondence of the *complementary colours*.

It is here to be observed, that according to the rule indicated, if from the white which is formed by the union of all the coloured rays we suppress, for example, the red rays, the remaining bundle ought to be seen under the colour of a very pale blueish green; but as the small white piece in the preceding experiment is in the shade, the black which results from it may be of the degree proper for destroying the effect of the white, and then the *blueish green* appears of a bright shade. The same reasoning is applicable to the cases of all the other colours.

To produce well the effects here announced, in repeating these experiments, it is necessary, when the opportunity of clear weather has been obtained, to guard against the reflections of neighbouring bodies, and against double contrasts. Thus,

Thus, when the bright light conveyed through a window surrounds the transparent paper, it may increase very sensibly the splendour of the colour of contrast, or injure it by producing another shade according to the colours of the bodies subjected to observation. In a word, one may always remove this inconvenience by concealing the troublesome objects by a piece of black pasteboard or stuff, or by looking through a blackened tube which confines the field of sight to the extent necessary.

This knowledge of contrasts may be applied with great advantage to those arts which have a relation to colours. The painter knows that one cannot be placed indifferently in the neighbourhood of another. But when one knows the law to which their re-action is subject, one knows better what must be avoided or done to increase the splendour of the colour which it is necessary to heighten: a successive comparison of them furnishes also valuable indications in regard to their nature or their composition. This is what the author himself put in practice with advantage in his manufactory of colours and paper-hangings.

These considerations in regard to contrasts led him to the examination of a very singular case proposed and treated of by Monge with his usual sagacity*; namely, the white appearance under which a coloured body is sometimes seen when viewed through a piece of glass of the same colour; some uncertainty remained in regard to the circumstances really necessary for producing this effect. The author determines them by the help of his particular experiments, and enumerates those which have a favourable influence, or the contrary. He concludes that, when one experiences the sensation of whiteness in these cases, it arises merely from the action of contrasts, by which the impression of the colour is lessened or annulled, while that of a certain brightness still exists, and is remarked by the opposition of a greater degree of obscurity. This manner of considering the subject leads to a new definition of whiteness, in which there is certainly nothing repugnant: *white to us is the sensation of light when no particular colour predominates or is perceived.*

In the subsequent part of his memoir the author employs himself in particular with the coloration of different opake or transparent bodies; that is to say, he endeavours to discover what are those luminous rays which any coloured body is really susceptible of reflecting or transmitting.

* Annales de Chimie, tom. iii.

His means of making experiments are simple. If the body be opake, it is placed on a piece of black stuff in order to be observed with the prism. He endeavours to give it a rectangular form; or, if it is not susceptible of being cut, it is covered with a piece of black pasteboard pierced with a hole of that form. The coloured fringes, then, manifested on the two opposite edges indicate the kind of rays which are reflected, and consequently those which are absorbed when the nature of the illuminating bundle is known; on which it is still to be remarked that, as the fringes themselves are of complex shades, we must separate the simple kinds. When a person has had some practice, a bare inspection will be sufficient. He may be formed to this habit, and the want of it may be supplied by guiding himself by cards representing each kind of rays placed over each other in order, removing them gradually agreeably to the different refrangibility; or he may employ a plate or board, constructed according to Newton's method, for determining the shades composed of different elementary colours.

If the body subjected to examination be diaphanous, it will be proper to view it through the aperture of the card before spoken of, in order to exclude extraneous light, in such a manner that the prism may show the fringes. Also, by placing yourself in the dark, a flame such as that of a wax taper will show through the transparent body, and, by the help of the prism, a series of coloured images corresponding to the rays transmitted.

By proceeding in this manner, the author found that many opake bodies which he had at hand of different kinds and of all colours, either yellow, orange, or red; or green, blue, or violet, were indebted for their coloured appearance to the following conditions:

1st, Each of the bodies always absorbs rays of the complementary kind of the prevailing colour.

2d, The absorption, in regard to some of them, comprehends, besides the complementary kind, other rays collateral to that species, and more or less numerous.

3d, The darker the same colour is, the fewer kinds of reflected rays it presents.

It must here be understood that we do not allude to mixed colours, but only to those which form a homogeneous compound or a real *combination*, according to the meaning attached by chemists to that word. It is also to be remarked, that we must not confound the colour reflected from the interior of the moleculæ susceptible of bright or dark shades with the light sent back from the anterior sur-

 face

face of the body. Though the latter overcharges more or less the proper colour, it is, however, easy to lessen the effects of it, and to distinguish them in experiments.

Another remark proper to be made is, that the expression *predominating colour* ought not to signify that the rays of that colour are more abundant than the rest: this would be an error. Several kinds of rays may co-exist in the bundle which produces the colour, without any kind being, on that account, more abundant. Strictly speaking, all the elements of the bundle are dissimilar, and consequently none of them is in greater quantity. But the general tone of the colour remains analogous to that of the rays distinguished by the name of *predominating*. Hence it is proper to retain this expression, provided an exaggerated signification be not given to it.

The author observed also transparent bodies, such as glass of different colours, and liquors contained in a flask having two broad parallel faces. By these he found a law of absorption similar to that of opake bodies, but still more striking, and without any ambiguity.

This law is constantly regular. It depends on the peculiar nature of the body which receives the light, and on its density and thickness. It is also determined by the light of the illuminating body, either in regard to its force, or to the two kinds of rays which compose it.

The absorption always begins with the rays most opposite to the predominating colour of the illuminated body. It continues by those which are next in the order indicated by the spectrum. It thus extends gradually, and never by jumps, to the last kind: consequently the body becomes more and more obscure, and always terminates by being black. Sometimes it extends from one side only of the first rays absorbed; sometimes on both sides at the same time; and it there proceeds either by an equal progress from the right and left, or by advancing more rapidly on one of the sides.

If each element be separately varied, there will be in the effects a peculiar progression. That depending on the densities is not always similar to that arising from the changes of thickness. By receiving also on the same body different kinds of light, the progress of the absorption is differently modified, and consequently the colours changed.

The author quotes examples of all these cases. He derives them from the numerous experiments which he made with coloured glass, with acid or alkaline metallic solutions, with the liquid tinctures of infusions or vegetable

 decoctions.

decoctions. They exhibited curious peculiarities; but we shall not detail them, both for the sake of brevity, and because they may be easily tried.

In short, very important consequences in regard to the reciprocal action of bodies and light arise from the whole of these observations, and perhaps they will throw some light on the grand question of the cause to which their permanent colours ought to be ascribed.

After these researches the author concludes with an examination of different phænomena of various kinds. He indicates the modifications experienced in their coloration by burning coals at different degrees of incandescence. His remarks are applicable also to other bodies, such as iron in a state of ignition, or a long series of reverberated lamps seen during foggy weather, or a white light seen through a piece of glass blackened by progressive strata of smoke. In all these cases, the colours necessarily pass through a series of shades, which proceed from white to yellow, to orange and to red, more and more dark; the reason of which he explains.

Metallic oxides have also a gradation of shades according to the proportion of oxygen. A certain continued alteration in vegetation produces one in some of the parts of plants. The arts or chemical processes present one also under a multitude of circumstances.

The manufacturer may, with advantage, derive from them indications either in regard to the progress of combinations, or to enable him to judge of the moment proper for performing certain operations of his labours.

The author then dwells more particularly on the appearance of the coloured clouds, and especially those seen near the time of the rising and setting of the sun. This phænomenon, so generally known, had hitherto never been explained, though the ablest philosophers had made it an object of their research.

It does not arise from the refraction of the rays of the sun, but the successive absorption of these rays when they strike the lower parts of the atmosphere and those most charged with vapours.

This absorption follows laws analogous to those already mentioned. As the quantity of the vapours, and even their nature, are not similar for two days in succession, this irregularity produces corresponding differences in their effects.

In general, the first rays attacked by these vapours are blue approaching to violet. Soon after, they absorb the contiguous rays, gaining with more rapidity the blues pro-

perly

perly so called, then the greens and yellows, and continuing thus to the red: hence the *yellowish*, *orange*, and *red* colours, under which the clouds appear. This period of shades, namely, the evening, manifests itself gradually in proportion as the sun approaches the horizon. Terrestrial objects, the part of the air near the sun, and even that luminary itself, are tinged with the same shades. When his rays can be received on a prism, it is seen that the rays really absorbed correspond to the general coloration of the moment.

In consequence of the successive increase and density of the vapours traversed by the light, clouds differently placed must at the same instant be tinged of different colours. The highest may be white, while the rest, at a less elevation, will be yellow, and others still lower will be proportionally redder. At an equal elevation, the most distant from the point where the sun sets will incline to red, and the nearest to yellow.

One may then see on bodies naturally white, blue or green shadows, as Buffon and other philosophers have remarked. They are only, as has been already said, the effect of the contrast of the actual colour of the illuminated and the obscure part.

Contrasts may also render complex the colour of the clouds; for example, when a great portion of the heavens shows a blue colour. There are some the colour of which arises merely from this cause, and some are observed sometimes during the day by those who are on a high mountain, or in any other situation which secures the eye from the too strong direct or reverberated action of the solar light; but in this case the clouds have only a yellowish shade, exactly of the complementary colour of the sky-blue.

It is under a similar colour that the moon is sometimes seen, when she is very high, a little before or after the sun has passed the horizon. It seems, also, that she appears thus, or even altogether white, when there exist at the same time in the atmosphere clouds variously coloured by the vapours of the east or the west. By this concourse of circumstances we have a new proof of the difference of the causes to which these colorations are owing.

We must remark, in the last place, that by the irregularity of terrestrial localities, and the state of the atmosphere, these phænomena may be concealed, or subject to different interruptions. In our climates, the coloration of the clouds for the most part does not attain to its utmost term. On certain evenings, however, if the sky is very se-

rene

rene towards the part corresponding to the sun, and if there be over our heads any of those light clouds which are exceedingly high, they will be seen at a later period clothed with a brilliant red, heightened by the diminution of the light on the earth, soon after darkened, and at length extinguished in the shade.

CONCLUSION.

Notwithstanding the many fine discoveries already made in regard to light, the theory of the production of colours has not yet acquired that generality which renders it applicable to all cases, and to that simplicity of principles to which we are always conducted when we discover the real laws of nature. Many phænomena have never been explained; and the explanation given to several necessarily requires to be rectified. The author proposes to establish changes in theory, the want of which he points out. He founds his reasoning partly on the doctrine and facts generally admitted, and partly on other information less diffused, though long known, and on his own observations. But he is far from flattering himself with the idea of having presented these objects properly in a sketch such as this memoir. He even was soon sensible that a subject so extensive and complex would require more time and labour.

To fill up many vacuities, to develop several points, and to rectify and extend others by researches, new experiments, and profound reflection,—such is the ample task of improvement. If his health and occupations permit, he will endeavour to undertake it.

It would be of utility, and also just, to give at the same time a short account of what we are indebted to the great Newton, who opened this career in so admirable a manner, and to the philosophers who have discovered new points, and removed difficulties. Greater precision ought also to be employed in the language relating to colours, proportioned to the increase of our knowledge, and the present state of science and the arts. In a word, it would not be too much, in such a branch of science, to add the resources of calculation and geometry to all the riches of experience, and, if possible, to the advantages of the best method.

XLIV. *On*

XLIV. *On the Variations of the Terrestrial Magnetism in different Latitudes. By Messrs.* Humboldt *and* Biot. *Read by M.* Biot *in the Mathematical and Physical Class of the French National Institute 26th Frimaire, An* 13. (17*th December* 1804.)

[Concluded from p. 257.]

The calculation is as follows:—I suppose that the point B (fig. 3.) is the north magnetic pole of the earth, and that the point A is the south magnetic pole: I suppose also that there is in the point M, at the surface of the earth, a molecula of the austral fluid which is attracted by B and repelled by A in the inverse ratio of the square of the distance; and I require what will be the direction of the power resulting from these two forces acting on that molecula. It is evident that this direction will be that also which would be assumed in the point M by the needle of a compass freely suspended: for, in consequence of the smallness of the needle in comparison of the radius of the earth, the lines drawn from its points to one centre, B or A, may be considered as parallel, especially if the points A and B are near the centre of the earth; which is the case with nature, as may be seen.

I shall first suppose that the earth has a spherical figure, and that the two poles A and B are equal in force; I shall then examine how far the latter supposition agrees with the results observed.

Let AM then $= D'$, BM $= D$, CP $= x$; PM $= y$, the angle MCP $= u$, CA $=$ CB $= a$, and I shall make $a = Kr$; r being equal to the radius of the earth, and K a constant but indeterminate quantity.

Let X, Y, also be the forces which attract M in a direction parallel to the axes of the co-ordinates, and β the angle which the resulting force makes with the axis ABC.

We shall first have the following equations, in which F is the magnetic force, at a distance equal to unity.

$$X = \frac{F}{D^2} \cos. MBD - \frac{F}{D'^2} \cos. MAD;$$

$$D'^2 = y^2 + (x + a)^2 = r^2 + 2\,\text{axis} + a^2$$

$$Y = \frac{F}{D^2} \sin. MBD - \frac{F}{D'^2} \sin. MAD;$$

$$D^2 = y^2 + (x - a)^2 = r^2 - 2\,\text{axis} + a^2;$$

or

or by putting for the cosines their values:

$$X = \frac{F(x-a)}{D^3} - \frac{F(x+a)}{D'^3}$$

$$Y = \frac{Fy}{D^3} - \frac{Fy}{D'^3};$$

and as we have

$$\text{tang. } \beta = \frac{Y}{X},$$

we shall have also

$$\text{tang. } \beta = \frac{\frac{y}{D^3} - \frac{y}{D'^3}}{\frac{x-a}{D^3} - \frac{x+a}{D'^3}} = \frac{y(D'^3 - D^3)}{x(D'^3 - D^3) - a(D'^3 + D^3)};$$

and putting for x, y, and a, their values, $r\cos. u$; $r\sin. u$; Kr;

$$\text{tang. } \beta = \frac{\sin. u}{\cos. u - K\left(\frac{D'^3 + D^3}{D'^3 - D^3}\right)};$$

$$D'^2 = r^2(1 + 2K\cos. u + K^2);$$
$$D^2 = r^2(1 - 2K\cos. u + K^2);$$

which gives the system of the two equations,

$$\text{tang. } \beta = \frac{\sin. u}{\cos. u - K\left(\frac{D'^3 + D^3}{D'^3 - D^3}\right)}$$

$$K\left(\frac{D'^3 + D^3}{D'^3 - D^3}\right) = \frac{(1+2K\cos. u + K^2)^{\frac{3}{2}} + (1-2K\cos. u + K^2)^{\frac{3}{2}}}{(1+2K\cos. u + K^2)^{\frac{3}{2}} - (1-2K\cos. u + K)^{\frac{3}{2}}} K.$$

These equations determine the direction of the magnetic needle in regard to each point M, the distance of which from the magnetic equator is known; but it is seen that this direction depends on the quantity K, which represents the distance of the magnetic centres from the centre of the earth: this distance being expressed in parts of the terrestrial radius, we must therefore first determine this quantity from observations.

To do it in the manner of approximation, and thus acquire a first idea of the value of K, I have chosen an observation made by M. Humboldt at Carichana in 7·2978° (6° 34′ 5″) of north latitude counted from the terrestrial equator, and 78·111° (70° 18′) west longitude reckoned from the

the meridian of Paris; which gives 16·526° (14° 52′ 25″) of latitude counted from the magnetic equator, and 53·7390° (48° 21′ 53″) of west longitude proceeding from the node formed by that equator with the equator of the earth. The inclination of the magnetic needle was observed in that place by M. Humboldt in the month of Messidor, year 8, and found to be equal to 33·78° of the centigrade division*. A comparison of this result, with the other observations of M. Humboldt, shows that it may be indeed considered as agreeing to that latitude.

To make use of it I have successively given to K different values in the formula: I have calculated the inclinations resulting from that latitude; and, comparing these results with that which M. Humboldt really observed, the progress of the errors naturally led me to the most proper supposition. The following is a table of these trials:

Values of K.	Inclinations of the Needle.	Errors.
K = 1	7·73°	26·04°
K = 0·6	18·80	14·97
K = 0·5	22·04	11·73
K = 0·2	29·38	4·39
K = 0·1	30·64	3·13
K = 0·01	31·04	2·73
K = 0·001	31·07	2·7

The first value of K would place the centre of the magnetic forces at the surface of the earth and the poles of the magnetic equator. It is seen that this supposition cannot be admitted, because it would give an increase of inclination much less rapid than that indicated by observations. The case is the same with the following results, which place the centres of action on the terrestrial radius at different distances from the centre of the earth; but it is seen also, in general, that they approach more and more to the truth in proportion as this distance becomes less; which evidently shows that the two centres of action of the magnetic forces are situated near the centre of the earth. All the other observations of M. Humboldt would also lead to the same consequence.

The most proper supposition would be to make K null, or so small that it would be needless to pay attention to it; which amounts to the same thing as to consider the two centres of action placed, as we may say, in the same molecula. The result, indeed, obtained in this manner is the most exact of all; it is equal to 31·0813°: this value is still

* All the measures of inclination which I have given in this memoir will be expressed, like those of M. Humboldt, in decimal parts of a quadrant.

a little

a little less than that which M. Humboldt observed, and the difference is equal to 2·69; but it must be considered also that the formula from which we derive these values supposes the position of the magnetic equator to be perfectly determined; but it may not be so with the utmost exactness, according to the only two observations of Lapeyrouse and Humboldt, which we have employed. It is therefore by studying the progress of the formula, and comparing it with the observations, that we are able to appreciate it justly; after which we may think of remedying the small errors with which it may be accompanied.

To obtain the result I have here mentioned, and which is, as it were, the limit of all those which may be obtained by giving to K different values, it is to be remarked that the quantity

$$K\left(\frac{D'^3 + D^3}{D'^3 - D^3}\right)$$

or

$$K\,\frac{(1 + 2\,K\cos.u + K^2)^{\frac{3}{2}} + (1 - 2\,K\cos.u + K^2)^{\frac{3}{2}}}{(1 + 2\,K\cos.u + K^2)^{\frac{3}{2}} - (1 - 2\,K\cos.u + K^2)^{\frac{3}{2}}}$$

becomes $\frac{0}{0}$ when K is null, but by applying to it the methods of known quantities it will be found that its value in this supposition is really determinate and equal to $\frac{1}{3\cos.u}$. By substituting this in the formula we shall have

$$\text{tang.}\,\beta = \frac{\sin.u}{\cos.u - \frac{1}{3\cos.u}}.$$

an equation which may be reduced to this form:

$$\text{tang.}\,\beta = \frac{\sin.2u}{\cos.2u + \frac{1}{3}};$$

which will easily give the value of β: and when this value is known we shall have the inclination I, by the following formula:

$$I = 100 + u - \beta,$$

which will serve throughout the whole extent of the two hemispheres.

From the progress I have traced out it is seen that the preceding formula is not merely an empyric construction of observations; on the contrary, it is totally independent, and only supposes the inclination of the magnetic needle to be produced

produced by a magnet, infinitely small, placed in the centre of the terrestrial surface; but by calculating from this formula the inclination for the different latitudes, I have found precisely the same numbers as M. Humboldt observed either in Europe or in America: and it is not his observations only that are represented in this manner; but those which have been made in Russia, and at Kola in Lapland, during the last transit of Venus, are also comprehended under the same law. This is proved by the table annexed to this memoir, in which I have calculated the observations of Mallet and Pictet, with a part of those of M. Humboldt, which I took at random, but, however, in such a manner as to include all the rest in the intervals.

It is seen that the results of the formula deviate very little from the observations; but these differences may be rendered still smaller. By examining, indeed, the progress of the errors, it is seen that the numbers given by calculation are a little too small in America for the low latitudes, and a little too great for the high latitudes; which shows that the whole may be allowed, with some slight modifications, either by changing, however little, the node and inclination of the magnetic equator, which two observations cannot determine with the utmost exactness, or by displacing ever so little our small magnet, leaving, however, its centre in the plane of the magnetic equator, and placing it in such a manner that it shall be a little nearer America than Europe. It is by the observations themselves, when we shall have a greater number, that we must be guided in these small corrections.

In a word, it must not be expected that we can represent in a rigorous manner, by a mathematical law, all the inclinations observed; for the phænomenon of the inclination, though more regular than the other magnetic effects, is not free from some anomalies: this may be easily seen on constructing the curve given by the observations themselves. Thus, for example, the inclination observed at Popayan is 0° 10′ greater than at St. Carlos del Rio Negro, though the magnetic latitude of the latter is 0·6852° (3° 7′) greater. The case is the same with observations made at Javita and Santa-Fé. Other anomalies are discovered in the comparative progress of the observations and formula. This is the case in regard to Carichana, St. Thomas de la Guyane, and Carthagena. The increase of the inclination from the first to the second of these points is by no means in harmony with the increase from the second to the third; and if we compare

compare together the intensities observed in these different places, the anomalies they exhibit announce in some measure those which the inclination ought to experience.

The cause of these anomalies becomes evident from what has been already remarked; they are merely the effect of local circumstances, and arise from the small systems of attraction by which the general phænomena are modified. This must be sensible in particular for that part of America which M. Humboldt travelled over, and which is traversed throughout its whole length by the grand chain of the cordillera of the Andes. It is also in these places that the most considerable differences exist. Popayan, for example, is situated near the volcanoes of Sotara and Pourace; it is joined to basaltic mountains abounding with magnetic iron. Near Sulumito, to the east of Popayan, these basaltic columns have very striking poles: in like manner Mexico is situated at the height of 1160 toises on the ridge of the grand cordillera of Lenschtitlan: the ground there is covered with porous basaltes and amygdaloids, which are almost all charged with magnetic iron. Must not all these causes have a sensible influence on the inclination of the magnetic needle; and must not the different dispositions of the ferruginous masses, or their change of state, in consequence of the action of nature, produce also variations? M. Humboldt made on this point a decisive observation: the earthquake of the 4th of November 1799 changed at Cumana the inclination of the needle. On the 1st of November it was 43° 65′; on the 7th it was only 42° 75′; and ten months after it returned to 42° 85′: but it did not regain its former value; the intensity of the magnetic force was not changed by the effect of this earthquake.

It is proved, then, by these observations, that local circumstances may have on the inclination a sensible influence; and this influence is remarked in the countries traversed by M. Humboldt*.

It appears, therefore, that the mathematical hypothesis which we have employed really expresses the law of nature at least to the north of the magnetic equator; for, though the first results observed towards the south seem to bend to it also, the uncertainty under which we are in regard to the true cause of these phænomena must stop our conjectures, and

* We can observe that the anomalies are sensible in particular in the islands.—*Note by the Authors of the Memoir.*

prevent

prevent us from extending too far the consequences of the laws which we observe*.

From the preceding results, we may calculate the points where the axis of the magnetic equator pierces the terrestrial surface; for their latitudes are equal to the complements of the obliquity of that equator, and their meridian is at 100° of longitude from its nodes. The north magnetic pole is found also at 97·7975° (79° 1′ 4″) of north latitude, and at 33·3719° (30° 2′ 5″) of longitude west from Paris, which places it to the north of America. The other magnetic pole, symmetric to the preceding, is situated in the same latitude south, and at 66·6281° (149° 67′ 55″) of longitude east from Paris, which places it amidst the eternal ice: indications entirely analogous to those of Wilke and Lemonnier.

If we could reach these poles, the compass would be seen vertical; but if any confidence can be placed in the law which we have discovered, this would be the only difference which would be observed in regard to the inclination, and we should be still as far distant as in Europe from the real centres which produce it. This result might appear to be of such a nature as to diminish the interest one might have in visiting these horrid regions, had we not also the hope of discovering there new phænomena in regard to the intensity of the magnetic force, and the influence of meteors.

These consequences do not entirely accord with the opinion pretty generally received, and which ascribes the increase of the magnetic effects towards the north to the great quantity of iron dispersed throughout these regions; but it appears to us that this opinion is not agreeable to the truth. The cordillera of the Andes contains an enormous quantity of magnetic iron: the native iron of Chaco, that problematic mass analogous to that of Pallas, and those of Xacateras in Mexico, is found even under the tropics†.

* Since this memoir was read, we can advance something more positive. Observations made at the Cape of Good Hope, Cape Horn, and New Holland, by different navigators, are very exactly represented by our formula; and it follows, that it extends also to the austral hemisphere. We hope soon to have numerous and very exact observations on the inclination of the needle in that part of the earth. But we have thought it our duty to add to our table such results as relate to it, and which we have been able to procure. We have inserted also two observations on the intensity, made with great care by M. Rossel, during the expedition of d'Entrecasteaux, which are very important, as they prove that the terrestrial magnetic force increases also in the austral hemisphere in proportion as one removes from the equator.—*Note by the Authors of the Memoir.*

† We may now add to the preceding considerations this decisive fact, that the intensity also increases when one approaches the south pole.—*Note by the Authors of the Mèmoir.*

 On

On seeing the inclinations of the compass so exactly represented in our hypothesis, we endeavoured to discover whether it could be applied also to the intensities observed by M. Humboldt; but we found that it did not apply. It gives, indeed, an increase of the magnetic forces from the equator to the pole; but this increase, which at first is too slow, becomes afterwards too rapid: I have not yet been able to try whether the small displacement of the terrestrial magnet will contribute towards representing them better: but it must be remarked, that the series of the intensities is extremely whimsical, and contains an infinite number of anomalies; so that local phænomena may have on this phænomenon a much more sensible influence than on the inclination.

On reviewing the results which we have given in this memoir, it is seen that we have first determined the position of the magnetic equator by direct observations, which had never been done before; we have then proved that the magnetic force increases in proceeding from that equator to the poles: in the last place, we have given a mathematical hypothesis, which when reduced to a formula satisfies all the inclinations hitherto observed.

Supposing, as we have done in this formula, the small corrections of which it is susceptible, its utility becomes evident, either for making known, in the course of time, the variations which may take place in the action of the terrestrial magnetism, or to ascertain or even foresee the value of the inclination, which in a great many cases is of great importance.

For example, near the magnetic equator, the increase or diminution of the inclination will indicate to a vessel on a voyage whether she has gained or lost in latitude by currents. This knowledge of the latitude is sometimes as important as that of longitude. On the coasts of Peru, for example, the currents tend from Chiloé to the north and north-east with such force, that one may go from Lima to Guayaquil in three or four days, and two, three, and sometimes five months are necessary to return. It is consequently of the greatest importance for vessels coming from Chili which stretch along the coast of Peru, to know their latitude. If they go beyond the port to which they are bound they must work to the southward, and every day's progress requires often a month of return. Unfortunately, the fogs which prevail during four or five months on the coasts of Peru prevent navigators from distinguishing the form of the coast: nothing is seen but the summits of the Andes,

Andes, and that of the peaks which rise above that stratum of vapours; but the figure of it is so uniform that pilots fall into mistakes. They often remain twelve or fifteen days without seeing the sun or the stars, and during that interval they come to anchor, being afraid of overshooting their port: but if we suppose that the inclination of the magnetic needle in the ports to the south of Lima is known, for example at Chancay, Huaura, and Santa, the dipping needle will show whether it be, in regard to Lima, to the south or the north. It will show at the same time opposite what point of the coast a vessel is; and this indication will be attended with more exactness than one could hope for, because in these seas the inclination varies with extraordinary rapidity. M. Humboldt, to whom we are indebted for these remarks, observed in these seas the following values:

Places.	South Latitudes.	Inclinations.
Huancey	10° 4′	6,80°
Huaura	11 3	9,00
Chancay	11 33	10,35

These observations prove that the error of three or four degrees in the inclination in these seas would produce but a degree of error in latitude; and, on account of the tranquillity of the Pacific Ocean, the inclination may be observed to within a degree nearly. Frequent instances of such results may be seen in books of voyages. In like manner, if one knew exactly the inclination at the mouth of the Rio de la Plata, it would be very useful to navigators, who, when the Pamperos blow, remain fifteen or eighteen days without seeing the heavenly bodies, and go on different tacks for fear of losing the parallel of the mouth of that river.

In a word, the inclination may indicate also the longitude in these seas: and this method may be employed when others fail. A vessel which sails there in the direction of a parallel could not find its longitude either by a chronometer or the declination of Halley, unless a star could be seen in order to take an horary angle or the magnetic azimuth. The dipping needle, then, throws light on the longitude amidst the thickest fogs. We point out this method as one of those which have only a local application; but hitherto little attention has been paid to it. These ideas may be extended and rectified by able navigators.

In general, if the inclination of the needle, and the law we have tried to establish, could be depended on, to observe the inclination and the terrestrial latitude would be sufficient to determine also the longitude: but we have not yet examined the extent of the errors of which this method may be susceptible, and consequently we confine ourselves to a mere indication of it.

The phænomenon of the inclination has in maritime observations a particular and very remarkable advantage, namely, that of not being subject to those great progressive variations which affect the declination. Without repeating what we have already said above on the supposed constancy of this phænomenon, it may be remarked that our formula even affords a new proof that it may comprehend in the same law the observations made thirty-six years ago in Lapland, those which Lacaille brought back in 1751 from the Cape of Good Hope, and those which M. Humboldt has lately made in America.

In short, when we tried to represent the inclinations in different latitudes by the supposition of a magnet infinitely small, very near the centre of the earth and perpendicular to the magnetic equator, we did not pretend to consider that hypothesis as any thing real, but only as a mathematical abstraction useful to connect the results, and proper to ascertain in future whether any changes exist. In regard to the declination and intensity, we freely confess that we are entirely unacquainted with their laws or their causes; and if any philosopher is so fortunate as to bring them to one principle, which explains at the same time the variations of the inclination, it will no doubt be one of the greatest discoveries ever made. But this research, exceedingly difficult, requires perhaps before it be attempted more observations, and in particular more precise observations than have hitherto been collected. For this reason we thought we might present to the class the preceding researches, imperfect as they are, begging it to receive them with indulgence. Should we be so happy as to find that our results appear of any utility, we propose to unite all the exact observations which have been made on this subject, in order to give the utmost degree of precision to the law we have discovered.

NORTHERN

S	41° 42′ 51″	10,6145	11,10	0,4355°	219
	228 56 50	32,4660	28,5185	+ 3,9475	204
	131 38 53	49,58	47,78	+ 1,8	- - -
	49 0 5	52,8889	55,555	— 2,6667	- - -
	57 13 52	70,04	68,89	+ 1,15	- - -
	263 21 18	78,7037	77,9667	— 0,737	265

ist longitude, reckoned from the node of the magnetic equator in the South Sea: this extent the magnetic equator is sensibly a great circle of the earth. We have plete the contour of that equator.—For the southern hemisphere we have given recasteaux. It results from them that the intensity of the terrestrial magnetism inclination observed by M. Derossel at Teneriffe being exactly the same as that comparable the results obtained by these philosophers in regard to the intensity: which he and M. Humboldt observed at Teneriffe. The result of this calcula- menon is very much modified by local circumstances, and incomparably more M. Humboldt is less than that which would result from our hypothesis; and g can be determined in regard to the law of this increase. The influence of ion and intensity of the magnetic forces experience there also sensible anoma- /. Derossel made at Sourabaya in the Island of Java. In a word, by comparing st be examined with critical accuracy, and must not be admitted but when n we should every moment fall into great errors, occasioned by the inco-

* From *Philosophical Transactions* of 1805, part i.

† See *Philosophical Transactions* of 1801, page 33C.

suspect

NORTHERN MAGNETIC HEMISPHERE.

Names of the Observers.	Places of Observation.	Latitudes reckoned from the Terrestrial Equator.	Longitudes reckoned from the Meridian of Paris.	Latitudes reckoned from the Magnetic Equator.	East Longitudes reckoned from the Node of the Magnetic Equator in the South Sea.	Number of Oscillations in 10′ of Time.	Inclinations given by Theory.	Inclinations given by Observation.	Differences in Degrees of the Centesimal Division.
		Old Division.	Old Division.	Old Division.	Old Division.		Centig. Division	Centig. Division	
Humboldt -	Magnetic equator in Peru - - -	7° 1′ 0″ S	80° 41′ 0″ W	0° 0′ 0″	40° 17′ 56″	211	0,000	0,00	0,00°
Lapeyrouse	Magnetic equator at sea between Brazil and the Island of Ascension - - - -	10 57 0	25 25 0	0 0 0	95 33 56	- - -	0,000	0,00	0,00
Humboldt -	Tompenda - -	5 31 4	80 27 0	1 30 54	39 52 51	213	[illegible]	[illegible]	[illegible]
The same -	Loxa - - - -	4 0 0	81 12 0	2 [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	+ [illegible]
The same -	Cuença - - -	2 54 9	80 43 0	4 [illegible] 4	[illegible]	[illegible]	[illegible]	[illegible]	— [illegible]
The same -	Quito - - - -	3 13 17	80 15 0	[illegible]	[illegible] 17 [illegible]	[illegible]	[illegible]	[illegible]	+ [illegible]
The same -	St. Antonio - -	0 0 0	80 12 0	7 0 53	39 18 52	220	[illegible]	[illegible]	— [illegible]
The same -	Popayan - - -	2 21 33 N	78 45 0	9 36 16	40 24 27	[illegible]	- - - -	[illegible]	- - - -
The same -	St. Carlos del Rio Negro - - -	1 52 4	70 10 0	[illegible] 11	[illegible]	[illegible]	[illegible]	[illegible]	— [illegible]
The same -	Javita - - - -	2 49 0	70 30 0	11 7 40	[illegible] 6	[illegible]	[illegible]	[illegible]	— [illegible]
The same -	Esmeralda - -	3 13 26	68 38 0	11 45 45	50 29 15	217	- - - -	[illegible]	- - - -
The same -	Santa Fe di Bagota	4 35 5	76 37 0	12 [illegible] 1	12 17 1	[illegible]	[illegible]	[illegible]	— [illegible]
The same -	Carichana - -	6 34 5	70 18 0	14 [illegible]	18 21 [illegible]	[illegible]	[illegible]	[illegible]	— [illegible]
The same -	St. Thomas of Guyana - -	8 8 21	66 26 0	16 [illegible]	[illegible] 7 [illegible]	[illegible]	[illegible]	[illegible]	— [illegible]
The same -	Carthagena - -	10 25 57	78 2 0	17 [illegible]	[illegible] 13	[illegible]	[illegible]	[illegible]	— 3,10
The same -	Mexico - - -	19 26 2	101 22 0	[illegible] 5 14	11 [illegible] 11	242	44,87	46,85	— 1,98
De Rossel in 1791 Humboldt in 1799	St. Croix in Teneriffe - - - -	28 28 30	18 37 0	39 12 40	72 0 26	238	61,9975	63,35	— 1,35
The same -	Atlantic Ocean -	38 52 0	16 20 0	49 28 22	106 30 10	242	74,29	75,76	— 1,47
The same -	Paris - - - -	48 50 15	0 0 0	57 57 0	128 22 47	245	80,69	77,62	+ 3,07
Euler junior	Petersburgh in 1755 - - -	59 56 23	27 58 0 E	64 41 0	173 30 25	- - -	85,21	81,67	+ [illegible]
Mallet - -	Kola, in Lapland, in 1769 - -	68 52 30	30 40 30 E	71 44 36	179 9 29	- - -	[illegible]	86,39	+ 3,20
Lord Mulgrave	In an island near Spitzbergen in 1773 - - -	79 50 00	7 38 00 E	83 9 50	127 40 5	- - -	96,1882	91,1111	+ 5,0771

SOUTHERN MAGNETIC HEMISPHERE.

Names of the Observers.	Places of Observation.	Latitudes reckoned from the Terrestrial Equator.	Longitudes reckoned from the Meridian of Paris.	South Latitudes reckoned from the Magnetic Equator.	East Longitudes reckoned from the Node of the Magnetic Equator in the South Sea.	Inclinations given by Theory.	Inclinations given by direct Observation.	Differences in Degrees of the Centesimal Division.	Number of Oscillations in 10′ of Time.
		Old Division.	Old Division.	Old Division.	Old Division.	Centig. Division	Centig. Division		
Humboldt -	Lima - - -	12° 2′ 31″ S	79° 33′ 0″ W	4° 48′ 36″ S	41° 42′ 51″	10,6145	11,10	0,4955°	219
Derossel, expedition of Entrecasteaux	Sourabaya in the Isle of Java -	7 14 23	110 21 28 E	15 37 22	228 56 50	32,4660	28,5185	+ 3,9475	204
Bayli in 1775	Cape of Good Hope	33 55 30	16 10 0 E	26 15 34	131 38 53	49,58	47,78	+ 1,8	- - -
Lapeyrouse	Bay of Talcaguara	36 42 0	75 53 0 W	28 42 14	49 0 5	52,8889	55,556	— 2,6667	- - -
The same -	In sight of the Isle of Patagonians	52 21 26	69 38 0 W	44 30 3	57 13 52	70,04	68,89	+ 1,15	- - -
Derossel, expedition of Entrecasteaux	New Holland -	43 34 30	144 36 33 W	54 12 43	263 21 18	78,7037	77,9667	— 0,737	265

The results comprehended in this table extend from 38° 55′ to 263° 21′ 18″ east longitude, reckoned from the node of the magnetic equator in the South Sea consequently, they comprehend more than 224°, and their agreement shows that in this extent the magnetic equator is sensibly a great circle of the earth. We have not yet calculated the observations for the 36 degrees of longitude which would complete the contour of that equator.—For the southern hemisphere we have given the observations, made with great care, by M. Derossel, during the expedition of d'Entrecasteaux. It results from them that the intensity of the terrestrial magnetism increases also in that hemisphere, when one removes from the magnetic equator. The inclination observed by M. Derossel at Teneriffe being exactly the same as that observed by M. Humboldt eight years after, this agreement has allowed us to render comparable the results obtained by these philosophers in regard to the intensity. for this purpose we have multiplied the results of M. Derossel, by giving the numbers which he and M. Humboldt observed at Teneriffe. The result of this calculation will be found in the column of oscillations. It is there again seen that this phænomenon is very much modified by local circumstances, and incomparably more than the inclination. The increase of the intensity deduced from the observations of M. Humboldt is less than that which would result from our hypothesis, and that given by the observations of M. Derossel is too great which proves that nothing can be determined in regard to the law of this increase The influence of local circumstances on the inclination is particularly sensible in the isles. The declination and intensity of the magnetic force experience there also sensible anomalies. This fact is indicated by several observations, and in particular by those which M. Derossel made at Sourabaya in the Island of Java. In a word, by comparing the results of our formula with the observations of different navigators, the latter must be examined with critical accuracy, and must not be admitted but when they agree with each other and with those of other navigators without this precaution we should every moment fall into great errors, occasioned by the incoherency of the results. besides, we present the preceding only as a first approximation.

XLV. *Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.**

MY DEAR SIR,

It is well known that the fluid, generally called the sap, in trees, ascends in the spring and summer from their roots, and that in the autumn and winter it is not, in any considerable quantity, found in them; and I have observed in a former paper, that this fluid rises wholly through the alburnum, or sap-wood. But Du Hamel and subsequent naturalists have proved, that trees contain another kind of sap, which they have called the true or peculiar juice, or sap, of the plant. Whence this fluid originates does not appear to have been agreed by naturalists; but I have offered some facts to prove that it is generated by the leaf†; and that it differs from the common aqueous sap, owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which Du Hamel has called the *suc propre*, and which I will call the true sap,) the whole substance, which is annually added to the tree, is derived. I shall endeavour in the present paper to prove that this fluid, in an inspissated state, or some concrete matter deposited by it, exists during the winter in the alburnum, and that from this fluid, or substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring, and thus furnishes those organs, which were not wanted during the winter, but which are essential to the further progress of vegetation.

Few persons at all conversant with timber are ignorant, that the alburnum, or sap-wood, of trees which are felled in the autumn or winter, is much superior in quality to that of other trees of the same species which are suffered to stand till the spring, or summer: it is at once more firm and tenacious in its texture, and more durable. This superiority in winter-felled wood has been generally attributed to the absence of the sap at that season; but the appearance and qualities of the wood seem more justly to warrant the conclusion, that some substance has been added to, instead of taken from it, and many circumstances induced me to

* From *Philosophical Transactions* of 1805, part i.

† See *Philosophical Transactions* of 1801, page 336.

suspect that this substance is generated, and deposited within it, in the preceding summer and autumn.

Du Hamel has remarked, and is evidently puzzled with the circumstance, that trees perspire more in the month of August, when the leaves are full grown, and when the annual shoots have ceased to elongate, than at any earlier period; and we cannot suppose the powers of vegetation to be thus actively employed, but in the execution of some very important operation. Bulbous and tuberous roots are almost wholly generated after the leaves and stems of the plants, to which they belong, have attained their full growth; and I have constantly found, in my practice as a farmer, that the produce of my meadows has been immensely increased when the herbage of the preceding year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the summer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the summer, in the preparation of matter calculated to afford food to the expanding buds and blossoms of the succeeding spring, and to enter into the composition of new organs of assimilation.

If the preceding hypothesis be well founded, we may expect to find that some change will gradually take place in the qualities of the aqueous sap of trees during its ascent in the spring; and that any given portion of winter-felled wood will at the same time possess a greater degree of specific gravity, and yield a larger quantity of extractive matter, than the same quantity of wood which has been felled in the spring or in the early part of the summer. To ascertain these points I made the experiments, an account of which I have now the honour to lay before you.

As early in the last spring as the sap had risen in the sycamore and birch, I made incisions into the trunks of those trees, some close to the ground, and others at the elevation of seven feet, and I readily obtained from each incision as much sap as I wanted. Ascertaining the specific gravity of the sap of each tree, obtained at the different elevations, I found that of the sap of the sycamore with very little variation, in different trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free from taste; but when obtained at a greater height, it was sensibly sweet.

The

The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance; and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progres of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above lighter by privation of its proper quantity of nutriment. The most eligible method therefore, which occurred to me, was to select and mark in the winter some of the poles of an oak coppice, where all are of equal age, and where many, of the same size and growing with equal vigour, spring from the same stool. One half of the poles which I marked and numbered were cut on the 31st of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to distinguish the winter-felled from the summer-felled poles, the bark being

being left on all, and all being placed in the same situation to dry.

In the beginning of August I cut off nearly equal portions from a winter and summer-felled pole, which had both grown on the same stool; and both portions were then put in a situation, where, during the seven succeeding weeks, they were kept very warm by a fire. The summer-felled wood was, when put to dry, the most heavy; but it evidently contained much more water than the other, and, partly at least, from this cause, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then ascertained the specific gravity of the winter-felled wood to be 0.679, and that of the summer-felled wood to be 0.609; after each had been immersed five minutes in water.

This difference of ten *per cent.* was considerably more than I had anticipated, and it was not till I had suspended and taken off from the balance each portion, at least ten times, that I ceased to believe that some error had occurred in the experiment: and indeed I was not at last satisfied till I had ascertained by means of compasses adapted to the measurement of solids, that the winter-felled pieces of wood were much less than the others which they equalled in weight.

The pieces of wood, which had been the subjects of these experiments, were again put to dry, with other pieces of the same poles, and I yesterday ascertained the specific gravity of both with scarcely any variation in the result. But when I omitted the medulla, and parts adjacent to it, and used the layers of wood which had been more recently formed, I found the specific gravity of the winter-felled wood to be only 0.583, and that of the summer-felled to be 0.533; and trying the same experiment with similar pieces of wood, but taken from poles which had grown on a different stool, the specific gravity of the winter-felled wood was 0.588, and that of the summer-felled 0.534.

It is evident that the whole of the preceding difference in the specific gravity of the winter and summer-felled wood might have arisen from a greater degree of contraction in the former kind, whilst drying; I therefore proceeded to ascertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when steeped in water. Having therefore reduced to small fragments 1000 grains of each kind, I poured on each portion six ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had sunk to 60°, I found

I found that the winter-felled wood had communicated a much deeper colour to the water in which it had been infused, and had raised its specific gravity to 1.002. The specific gravity of the water in which the summer-felled wood had in the same manner been infused was 1.001. The wood in all the preceding cases was taken from the upper parts of the poles, about eight feet from the ground.

Having observed, in the preceding experiments, that the sap of the sycamore became specifically lighter when it had continued to flow during several days from the same incision, I concluded that the alburnum in the vicinity of such incision had been deprived of a larger portion of its concrete or inspissated sap than in other parts of the same tree: and I therefore suspected that I should find similar effects to have been produced by the young annual shoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain less extractive matter than an equal portion taken from the lower parts of the same pole, where no annual shoots or leaves had been produced.

No information could in this case be derived from the difference in the specific gravity of the wood; because the substance of every tree is most dense and solid in the lower parts of its trunk: and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces several equal portions of wood taken from different parts of the same poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I suffered to remain twenty hours, as in the preceding experiments: and I then found that in some instances the wood from the lower, and in others that from the upper parts of the poles, had given to the water the deepest colour and greatest degree of specific gravity; but that all had afforded much extractive matter, though in every instance the quantity yielded was much less than I had, in all cases, found in similar infusions of winter-felled wood.

It appears, therefore, that the reservoir of matter deposited in the alburnum is not wholly exhausted in the succeeding spring: and hence we are able to account for the several successions of leaves and buds which trees are capable of producing when those previously protruded have been destroyed by insects, or other causes; and for the extremely luxuriant shoots, which often spring from the trunks of trees, whose branches have been long in a state of decay.

I have also some reasons to believe that the matter deposited in the alburnum remains unemployed in some cases

 during

during several successive years: it does not appear probable that it can be all employed by trees which, after having been transplanted, produce very few leaves, or by those which produce neither blossoms nor fruit. In making experiments in 1802, to ascertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear-tree, at a small distance from the trunk; and I pared off, at the same time, the whole of the lifeless external bark. The age of this tree, I have good reasons to believe, somewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every successive year to terminate its existence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the succeeding spring, or early part of the summer: but in the beginning of July numerous buds penetrated through the bark in every part, many leaves of large size every where appeared, and in the autumn every part was covered with very vigorous shoots exceeding, in the aggregate, two feet in length. The number of leaves which, in this case, sprang at once from the trunk and branches appeared to me greatly to exceed the whole of those, which the tree had borne in the three preceding seasons; and I cannot believe that the matter which composed these buds and leaves could have been wholly prepared by the feeble vegetation and scanty foliage of the preceding year.

But whether the substance which is found in the alburnum of winter-felled trees, and which disappears in part in the spring and early part of the summer, be generated in one or in several preceding years, there seem to be strong grounds of probability, that this substance enters into the composition of the leaf: for we have abundant reason to believe that this organ is the principal agent of assimilation; and scarcely any thing can be more contrary to every conclusion we should draw from analogical reasoning and comparison of the vegetable with the animal economy, or in itself more improbable, than that the leaf, or any other organ, should singly prepare and assimilate immediately from the crude aqueous sap, that matter which composes itself.

It has been contended* that the buds themselves contain the nutriment necessary for the minute unfolding leaves: but trees possess a power to reproduce their buds, and the matter necessary to form these buds must evidently be derived from some other source: nor does it appear probable that the young leaves very soon enter on this office; for the

* Thomson's Chemistry.

expe-

experiments of Ingenhousz prove that their action on the air which surrounds them is very essentially different from that of full grown leaves. It is true that buds in many instances will vegetate and produce trees, when a very small portion only of alburnum remains attached to them: but the first efforts of vegetation in such buds are much more feeble than in others to which a larger quantity of alburnum is attached; and therefore we have, in this case, no grounds to suppose that the leaves derive their first nutriment from the crude sap.

It is also generally admitted, from the experiments of Bonnet and Du Hamel, which I have repeated with the same result, that in the cotyledons of the seed is deposited a quantity of nutriment for the bud, which every seed contains; and though no vessels can be traced* which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous passage, which is perfectly similar to that through which I conceive the sap to be carried from the leaves to the buds in the subsequent growth of the tree; and I am in possession of many facts to prove that seedling trees, in the first stage of their existence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many instances killed, by being put to vegetate in rich mould.

We have much more decisive evidence that bulbous and tuberous rooted plants contain the matter within themselves which subsequently composes their leaves; for we see them vegetate even in dry rooms on the approach of spring; and many bulbous rooted plants produce their leaves and flowers with nearly the same vigour by the application of water only, as they do when growing in the best mould. But the water in this case, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by dissolving the matter prepared and deposited in the preceding year; and hence the root becomes exhausted and spoiled: and Hassenfratz found that the leaves and flowers and roots of such plants afforded no more carbon than he had proved to exist in bulbous roots of the same weight, whose leaves and flowers had never expanded.

As the leaves and flowers of the hyacinth, in the preceding case, derived their matter from the bulb, it appears extremely probable that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many species precede their leaves: and as the roots of plants become weakened and apparently exhausted, when

* Hedwig.

they

they have afforded nutriment to a crop of seed, we may suspect that a tree, which has borne much fruit in one season, becomes in a similar way exhausted, and incapable of affording proper nutriment to a crop in the succeeding year. And I am much inclined to believe that were the wood of a tree in this state accurately weighed, it would be found specifically lighter than that of a similar tree, which had not afforded nutriment to fruit or blossoms, in the preceding year, or years.

If it be admitted that the substance which enters into the composition of the first leaves in the spring is derived from matter which has undergone some previous preparation within the plant, (and I am at a loss to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at least,) it must also be admitted that the leaves which are generated in the summer derive their substance from a similar source; and this cannot be conceded without a direct admission of the existence of vegetable circulation, which is denied by so many eminent naturalists. I have not, however, found in their writings a single fact to disprove its existence, nor any great weight in their arguments, except those drawn from two important errors in the admirable works of Hales and Du Hamel, which I have noticed in a former memoir. I shall therefore proceed to point out the channels, through which I conceive the circulating fluids to pass.

When a seed is deposited in the ground, or otherwise exposed to a proper degree of heat and moisture, and exposure to air, water is absorbed by the cotyledons, and the young radicle or root is emitted. At this period, and in every subsequent stage of the growth of the root, it increases in length by the addition of new parts to its apex, or point, and not by any general distension of its vessels and fibres; and the experiments of Bonnet and Du Hamel leave little grounds of doubt, but that the new matter which is added to the point of the root descends from the cotyledons. The first motion therefore of the fluids in plants is downwards, towards the point of the root; and the vessels which appear to carry them, are of the same kind with those which are subsequently found in the bark, where I have, on a former occasion, endeavoured to prove that they execute the same office.

In the last spring I examined almost every day the progressive changes which take place in the radicle emitted by the horse chesnut: I found it, at its first existence, and until it was some weeks old, to be incapable of absorbing coloured

coloured infusions, when its point was taken off, and I was totally unable to discover any alburnous tubes, through which the sap absorbed from the ground, in the subsequent growth of the tree, ascends: but when the roots were considerably elongated, alburnous tubes formed; and as soon as they had acquired some degree of firmness in their consistence, they appeared to enter on their office of carrying up the aqueous sap, and the leaves of the plumula then, and not sooner, expanded.

The leaf contains at least three kinds of tubes: the first is what, in a former paper, I have called the central vessel, through which the aqueous sap appears to be carried, and through which coloured infusions readily pass, from the alburnous tubes into the leaf-stalk. These vessels are always accompanied by spiral tubes, which do not appear to carry any liquid: but there is another vessel which appears to take its origin from the leaf, and which descends down the internal bark, and contains the true or prepared sap. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon; but being exposed to the air, and without the same means to acquire, or the substance to retain moisture, it is fed by the alburnous tubes and central vessels. The true sap now appears to be discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark, and to be employed in the formation of new alburnous tubes between the base of the leaf and the root. From these alburnous tubes spring other central vessels and spiral tubes, which enter into, and possibly give existence to, other leaves; and thus by a repetition of the same process the young tree or annual shoot continues to acquire new parts, which apparently are formed from the ascending aqueous sap.

But it has been proved by Du Hamel that a fluid, similar to that which is found in the true sap vessels of the bark, exists also in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whose true sap is white, or coloured. The vessels, which contain this fluid in the alburnum, are in contact with those which carry up the aqueous sap; and it does not appear probable that, in a body so porous as wood, fluids so near each other should remain wholly unmixed. I must therefore conclude, that when the true sap has been delivered from the cotyledon or leaf into the returning or true sap vessels of the bark, one portion of it secretes through the external cellular, or more probably glandular substance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it secretes through the internal glandular substance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and subsequently mingles with the ascending aqueous sap; which thus becomes capable of affording the matter necessary to form new buds and leaves.

It has been proved in the preceding experiments on the ascending sap of the sycamore and birch, that that fluid does not approach the buds and unfolding leaves in the spring, in the state in which it is absorbed from the earth: and therefore we may conclude that the fluid, which enters into and circulates through the leaves of plants, as the blood through the lungs of animals, consists of a mixture of the true sap or blood of the plant with matter more recently absorbed, and less perfectly assimilated.

It appears probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap; for this fluid in the sycamore has been proved to become more sensibly sweet in its progress from the roots in the spring, and the liquid which flows from the wounded bark of the same tree is also sweet; but I have never been able to detect the slightest degree of sweetness in decoctions of the sycamore wood in winter. I am therefore inclined to believe that the saccharine matter existing in the ascending sap is not immediately, or wholly, derived from the fluid which had circulated through the leaf in the preceding year; but that it is generated by a process similar to that of the germination of seeds, and that the same process is always going forward during the spring and summer, as long as the tree continues to generate new organs. But towards the conclusion of the summer I conceive that the true sap simply accumulates in the alburnum, and thus adds to the specific gravity of winter-felled wood, and increases the quantity of its extractive matter.

I have some reasons to believe that the true sap descends through the alburnum as well as through the bark; and I have been informed that if the bark be taken from the trunks of trees in the spring, and such trees be suffered to grow till the following winter, the alburnum acquires a great degree of hardness and durability. If subsequent experiments prove that the true sap descends through the alburnum, it will be easy to point out the cause why trees continue to vegetate after all communication between the leaves and

and roots, through the bark, has been intercepted: and why some portion of alburnous matter is in all trees* generated below incisions through the bark.

It was my intention this year to have troubled you with some observations on the reproduction of the buds and roots of trees; but as the subject of the paper, which I have now the honour to address to you, appeared to be of more importance, I have deferred those observations to a future opportunity; and I shall at present only observe, that I conceive myself to be in possession of facts to prove that both buds and roots originate from the alburnous substance of plants, and not, as is, I believe, generally supposed, from the bark.

I am, &c.

Elton, Dec. 4, 1804. T. ANDREW KNIGHT.

XLVI. *Description of the Coming-up Glass Telescope, as made by Mr.* THOMAS JONES, *Mathematical, Optical, and Philosophical Instrument Maker; Pupil of the late Mr.* RAMSDEN.

As it is of much importance, especially in war time, during the chase of a vessel, that the chasing ship should be able to ascertain whether she lessens or increases her distance from the object of which she is in pursuit, I am persuaded the philosophical world will be pleased with a description of an instrument adapted to this useful purpose, invented by the late Mr. Ramsden. It consists in applying a micrometer to a refracting telescope of about two feet long, or to a day and night telescope; and I have been informed by some who were furnished with them by Mr. Ramsden, (and by myself since his death,) that they answer the purpose remarkably well.

To this combination the English sailors have given the name of a *coming-up glass*. To fit the telescope for this use, the third glass from the eye, in the drawer of the telescope, is divided in two by cutting it across its centre at right angles to its surface; and, in using it, these two semi-lenses are separated from each other in the direction of their line of separation. By turning a finger-screw on one side

* I have in a former paper stated that the perpendicular shoots of the vine form an exception. I spoke on the authority of numerous experiments; but they had been made late in the summer; and on repeating the same experiments at an earlier period, I found the result in conformity with my experiments on other trees.

of the eye-tube, each semi-lens forms an image of the same object. These two images will be more or less separated in the proportion of the distance of the centres of the semi-lenses from each other, which distance is shown in revolutions and parts of a revolution of a finger-screw that separates them. For this purpose a circular head is fixed on the finger-screw, the edge of which is divided into a hundred parts; and in order to know the number of revolutions, a small slip of brass (that passes over the graduated surface of the head, and serves as its index for showing the centesimal parts of a revolution) is fixed to the eye-tube, and has its chamfered edge also divided, each division being equal to one entire revolution of the screw.

To adjust the Telescope.

Having, by drawing out the eye-tube more or less, adjusted the telescope to distinct vision, turn the finger-screw till the two images of the same object appear in one, and the edge of the head together with the division numbered 100 will be found at the first division on the index that shows the revolutions.

The use of the Coming-up Glass.

Having directed the telescope to the vessel chased, turn the finger-screw till the two images of some well defined part of the vessel appear to have their extreme edges in contact with each other; then read off the number of revolutions of the screw shown on the chamfered edge, also the parts of a revolution shown on the edge of the head: then if, after some time, it be required to know whether we have gained or lost in the chase, again bring the edges of the images of the same object in contact as before. If the number of revolutions and parts of the screw be the same as was shown before, we have neither gained nor lost in the chase. But if the number of revolutions and parts be less, the distance from the chased vessel will be greater in the proportion of the difference of these numbers to the former. On the contrary, if the number of revolutions and parts be greater, we come nearer the vessel in the proportion of the difference of these numbers to the number of revolutions and parts of the first observation.

Example.

Suppose in the chase of a vessel, by turning the finger-screw, I bring the image of the main-top-yard to coincide with the main-yard; and, reading off the value, I find it to be three revolutions on the chamfered edge, and 20 parts of a revoa revolution on the circular head, which may be wrote 320; if, some little time after, by bringing the two images of the same object in contact, I find the number of revolutions and parts to be 360, the difference of the two observations is 40, therefore 40 : 320 : : 1 : 8 (that is, 40, the difference, is to 320, the first observation, as 1 is to 8); consequently, we shall have gained in the chase one-eighth part of the distance: but if at the second observation the number of revolutions be less, for instance 280, or two revolutions and 80 parts of a revolution; then, as 40, the difference, is to 320, the first observation, so is 1 to 8: consequently we should have lost one-eighth part of our distance in the chase.

No. 120, Mount-street,
Berkeley-square.

XLVII. *On the Buds and Ramifications of Plants; the Birth of these Organs, and the organic Relation between the Trunk and the Branches: in a Letter from* G. L. Koeler, *M. D. Professor of Botany and the Materia Medica in the Provisional School of Medicine at Mentz, to M.* Ventenat, *Member of the French National Institute.*

[Concluded from p. 241.]

But it may be said, if these observations on the origin of buds are founded in nature, how comes it to pass that herbaceous and so tender bodies should penetrate through a considerable number of ligneous zones, which must be the case in branches of two or three years of age, which, though rarely, produce, however, sometimes buds? It appears to me very probable, that at the period when the *bud* becomes expanded, a period which corresponds with the elongation of the medullary sheath, a certain number of the bundlos of the tubes of that sheath proceeds laterally from interval to interval towards the bark. Soon after, the first *cambium* of the bud appears between the medullary sheath and the bark, and separates these two parts from each other. This cambium does not long retain its mucilaginous form; it is soon metamorphosed into the first *liber* or bark. The bent vessels of this first liber, in hardening and becoming every day straighter, proceed towards the medullary sheath, round which they compress themselves, and form there the first *alburnum*, which becomes then the first ligneous zone. The prolongations of the medullary sheath are not all pinched by the formation of this first

 stratum

stratum of the wood; for the latter quits its mucilaginous state slowly, and its tubes have not straightened themselves by a great effort: on the contrary, they retain their curvature around the prolongations, as may be readily seen. The straightening, however, and compression of the tubes of the liber one against the other, ought to be attended with this necessary consequence, that the prolongations should be more or less pressed; and as this is repeated every year in the formation of the new ligneous strata, the result must be, that not only a certain number of prolongations are at length stifled, but also that the quantity increases in proportion as the twig becomes a branch, and as the latter acquires more size. This opinion appears to me to be very well founded, for we observe that the number of the buds is generally in the inverse ratio of the age of the branch: nature will have it that this number should decrease more and more every year, and she fixes a period at which branches generally cease altogether to produce any. On this rule, modified according to the nature of each species of vegetable, and according to the particular circumstances under which the plants are placed, depends in a great measure the *habitus* of the vegetables. Nature rarely deviates from this law: there are, however, some exceptions; and it sometimes happens that it retains, in case of need, some of these prolongations, which have not expanded into buds during the first years of the branch. If, for example, the sap rises in too great abundance, or if it be too nourishing, especially when the tree cannot sufficiently discharge it, such of these prolongations as are not stifled acquire vigour, lengthen themselves towards the bark, pierce it, and give birth to gluttonous branches, or branches with false wood, which often preserve the tree from plethoric diseases which might become mortal. The birth of these twigs explains to us, in my opinion, why the buds from which they have been produced are always thin and herbaceous; why they almost always expand very speedily, and often out of season; and why they do not adhere firmly to the tree. It appears besides, that my opinion on the question itself is confirmed by the consideration, that if nature forms more wood than usual, and very thick zones, the branches commonly give fewer buds the following year: but nature seems then to gain on the one hand what it loses on the other; for it multiplies then the number of the young branches, which, as we know, produce more buds than any other part of the vegetable. Another observation, which renders my opinion still more probable, is, that I have seen at the lower part of the branches, and the

the twigs of several trees and shrubs, such as the young branches of the common willow (*salix caprea*), a considerable number of small herbaceous prolongations of the medullary sheath, which were as insulated and dispersed throughout the ligneous body, and which had neither pierced nor raised up the bark that covered them. They appeared to me to be prolongations reserved to form, in case of need, leaves or branches.

But how can we account for the origin of buds from a trunk the interior part of which is rotten, and where the medullary sheath no longer exists? and how comes it to pass that the branches of a hollow tree, or those grafted on another tree, vegetate with the greatest vigour? It appears to me that the answer to this question presents no difficulty. These phænomena will soon be explained, if we shall be able to prove that the prolongations of the sheath may be preserved, and live, when they are separated by art or by nature from the sheath whence they have arisen. In these cases the branches must receive the sap by other tubes than those of the sheath. But does not the observation made by Coulomb, that in sound trees the sap ascends chiefly by the tubes which surround the pith, oppose what I have said? I do not think it: on the contrary, I am of opinion, that, if the observation of that celebrated philosopher had still need of being corroborated, it certainly would be by my experiments on the prolongations of the sheath. It is in these organs indeed, more than in any other, that we are to search for the proof that the sap ascends chiefly in the vessels of the medullary sheath of a sound tree, because when cut transversely they are always found, as long as they are not ligneous, more or less filled with juices. But is the medullary sheath the only way by which the *sap* proceeds from the root to the extremities of the vegetable? Has nature confined herself within so narrow limits, or has she established other canals to conduct the nourishing juices from the root to all the ramifications of the plant? The existence of so many hollow trees will furnish us with an answer. In hollow trunks it is only by the *liber,* and before the birth of that organ by the *alburnum,* that nature makes the sap to ascend. It happens also, but more rarely, that the bark is provided with small tubes which convey a part of that limpid liquor. If we examine the hollow willows on the approach of spring, it will be observed that the *alburnum* is full of juice. If we cut the stems, branches, and stalks of the vine, euphorbia, &c. we shall see the tubes next the bark emit not only their own juices but also an

 aqueous

aqueous humour, which is the ascending sap. The *liber*, *alburnum*, medullary sheath, and sometimes the tubes of the bark, are the only organs which nature employs in the cotyledons to convey the sap to the ramifications of the trunk. But after the sheath has produced the prolongations or germs of the buds, the liber and alburnum secure in preference the preservation of the individual; for, when the new production of these organs is prevented, the plant infallibly dies: on the other hand, if nature destroys the sheath, the tree very often vegetates as before; and to be fully convinced of it we need only mention the enormous boabab of the coast of Africa. Besides, the ingenious experiments of Mirbel have fully shown how, by the help of the pores and fissures with which almost all the tubes and cells of vegetables are pierced, the sap may be conveyed to all the parts; and how nature, to convey the juice from one organ to the other, may find ways which to us appear extraordinary. Hence we are obliged to believe, that in the case where the sheath does not exist, or a graft has succeeded, and where the pith is converted into wood, the prolongations may receive the nourishing liquor with which they are filled, from the living tissue with which they are in immediate contact. In plants, even the interior part of which is very sound, the sap cannot be conveyed immediately from the tubes of the sheath of a trunk to those of the sheath of an old branch. In this case, the prolongations are not composed of tubes; they have become entirely ligneous, and their vessels, changed into fibres, no longer convey the juices. There is no longer any organic communication but by means of the *alburnum*, the *liber*, the medullary radii, and the bark: even the latter rarely serves for conveying the sap. It thence follows, that the sap of the trunk ascends in the sheath, passes thence into the *liber*, or the *alburnum*, to be conveyed to the sheath of the branch through the cellular and vascular tissue. When it reaches the sheath of the branch, it is conveyed to the following ramification in the same manner as from the trunk to the principal branch.

The origin of buds, of which I have spoken, enables us to conceive the cause of the firm insertion of the branch into the trunk. The bud has been produced by a prolongation of the medullary sheath: the first stratum of the wood is formed while that bud has been elongated into a branch: the trunk has produced one at the same time; the ligneous zones have succeeded regularly from year to year in the trunk and in the branches, and each zone of the branches

is

is in an uninterrupted relation with another zone of the trunk, and seems to be thus produced by the prolongation or increase of the latter. It appears to me that the cause is explained by the following observation:—It is generally known that the *cambium*, of which the *liber* is formed, has its birth in the *alburnum* and bark, and that the latter, during the formation of the *cambium*, is altogether separate from the *alburnum* even at the base of the branch. By these means the *cambium* of the trunk and that of the branch are never divided; on the contrary, they adhere to each other, and the result is, that the wood thence arising forms thus an uninterrupted zone. The old trunks of the pine, fir, oak, &c. seem, however, sometimes to show the contrary; for it is not uncommon that the branches, even those covered with bark, traverse a greater or less part of the wood of the trunk, without being there united to the exterior zones. Nevertheless, if such a trunk be cut vertically, it will be seen that all the zones of the branch have an immediate reference to as many zones of the trunk; the reason of which is, that this branch has been cut, or has ceased to grow, and that the trunk, which has continued its zones, has surrounded and inclosed the branch sometimes even entirely.

It may perhaps be asked, why all the germs of a twig do not pierce the bark at the same time the first year. This question I am not able to answer, because I cannot unveil the mysteries of nature, and, in regard to the birth of buds, know only the above phænomena. It may, however, be conjectured that in the expanded buds there were germs, that is to say, prolongations of the medullary sheath, differing in size and vigour; and that, besides, the time of their respective development depended on external circumstances, the most of which are still unknown to us.

It is observed sometimes in shumac, the plane-tree, &c. that the lateral prolongations are entirely dried and ligneous, though that in the middle be still perfectly herbaceous. In this case the petiole which covered the great prolongation, and below which the bud that produces the twig must have been formed, has been torn or dropped off the preceding spring, or at least before the autumn.

The means which nature has made choice of to nourish the buds during the winter have been the object of several hypotheses. I shall abstain from mentioning them all, in order that I may attach myself to the only one which merits particular consideration. The interior of the bud has been compared to the embryo of a seed, and it has been asserted that

that the body of this bud was nourished by the scales which cover it, as the embryo is by the cotyledons. I shall not here examine whether the embryo be really nourished by the cotyledons; but I must deny that the bud is nourished by the scales, sometimes very arid, which protect it. On the contrary, it is nourished by the juice conveyed to it by the prolongations of the sheath; for I have had opportunities of remarking that the motion of the sap does not entirely cease in the interior of the tree but when the cold is severe, and the buds are then preserved without having need of nourishment. To insure the conveyance of the sap to the buds, and by these means to shelter them as much as possible from the cold, nature, perhaps, has caused the prolongations of the sheath to be traversed by one or more zones of wood.

In regard to the symmetry which nature exhibits in the disposition of the prolongations of the sheath and the buds, it is as yet inexplicable to us: the hypothesis that the bud pierces the bark at the place where it is thinnest, as for example in the eyes of the leaves, is still very far from the truth. The question is, to know how the prolonged tubular bundles of the medullary sheath which is directed towards the bark, may meet that thin place without having crept along the interior side of that organ. A thousand other questions might be added, which present difficulties equally great.

It appears to me very probable that Hales, Linnæus, and several other observers who had adopted the hypothesis that the pith is the most active organ in the interior œconomy of plants, the reproducing organ of the ramifications of the trunk, were not far from discovering the tubular prolongations of the sheath, which I discovered by chance. It appears to me also that the *green substance* contained in the medullary sheath has a different origin from that ascribed to it; and that in regard to the *medullary radii*, to which Daubenton has given the name of *medullary prolongations*, they are very different from the organs to which I have given the name of *prolongations of the sheath*.

My researches in regard to these organs have made me perceive also some phænomena respecting the origin of leaves, which were entirely unknown to me. These organs are considered as expansions of the herbaceous tissue of the bark, and of the tubular tissue of the *liber*. This opinion is among the number of those which cannot be admitted. A leaf, whatever be the place of its insertion, can never be torn off without finding, exactly at the place where it was fixed,

fixed, one or more prolongations of the medullary sheath penetrating the wood, and sinking into the petiole, or into the base of the leaf to which they convey sap. In the species of the family of the *pinnifera* one single prolongation of the like kind enters into the petiole: in all the other vegetables which I have examined, I found two, three, and even more.

The *fall of the leaves* is explained, not only by the influence of the air and the sun, but also by the mechanism of nature, which, according to the opinion most generally admitted, prevents the sap from ascending from the branch to the petiole. "The tubes of the *liber* (it is said) in daily extending more press against each other, and by a necessary consequence transport themselves towards the centre of the vegetable; at the same time the bark, which dilates itself, recedes; and the necessary result of these two movements is, that the leaves no longer receive sufficient nourishment." There is no doubt that the influence of the air and the sun contributes powerfully to harden the substance of the leaves, as well as to approach towards the ligneous state, in which the passage of the juices ceases, and then the leaves no longer transpire or absorb. But I do not believe that the mechanism in question can in any manner prevent the ascent of the sap. But do we not, indeed, see that in a great number of plants some leaves drop at the period when the liber has not yet been changed into *alburnum*, and while the sap is still in full motion? In a word, when the bark is separated at the place where the petiole has been fixed, it is seen that the prolongations of the medullary sheath, which have conveyed the nourishing juice from the stem to the leaves, are still green and herbaceous, and have sustained no injury; which seems to prove that they were not choked, nor attracted with force. They dry a long time after, and acquire very slowly the consistence of the wood in which they are placed.

XLVIII. *Progress of Vaccination in India**.

Never was a discovery corroborated by more numerous and distant evidences than the cow-pock.

The bramins in India were accustomed to inoculate at certain periods of the year; now they vaccinate throughout the year; and the following testimony has appeared in the

* Communicated by Dr. Thornton.

government

government gazette at Fort St. George, Dec. 19, 1804, being a letter from the head bramin to the physician-general at Madras:

To Dr. James Anderson.

SIR,

I beg leave to assure you that I am an eye-witness, as well as many other bramins, of that wonderful, healthful, immortal vaccine matter, discovered on the nipples and udders of some cows in England by that illustrious doctor, Jenner; whereby that loathsome, painful, and fatal small-pox has been prevented attacking persons in India as well as in England. Numbers of children and others have been inoculated by us without any injury whatever, except a small blemish or spot in the place where the matter is applied, generally on the arm.

This preserving power should be experienced not only by the honorary but also those of the low cast. On which account permit me to observe, that the term *cow-pox*, advertised in our Tamul tongue by you, translated *comary*, should be altered, not to give room for the prejudices of the very common people; and it should be styled, no doubt, *a drop of nectar from the exuberant udders of some cows in England*, and not by any appellation similar to the humour discharged from the feet of diseased cattle in this country.

I am, sir, with much respect,

Your faithful, obedient, very humble servant,

Dec. 19, 1804. MOO-PE-RAL STEE-NE-VA-SA-CHA-RY.

XLIX. *Memoir on a new Genus of Mammalia called Hydromis.* *By* E. GEOFFROI (SAINT-HILAIRE).*

THIS new genus, introduced into the system, is composed of three species, two of which have never been described, and the third cannot be exactly known but by means of the illustrations which I am about to give. It was first found in the waters of Chili by Molina, who published an account of it under the American name of *coypou:* it has since been met with only by Felix d'Azzara, who has given a long description of it† under the name of *quouyia*, by which it is known in the province of Tucuman.

A long time, however, after these travellers had discovered the *coypou*, it had engaged the attention of Commer-

* From *Annales du Muséum d'Histoire Naturelle*, No. 31.

† Histoire des Quadrupedes du Paraguay, vol. ii. art. 2.

son;

son; and it would then have been more perfectly known, had that respectable and indefatigable naturalist lived long enough to publish the immense materials with which he enriched natural history. There was found, indeed, among Commerson's papers, a drawing of this singular animal reduced only to half its size: he had been indebted for it to "M. Bougainville de Nerville, governor of the Malouine Islands, an enlightened amateur of natural history." These are the words of the note which our respectable correspondent put at the bottom of the drawing which had been given to him. The name *myopotamus bonariensis*, which he provisionally gave to this species, proves that he then considered it as the type of a new genus.

I have not employed the same denomination as Commerson. It appeared to me more proper to prefer that of *hydromis*, composed nearly of the same radicals, because it presents the same termination as the names already employed to denote the most of the analogous genera, such as those of *pteromis*, *phascolomis*, *lagomis*, *cheiromis*, &c.

We long neglected the drawing of the *myopotamus*, either because, not having been executed by Commerson himself, it did not inspire us with sufficient confidence, or because our mistrust was authorized by the difficulty of comprehending it in the genera already established.

It was only after examining the rich collection of furs of M. Bechem*, where I found a considerable quantity of the skins of an animal which appeared to me to be unknown to naturalists, that I recollected the drawing of Commerson. M. Bechem informed me that these skins had been introduced into commerce about nine or ten years before; that they were transmitted by the way of Spain; that he never received fewer than a thousand at a time; and that in some years the number amounted to fifteen or twenty thousand. M. Bechem found that they had an affinity to the beaver skins, and could be applied to the same purposes: he sold them under the denomination of *racoonda*, which he derived from that of *racoon*, the name given by the English to an animal of North America. They are sought for in commerce, and are employed in particular by the manufacturers of hats. Thus our arts were supplied with, and a part of our dress was formed of, the hair of an animal with which we were unacquainted, and which, however, was sufficiently different from others of the same genus to deserve to be classed in a small particular tribe.

It was so humiliating a condition for science to be thus

* A fur-merchant of Paris.

outstripped

outstripped by commerce, that I could not help embracing the earliest opportunity of endeavouring to discover to what animal this useful fur belonged. Among the number of skins which M. Bechem possessed, I had the good fortune to find a sufficient number entire, to enable me to hope that I should be able to determine this point. I soon perceived that the description of the *quouyia* of M. d'Azzara corresponded to them perfectly, and that this description could be exactly applied also to the drawing of the *myopotamus*. I should have been sooner conducted to this comparison, had I not found, in the French translation of M. d'Azzara's work, the epithet of red twice substituted for that of ruddy: the author, by the Spanish word *rubro*, had denoted only the latter quality.

It was with reason that Commerson had provisionally considered his animal of Buenos-Ayres as a new genus: it belongs to the order of the *rodentia* by its two strong incisor teeth in each jaw; but not to any of the genera of that order, in consequence of its tail and its hind feet. The conjecture of Commerson is now fully justified by the existence of two other species in New Holland, which have exactly the same combinations of form: such are the three animals which I comprehend under the same generic denomination of *hydromis*.

To be able to subdivide with more precision the order of the *rodentia*, and particularly the numerous genus of rats, we have attended to the consideration of the molar teeth, the form of which has furnished us with excellent characters, which have gone hand in hand with the different configurations of the feet and the tail. Thus, all the rats analogous to the field rat, the water rat, &c. have the molar teeth formed of laminæ placed one before the other, and the tail short and hairy: on the other hand, those which have a relation to the common rat, &c. Norway rat, &c. are distinguished by molar teeth with a simple crown, and by their long tail, which is in part naked and scaly; others, such as the hamster, have these teeth single, and the tail short and hairy.

My first care, after these observations, ought to be to attend to the molar teeth of my three species, and deduce from them characters applicable only to them. I was deprived of the means of doing this in regard to the American species. M. d'Azzara neglected to speak of its molar teeth: Molina, however, has in part supplied this deficiency, if it be true that he extended his remarks to their number. We have reason to believe this from the extract in regard to the *mus coypus*, which Gmelin made from the account of that jesuit. M. Gruvel in the French translation must, then, have omitted this important character.

However, the following are the observations, in regard to the teeth, which I made in the two species of New Holland.

These molar teeth present themselves first in a number worthy of remark: there are only two on each side, which carries the whole number to eight. Their form exhibits no less singularity: the length of each is double its breadth: the enamel traverses it in the middle, turning round in such a manner, that the section forms pretty nearly the figure 8, which is rendered sensible in particular by two excavations, pretty deep, corresponding to the vacant spaces which exist in that figure.

The feet of the *hydromis* have five toes; those of the fore feet are very short, and almost entirely enveloped: the other toes are free. On the other hand, those of the hind feet are engaged in a membrane: the external toe only is at liberty, because the general membrane which extends over it, and which borders the interior side, arises only from the extremity of the metacarpian bone of the annular toe; it forms on the last a small interior border, which does not retain it in its deviation. The nails are compressed, pretty long, hooked, and very sharp.

The head, as far as could be judged from the remains which I examined, is broad and depressed like those of the beaver and the water-rat: the muzzle appears to be less obtuse; the neck is thick and short; the ears small and round; the whiskers long and thick.

The hair is of two kinds, as in the beaver; under the long silky hair is a short felt, thick, and exceedingly fine.

The hydromis has a resemblance also to these animals by the proportions of its body, and particularly by the shortness of its paws; but they differ sensibly by the form of the tail, which is almost as long as the body, perfectly round, and terminating in a point.

In general, there can be no doubt that in the natural order these animals ought to occupy an intermediate place between the beaver and the water-rat: being destitute of membranes on the hind feet, they would naturally belong to the tribe of the latter; or, if their tail had the form of that of the beaver, they might be united with the species of that genus.

1st. *The Hydromis Coypou.*—I have said that it was first made

made known by Molina. His description comprehends pretty well the essential points, but is not sufficiently minute. This traveller speaks of the *coypou* as a species of water-rat, of the size and colour of an otter. Gmelin has confined himself, in what he has said of the *mus coypus*, to copying Molina. M. d'Azzara, on the other hand, has given a complete description of that species, with the measures of the different parts of the body; and, in general, has not omitted what relates to the molar teeth.

The *coypou* is distinguished in particular from the two other species, of which we shall speak hereafter, by its great size.

	In.	Lines.
Length of the body - - -	9	6
——— of the tail - - - -	2	3
——— of the head - - - -	4	3
——— of the extremities - -	4	6

The general tint of the hair and on the back is a chestnut brown. This colour becomes brighter on the flanks, and passes to bright red; under the belly it is only a dirty and almost dark russet. Yet this colour is sufficiently changeable according to the manner in which the *coypou* raises or lowers its hair. This mobility in the tone of its fur arises from each hair being of an ash-coloured brown at the root, and bright red at the point.

The felt concealed under the long hair is an ash brown, of a brighter tint under the belly. The long hair on the back has the points only reddish, and that on the flanks is of the latter colour throughout the half of its length.

As in all animals which go frequently into the water, the hair of the tail is thin, short, stiff, and of a dirty red colour; in its naked parts it is scaly.

The contour of the mouth and extremity of the muzzle are white; the whiskers, which are long and stiff, are also white, some black hairs excepted.

Among the great number of skins which form part of the collection of M. Bechem, I saw some belonging to animals which had no doubt been affected with the albine disease; in one of these the silky hairs were entirely russet, so that the back appeared of the same tint as the sides and the belly; in another, the grand dorsal stripe, instead of being chestnut, had passed entirely to a red colour, the flanks being of a very pale red. I cannot believe that these varieties, on the one hand, were the character of youth or of the female, because these accidents were rare, considering the

great

great number of skins which I examined; and on the other, because M. d'Azzara has expressly told us that the female is entirely similar to the male.

Molina and d'Azzara agree in regard to the mild qualities by which the coypou is distinguished. It eats every thing given to it, and seems to attach itself to those who take care of it. It may be easily tamed, and soon becomes accustomed to the state of domesticity. It is never heard to cry but when harshly used; it then emits a piercing cry. The female produces five young, which she always carries with her.

The *coypou* is very common in the provinces of Chili, Buenos-Ayres, and Tucuman. On the other hand, it is rarely found in Paraguay.

2d. *The yellow-bellied Hydromis*.—This species is nearly half as small as the *coypou*. The length of its body is one foot, and that of the tail two inches six lines.

Its long hair is not sensibly distinguished from the felt; it is proportionally shorter and finer, which renders the fur of this hydromis more valuable than that of the coypou: there are few furs thicker, or softer to the touch. The hair in its apparent part is above of a chestnut brown, and below of a most beautiful orange colour. At the root it is ash-coloured, and gray under the belly. The tail is entirely covered with very short and stiff hair: towards the root it is pretty large, and well furnished with hair: in three-fourths of its length its colour is the same as that of the back; but in the other fourth, towards the point, it is of a very pure white. The membrane which incloses the toes of this second species is not so extensive as in the coypou; its interior cut is a little deeper.

This animal was killed by a sailor in one of the islands which form d'Entrecasteaux's channel, at the moment when about to shelter itself beneath a heap of stones: it was preserved to us by the care of M. Levillain, one of the zoologists on the expedition to the austral lands.

3d. *The white-bellied Hydromis*.—This species has a great resemblance to the preceding, and is of the same size. Its head, however, is a little longer, and its fur not so fine, and less soft to the touch. The hind-feet are only half palmated; its hair is brown above, and dirty white below. The tail is also terminated with white, but for a more considerable extent: the white part forms a little more than a third of the whole length.

Four individuals of the white-bellied Hydromis were transmitted to us, all of which had a great resemblance to

each

each other: they were found in the island Maria, which is not far from d'Entrecasteaux's channel, by Messrs. Peron and Lesueur, to whom we are indebted for almost the whole of the zoological riches brought to us from New Holland.

I have comprehended in the following table all the characters of the genus and species of these three animals.

HYDROMIS.

NAT. CHAR.—Incisor teeth two in each jaw; canine; two molar teeth in each row, furrowed on the side, and with a double excavation on the crown.

Feet pentadactyles; the anterior free, the posterior palmated.

Tail round, and covered with short hair.

ESSENT. CHAR.—*Hind-feet palmated. Tail round.*

I. *Species.* HYDROMIS COYPOU. *Hydromis coypus* (Plate VI.)

Hair chestnut brown on the back, red on the flanks, and bright brown under the belly.

Coypou. Molina's Hist. Nat. du Chili, p. 255. French Translation.
Mus coypus. Gm. Syst. Nat.
Quoiuya. D'Azzara, Hist. des Quad. du Paraguay, tom ii. p. 1.

COUNTRY.—Chili, Paraguay, Tucuman.

II. *Species.* YELLOW-BELLIED HYDROMIS. *Hydromis chrysogaster* (Plate VIII.)

Hair chestnut brown above, orange below.

COUNTRY.—One of the islands in d'Entrecasteaux's channel.

III. *Species.* WHITE-BELLIED HYDROMIS. *Hydromis leucogaster.* (Plate VII.)

Hair brown above, white below.

COUNTRY.—The island Maria.

L. *Physico-mechanical Experiments and Discussions of the Phænomena observable in that casual Product of Art the Hand Granade, Prince Rupert's Drop, or Glass Tear.* *By Mr.* JOHN SNART, *Optician.*

ALTHOUGH the subject of this paper is very inconsiderable in itself, being, as was said above, "the casual and easy product of art;" yet as it tends, in my humble opinion, to develop some very obscure and important phænomena in nature as well as the arts, I shall not take an operose route to apologize for offering my explanation of it to the public; because every accurate account of the *arcana naturæ,* &c. is is always sufficient to justify itself, and consequently does not need any apology. I therefore proceed.

The pyrometer long ago has demonstrated to the satisfaction of every one in the least acquainted therewith, that all bodies, whether in a fluid or solid state, expand by heat, and contract by cold, and from a very obvious cause; viz. because the particles of that fluid element, (fire,) by insinuating themselves between the component parts of such bodies, not only fill the most minute interstices, and pervade every pore with the greatest facility, but absolutely disunite or separate the constituent particles themselves, and thereby make them, in the aggregate, of a greater volume; and ultimately render such substances (if capable of fusion) a fluid, or running mass: in which state such substances occupy their greatest possible extension,—unless they were volatilized, or changed into vapour.

Thus much for the laws of fusion themselves. The application and inferences from them are quite appropriate.

It is while the greatest possible degree of expansion of the vitreous matter exists, that the portion which forms this molten tear or drop I am about to treat of, is separated and made. In fact, its fusion is the radical consequence of its extreme rarefaction, or expansion, being physically and mechanically superinduced thereby. For, by the introduction of the accumulated igneous particles, a separation of the once continuous particles of the glass takes place, until the attraction of cohesion entirely ceases, and fluidity eventually ensues.

Now, at this instant of extreme excitation it is, that, with a sudden and forcible jirk, the artist ejaculates or throws it forth to be suddenly quenched in cold water; when it is evident, that the parts which first came into contact with the water will become cooled and indurated first;—which is the exterior surface. And this stratum (if I may so speak, and suppose the whole composed of a number of strata) having taken its form and dimensions, no internal mutation or exertion it is capable of can either alter or lessen its form. But as all bodies, as was said before, must contract in cooling, the interior parts (contrary to gradual cooling) must give way: they therefore become vacuous, of necessity. And because the central parts cannot coalesce, they being the last which feel the refrigerating quality of the water, they remain longest in a fluid state: and as there is not a sufficient quantity of matter, when cooled, to occupy all the space it did when in an ignited or rarefied state, the parts which are most fluid, being most easy to move, and finding they cannot fill the whole space, by close union, on account of their paucity, make the best junction they can, by taking a lateral course, and a vacuum is the result of unavoidable necessity.

And such a vacuum as cannot be produced by any other means that we know of;—a vacuum only assailable by that subtle agent that knows no impediment,

> But walks alike through solid gates of brass,
> Or bars of steel, or doors of molten glass;
> This *primum mobile* all forms pervades;
> Our ev'ry act depends upon its aid.

It is true, the same event happens to other bodies, upon being brought into a state of fusion and suddenly quenched, as to glass. Thus, if a bar of steel be overheated in our attempts to harden it, we see upon breaking it that the interior parts have changed their position, and formed themselves into a kind of irregular crystals, for the same reasons: yet, as no substance but glass is so generally impermeable to all agents, except fire, the result is not so satisfactory as when this material is used; because, on account of its diaphanous nature, or transparency, the appearances are more ocular, consequently less doubtful. For here, without analogy or hypothetical inference, our senses are called to witness the most complete vacuum in nature; and, as far as relates to heterogeneous effluvia, quite perfect.

A vacuum not of the Boylean kind, where that air which is driven out is ejected by means of that which remains behind; but of the Torricellian kind, yet infinitely more perfect: the very formation of which precludes the possibility of the most subtle agent in nature (except fire) insinuating one particle of its substance therein. And, were it possible that we could make this vacuum subservient to our experiments on the mechanical pressure of the air, (whether incumbent or lateral,) I am persuaded, from what I have herein discovered, that we should have a greater result than (the maximum allowed) 14 or 15 lbs. per inch for its energy. The excess of which energy would be in a direct ratio to the perfection of this vacuum, in comparison with that of the barometer: and those of the air-pump cannot stand one moment in competition, for the reason assigned above.

And yet so well grounded were the antient philosophers "in all space being occupied by some agent or other," that they have, as it were, anticipated us in our most modern discovery; for it was one of the dogmata of the Peripatetic or Aristotelian schools, "that Nature abhorred a vacuum."

cuum." And although I will not take upon me to justify all their decisions, or even this dogma itself upon their principle; yet as this seems the *ne plus ultra* of human means to procure one, and which, after all, appears not to be absolutely so, I think their assertion is not remote enough from truth to justify a rash contradiction on our part, or to arraign the profundity of their knowledge. But to return: How great must be the velocity, how great the mechanical collision, to rend asunder with such force so well compacted a substance as the glass of which these granades are made, even after making every reasonable allowance for its want of annealing, which no doubt increases its fragility, while it hardens the surface! There must be something more than mere mechanical influx, or consequent collision, to rend asunder and reduce to powder an impregnable mass, which is capable of resisting the weight of many tons, sustaining the force of a large vice, or the rude strokes of a hammer (after allowing for the geometric resistance on the external attack, and the want of the same in the internal force) used in breaking them, before this phænomenon will be satisfactorily explained.

And without giving a more elaborate definition than the thing is obviously susceptible of, I conceive this cause to be electricity: and that, in the formation of the drop, after the caloric particles have pervaded the glass and subsided, this matter, like a lambent flame, attaches to and lines the interior surface or cavities, as in the Leyden jar; and by this hermetic accident (for it is not properly art) may remain prisoner for many centuries; but yet, though pent up, (being perfectly insulated) is not diminished: therefore, upon opening the conduit pipe of communication in the tail, the affinity the inclosed effluvium has with that in the air, our hands, or whatever else breaks them, causes that violent detonation, and the destruction thereof considered therewith to take place, and the force of the explosion is *(cæteris paribus)* in a compound, direct, ratio with the capacity of the cavities, and the strength of materials.

Thus is is, when a vacuum is induced of necessity, and of materials which, while they are extremely friable on the one hand, are impervious to both influx and efflux on the other; (hence the want of these appearances in breaking steel, &c.) and void of every other occupant than this elastic vapour, which is generated in their first formation, which, like seed in the womb, seems to be the very germ or rudiment of elementary fire, and only waits the invitation of excitation to manifest itself: and whenever this excitation is begun, how irresistible are the effects produced by it! I believe nothing in nature (if small things might be compared with great) is more analogous to an earthquake than the bursting of one of these little tears of glass, or more like it in its cause; for both arise from a sudden combustion of a latent and inflammable principle contained in their interior parts; and both alike cause a derangement of the parts which heretofore formed their orbicular prisons.

To ascertain whether they were charged *plus* or *minus* *, I placed them in an insulated situation, on a large flint glass condensing receiver; and with a hammer of the same kind of glass (both being well excited) I broke some of them at the small end: but, contrary to my expectation, the result was the same as if broken in my hand.

I next placed some of them on a table, which I knew to be a good conductor, and with my glass mallet or hammer I broke several more with precisely the same effect.

I also broke some with a conducting, *i. e.* a common hammer, upon the excited receiver, but there was no difference in the effect: for all the means I made use of (however varied) were productive of the same appearances as when broken in my hand, or by the collision of a conducting substance, while they were placed upon another conductor.

And now, having varied the experiment by all the means I could devise, except I had tried the air-pump, (which I could not do, as I had not got an open receiver,) I began to doubt whether electricity had any share in this phænomenon or not, and to conclude the effect must be purely mechanical (for I would not call it a *lusus naturæ*); when what compensated all my trouble, and put the matter past doubt, was this:

I darkened the room entirely, having previously furnished myself with a clear strong glass receiver to prevent the accident of the pieces flying into my eyes; then directing these organs of vision by the feel, I broke several in this inclosure, where they uniformly produced a bright corruscation like lightning; which, together with the more than mechanical snap they make upon breaking, quite satisfied

* To those who are unacquainted with the history of electricity, it may be needful to observe, that the terms *plus* and *minus*, + —; positive and negative; vitreous and resinous; are not six different electrical qualities; but three sets of equivalent terms, in different systems of the same science, indicative of the two or contrary principles which reciprocally attract each other, and cause what is called the *electric shock*. There are also other terms by way of designating the same principles, as *terrene* and *atmospheric*; but these are not so common.

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me that it was not the mere mechanical collision of the air striking the internal surface which so forcibly rends them asunder, but a compound effect of that and electrical affinity together; which, being corroborated by so many actual experiments, makes me believe the phænomenon of these little philosophical ænigmas is nearly explored. And although I would not say the subject is entirely exhausted, yet this solution has so satisfied my mind, that I cannot think it any longer a desideratum.

I remain, sir,
Your obliged servant at command,
JOHN SNART.

Tookey-street,
Sept. 14, 1805.

P. S. Since the publication of my paper in the 86th number of the Philosophical Magazine, from a comparison of the creature therein described with what I have met with in Dr. Shaw's work, I am inclined to think it is a *larva* of the genus *dytiscus*, of the order *coleoptera*. It might not improperly be denominated *ranumalgus*, or frog-sucker.

LI. *Twenty-third Communication from Dr.* THORNTON.

To Mr. Tilloch.

Sept. 14, 1805.
No. I, Hinde-street, Manchester-square.

DEAR SIR,

PERMIT me the honour of continuing, in your excellent magazine, facts confirming the propriety of employing pneumatic medicine.

Case of Consumption cured by Hydro-azotic Gas.

James Davis, æt. 25, living with Mr. Chambers, banker, Bond-street, as groom, had all the marked symptoms of a confirmed galloping consumption, violent cough, frequent pains in the side, shortness of breathing, the expectoration very copious, of an opaque appearance, night sweats constantly, wasted almost to a shadow, and extremely weak, arising from a cold proceeding from putting on a damp shirt, and being wet through, in March 1805. The violence of this disease had existed two months. He was recommended by Joseph Workman, who had been similarly affected, servant of captain Bond, also cured by the same plan. This patient inhaled the hydro-azotic gas, and he took at the same time tonic medicines, as bark, columbo, with myrrh, with occasional aperients; and, following this plan for six weeks, he was restored to perfect health. The patient is now before me, Sept. 14, 1805; and in excellent health, looks well, is fat, has since had no complaint of any kind, and is an additional proof of the virtue of the aërial remedy.

I have the honour to remain,
Dear sir, &c.
ROBERT JOHN THORNTON.

Observations on this Case.—The same opinions arise here as in other cases: but the pneumatic practice is more particularly necessary, as tonics would only augment the malady, if applied alone, as the lungs are locally inflamed; and the lowering plan would only have aided the debility induced from this local cause, exciting an immoderate morbid secretion from the lungs.

LII. *A simple Method of making Tubes of elastic Gum or Caoutchouc, to avoid the Expense of Solution in Æther.*

SPLIT a stick of cane, and then apply together again the split pieces, but with a slip of whalebone interposed between them. Cut the elastic gum into slips fit for twisting over the prepared cane, so as to cover it; then, by duly heating the surface of the cane covered with the caoutchouc, it will melt so as to form one piece. When cold, draw out the interposed whalebone from between the split cane; by which means, without difficulty, the whole substance of the cane may then be readily withdrawn from under the covering, thus leaving the tube formed as desired.

Some recommend winding small thread round the twisted gum elastic, to help to unite the joinings, and employ the heat of boiling water.

LIII. *Notices respecting New Books.*

Medical Sketches of the Expedition to Egypt from India. By JAMES M'GREGOR, *A. M. Member of the Royal College of Surgeons of London; Surgeon to the Royal Regiment of Horse Guards; and lately Superintending Surgeon to the Indian Army in Egypt.* 1804.

THE health of soldiers, and particularly when engaged in distant expeditions, is of so much importance, that every publication which can tend to promote so desirable an ob-

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ject deserves to meet with a favourable reception. This observation, in our opinion, is very applicable to the present work, as it relates chiefly to a country interesting to Great Britain, under various points of view, and which, in the present situation of public affairs, may again afford British soldiers an opportunity of showing what they are able to achieve, when commanded by able and experienced officers.

These sketches are divided into three parts. The first gives the medical history, or rather the journal, of the expedition: in the second, the author, after attempting to assign the causes of the diseases which prevailed, proposes some modes of prevention: and in the third, some account of the diseases is given.

"The first division of the army intended for the expedition to Egypt, under colonel Murray, sailed from Bombay in January, 1801. Their voyage was rather a tedious one, and the small-pox and a remittent fever broke out among them. They touched for refreshments at Mocha and at Jedda, and on the 16th May, 1801, came to anchor in Kossier-bay; the prevailing winds in the Red Sea, at this time, rendering it impossible to get so far up as Suez.

"The second division of troops (originally intended for another time), under colonel Beresford, sailed from Point de Galle, in Ceylon, on the 19th February; and on the 19th May disembarked at Kossier.

"The last division, under colonel Ramsay, sailed from Trincomalée, in Ceylon. They were later of arriving at Kossier, and were not able to cross the desert before July.

"At Kossier there is a fort and a town, if they deserve the name. They are built of mud, and the Arabs inhabit them only at the season when caravans arrive with the pilgrims for Mecca, and with corn for that and the other ports on the opposite Arabian coast."

Soon after the arrival of the troops at Kossier, they were all attacked with a diarrhœa, occasioned by the water, which contained much sulphate of magnesia. At first the men were greatly debilitated by it; but as they became used to the water, it ceased to affect their bowels: on the whole, however, it appeared to have produced salutary effects, and the army for some time was uncommonly healthy.

On the 19th of July, 1801, the 88th, with two companies of the 80th regiment, under the command of colonel Beresford, as the advance of the army, commenced the march across the desert; but as they had the digging of wells, and other duties to perform, they did not reach the banks of the Nile

Nile until the next month. The rest of the army marched on the following days, the marches being always performed by night, and the army, with very inconsiderable loss, reached the banks of the Nile in a very healthy state. The course it pursued was nearly that travelled by Mr. Bruce. During almost the whole of July the army was encamped on the banks of the Nile, which now began to overflow its banks near Ghenné; they, however, soon prepared to move, and detachments went up to Thebes, Luxor, and the cataracts, to press all the boats; and about the end of the month the army began to move to Lower Egypt. The 10th regiment marched to Girgé, the capital of Upper Egypt, sixty miles below Ghenné, and on the 27th and following days the rest of the army was embarked in boats. The thermometer had a wide range at Ghenné; in the author's marquee it varied from 71° to 108°, on the 20th it rose to 111°, and in the open air the heat was from 70° to 115°.

By the 12th of August, the greater part of the army, after a navigation on the Nile of nearly four hundred miles, arrived at Ghiza. As they landed, the troops were uncommonly healthy; but in three weeks the sick of the army exceeded one thousand. A considerable number of ophthalmic cases occurred, but the prevailing disease was fever; in general it was of short duration, of two, three, or five days at most, and rarely proved fatal. In the month of September the plague made its appearance in the hospital of the 88th regiment, in the neighbourhood of Rosetta, which rendered it necessary to adopt measures for preventing the further progress of this destructive scourge. Next to the plague, the most formidable disease in the army, from its general prevalence, was ophthalmia. In the 10th and 88th regiments there were upwards of three hundred and fifty cases, and the total number in the army exceeded six hundred. Dysentery and hepatitis prevailed very generally among all the European corps, and the mortality of the month was very considerable. In the month of January, 1802, the cases of plague in the Indian army amounted to 72, in March the number was 46, and in May 26.

On the last day of April, orders arrived from England to general Baird, to return with his army to India, and to detach the 10th, 61st, and 88th regiments, which were placed on the British establishment. On the 3d, the Indian army began to march to Ghiza, where it remained encamped by the pyramids for some days, until water and other necessaries for the passage over the desert were reported to be ready. They crossed the river, encamped at Boulac, set off from

from Cairo, and, passing the ruins of Heliopolis, made El Hadje their first stage. Their marches over the desert of Suez, as in crossing the great desert, were all performed during the night, and they always encamped by sun-rise in the morning. By the end of the month, the whole corps, except the 7th Bombay regiment, had crossed the desert, and arrived at Suez. Part of the army was encamped near the town of Suez, and part at Moses Wells, nine miles on the eastern side of the Red Sea. The march over the desert of Suez was performed with much greater ease than that over the desert of Thebes. The weather was cool and favourable; the hot winds were less felt, and they found abundance of good water provided at the different stations. On the 2d of June, the embarkation commenced, and by the 15th the whole army was embarked, and had sailed for the different presidencies, except the 7th regiment, which, on account of the plague still prevailing in it after the rest of the army had embarked, was ordered to remain two months. Most of the corps of the army embarked in the most healthy state.

"To conclude," says the author, "never, perhaps, was there an army embarked for any service more healthy than the Indian army was when it re-embarked on its return from Egypt.

"Previously to the arrival of the army from Egypt, in order to provide against the introduction of the plague into India, quarantines were established at the presidencies of Bombay, Bengal, and Madras, as well as at the island of Ceylon. The principal of these was at Butcher's Island, near Bombay, where there were pest and quarantine establishments, of which, on my arrival in June, I took the charge. At this period, letters from Dr. Short, at Bagdad, and from Mr. Milne, at Bassorah, described the plague as raging in Persia, and particularly at Ispahan and Bagdad: in consequence of this information, every vessel, both from the Red Sea and Persian Gulf, was ordered to Butcher's Island.

"As the ships arrived, the troops from the Red Sea were landed; but the artillery, 86th regiment, 1st Bombay regiment, and the commissariat department, were so uncommonly healthy, that I detained them but a very few days on the island.

"The 7th Bombay regiment landed at Butcher's Island in August. As this was the corps in which the plague had principally prevailed, though they were not unhealthy, I judged it prudent to detain them a month. On my last inspection

inspection of them before they left the island, of a total of seven hundred, including sepoys, their wives, and the public and private followers of the corps, I found only four sick, and these I believe were all catarrhs.

"Dr. Henderson, with the pest-establishment, and all those whom we had left at Suez, arrived at Butcher's Island on the 1st September. The convalescents from the plague, as well as the guard, and the pest-house servants, were, on their arrival, all of them very healthy: but I thought it safe to keep them in quarantine on the island till October; when, like all the others who had been in quarantine, they were provided with new clothing and sent over to Bombay.

"The company's packets from Bassorah, and the vessels which arrived from the Persian Gulf, had none of them the least suspicious appearance, and I found that their crews were all very healthy.

"I had likewise the satisfaction to receive accounts from the medical gentlemen employed in the expedition, after their arrival at Calcutta, Madras, and Ceylon: their accounts were so late as November. In none of the corps did any death occur from the time of embarkation at Suez."

In part second, the author gives some observations on the climate of Egypt, as connected with the diseases which prevail in that country.

"The cultivated part of Egypt, particularly the Delta, is a very rich country; in fertility and luxuriance of soil yielding to none under the face of heaven. The art of husbandry is there but imperfectly known; and at their harvests there is a very great destruction of vegetable matter, from which hydrogene gas, or hydro-carbonate, is extricated in great quantities. Under similar circumstances, in America as well as in India, I have seen a bad fever of the intermittent or remittent type appear. But in Egypt after the subsiding of the Nile, which in many places had covered a great extent of country, there is a great exhalation from the mud, and from the putrid animal and vegetable matters left behind. The effluvia of these substances, acting on the human body, will readily account for much disease. If we add to these the extreme filth of the inhabitants of Egypt, their poor diet, their narrow, close, and ill-ventilated apartments, generally much crowded, with the extreme narrowness of their streets, and the bad police of their towns, we shall not be astonished if a fever, at first intermittent or remittent, should have symptoms denominated malignant, superadded to the more ordinary symptoms of the disease. If an imported contagion should make its appearance at the same time, and under

under the above circumstances, we expect a most terrible disease.

"The dry parching wind, which comes over the desert, and which at certain seasons blows in Egypt and in Arabia, is well known, and was often severely felt by the army on their march, both across the desert and the isthmus of Suez. The whirlwinds of sand roll with great impetuosity, are very troublesome, and insinuate fine sand and dust every where. It is hardly possible to keep the minute particles out of the eyes.

"The dews, which fall in Egypt, I always heard were very heavy, and were a cause of the diseases of the country. I had occasion too, more than once, to hear the natives attribute much to them as the cause of their diseases; with what justice I will not pretend to decide. From some experiments which I made in India, on the Red Sea, and lastly in Egypt, I am inclined to think that they are equally heavy in the two former as in the latter quarter. After weighing the matter carefully, I took a quantity of lint, twelve inches square, exposed it for a night to the dew, and, by weighing it in the morning again, ascertained the quantity which it had gained. I am aware that this is by no means a nice experiment, and that in the performance of it several particulars demand attention; but it is sufficient to our purpose, and I learned by it, that, in the island of Bombay, on the Red Sea, and in Lower Egypt, the quantity of dew which falls is nearly equal.

"It ought to be mentioned, that, during the year we were in Egypt, the season was not the usual one. There was a greater overflow of the Nile. It rose higher on the Nilometer than it had done for several former years, and it was remarked to be much later in subsiding at Rosetta.

"The fall of rain at Alexandria was greater than on former years; and, at Rosetta, the rains were in setting-in later than usual. The season of the plague set-in much earlier than usual*.

"In general, the Thebaid, or Upper Egypt, is healthier than the Lower. Never were troops more healthy than the army when encamped near Ghenné.

"Ghiza, the antient Memphis, at the time the army disembarked there from Upper Egypt, we found to be a very unhealthy quarter. For a considerable time, and immediately before the arrival of the Indian army, it had been

* These circumstances I learned from a member of the French Institute, and from the Pharmacien en Chéf to the French army, who often related to me the order which Bonaparte gave him to poison the wounded with opium.]

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the station of large armies: alternately of Turks, Mamelukes, French, and English. From all these armies a number died at Ghiza, and there was much filth and noxious effluvia. We saw there enough of putrid animal matter to generate contagion. Whether this was or was not the cause of the fever which prevailed, I will not attempt to decide. One circumstance may be mentioned: we were here joined by a detachment of the 88th regiment under colonel Lloyd, which, for some months before, had been doing duty with the vizier's army, which never was healthy. That the circumstances which existed at the time of our occupying Ghiza were the cause of the fever, is manifest from this, that, subsequently to the army going to the coast, the garrison left in it found Ghiza a most healthy quarter. The same objections are to be made to Rhoda that are applicable to any marshy situation."

In the third part of this work, which gives an account of the diseases of Egypt, Mr. Macgregor has brought forward some new facts in regard to the plague, and particularly the treatment, which are well worth the attention of medical men in general, and of those in particular who may visit countries where this disease is prevalent. It is commonly believed, that the progress of this contagion is stopped by extremes of both heat and cold: but if this be true in regard to heat, it did not appear to be so in the army of Egypt in regard to cold; for the period at which the plague raged most was in the coldest months. In regard to the treatment, nitric acid was given internally, and where the patients would drink it it showed good effects. Bark, wine, and opium, were largely administered, and at a certain stage the cold bath, for the purpose of obviating that debility which always appeared to be very great. At first, calomel was used only as a purgative, but at last the use of this remedy was carried farther. "On the whole," says the author, "in mercury and the nitric acid we appear to have excellent remedies for the plague; but they must be very early and very liberally exhibited. If the first stage is allowed to pass over before they are given, the season of doing it with advantage is in danger of being lost." In regard to preventive means, the following observation seems so well calculated to remove that despondency which generally prevails when the plague exercises its ravages, that we cannot help quoting it.

"There was hardly a corps in the army," says the author, "where, at one period or other, the disease did not make its appearance; but it was always in our power to arrest its progress. In well regulated corps, where a rigid discipline was

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enforced, and proper attention to the interior economy was paid, it rarely happened, indeed, that much difficulty was experienced in eradicating the contagion. As our success in the prevention," adds he, "was so great, all that remains for me is to mention the substance of general Baird's order to the army on the subject.

"1st, To every hospital, an observation-room, or in lieu of it a tent was attached; and to it every case whatever with febrile symptoms was sent, as soon as discovered, and was there most strictly watched by the surgeon.

"2dly, On any symptoms of the plague appearing, the case was instantly sent to the pest-house from the observation-room of the regimental hospital: the patient was accompanied by the medical gentlemen of this corps who attended him, and who gave the medical gentlemen at the pest-house an account of the previous treatment of the case.

"If any doubt remained, the patient in the first instance was placed in the observation-room of the pest-house; and, if the disease did not turn out to be plague, he was sent to the quarantine.

"3dly, In every corps, and in every department, a minute inspection by the surgeon was made twice a week; and every person with the smallest appearance of ill-health was sent to the hospital.

"4thly, Every corps or hospital, where a case of plague had appeared, was put into a state of quarantine; and, in such corps or hospital, an inspection by the surgeons was made at least two or three times a day; and every case with suspicious symptoms was ordered to the observation-room.

"5thly, In suspected corps it was ordered, that, under the inspection of a commissioned officer, every person should be bathed more frequently, and at stated periods; and, likewise, that all their clothing and bedding should be frequently washed and baked. To all the hospitals, ovens and smoking-rooms were attached.

"6thly, Quarters of corps, hospitals, and ground of encampments, were frequently changed.

"7thly, Much is to be attributed to the nitrous fumigation. In several instances it was attended with the best effects. The lamps, with this, were kept constantly burning in the observation-rooms, and in the rooms from which the cases of the plague had come. Vessels, with the materials for the fumigation, were likewise placed under the beds, and in the corners of the rooms. When our stock of nitre was at length exhausted, we substituted marine salt for

for it; but this fumigation could not be kept up in rooms where the patients were all confined to their beds."

The next malady which engages the author's attention is the ophthalmia, which in Egypt at particular seasons is a most generally-prevailing disease. It is not, however, confined to the human race; the animals of the country, particularly the dogs and camels, are subject to its attacks. In Egypt it proved most distressing and obstinate. The French, it was said, sent from Egypt to France 1000 blind men. The number sent home from the English army was very considerable likewise. Of the Indian army 50 were sent home invalids from blindness; most of whom were from the 10th and 88th regiments.

"The disease, I think," says the author, "might generally be resolved into, 1st, either of Cullen's two species, the ophthalmia tarsi and the ophthalmia membranarum; 2dly, to a combination of these two; or, 3dly, to a species of ophthalmia, frequent in India, symptomatic of disease in the biliary secretion.

"The appearance which the disease put on, particularly the two first species of it, was nearly what we have seen in other parts of the world; except that the symptoms advanced with alarming rapidity to the highest inflammatory stages. In most cases the attack was sudden, and very generally at night. Speedily, the patient complained of a burning heat of the eye-ball, or of a sensation of needles being passed through the eye. There was a considerable swelling of the ball of the eye, of the eye-lids, and sometimes of the neighbouring parts. Almost always, there was a copious flow of tears, which felt hot and scalding, and, as they flowed, excoriated the face down. Very frequently, there was a racking head-ach and general fever. Œdema of the eye-lids was frequently met with in the early stage of the disease, and inversion of the cilia in the last stages.

"The disease very often continued two or three months: after it had continued some time, the general health became much impaired. It often terminated in diarrhœa or dysentery, and sometimes the patient became hectic.

"In the third species of the disease, which I have mentioned, there was not so much active inflammation as in the other two species; and it was generally known by a yellow tinge of the adnata, or by dyspeptic symptoms being present: though, sometimes, we have seen those appearances absent: and no topical application had any effect in removing the ophthalmia, till the gums were affected by calomel, or some mercurial preparation.

"In

"In the two first species of the disease, the inflammation, in a great many instances, induced fever of many days duration, and the disease too frequently terminated in opacity of the cornea, or in suppuration of the eye-ball.

"In the treatment, it appears, from the reports, that different gentlemen followed very different modes. We said, in general, that the European practice did not succeed. Scarification and astringent collyria, in the first stage, gave intolerable pain, and generally aggravated the symptoms.

"The practice of the natives was, to apply, in the first stage, emollient decoctions of their plants, and poultices of the kali. In the last stage, they rely much on the frequently bathing of the eye in the cold water of the Nile; they are likewise very fond of bleeding; and I understood that sometimes they use the actual cautery, burning behind the ear where we usually apply blisters.

"The practice, which appeared to be by far the most successful, was the following:

"For the first twenty-four or thirty-six hours after admission, the eyes of every patient were carefully syringed with tepid water, which had been filtered carefully. The syringing was performed from three to six times in the day; the light was carefully excluded, the patient kept cool, and every other part of the antiphlogistic regimen strictly enforced. After the above period, a weak solution of sugar of lead, or of camphir, or vitriolated zinc, was applied. Where the pain was much complained of, a solution of opium was added to the collyrium; opium was applied in a cataplasm, or two or three drops of laudanum were let fall into the eye.

"If there was much swelling, a saturnine poultice, or the coagulum aluminosum, was applied to the eyes. I observed, that blistering a large surface, and as near as possible to the seat of the pain, if kept discharging for some time, always afforded great relief.

"To remove the fever and to alleviate the distressing pain, we often gave opium internally in a considerable quantity, and with great advantage.

"Setons in the neck and the free use of bark appeared to be of the greatest service, when the disease was of long standing.

"In opacity of the cornea, and when there were specks, several gentlemen thought highly of the aqua phagedænica of the old pharmacopœias, after having divided the vessels which went to the speck. It gave very pungent pain; but

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I have seen great relief from it, and also from a solution of lunar caustic.

"As a collyrium in Egypt, I often gave with considerable benefit what I found in the hands of the black doctors in India, viz. a tea spoonful of lime-juice to four table spoonfuls of water, or a tea spoonful of arrack to two table spoonfuls of water. In the first stage, I would have applied leeches, but never could procure them.

"In Persia, Dr. Short informs me, that he was very successful in the general use of an ointment, composed of white vitriol, tuttey, and cinnabar, after the application of leeches and scarification.

"From the days of Prosper Alpinus, the salts contained in the soil of Egypt have been supposed to be among the principal causes of the ophthalmia of the country. Though the various modifications of light and heat no doubt act as existing causes; yet to the particular soil of Egypt, and to the constitution of the air there, we must look for the regular and the principal causes of this disease.

"In Egypt several causes occurred, which in any country, separately applied, would be adequate to the production of violent ophthalmia: the dry, white, dazzling soil, and the fine sand and dust constantly thrown about in whirlwinds and entering every crevice. If an ophthalmia is epidemical or is endemic in Egypt, the above causes will render it a very violent disease."

The other diseases mentioned by the author as prevalent in the Indian army, are, fever, hepatitis, or the liver complaint, dysentery, pneumonia and rheumatism, small-pox, diarrhœa, scurvy, syphilis, the guinea-worm, ulcers, and tetanus. In regard to the guinea-worm, it did not frequently appear in the Indian army while they were in Egypt, but on the voyage thither it prevailed very much. Soon after sailing from Ceylon, it made its appearance in the 88th regiment, and by the time the army reached the straits of Babelmandel it was in the most alarming state. Of 360 men whose services might be daily required, no less than 161 were crippled, and laid up with this loathsome disease.

"The disease was pretty uniform in the manner of its appearance. The patient was first sensible of an itching; and, on looking at the part, generally observed a small blister: sometimes I have seen three or four small blisters, and the part having the appearance of being stung with nettles. When the blister was snipped, a piece of mucus of

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of the breadth of sixpence was seen underneath; which being removed, the head of the worm was seen. It was in general firmly attached, and required force to detach it from the parts underneath. When detached with the forceps, we twisted it round a ligature or piece of lint, and thus, often on the first day, succeeded in extracting a foot, or even two, of the worm. It resembled much what is called bobbin, and was about the same size. It was transparent and moist, a white liquid being seen in it. We continued, daily, extracting as much of it as would come out with gentle pulling. It was always dangerous to pull strongly, for fear of breaking the worm: it then occasioned the most acute pain, and there followed much swelling, with inflammation of the neighbouring parts, sometimes of two or three weeks continuance; when the worm would show itself at another part, as at first, with itching and a blister.

"It seldom appeared to be deeply seated; generally, under the cutis, or among the tela cellulosa, when we could often trace it in its course, and sometimes see it: sometimes it was under the fascia, and but seldom among the muscles.

"If not ushered in with fever, it was almost always attended with it in its course: when there was considerable inflammation, it ran very high. In seven cases mortification took place, and very large sloughs were cast off. In a few cases, there was a very considerable and alarming hæmorrhage.

"By presenting itself at different places, it would often leave two or three large, foul, and fistulous ulcers in different parts of a limb. When the inflammation has run very high, as I have often seen of the whole leg or thigh; and when a profuse suppuration followed; the worm frequently has come out dead, often in pieces, with the sanies; by which, probably, it had been eroded and killed.

"Frequently, after extracting one worm from a patient, a second, a third, or even a fourth, would appear: after getting one out of a leg, a second would appear in the other, a third in one hand, and a fourth in the other hand.

"The guinea-worm, I believe, has been seen in every part of the body. Though the extremities appear to be its favourite seats, yet the face, breast, back, penis, &c. are not exempted from its visits. I heard of a gentleman in Bombay who had one in his scrotum and penis, and of a lady who had one in the pudenda.

"The following I extract from my case-book and notes, taken on board the Minerva, by which it will be seen that

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the extremities are as much more frequently its principal and first seat, as in the itch.

	Feet	Legs	Thighs	Scrotum	Groin	Hands	Arms	Body	Total Cases
Feb.	34	3	1	—	—	1	—	—	39
Mar.	70	21	5	2	2	3	—	—	103
April	20	9	5	—	—	3	—	2	39
Total	124	33	11	2	2	7	—	2	181

"As to the causes of the appearance of the guinea-worm, and the mode in which it is generated, I must confess that I have no account that I could venture to offer here.

"In different parts of the world, the water drank is accused of occasioning intestinal worms, as the tænia in Switzerland, and the tænia and the teretes in the West Indies; where, likewise, I have heard the mucilaginous vegetables eaten assigned as a cause of the frequent appearance of worms. In Russia, there is a worm, the lumbricus militensis, common near swampy grounds. In Russia and in Siberia, in the same situations, the tænia infernalis prevails. But, after what has been here stated, we cannot bring the water, drank on board the Minerva, or at Bombay, to account for the guinea-worm which prevailed: in fact, the water came from different and distant quarters, Bombay, Ceylon, and Madras. Besides, the officers of the 88th, and the artillery, drank the same water, and escaped.

"No case of guinea-worm had been known among either the Lascars or European sailors in the Minerva, when the 86th and 88th embarked in her.

"I have good reason to think that the spreading of the guinea-worm may be stopped, whenever it does appear. The means which we adopted appeared to succeed. Extreme attention to cleanliness is indispensably necessary.

"In India the native doctors are much more successful in getting out the worms, than Europeans. After long feeling with their fingers, for the body of the worm, they make an incision as nearly as they can judge over its middle, and, pulling the worm by a duplicature of it, draw out both ends of the worm at one time. I have often endeavoured to imitate this practice. My sense of touch was not so delicate, and did not guide me so correctly as it did the Hindoo doctors; but I always found that when, on cutting down

down to it, I got on the middle of the worm, and by the forceps pulled this out, I could with ease extract a large portion, and, not unfrequently, the whole worm.

"Leeches, astringent and sedative lotions, cataplasms, fomentations, &c. were applied, as required by the circumstances of the case. A good deal of attention was paid to the disease, in all its stages; and several experiments were made on the worm, which, however, it is needless to detail here.

"After using a variety of articles, in the treatment of the guinea-worm, and making them enter the system by the absorbents, I think that unctuous substances succeeded the best, particularly mercurial ointment. Passing an electrical shock through the part had no effect."

We cannot conclude this article without expressing our thanks to Mr. Macgregor, for the information he has added to our stock of medical knowledge. Having enjoyed opportunities which seldom fall to the lot of medical practitioners, he seems to have exerted himself as much as the shortness of the time would permit, to collect every observation that might be useful either to himself or to others in the same department. Many of the facts which he presents are new, and therefore worthy of more attention; his remarks on them are judicious, and appear to be the result of a sound judgment united to long experience. In a word, we do not hesitate to recommend these sketches to the notice of medical men in general; and we have no doubt that they will be found of great utility to those whose employment may lead them to the same countries which were visited by the author.

LIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

In an ingenious paper lately sent by Dr. Herschel to this learned body, that eminent astronomer announces a new discovery respecting Saturn. The form of that planet he has discovered to be that of a cube with its angles and edges truncated, which he ascribes to the attraction of the belt.

TEYLERIAN SOCIETY OF HAERLEM.

This society, in the sitting of October 30, last year, proposed as the subject of a prize the following question:—"To discover in history, and explain briefly, what have been the revolutions in poetry, not only among the antient nations best known, but among modern nations, dating from the epoch at which they can be reckoned among civilized nations."

The society desires that the author will examine whether the revolutions, progress, and decline of poetry, as well as the variations of its forms, among civilized nations, have followed the progress of their moral, civil, and religious knowledge, and of the other sciences, or have been independent of them?

The memoirs must be transmitted to the society, with the proper address, before the 1st of April 1806. The prize is a gold medal value 400 florins.

The society renewed at the same time, and with the same prize, another question, before proposed, to which no sufficient answer had been received:—" Does the history of the moral sciences prove that the application of metaphysical theories has been useful to their progress? or does it teach us, on the contrary, that no progress can be made in these sciences but by observation, experiments, the consequences deduced from them, and the scientific calculations established on these data? And what rules does the history of science prescribe in this respect, to those who wish to contribute in the most effectual manner to its progress?

The memoirs for these two prizes may be written in Dutch, Latin, French, English, or German; but not in German characters. The address is, *Aan Tylers Fundatie Huis,* at Haerlem.

FRENCH NATIONAL INSTITUTE.

An Account of the Labours of the Class of the Mathematical and Physical Sciences of the French National Institute from the 20th of June 1804 *to the same Day* 1805. *By M.* CUVIER, *perpetual Secretary.*

[Continued from p. 278.]

M. Dessaerts has communicated a singular medical fact, made known to him by M. Burtini, physician of Asti. A young woman, after a severe indisposition, accompanied by a tumour in the region of the liver, voided fourteen bladders of the size of an egg, the shells of which have not become hard, and filled with a glutinous liquor a little yellowish in the middle. A report had been spread among the people that this young woman laid real eggs. These bladders, according to M. Burtini, had no appearance of bodies that had ever been animated.

M. Dessaerts,

M. Dessaerts, extending this latter observation to the bladders to which naturalists have given the name of *hydatides,* and which they consider as real animals, thence concludes that this opinion of naturalists is very doubtful; and he announces that he proposes to combat it in a memoir which he will soon present: he flatters himself that he shall be able to show, from powerful authority, that the melancholy decision of the hydatides being an incurable disease, is void of foundation.

A great and important work in medicine has appeared this year. It is the medical anatomy of M. Portal, in which, besides a new and detailed description of the human body, he has given an account of every thing he has acquired by long and extensive practice in regard to lesions of the organs, and their relation to the apparent symptoms of different affections.

MATHEMATICAL PART,

By M. Delambre, *perpetual Secretary.*

GEOMETRY.

Huygens has given in his treatise *De Horologio Oscillatorio* the two following theorems, which may be applied to all solid bodies: *The centre of oscillation and that of suspension are always reciprocal one with the other*; the same body is *always isochronous to itself when it oscillates around parallel axes taken at equal distances from the centres of gravity.* M. Biot has given to these theorems a remarkable extension.

All these parallel axes form the surface of a right cylinder, the axis of which passes through the centre of gravity. But the analytical expression under which M. Biot presents the theorem of Huygens showed him that an arbitrary inclination might be given to this axis, provided that the radius of the cylinder should be also changed in a proper manner. By these means there will be obtained, according to the different values of the inclination, an infinite number of cylinders the edges of which have the same properties as those of the primitive cylinder. But this is not all: the axis, without changing its inclination, may describe a conical surface around its primitive position, which still multiplies the number of the cylinders already found, as many times as there may be conceived ridges on the surface of the cone.

The same analytical expression being of the second degree in regard to the radius of the cylinder, or, what amounts to the

the same thing, in regard to the distance of the centre of gravity from the point of suspension, this consideration alone conducts to a theorem analogous to the first of the two, for which we are indebted to Huygens;—a new proof of the great fecundity of algebraical expressions, when one has the art of giving the most proper form to all their developments.

Measuring of Heights by means of the Barometer.

The celebrated experiment devised by Pascal, and which proved that a column of mercury decreased in proportion as the barometer was carried to a greater height, after having proved the gravity of the air, must have made the mercury be considered as a scale capable of measuring the height to which it is carried. But this scale being very small in comparison of the heights which it ought to measure, it was soon perceived that it would be necessary to improve the construction of the barometer so far as to render sensible and appreciable the smallest changes in the height of the mercury. The necessity of avoiding or of calculating the continual variations which the barometer experiences, even without changing its place, presented another obstacle much more formidable, and which seemed to take away all hope of approaching the truth, or coming near it. These difficulties, however, philosophers have been able to surmount; so that barometric measures, properly employed, may vie in exactness with the trigonometrical measures, to which they are superior on account of their facility and the generality of the method.

Among the different formulæ given for the solution of this problem, that of M. Laplace is distinguished by the manner in which it has been deduced from theory; but the principal co-efficient, drawn from an observation which appears not to have been free from error, might have need of some modification. This M. Ramond has examined in a memoir, of which we shall give some account. By his numerous experiments made on different mountains, he found what are the circumstances most favourable to such observations, as well as the hours which ought to be chosen or avoided; for there are some causes the effects of which must be very sensible, and which, however, it is impossible always to take into account in calculations. Such are the ascending or descending winds, which, according to M. Ramond, prevail almost constantly at certain hours. Some, by lessening the weight of the column of air with which the mercury is in equilibrium, must also lessen that column, and

and make heights to be considered as too great: descending winds necessarily produce a contrary effect. The moment when the equilibrium of the atmosphere is not disturbed by either of these causes must therefore be chosen, and that moment is the middle of the day. But M. Ramond observed also, that the descending winds prevail oftener than others; and he concludes that in general the mean results of observations must give heights too small.

To make a proper choice of the moment is not all; no less care and attention is requisite in the choice of the stations: simultaneous observations, some made in the place the height of which is required, and others in a fixed place the height of which above the level of the sea is perfectly known, are necessary. Those also who wish to verify a formula must have the same knowledge of the height of the mountain to which the barometer is carried; and that no objection may be made to the conclusion, it is necessary that the two stations should be sufficiently near, and that nothing interrupts the communication, so that the atmospheric variations which arise in the one may also take place in the other. M. Ramond found all these advantages united in the Peak of Bigorre, and the town of Tarbes, where M. Dangos, a celebrated astronomer, was pleased to take upon himself the corresponding observations.

It was by these means, and with these attentions, that M. Ramond found the correction of the co-efficient of Laplace; after which he applied the formula thus corrected, in conjunction with several other known formulæ, to calculate the aërostatic ascent of M. Gay-Lussac, who rose to the greatest height ever attained by man, since it surpasses that of all the mountains on the earth.

He applies also all these formulæ to the observations communicated to the class by M. Humboldt, and which were made on the highest mountains of Peru, and particularly Chimboraço, several hundreds of metres above the point at which Condamine, the most intrepid of our academicians, was obliged to stop.

It results from all these formulæ, that the formula of Laplace forms a pretty exact mean between alll the other formulæ; that it gives errors always very small, sometimes more sometimes less; and that the sum of these errors divided by the number of observations scarcely indicates 1-100dth as the ulterior correction of the co-efficient determined by M. Ramond.

This memoir is terminated by an appendix, in which are given models of all the calculations, tables for shortening the

the operations, and reflections on some small corrections which may be neglected in the most common circumstances.

Terrestrial Magnetism.

The observations of the magnetic needle which M. Humboldt made with great care in the countries which he visited, have given M. Biot the idea and the means of making researches in regard to the mathematical theory of terrestrial magnetism *.

GEOGRAPHY.

The results of the attempts hitherto made by the society in England for making discoveries in the interior parts of Africa are well known. By the accounts of the different travels it caused to be undertaken, the difficulties and danger which attend such expeditions are seen, and it may therefore be readily conceived why that part of the world is so little known. There was reason to think that the antients had a more extensive and correct knowledge of it; and in consequence of this idea the Class of the Moral and Political Sciences proposed, as the subject of a prize in the year 9, a comparison of the geography of Ptolemy concerning the interior of Africa with what has been since written on it by modern authors and those of the middle ages. As no memoir was presented, and as the subject of the prize was withdrawn, M. Buache thought it his duty to communicate to the class a detail of the researches which he had before made on the same subject. In 1787, before the formation of the English African Society, he had announced his opinion in regard to it, in a memoir read in a public sitting of the Academy of Sciences; and this opinion, contrary to all the ideas before received, was of such a nature as to excite the attention of geographers, had it been developed as it is in the new memoir which M. Buache presented to the class this year.

In this memoir, which is entitled *Researches in regard to the Interior of Libya of Ptolemy*, the author examines in succession, and in the order in which they are, all the details contained in the sixth and fourth books of Ptolemy; and according to the various information he has found, and which he discusses by presenting them under their real point of view, he endeavours to indicate nearly the position of the different objects described by the antient geographer. He presents this immense labour only as a mere commentary, destined to throw light on the knowledge which the antients

* See his paper on this subject in the last and present Number.

antients had acquired in regard to the interior parts of Africa, and to furnish some information useful to new travellers, and to the learned who are interested in the progress of discovery. By allowing to the knowledge of the antients much more extent than is generally supposed, he is obliged, by the gross errors which he corrects, and of which he shows the cause, to deny the exactness ascribed to them by some authors. As we cannot here give an idea of this labour, we shall confine ourselves to the principal results. According to the opinion of M. Buache, the knowledge of the antients along the western coasts of Africa extended as far as the Cape of Palms, and to the commencement of the Gulph of Guinea: they had only a vague idea of that gulph, because they durst not enter it; but they sailed without difficulty as far as Sierra Leone and the banks of St. Anne, which represent to us the *Hypodromos Æthiopiæ*; and all the coast to that place was well known to them. On this first point M. Buache agrees with Danville and major Rennel.

In regard to the interior of that country the antients distinguished two large rivers, the Niger and the Gir. According to Danville, whose opinion has been hitherto adopted, the Niger was that great river which waters Nigritia, directing its course from west to east; and the Gir, that which waters the kingdom of Bournon from north to south, and which then proceeds to the Nile. According to M. Buache, the Niger of Ptolemy is composed of the river Senegal and that part of the Joliba discovered by Mungo Park, and the Gir is a river which waters Nigritia along with the Joliba. M. Buache, therefore, establishes on the Joliba and the Senegal the people and towns which Ptolemy has placed near the river Niger; and transports to Nigritia, on the Niger of Danville, the people and towns which Ptolemy indicates on the Gir.

It appears to M. Buache, that the antients carried on along the coasts and into the interior of Africa the same trade that they do at present, and in the same manner. They had establishments on the coast, and on the great rivers which proceed thither, such as the Senegal and the Gambia: they extended their commerce as far as the banks of the Gir, but they did not penetrate beyond that river towards the south. Ptolemy speaks of no town beyond the Gir, but gives only the names of different tribes.

A very curious observation, which is worthy of further research, is that of several tribes whose names are twice mentioned, and which are at considerable distances in Ptolemy's lemy's map. There are six in that part of the Barbary coast comprehended between the two Syrtes, and depending on the kingdom of Tripoli. Such are the Astacuri, the Dolopians, the Mimaci, the Samamyci, the Nigbeni, and the Esopæi. It is well known that this part of the coast of Africa is that by which a communication may be most easily opened with the country of Nigritia, because there are fewer deserts to be traversed, and because the kingdoms of Fezzan, Agadez, and others, where refreshments can be procured, are found on the route. It is to be remarked, that it is to the south of the sources of the Gir that the names of the above tribes are found; and they are at a small distance from each other, and near the coast of Barbary. M. Buache presumes, till further information be obtained, that these tribes were colonies of those on the coast of Barbary, and that the countries which they occupy to the south of the sources of the Gir, are the most fertile and richest of the interior of Africa. This memoir contains other observations equally interesting, which may afford encouragement to the prosecution of discoveries in this part of the earth.

ASTRONOMY.

M. Burckhardt, who received the first intelligence of the new planet discovered by M. Harding, has constantly followed, more than any of our astronomers, the progress of this almost imperceptible body; he has endeavoured also to determine the elements of its orbit. On the 16th Vendemiaire he presented to the class an ellipsis, the great semi-axis of which, or the mean distance from the sun, was nearly the same as that of Ceres and Pallas, and its eccentricity greater than that even of Mercury. In regard to the inclination, it is much less than that of Pallas, but greater than that of any other planet. M. Burckhardt, before he arrived at this ellipsis, had tried a parabola, and then a circle. Twenty days after he read a new memoir, which confirmed all his former results; but he gave to each element a more approximate value. On the 3d of Nivose he made known to us a third ellipsis, which differs from the second only by quantities almost insensible, and which cannot be improved but by means of more numerous observations, and made at more remote periods.

M. Gauss, correspondent of the Institute, published also in Germany the elements of the same planet, founded, in a great measure, on other observations, and which differ very little from those of M. Burckhardt.

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This new planet, therefore, so difficult to be seen, and the theory of which still announces greater difficulties to those who may wish to determine it; by calculating all the perturbations it may experience, seems sufficiently known to be found without much trouble, when disengaged so far from the solar rays as to be again visible.

Solstitial Equinoxes.

We gave an account the preceding year of the observations made with Borda's circle at the observatory *Rue de Paradis*, to determine the equinoxes and the solstices. Since our last public sitting the same astronomer has observed two new equinoxes and two solstices; for the sky was too often obscured during the latter part of Prairial: yet, as he let slip no opportunity, we have already collected a great number of observations of the solstice which took place in the night between the second and third of Messidor, to be assured that the observations which we hope still to make may produce great changes in the definitive result.

The observation of the solstices was employed by the antient astronomers to determine the length of the longest day in every climate, and the height of the pole for the place of observation. At present, we have means much more precise for ascertaining the height of the pole; and in regard to the length of the longest day, we already know it with more than sufficient exactness. But the solstices are still no less interesting to astronomy, which has no other method so natural of determining the obliquity of the ecliptic; that is to say, the angle formed by the planes in which the annual and diurnal revolutions of the earth are effected; the fundamental element which enters into all our calculations, and the fixing of which is a matter of so much delicacy, that observations cannot be multiplied too much to determine properly either the precise extent it had at a given period, or the variation which it annually experiences.

It results from the observations of which we here give an account, that by a mean of twelve solstices, both of winter and summer, the mean obliquity must have been 23° 27′ 57″ at the commencement of the 19th century, and that it would be less by 1″ or 2″ if we referred merely to the last summer solstice. The annual diminution is still much more difficult to be known, since it supposes excellent observations made at two periods sufficiently distant from each other. Theory would give it with more precision, were we not obliged to suppose a mass respecting which there still remains

remains some doubt. The observations of Lacaille, Bradley, and Mayer, compared with those which we have mentioned, and those of the most celebrated modern observers, furnish quantities the extremes of which are 44″ and 56″ for the present century; theory gives 52″; and this result has been adopted in the solar tables now printing.

The observation of the equinoxes furnishes the most natural and most exact means of knowing the length of the year, the apparent motion of the sun, and the point of the heavens from which the motion of all the stars is reckoned. The five last equinoxes, and more particularly those of the year 13, have fully confirmed the correction of from 4″ to 5″, made some years ago, in the right ascensions of the stars, which serve as a foundation to all our calculations.

M. Pictet, correspondent, has communicated to us the observation of an occultation of the Pleiades by the moon, made at the observatory of Geneva.

An occultation of π of Scorpio, observed on the 28th of Messidor, year 12, on the summit of Casuelta, a mountain in the kingdom of Valencia, has been found among the papers of M. Mechain, and will appear in the 6th volume of the Memoirs of the Class. This is the last observation made by an astronomer whose premature loss the Institute will long regret.

There was found also among his papers a series of observations of the comet which he discovered at Barcelona in 1793; it is also printed in the 6th volume of the Memoirs, and will soon appear.

M. Humboldt read in one of our sittings a memoir on the longitude of Mexico, the capital of the kingdom of the same name.

Geographers were little agreed in regard to the position of that important point. The considerable difference which M. Humboldt found between his first observations and the last which were made by him, induced him to repeat them as often as he could, and by different methods. The distance of the moon from the stars, and the eclipses of several satellites of Jupiter, constantly gave him the same result; which is incontestably preferable to all those which had appeared before.

[To be continued.]

LV. *In-*

LV. *Intelligence and Miscellaneous Articles.*

ANTIQUITIES.

A LETTER from Italy, dated August 10, says: "Mr. Hayter, who obtained permission from the king of Naples to unroll the manuscripts found in Herculaneum, begins now to reap the fruits of his labours and patience. The eleven young men whom he employs for this purpose have become very expert, and labour with more dexterity than their predecessors. Mr. Hayter entertains hopes that he has found a whole Menander, Ennius, and Polybius. He has found a Greek writer named Colotos, whose philosophical works were before totally unknown. A valuable discovery is an entire copy of Epicurus, of which we had before only fragments. There are still 600 manuscripts in the Museum of Portici."

Naples, June 25.—According to the reports made to government of the state of the famous ruins of the antient city of Pæstum; and particularly the largest of the three temples, which having been damaged by lightning was in danger of falling down, orders were given for its restoration. This design, however, having been prevented by different obstacles, the counsellor of state charged with the department of the fine arts, M. Seralli, sent to Pæstum, at the end of the last year, don Felix Nicolassi, superintendant-general of the search for antiquities throughout the kingdom, in order to remove the rubbish from the largest of the three temples, to examine it, and draw up a plan for its restoration. When the superintendant arrived along with Antonio Buonucci, he drew up a plan for the restoration of this monument of antiquity; which, being approved by government, was carried into execution in the course of the present year. During his stay at Pæstum, while superintending the works undertaken for removing the rubbish which disfigured this antient monument, M. Nicolassi caused researches by digging to be made in several places, which were attended with the best success. He found, in different tombs into which he entered, bronze arms, the sculpture of which was perfect, and which were highly interesting on account of the very remote period to which they belonged. He found there also bronze urns of the most elegant form; some of baked clay, exceedingly curious, both in regard to their form, the subjects represented on them,

them, and the perfection of the design: in a word, a great number of military instruments and utensils, used for sacred as well as domestic purposes. Exact descriptions of these articles, as well as of the paintings found in these tombs, are to be published.

Government, no less attentive to the preservation of the objects of antiquity which exist in the environs of Puzzoli, has charged M. Nicolassi to clear away the ruins which encumber the temple of Jupiter Serapis. The labour has been already begun; and this antient temple will soon be entirely discovered, and freed from the stagnant water formed around it, and which occasioned the greatest inconvenience in regard to the salubrity of the air in that country.

EARTHQUAKE.

Messrs. Wombwell, Gautier, and Co., a highly respectable mercantile firm in the city, have favoured us with the following particular account of the earthquake that took place in the kingdom of Naples, the 26th of July, and the eruption of Mount Vesuvius on the 12th of August, as transmitted to them by Mr. Falconnet, a merchant of veracity, and much respected at Naples:

" *Naples, August* 13.—The following particulars of the dreadful earthquake, on the 26th July, as received by government, down to the 29th July, are the most accurate that have yet appeared. I procured a copy to be sent to you this post for your information, that of your friends, and correspondents. Although I expressed to you by my former letter my regret that no eruption of Mount Vesuvius took place, and that, on the contrary, the little columns of fire that arose now and then were less since the earthquake, and how desirable it was that a vent should be given by an eruption to the inflammable matter that seemed to exist in the bowels of the earth, I did not expect to have this day to announce to you, that my wishes were accomplished last night, by an abundant eruption of lava from Mount Vesuvius; which, though we did not feel any fresh shock of earthquake since July 26, yet now relieves us, in my humble opinion, from any further apprehension of new shocks.

" In the course of yesterday, till seven o'clock in the evening, Vesuvius was very quiet, emitting but little smoke; it then increased, with flames at intervals: past nine o'clock, they became frequent, and I observed, when they fell, that the mouth of Vesuvius appeared still as a furnace. I was then on the terrace of my country house, at St. Jeriv, west from Vesuvius, and very near it. Mrs. Falconnet had just left

left me to sit down to supper in the dining-room next the terrace, and wished me to come;—but the scene before me kept me some minutes longer.—I joined her, and had not sat down a minute when her English maid called to us that the eruption was beginning. In an instant we were on the terrace, and observed its having overflowed on the same side as last year, and rushed down with such rapidity as to run more than a mile in ten minutes, and in a very short time it reached the valley towards Torre del Greco. This stream of lava was immense, and extended with amazing rapidity over the country; it divided itself in three branches, one of which, beyond the Torre del Greco, surrounded the country-house of the cardinal archbishop of Naples, and before morning reached the sea, and continued running into it. The stream of lava is much diminished; but when it broke out last night, about ten minutes after ten o'clock, until twelve, it was a grand and splendid sight; and as it ran from north to south, and I was west of it, it represented the back scenery of Hell at an opera; figure to yourself an immense sheet of flames rising at least half a mile from the ground, and crowned by a black cloud which vanished by degrees.

"Many very valuable vineyards and farm-houses have been destroyed; and as the lava rushed out with very little noise and great rapidity, I am afraid some habitations on the brow of the hill will have been surrounded before the people were aware of the danger, or had time to escape: but a great part of the lava ran on that of last year, 1779, which renders the mischief less. It surprises many strangers, how people will cultivate and live on such a spot, as the lava constantly takes that direction, south and south-east; but the land is so very productive, that the temptation is not easily combated.

"All my family were perfectly composed during the whole of the eruption, and returned quietly to bed at midnight, as I had often taught them to wish for it since the earthquake, as a security from new shocks.

"One cannot but regret that such a beautiful country as this, blessed with an admirable soil, fine situation, healthy climate, and pure sky, should be liable to such drawbacks and convulsions of nature. But in this world we cannot expect enjoyments without some alloy, and we must submit to Providence, who has perhaps decreed in its wisdom, that a people too much inclined to vices and immorality, should be now and then recalled to a sense of its duties by

 such

such uncommon events, which happen when least thought of. I am truly, sir, your most obedient servant,

(Signed) J. L. FALCONNET.

"It is now eleven o'clock in the evening, and before closing this letter I looked at the lava: the stream continues, but it is nothing in comparison to last night. I heard a few hours ago, that there was an eruption of Ætna in Sicily, but I could not trace that report to its source. Our last letters from Sicily make no mention of it, though much alarmed by an intense heat, that lasted five days, and in great apprehensions."

Particulars of the Damages caused by the Earthquake of Friday, July 26, from Reports to the Secretary of State's Office, down to the 29th July.

Towns and Villages.	Damages.	Families perished.	Total dead.
Isernia	destroyed	339	1506
Castel Petroso,	ditto	131	443
Cantallipa,	ditto	142	568
Santo Massimo	ditto	74	227
Tresolone,	part destroyed	390	1440
St. Angelo in Grotta,	ditto	43	174
Carpinone,	ditto	193	579
Baranella,	ditto	180	720
Sassano,	entirely destroyed	inhabitants lost	
Bassano,	become a lake	220	672
St. Angelo di Lombardi,	part destroyed,	no particulars	
Camelli,	a volcano opened	ditto.	
		1772	6329

Other Places, with general Information.

Bassano, destroyed, and was the centre of the earthquake, which extended in a circuit of 150 miles. The following places were also destroyed:—Rucca Mandolfi, Macchia Godena, Mirabello, Vinghiatura, and other villages round.

The following places partly destroyed:—Campobasso, Saverna, Supino, Ducameno, Santa-buono, Colle Danchese, Castor Petrone, Civita Narva, Bolino, and other villages round.

N.B. Of the different places in Abruzzo, and the Contado di Molise, that have suffered, no particulars are as yet given, no certain account having as yet been received of the number of fam lies or persons dead or missing; but as many are supposed to be dead that are missing, the number is likely to be less, at least we hope so.

ASTRONOMY.

Table of the right Ascension and Declination of Ceres and Pallas for October 1805.

	Ceres.					Pallas.				
	AR.			Decl. N.		AR.			Decl. S.	
1805	h	m	s	°	′	h	m	s	°	′
Oct. 2	6	58	16	22	51	5	8	24	16	40
5	7	1	32	22	55	0	10	20	17	37
8	0	4	36	22	59	0	12	0	18	34
11	0	7	32	23	3	0	13	28	19	32
14	0	10	16	23	8	0	14	40	20	30
17	0	12	52	23	13	0	15	40	21	27
20	0	15	16	23	18	0	16	24	22	25
23	0	17	32	23	24	0	16	48	23	23
26	0	19	36	23	31	0	17	0	24	19
29	0	21	24	23	39	0	16	52	25	14

VACCINATION.

Certificate of the Evidence of Mr. Jesty, the Inoculator of his Family for the Cow-pock in 1774.

Mr. Benjamin Jesty, of Downshay in the isle of Purbeck, having, agreeably to an invitation from the Medical Establishment of the Original Vaccine Institution, Broad-street, Golden Square, visited London in August 1805, to communicate certain facts relating to the cow-pock inoculation: We think it a matter of justice to himself, and beneficial to the public, to attest, that, among other facts, he has afforded decisive evidence of his having vaccinated his wife and two sons, Robert and Benjamin, in the year 1774, who were thereby rendered unsusceptible of the small-pox; as appears from the exposure of all the three parties to that disease frequently during the course of 31 years: and from the inoculation of the two sons for the small-pox 15 years ago—That he was led to undertake this novel practice in 1774, to counteract the small pox, at that time prevalent at Yetminster (where he then resided), from knowing the common opinion of the country ever since he was a boy, now above sixty years ago, that persons who had gone through the cow-pock naturally, i. e. by taking it from cows, were unsusceptible of the small-pox—by himself being incapable of taking the small-pox, having gone through the cow-pock many years before—From having personally known many individuals, who, after the cow-pock, could not have the small-pox excited—From believing that the cow-pock was an affection free from danger—And from his opinion that by the cow-pock inoculation he should avoid ingrafting various diseases of the human constitution, such as "the evil, madness, lues, and many bad humours," as he called them.

The remarkably vigorous health of Mr. Jesty, his wife, and two sons, now 31 years subsequent to the cow-pock; and his own healthy appearance, at this time 70 years of age, afford a singularly strong proof of the harmlessness of that affection. But the public must with particular interest hear that, during the late visit to town, Mr. Robert Jesty very willingly submitted publicly to inoculation for the small-pox in the most rigorous manner, and after the most efficacious mode, without having been infected.

The circumstances in which Mr. Jesty purposely instituted the vaccine pock inoculation in his own family, viz. without any precedent, but merely from reasoning upon the nature of the affection among cows; and from knowing its effects in the casual way among men; his exemption from the prevailing popular prejudices; and his disregard of the clamorous reproaches of his neighbours, in our opinion well entitle him to the respect of the public for his superior strength of mind: but, further, his conduct in again furnishing such decisive proofs of the permanent anti-variolous efficacy of the cow-pock in the present discontented state of many families, by submitting to inoculation, justly claims at least the gratitude of the country.—As a testimony of our personal regard, and to commemorate so extraordinary a fact as that of preventing the small-pox by inoculating for the cow-pock 31 years ago, at our request a three-quarters length picture of Mr. Jesty is painted by that excellent artist Mr. Sharp, to be preserved at the Original Vaccine Pock Institution.

John Heaviside, Thomas Payne,	Treasurers.
George Pearson, Laurence Nihell, Thomas Nelson,	Physicians.
Thomas Keate, Thompson Forster,	Consulting Surgeons.
Joseph Constantine Carpue, John Doratt,	Surgeons.
Francis Rivers, Everard Brande, Philip De Bruyn,	Visiting Apothecaries.

FINSBURY

FINSBURY DISPENSARY.

At the late election for Surgeon to this Institution the numbers were; for Mr. Taunton 350; for Mr. Smith 143. —Majority in favour of the former, 207.

Mr. Taunton's Lectures on Anatomy and Physiology will commence at the Finsbury Dispensary, on Saturday, October 5.

LIST OF PATENTS FOR NEW INVENTIONS.

James Noble, of Coggershall, in the county of Essex, worsted-spinner; for a machine for discharging a wool-comb or combs, by separating the tears from the noiles, and drawing what is commonly called a sliver or slivers from the comb or combs after or before the combs are worked, or the wool is combed upon the same.

William Kent, of the borough of Plymouth, in the county of Devon, merchant and agent; for certain additions and improvements in a sort of candlestick (in common use), which will be found to prevent accidental fires in the use of candles, by which so many valuable lives are lost, and such immense property consumed; and which will not be confined to chamber use, but, being made on a larger scale, will be found equally useful in shops, warehouses, oil and spirit cellars, and other places where the use of a candle is found necessary.

Arthur Woolf, of Wood-street, Spa Fields, in the county of Middlesex, engineer; for certain improvements in steam-engines.

James Boaz, of the city of Glasgow, in Scotland, civil engineer; for a new and improved method of raising water, and working machinery by means of steam.

Alexander Wilson, of Tichborne-street, Piccadilly, in the county of Middlesex, gun-maker; for certain improvements applicable to shot-belts and powder-flasks, and to fire-arms of all descriptions.

Benjamin Batley, of Queen-street, in the city of London, sugar refiner; for a new and improved method of refining sugars.

Henry Edward Witherby, of Islington, in the county of Middlesex, gentleman; for an apparatus for purifying and improving water and other liquors by filtration.

Johan Gottlieb Frederic Schmidt, of Greek-street, Soho, in the county of Middlesex, gentleman, and Robert Dickinson, of Tavistock-street, Covent Garden, gentleman; for methods of sustaining animal life and combustion for a great length of time, at considerable depths beneath the surface of the sea, or other bodies of water, in such a manner as to enable a person making use of such means, to exist, and to move from place to place, at the bottom of the sea, or at any required depth between the surface and the bottom, with much more facility and advantage than by any other apparatus or contrivance which has been hitherto invented for that purpose.

Peter Marsland, of Heaton Norris, in the county of Lancaster, cotton spinner; for improvements in sizing cotton yarn.

Peter Marsland, of Heaton Norris, in the county of Lancaster, cotton spinner; for an improvement in the process of dyeing silk, woollen, worsted, mohair, furhair, cotton, and linen, or any one or more of them, as well in a part-manufactured as in an unmanufactured or raw state.

Thomas Chapman, of Witham in Holderness, in the county of York, thrashing-machine-maker; for a mill for tearing, crushing, and preparing oak-bark to be used by tanners in the process of tanning of hides.

Henry Maudslay, of Margaret-street, Cavendish-square, in the county of Middlesex, mechanist: for a process, upon an improved construction, for printing of calicos, and various other articles.

William Wilkinson, of Needham Market, in the county of Suffolk; for improved pantiles for covering houses and other buildings.

William Scott, of the London Glass Works, East Smithfield, in the county of Middlesex, glass manufacturer; for certain improvements in the manufacturing and working of various kinds of glass.

Thomas Johnson, late of Stockport, in the county of Chester, but now of Preston in the county of Lancaster, weaver; and James Kay, of Preston, aforesaid, machine maker; for an improved machine or loom for weaving cotton and other goods by power.

William Deverell, of Blackwall, in the county of Middlesex, engineer; for certain improvements on the steam-engine.

Samuel Caldwell, of Hathern, in the county of Leicester; for new machinery and apparatus to be attached or annexed to certain plain frames or machines called stocking-frames, plain piece-frames, or any other plain frames for the purpose of working, making, or manufacturing silk, cotton, mohair, worsted, or any other sort of stuff whatsoever, into plain hose, or any plain sort of piece-work whatsoever, whereby these frames will work, make or manufacture all kinds of plain stockings and plain piece-work by mechanical machinery and motion.

METEOR-

METEOROLOGICAL TABLE
BY MR. CAREY, OF THE STRAND,
For September 1805.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Aug. 27	61°	72°	61°	29·97	48°	Fair
28	62	69	64	·98	30	Cloudy
29	63	71	64	30·03	41	Fair
30	64	71	63	29·98	34	Fair
31	64	69	64	·72	25	Cloudy
Sept. 1	63	68	58	·68	42	Showery
2	57	64	58	·96	34	Cloudy
3	61	69	56	·85	34	Cloudy
4	60	69	64	·72	35	Cloudy
5	63	70	57	·69	40	Fair
6	60	69	58	·50	40	Fair, with thunder and rain in the morning
7	60	66	58	·36	17	Stormy
8	59	67	55	·62	25	Showery
9	56	66	56	·95	39	Cloudy
10	57	67	58	30·16	40	Fair
11	56	70	64	·06	36	Fair
12	64	70	55	·05	25	Showery
13	54	69	56	·10	30	Fair
14	57	71	57	·19	45	Fair
15	55	71	55	·16	27	Fair
16	54	72	59	29·96	37	Fair
17	61	70	57	·96	32	Fair
18	60	75	68	·98	42	Fair
19	64	71	64	·89	22	Showery, with thunder at night
20	57	66	55	30·12	51	Fair
21	56	65	54	29·80	26	Showery
22	55	56	51	·93	10	Showery
23	53	58	49	30·10	12	Showery
24	47	57	42	·11	45	Fair
25	41	59	54	·16	35	Fair
26	56	63	55	·15	27	Cloudy

N. B. The barometer's height is taken at noon.

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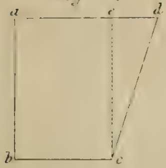
Carriages.

Hydromis.

END OF THE TWENTY-SECOND VOLUME.

Printed by R. Taylor and Co., 38, Shoe Lane, Fleet Street.

Fig. 1.



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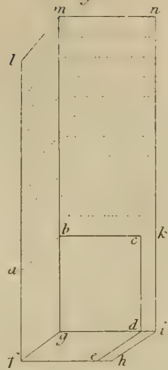

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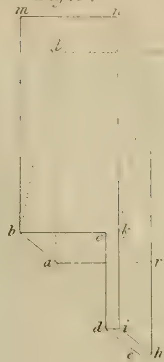

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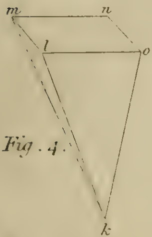

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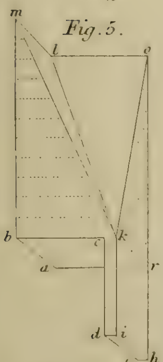

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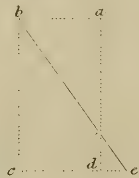

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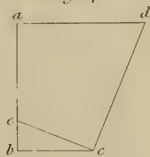

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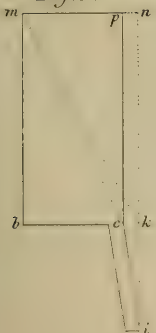

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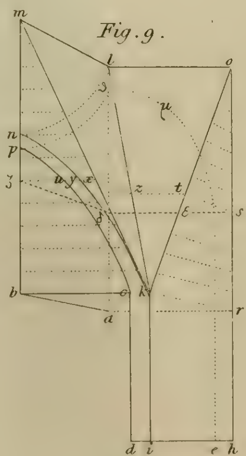

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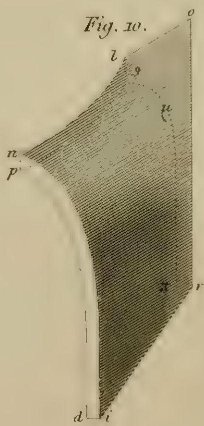

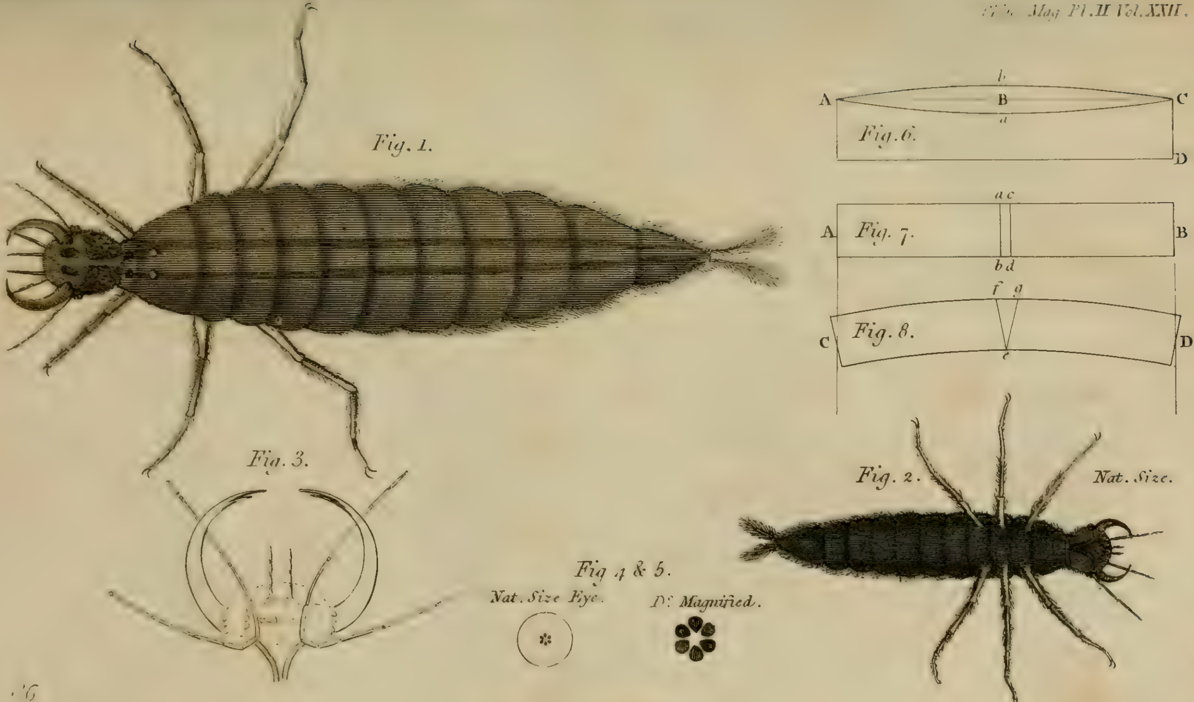
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Fig. 6.
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Fig 4 & 5.
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Fig. 2.
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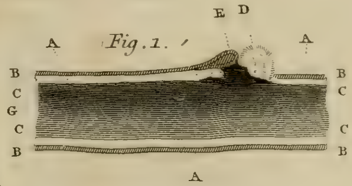
Buds and ramifications of Plants.

Fig. 1.



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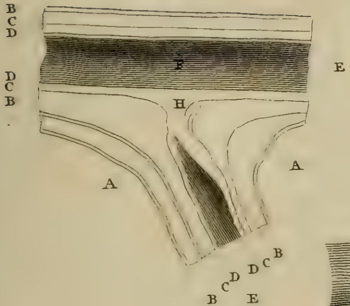

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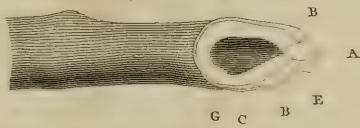

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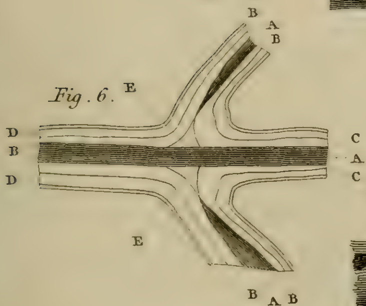

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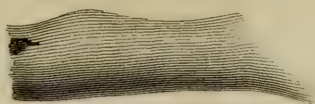

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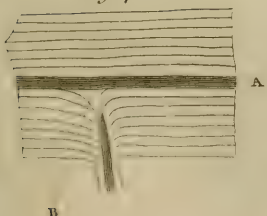

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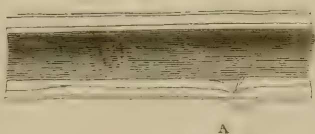
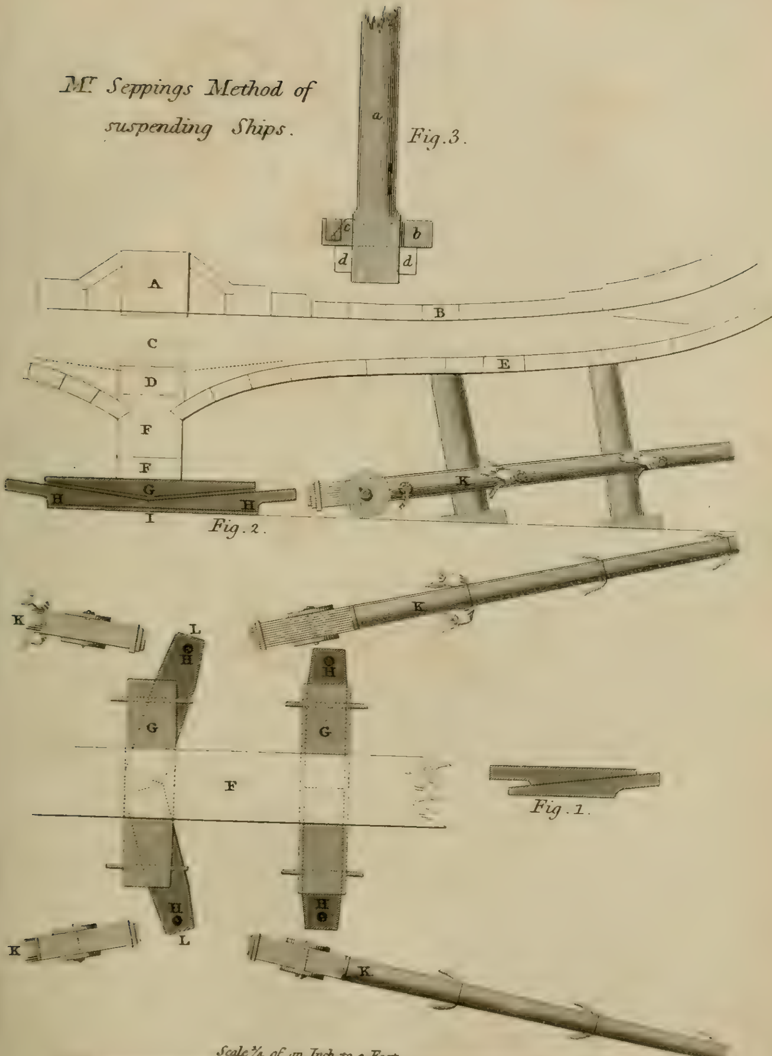
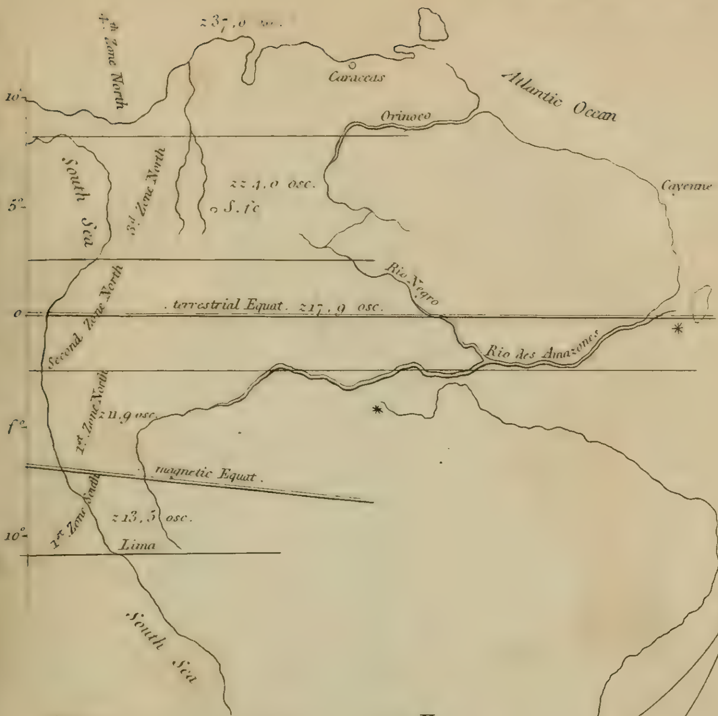


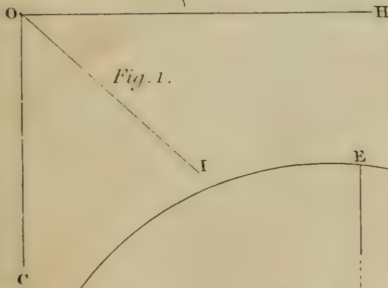
Mr. Seppings Method of suspending Ships.



Scale 3/4 of an Inch to a Foot.

1 2 3





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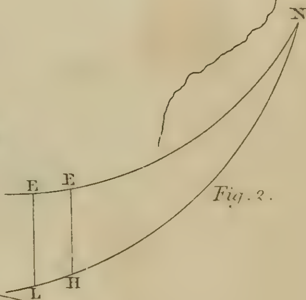

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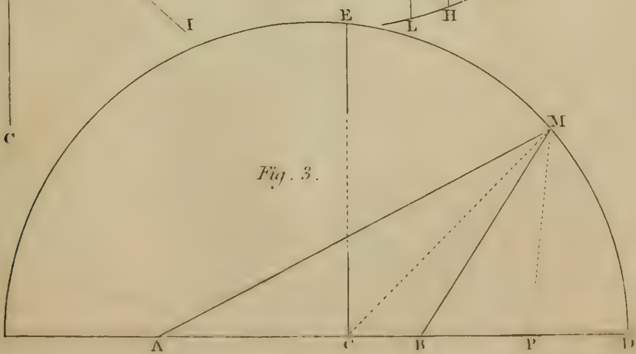

Fig. 3.

Hydromis Coypou.

A. *Red bellied Hydromys.*

B. *White bellied Hydromys.* C. *Scull of the same.* D. *its lower Jaw.*