to express a decided view on the subject (such as Mr. Burt, Sir G. Elliot, and the late Mr. Knowles), that blasting should be prohibited, at any rate in fiery mines. It was admitted that the cost of working coal would be much increased by the enforcement of the suggested prohibition, and the majority of competent witnesses examined afterwards by the Royal Commission maintained that the abolition of shot-firing in coal-getting must be attended by very formidable difficulties, and must, in fact, cause the closing of many pits.

I have shown that even the comparatively very small amount of fire-damp which may, at any rate occasionally, pervade the air in portions of mine-workings where thorough ventilation is most effectually provided for, and may escape detection, suffices to determine the production of a disastrous explosion, if, under these circumstances, a blown-out shot occurs where an accumulation of dust exists; and that it is even possible, in the complete absence of fire-damp, for a blown-out shot to give rise to an explosion in a very dusty working or mine, where the coal is of a specially inflammable and sensitive character. Such being the case, the fact cannot be ignored that last year's decision of the late Home Secretary—which raised consternation in many mining districts—to prohibit the firing of shots in any colliery within a period of three months after the existence of gas had been there reported (while the workmen were in any part of the mine), is far from affording the contemplated protection against disaster resulting from the use of explosives *in the ordinary manner*.

This most grave aspect of the question has received the

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To this Society, which has always distinguished itself by its encouragement of earnest workers, and by its just judgment of their labours, I have ventured, as one of its members, to make these comments, which could not be uttered by me in my capacity as a member of Her Majesty's Commission, whose duty it is simply to report the results of their labours when they have, to the best of their judgment, fulfilled the duties imposed upon them.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 19.—Abstract of "Report on a Series of Specimens of the Deposits of the Nile Delta, obtained by the recent Boring Operations." By J. W. Judd, F.R.S., Professor of Geology in the Normal School of Science and Royal School of Mines. Communicated by order of the Delta Committee.

suppose, so thoroughly established that the notable absence of them on these occasions may fairly be taken to suggest a possibility that the phenomena were not truly auroral. If so, the coincidence with the star-shower becomes more noteworthy.

Rugby, January 5 J. B. HASLAM

A SPLENDID shower of meteors occurred on the night November 27, 1885. Seen from Ava, near Mandalay, 10 p.m. mean time of place, the point of emergence was ne the zenith, and the shower radiated to each point of the horizo The rate at that hour was 450 to 600 per minute, as near as could judge lying on my back on the steamer's awning. It probable, however, that I missed a great many. The point emergence was at one-fifth the distance from γ Andromed (Almach) towards β Andromedæ (Mirach). The followir night the shower was still plentiful, but I did not count ther The nights have been very clear and beautiful here.

December 1, 1885 ALFRED CARPENTER

Deposits of the Nile Delta

IN the abstract of the Report of the Committee of the Roya

The striking peculiarities of these sands and muds of the Nile-Valley appear to be capable of a simple explanation. In countries where rain falls and vegetation abounds, water charged with carbonic acid is constantly tending to break up the compound silicates; the silicates of the alkalies and the alkaline earths being decomposed and their constituents removed in solution, while the silicate of alumina becomes hydrated, and is carried away in suspension by water in the form of kaolin. In this way, the felspars and nearly all other compound silicates are affected to such an extent that in most granitic and metamorphic rocks they show evidence of extensive "kaolinization," while the clays derived from them are made up for the most part of crystalline plates of kaolin. But in a rainless country, like Northern Africa, none of these agencies will operate, and the disintegration of the solid rocks is effected by mechanical means; the most potent of these mechanical agents are the heat of the sun, causing the unequal expansion of the minerals which build up the rocks, and the force of the wind, producing constant attrition of the disjoined particles.

This being the case, it will be readily understood that the coarser sand-grains will include felspar and other minerals in a nearly unaltered condition, while in countries where the chemical agents of the atmosphere come into play, such particles would be more or less completely converted into kaolin. In the same way the mud, instead of consisting of scales of kaolin originating from chemical action, will be formed of particles of the chemically unaltered minerals reduced to the finest dust by purely mechanical agencies.

The chemical analyses which have been made of these Nile muds entirely support these conclusions. Instead of containing a considerable proportion of combined water, as do all the ordinary clays, their composition is that of a mixture of anhydrous minerals.

But there is fortunately a kind of evidence, derived from chemical analysis which is of the greatest value from its bearing on the questions we are now discussing—that, namely, which is derived from a study of the composition of the Nile-waters.

It must be remembered that the Nile is a river of a very peculiar and exceptional character. The last tributary which it receives is the Atbara, which falls into it in lat. 17° 38′ N.; from that point to its mouth, in 31° 25′ N. lat., the river does not receive a single affluent; for a distance of 1400 miles it acquires no fresh supply of water except what is brought to it by superficial torrents after heavy rains in Lower Egypt. It has been clearly demonstrated that, after receiving the Atbara, the Nile undergoes a continual diminution in volume in its course through Egypt. This is no doubt in part due to percolation of the water through the delta-deposits, and in part to the water being drawn off in canals for purposes of irrigation; but a large part of this diminution in volume must certainly be ascribed to the great evaporation which must be going on from the surface of the river during the last 1400 miles of its course.

Although we shall not be able to calculate the exact loss of the Nile by evaporation in the course of 1400 miles through one of the hottest and driest regions of the globe, yet we cannot doubt that this loss is enormous. Now the effect of this constant evaporation must be to concentrate the saline matters held in solution, and we might therefore anticipate that the waters of the Nile in Lower Egypt would contain an exceptionally high percentage of saline matters in solution.

But what are the actual facts of the case?

According to the analyses of Dr. C. Meymott Tidy, the Nile contains only a little more than one-half of the proportion of soluble materials which exists in the Thames, the Lea, the Severn, or the Shannon!

A little consideration will show, however, that this startling and seemingly anomalous result is capable of simple and easy explanation. The substances dissolved in the water of rivers is of course derived from the materials composing the rocks of the river-basin, through the action of water holding carbonic acid or other acids in solution.

Hence we are led by the study of the composition of the Nile water to the same conclusion as was reached by the study of microscopical characters of the muds and sands of the delta, that while in the rainy districts of the temperate zones the disintegration of rocks is mainly effected by chemical agencies, in the rainless areas of the tropics the same work is almost exclusively effected by mechanical forces.

The products of these two kinds of action are, however, essentially different. In the former case we have formed crystals of kaolin, which constitute the basis of all the true clays, a large quantity of lime, magnesia, iron, soda, and potash salts with silica passing into solution; while, in the latter case, the several minerals of the rock are simply reduced to fragments of varying size and form, and but little matter passes into solution.

The whole of the observations described in the present report are in entire harmony with this explanation. The comparatively unaltered condition of the felspars and other complex silicates in the sands; the absence of kaolin from the muds, and the presence of the chips and flakes of the unattacked minerals in the muds; and finally the small quantity of dissolved matter in the Nile-water, in spite of the enormous concentration it must have undergone by evaporation—all point to this same conclusion.

In the estimates which have been made of the rate of subaërial denudation in different parts of the globe, it has usually been assumed that this action is similar to what is seen taking place in our own country and in North America. But the observations detailed in this report prove that in rainless tropical districts, where little or no vegetation exists, the disintegration of rocks, though not, perhaps, less rapid than in temperate climes, is different alike in its causes and in its products.

It has often been pointed out by chemical geologists that metamorphic action could not have produced many of the schists from sedimentary rocks, for the former are rich in potash, soda, and other materials which have been dissolved out from the latter during the disintegration of the rock-masses from which they were derived. The recognition of a kind of action whereby great masses of sedimentary materials can be produced, rich in those substances which are usually removed in a state of solution, is not destitute of interest at the present time, when the question of the origin of the crystalline schists is one that presses for solution.

PARIS

Academy of Sciences, November 30.—M. Jurien de la Gravière, Vice-President, in the chair.—The Vice-President announced the death of the President, M. Henri Bouley, who died on the morning of the same day. The speaker referred in warm terms to the career of M. Bouley, his entire devotion to science, and the courage with which, although suffering from a fatal disease, he continued to the last to fulfil the duties of his office.—Obituary notices of M. Bouley: by M. Hervé Mangon, in the name of the Academy of Sciences; by M. A. Milne-Edwards, in the name of the Natural History Museum; by M. A. de Quatrefages, as Vice-President of the Acclimatisation Society; and by M. Fremy, Member of the Academy.—As a mark of respect for its late President, the public meeting of the Academy was immediately adjourned.

BERLIN

Physiological Society, October 30.—Prof. Zuntz spoke on the apnœa of the fœtus and the cause of the first respiration, setting forth the present state of the question, and then passing to consider the assertion of Prof. Preyer, who, by experiments on rabbits and guinea-pigs, sought to prove that it was not the change in the gas of the blood which was the cause of the first respiration, but a stimulus exercised

on the integument. Prof. Zuntz had quite recently, in conjunction with Dr. Cohnstein, made observations on a new-born lamb that, connected by the umbilical cord with the ewe, came into the world completely apnœic, and, notwithstanding that the most varied stimulations were exercised on the skin, continued apnœic for ten minutes long, though in all other respects these stimulations were normally responded to. Not till the placenta had detached itself did the respiration begin. This observation proved with all certainty that apnœa was dependent on the sufficient supply of oxygen, and that the first respiration was induced by a deficit of oxygen. They therefore repeated the experiments of Prof. Preyer, and came to the conclusion that under them the circulation of the blood always suffered disturbance in consequence of the pressure exerted, whereby the supply of oxygen to the fœtus was impaired, and that the fact which Prof. Preyer adduced in support of the accuracy of his view, namely, that the blood of the umbilical vein always appeared of a bright scarlet red, served exactly to disprove it. The brighter blood of the umbilical vein was, accordingly, an argument of a disturbance in the circulation of the blood, in consequence of which less arterial blood reached the fœtus, and, notwithstanding its greater saturation of oxygen, the blood was, therefore, unable, on account of its deficient quantity, to convey the requisite amount of oxygen to the whole blood. The respiratory centre in the brain thus got supplied with blood poorer in oxygen, and when a stimulation of the skin was superadded the first respiration ensued. In the case of the less excitable brain of the fœtus it was necessary that the outward stimulation should supplement the deficiency of oxygen. In the case of the normally born, however, the detachment from the placenta and the absolute want of fresh oxygen sufficed to stimulate the respiratory centre to activity. In the case of the adult, finally, with excitable brain, a slight reduction of oxygen was itself sufficient to excite respiration.—Referring to the beautiful discovery by Mr. Haycroft, of the fact that the ferment of the saliva in the leech prevented coagulation, Prof. Zuntz recommended the use of this ferment of the leech in measurements of blood-pressure, with a view to avoiding coagulation. This substance had the advantage over all other preventives of coagulation, that in no respect had it any toxic effect. Into the tube conjoining the artery of the animal examined with the manometer of the kymographion a T-tube was intercalated, and by its means a cubic centimetre of the ferment of the leech was squirted per hour into the separate fluid. This was sufficient for the marking of curves of blood-pressure for seven hours consecutively, without the least trace of coagulation.—In view of the divergence of opinions prevailing regarding the alimentary value of the peptones—some maintaining that peptone was used as an alimentary deposit in the body, while others considered that only the albumen absorbed as such was capable of being deposited, the peptones getting, on the contrary, further decomposed—Prof. Zuntz had a number of feeding experiments instituted with peptones. A somewhat long series of experiments was executed on a little dog, first with meat, then with peptone furnished from fibrine, next with albumose substances or propeptones, and, further, with lime. The experiment was arranged in such wise that the dog, along with equal quantities of fat and starch, received daily the like amount of nitrogen. The quantity of secreted nitrogen was daily determined, and thereby the deposit of nitrogen ascertained. The dog first got meat for some days, then peptones for some days, next thereafter meat again, and, following thereon, albuminose substances; this in turn was succeeded by meat days again, then lime days, and, finally, meat days anew. The deposit of nitrogen was now found to amount to—(1) with meat diet, 0·502 grammes nitrogen daily; (2) with peptone, 0·584 grammes; (3) with meat, 0·513 grammes; (4) with propeptone, 0·70 grammes; (5) with meat, 0·46 grammes; (6) with lime, – 0·5 grammes; (7) with meat, 0·48 grammes nitrogen. Meat feeding, accordingly, yielded about the same quantity of nitrogen deposit on each occasion of its being used; in the case of feeding with peptone and propeptone the nitrogen deposit was somewhat greater than in the case of meat-feeding, a result explained by the fact that all the nitrogen of meat did not belong to the albumen, but in part appertained to the nitrogenous bases, which could yield no nitrogen deposit. In the case of lime-feeding, on the other hand, a loss of nitrogen for the body was the result. Prof. Zuntz next had a further series of feeding-experiments performed with the peptones occurring in trade. The dog in question received only fat in addition to the nitrogenous nutriment. In the first days, with meat-feeding,

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SOCIETIES AND ACADEMIES

LONDON

results of his further investigation into the constitution of the derivatives obtained from cholestearine, which, at a meeting of the Society before the vacation, he had declared to be terpenes. He endeavoured to determine the molecular weight of those carbo-hydrates which, according to the nature of terpenes, had the composition $(C_5H_8)^n$. The vapour density, determined according to the method of Victor Meyer, showed itself in the lead bath not normal. It corresponded with the composition C_5H_8, thus indicating decidedly that a dissociation had set in during the process of heating. Other terpenes also, such as turpentine oil and camphor, yielded results which were not normal and showed a dissociation into the radical C_5H_8, a circumstance which likewise argued the terpene nature of cholestearine. Dr. Weyl was able, finally, to demonstrate the connection of cholestearine with the terpenes by showing that the latter very beautifully produced the well-known cholestearine reaction. Further experiments with a view to determining the vapour density in a vacuum would perhaps yield the molecular weight of these interesting carbohydrates.

CONTENTS

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Rugby, January 5 J. B. HASLAM

A SPLENDID shower of meteors occurred on the night of November 27, 1885. Seen from Ava, near Mandalay, at 10 p.m. mean time of place, the point of emergence was near the zenith, and the shower radiated to each point of the horizon. The rate at that hour was 450 to 600 per minute, as near as I could judge lying on my back on the steamer's awning. It is probable, however, that I missed a great many. The point of emergence was at one-fifth the distance from γ Andromedæ (Almach) towards β Andromedæ (Mirach). The following night the shower was still plentiful, but I did not count them. The nights have been very clear and beautiful here.

December 1, 1885 ALFRED CARPENTER

Deposits of the Nile Delta

IN the abstract of the Report of the Committee of the Royal Society, on recent borings in the Nile Delta (NATURE, Dec. 10, 1885, p. 142), there is a reference to my "Notes on the Geology of the Nile Valley" (*Geological Magazine*, 1884), which calls for some explanation in the interests of Egyptian geology. When I saw a portion of the borings in Cairo, in the early part of 1884, the work had extended to a depth of only about 40 feet. At a depth of between 30 and 40 feet the boring-rod, after passing through continuous Nile mud, had entered into quicksand, consisting of polished and rounded grains of quartz and other hard rocks (desert sand), and the difficulties incident to this material had for the time arrested the operations. In connection with this and with the insufficiency of the funds on hand for overcoming the difficulties of the work, I wrote a letter at the time to the President of the Royal Society, strongly urging an additional grant, in order that greater depths might be reached.

I then believed, and still believe, that the quicksand marks the true base of the modern Delta alluvium, and corresponds with the similar sand which in certain parts of the Delta protrudes itself from beneath the fluviatile deposit. I did not, however, suppose that this sand rests directly on the rocky floor of the valley. On the contrary, as might be inferred from my short statement in the *Geological Magazine* (July 1884, p. 292 and footnote), I anticipated that below the sand would be found the Pleistocene clays, marls, sands, and concretionary limestones of the "Isthmian" formation seen at El Guisr on the Suez Canal, and the equivalents of which rise from under the alluvium in several places on the sides of the Nile Valley. These also constitute the lower strata of the borings reported by Figari Bey; and it appeared to me that in the colour and texture of the sediment mixed with the lower samples of the sand there were indications of the approach to these deposits.

Though I have not seen the borings between 40 and 80 feet, I still think that the question whether these are modern, or belong to the Pleistocene, remains to be disposed of, and will require comparison of the lower samples, if they can be separated from the mud and sand introduced from above, with the overlying deposit. This may have already been attended to, but if so, the fact is not stated in the published abstract. With reference to such comparisons I would ask particular attention to the chemical character and depth of the specimens containing calcareous concretions, which are characteristic of the Isthmian rather than of the Nilotic formation.

Of course I do not affirm that the modern deposit of the Delta is in no place thicker than 40 feet, on the contrary, on my view of the history of the district, there must be old buried channels of the Nile in which it is much thicker, but it should be possible to recognise these by the character of the material filling them.

The softness of the Nile water and the minutely arenaceous character of the Nile mud, as well as the connection of this with its fertility, have been remarked from the most ancient times; and the microscopic details given by Prof. Judd have done much to give precision to our views on these points. With respect however to the causes and geological significance of these phenomena, the conclusions stated in the abstract seem open to serious objections, suggested by the physical features of the area drained by the Nile, and the conditions under which the fluviatile deposits are laid down. As this subject is of some importance both with reference to the geology of Egypt and general geology, I would ask your permission to refer to it in a second short communication. J. WILLIAM DAWSON

McGill College, Montreal, December 24, 1885

The Discovery of the Source of the Mississippi

IT is a matter of little importance or interest in what spot is located the ultimate spring of the longest branch of even the greatest river. Especially is this the case with the Mississippi, where it may easily be an open question which of a dozen branches is the longest, when traced through its innumerable lakes and windings. By common consent, however, a certain branch of the Mississippi has been assumed as the river proper, and its head as Lake Itasca, in northern central Minnesota. The river was explored to this point, and the lake discovered in 1832 by Schoolcraft, who published a map of the lake, and of the river from this point downwards. He spent but one night on the lake, and did not explore its tributaries. Four years later Nicollet led an expedition to the head waters of this stream, reached Lake Itasca, and spent several days in making a thorough exploration of the country about it. In his narrative, published in 1841, he gives a full description of the tributaries to the lake which constituted, according to general acceptation, the extreme head waters of the river. The report is accompanied by a map, on which the geographic features described in the narrative are delineated, and which agrees in general with later and more accurate maps.

During the half century which has passed since the time of these explorers, settlement has crowded upon this region, railroads have been built in close proximity to it, and the country has been explored in every direction in the interest of the lumber industry. Furthermore, in 1876, the surveys of the General Land Office were extended over it. Lines were run at intervals of a mile over the whole region, and every lake and pond of any importance was mapped by traverse survey. In short, the country has long since ceased to be a *terra incognita*.

It is therefore with astonishment, not unmixed with a feeling akin to disgust, that we read in the daily papers, in certain magazines, and finally in the *Journal* of the Royal Geographical Society, an account of the alleged "discovery" of the source of the Mississippi, made by a Capt. Glazier, in the summer of 1881. It appears from his narrative, published in great fulness of detail in the *American Meteorological Journal*, September to December, 1884, that his expedition started at St. Paul and pushed its way manfully by rail and stage to the Leech Lake Indian agency. After obtaining at this place a full complement of men and material (except provisions) for a life in the wilderness, they started westward for Lake Itasca. They fortunately escaped all the perils of the journey, and arrived there on the third day safely. Coasting along the shore of the lake, they found a stream coming in at the head of the south-west arm, up which they journeyed, some two hundred yards, when they entered a second lake, which Capt. Glazier claims to be the ultimate source of the Mississippi, and to which, probably in virtue of his heroic achievement in being paddled to it, he claims the right to give his own name. The failure of provisions prevented him from making any further exploration or discovery, and the expedition returned to settlements.

It appears from the explorer's description and from the extremely incorrect map which accompanies his narrative—made, as he naïvely informs the reader, from information furnished by his Indian guide—that his so-called Glazier Lake is identical with a lake in Township 143 north, Range 36 west, which had been carefully mapped by traverse survey by the General Land Office in 1876, or five years prior to his "discovery." This lake, or pond, has an area of about half a square mile. On the Land Office plat it is called Elk Lake, and its connection with Lake Itasca is plainly indicated. By a mere inspection of this plat Capt. Glazier might have made his discovery, and thus have avoided all the hardships and labours of his perilous journey. Since his claim to the discovery of this lake must be considered as altogether baseless, his desire that his name shall be for ever associated with it as the source of the Mississippi River is preposterous, especially as he cannot be ignorant of the above facts. HENRY GANNETT

Washington, D.C.

Chætoderma

YOUR biological readers will probably be interested to learn that I dredged a specimen of *Chætoderma* last August off the

south end of the Isle of Man from a depth of about 20 fathoms. It is about 1·5 cm. in length, and differs somewhat in shape from both *Chætoderma nitidulum*, Lovén, and the new species (*C. militare*, Selenka) found during the *Challenger* Expedition. The calcareous spicules are also different from those of both the previously described species, but they seem to vary considerably in shape. The specimen—along with the other Vermes obtained during the various dredging expeditions carried on last summer by the members of the Liverpool Marine Biology Committee—has been placed in the hands of Mr. R. J. Harvey Gibson, M.A., for detailed examination, and will be described in the First Report upon the Fauna of Liverpool Bay, to be published shortly.

W. A. HERDMAN

University College, Liverpool, December 30, 1885

A Solar Halo

AT about noon on this day a fine halo with its mock suns was well seen at the Radcliffe Observatory. Measurements of the vertical radii of the first circle gave 22° 24′, whilst the angular distance between the true and mock suns was 22° 30′. The radius of the second circle was rather difficult to determine, but the mean of several measures gave 46° 40′. The inverted arcs at the vertices of the two circles were clearly seen. The zenith distance of the sun's centre was nearly 75° at the time of the observation.

E. J. STONE,
Radcliffe Observer

Radcliffe Observatory, Oxford, December 30, 1885

Ventilation

MR. FLETCHER'S letter in your issue of December 17 (p. 153) illustrates the difficulties encountered by people who adopt patent ventilators and so-called systems of ventilation without considering the natural laws ruling the flow of currents of air.

The exit-shafts recommended by the writer of your article on the subject, as he himself confesses, may act as inlets, and generally do, if there is no other free inlet for air. This there seldom is in cold weather when the windows are closed, unless a hot-air grate on the Galton or other model is adopted. There is very little objection to running the exit-tube from the chandelier into the chimney flue, on the same principle as that of the chimney to each ventilator, now so much used.

I think the writer of your article hardly appreciates the difficulties to be encountered in ventilating an English house or assembly-room. Irrespective of the ignorance of the public generally on the subject, we are met by the fact that in most town houses it is very difficult to place a stove, with proper fresh-air inlet, in the entrance, where it may afford a supply of fresh warmed air to the house. As a rule the nearest flue is a very long way off. Again, fire-places being as a rule on inside walls in such houses, the flue to supply a hot-air grate (by far the best method of warming) has to be very long, and there is difficulty in arranging for its due cleansing.

Your correspondent speaks of expense being no object in the erection of public buildings. This is far from my experience. In the cases of churches, schools, and assembly-rooms, the question of ventilation is entirely bound up with that of heating, and in conversation with various makers of heating apparatus I have found their views quite unanimous on the peculiarities of building Committees on this subject. The lowest tender is as a rule accepted, and this never provides for ventilation. They are asked to heat only.

The real objection to ventilation in large rooms is the cost of the necessary heating apparatus. For instance, a large concert-room has recently been erected in this neighbourhood to seat 3800 persons, with a cubical content of 514,800 feet.

Now to warm this in the ordinary manner by hot-water pipes would require about 2600 feet of four-inch piping. But to supply a thousand feet of air per head, heated from 30° to 60° Fahr. would, according to the formula given in Hood's work, require no less than 10,600 feet, or more than four times the amount, while the space occupied by more than two miles of large piping would have to be taken into consideration.

No doubt the heating could be done more economically by steam coils or large stoves if care be taken not to over-heat the air.

Until ventilation is considered as necessary as drainage, and is paid for accordingly, and till failure on the part of architect and builder to secure it is visited with as severe penalties as failure in points of construction or design, I see no chance of improvement on the present state of chaos.

ERNEST H. JACOB

Leeds, December 22, 1885

Travellers' Snake-Stories

TRAVELLERS' "stories" are not expected to be quite matter-of-fact. One of the best of these jokes occurs in an article on "Travellers' Snake-Stories" in the December number of *Good Words*. Among the natural enemies of snakes the mongoose is thus described :—

"The mongoose, a bird known as the kingfisher of Australia, and secretary-bird of Africa, is well known in some of the West Indian Islands almost always to come off victorious in its encounters with the rattlesnake, and it has even been proposed to breed it specially for its extirpation."

From the use of the singular number in the above extract it is clear that only one animal is intended to be described, and that one is a *bird*. Next follows an interesting description in considerable detail (quoted from the *Standard* of January 22, 1883), of fights between the *Indian* mongoose and the Indian cobra in Lucknow, ending with the sentence :—

"He adds that these birds make affectionate pets," &c.

This is the best joke of all. It may be that the Australian kingfisher and African secretary-bird are locally called "mongoose" (this is not within the present writer's experience), but the *Indian* mongoose is a small animal, in shape very like a weasel or a ferret. It is impossible that the writer in the *Standard* (who is stated to have himself arranged the mongoose and cobra duels) could have described the mongoose as a *bird*. What does the man mean?

ALLAN CUNNINGHAM

Blackbird with White Feather

I NOTICE a letter from Mr. Murphy in your issue of December 24, 1885, about a blackbird with a white feather in its tail. Allow me to say that last month I saw a cock blackbird with a pure white tail; the rest of its plumage was natural. I saw it very distinctly, as it was flying away from me at the time, not more than ten yards off when I first noticed it, with its tail extended; I saw it again last week, within a few feet of the same place, this time running under a gate. My wife says she saw a similar bird, at the same spot, about a year ago.

THOMAS J. BUSK

Ford's Grove, Winchmore Hill, January 4

IT may interest your correspondent, Mr. J. J. Murphy, to know that for the last two years we have had a cock blackbird about our garden with a patch of pure white on each side of the head.

E. BROWN

Further Barton, Cirencester, January 3

DURING the frost of January 1880 I frequently noticed a hen blackbird with several white feathers on the head, breast, and back. It was quite tame, and came for food every day.

Hartford, Cheshire, December 30, 1885 E. K.

ON THE METHOD OF RECIPROCANTS AS CONTAINING AN EXHAUSTIVE THEORY OF THE SINGULARITIES OF CURVES[1]

IT is now two years and seven days since a message by the Atlantic cable containing the single word "elected" reached me in Baltimore informing me that I had been appointed Savilian Professor of Geometry in Oxford, so that for three weeks I was in the unique position of filling the post and drawing the pay of Professor of Mathematics in each of two Universities: one, the oldest and most renowned, the other—an infant Hercules—the most active and prolific in the world, and which realises what only existed as a dream in the mind of Bacon—the House of Solomon in the New Atlantis.

To Johns Hopkins, who endowed the latter, and in conjunction with it a great Hospital and Medical School, between which he divided a vast fortune accumulated

[1] Inaugural Lecture of Prof. Sylvester, F.R.S., delivered before the University of Oxford, December 12, 1885.

same black character, the lava broken in innumerable blocks, and setting out in vivid colour the verdure of the river banks."

A good deal of what has been said respecting the volcanic district of Itasy also holds good in regard to that of the Betafo valley and neighbourhood, where, however, the volcanic cones are fewer, and where trachytic domes do not appear to exist. One of the volcanoes in the Betafo valley, Iavoko, is of greater dimensions, and has a much larger crater than any to be found about Itasy. From this volcano a large sheet of basaltic lava has issued, upon which are to be found in abundance various species of plants, notably a Euphorbia and a stonecrop (Kitchingia). Almost all the plants growing on this lava-bed, however, are of a succulent character, and can dispense with soil, requiring merely a foothold. On the sides of Iavoko may be picked up fragments of calcined gneiss, which have been torn from the sides of the vent in the passage upward of the volcanic matter. On some of the cones numerous crystals of augite as large as marbles may be found among the volcanic debris. There is one volcano, Tritriva, near Betafo, which, inasmuch as it is different in character from any others mentioned above, deserves a few words. It is one of those volcanoes off which the summit has been blown by explosive action, leaving what is known as a crater-ring, which is now the site of a small lake. The lake is not more than 100 or 200 feet in diameter, perhaps not as much as that; but there is reason to suppose that it is of very great depth. The inner sides are steep for the greater part of the circumference, but on one side the lake is easily accessible.

It is possible that, when the country is more thoroughly explored, it may be found that the volcanoes near Itasy and those in the Betafo valley are connected by intermediate ones; indeed, on Dr. Mullens's map several craters are shown somewhat west of a straight line drawn between these two volcanic districts.

About 25 or 30 miles to the north-east of Antananarivo I discovered, a couple of years ago, several small volcanic craters. These also seem to belong to the class of crater-rings or explosion craters. Although fragments of volcanic matter have been ejected from them, they are not in such quantity as to form a cone; and the craters, none of which exceed 100 yards in diameter, and 30 feet in depth, have been formed probably by a single explosion of the pent-up forces below. With the exception of scoriæ and lapilli, which are sparingly scattered about, there is no visible sign of volcanoes, and one may come to the very verge of the craters before being aware of their existence. Two of the largest craters consist of saucer-shaped depressions, but are rather elliptical than circular in form; the others consist mostly of small cavities, deep in proportion to their width. Several of the craters are occupied by sheets of water, with rushes and other aquatic plants growing around their margin.

Besides the volcanic phenomena mentioned above, thermal springs occur in various localities in the interior of Madagascar. The following is an analysis by Dr. Parker of water from springs in the district of Antsirabe:—

"On evaporation, one pint (20 oz.) of water from each spring yielded the following quantities of solid salts:—

Spring No.	1	yielded	40	grs.	of salt,	or	2	grs.	to	1 oz. of water.
,,	,,	2	,,	38	,,	,,	1·9	,,	1	,, ,,
,,	,,	3	,,	42	,,	,,	2·1	,,	1	,, ,,
,,	,,	4	,,	28	,,	,,	1·4	,,	1	,, ,,

All these springs contain the same ingredients, viz. lime, magnesia, soda, and potash, in combination with chlorine, iodine, sulphuric acid, and carbonic acid, with the addition of free carbonic acid gas."

At Antsirabe there is a deposit from one of these springs of carbonate of lime, which is occasionally used for building purposes in the capital. Bubbles of carbonic acid may be seen rising from the surface of the deposit, and at one point, where there is a small spring, a mass of calc-sinter has been formed which, speaking from memory, is probably 12 feet high by 18 feet long.

In one of the valleys in the vicinity of the crater-rings of Ambohidratrimo, spoken of above, there is a deposit of siliceous sinter. It appears in one or two places, scarcely rising above the surface of the ground, in a valley of rice-fields, and has been deposited by springs which have long since ceased to flow. The sinter is exceedingly hard and compact, and is used by the natives for fire-flints. In some portions of it numerous fossils of a species of Equisetum are embedded. The longitudinal striæ leave no doubt as to the nature of the plant. The fistular stem has been filled in, and the vegetable substance entirely replaced, by silex. The stems of some of these fossil plants are quite half an inch in diameter. Now, the only Equisetum found in Central Madagascar at the present time is *E. ramosissimum*, but this never attains to such a thickness as the Equiseta in the sinter; so that the fossil species have become extinct since the springs which deposited the geyserite were in a state of activity.

So little is known respecting earthquake phenomena in Madagascar, no scientific observations ever having been instituted, that it is scarcely worth while to refer to the subject. However, it may be stated that scarcely a year passes without one or more shocks being experienced in Central Madagascar, though they are never severe or of long duration; and the destruction caused by these earth-waves in some parts of the world is entirely unknown here. The natives, I may say in passing, strangely imagine that earthquakes are caused by a whale (Trozona) turning on its back.

Extinct volcanoes and thermal springs exist also in other parts of the island, but so little is known about them that I can do no more than merely allude to their existence. R. BARON

Antananarivo, Madagascar, December 2, 1885

Coal-Dust and Explosions

THOSE who have given the labours and conclusions of workers antecedent to, and contemporaneous with, Mr. W. Galloway, on the subject of the part played by coal-dust in mine explosions, the careful consideration which these merit in common with the results and writings of that zealous exponent of the question, will hardly feel disposed to concur in his conclusion that, except by him, "the very simple, and yet all-important, element" to which he refers in his recent letter has been treated with neglect.

On the other hand, they will consider that when Mr. Galloway "goes the length of crediting coal-dust with the *rôle* of principal agent (in coal-mine explosions), and of relegating fire-damp to a secondary position," he altogether loses sight of some very obvious facts which forbid so sweeping a conclusion.

Any one who is led, by special interest in the subject, to study the forthcoming Report of the Royal Commission on Mine Accidents, will find that the important part which may be, and no doubt frequently is, taken by dust in coal-mine disasters is recognised to its full extent, and that, in a careful consideration of the accumulated knowledge on this subject, all due weight has been given to the experimental results arrived at by Mr. Galloway and others. FREDK. A. ABEL

March 3

Deposits of the Nile Delta

PERMIT me to say that Prof. Judd is in error in supposing that I intended to withdraw my statement that desert sand underlies the Nile alluvium at a very moderate depth. The general succession of the newer deposits of Lower Egypt, according to the information I have been able to obtain (and which I have endeavoured to state as plainly as possible) is as follows, in descending order: (1) Modern alluvium, varying from zero to about 40 feet, and of course more in old eroded channels. (2) Desert sand of the Post-Glacial continental period. (3) Pleistocene or Isthmian deposits, lacustrine, estuarine, or marine. The question is not whether this succession exists—that I am prepared to argue on other grounds—but whether it appears in any or all of the recent borings. It is scarcely necessary to say that such general succession admits of alternations at the junctions of beds, and of local absence of some of its members. On finding, however, that the recent borings had been stopped by quicksand at the depth of about 35 feet, and that this quicksand consisted of the rounded grains of desert sand, and was mixed with gray clay or marl, and concretions like those of the Isthmian formation, I naturally concluded that the succession above referred to was distinctly indicated. Prof. Judd now affirms, as I understand, that, in all the Delta borings, mud of "precisely similar mineral character" to that of the surface extends to the bottom. The evidence of this, as well as the promised consideration of the other points to which I have alluded, I am content to wait for till the report appears in full. J. WM. DAWSON

Montreal, February 18

On the Intelligence of Dogs

WHEN reading in NATURE of November 12, 1885, the abstract of Sir John Lubbock's paper "On the Intelligence of Dogs," I called to mind an incident of a little Blenheim spaniel which belongs to my mother.

The readers of NATURE may perhaps be a little tired of stories relating to the intelligence of the dog, especially when these are illustrations of the effects of training. My excuse for troubling you now is that the following incident seems to indicate a singular power of *reasoning*.

"Middy" was about nine months old when he was picked off the streets of Melbourne, and he had many traits of the "larrikins," as the human waifs there are called. He had been three months in our family, and we had almost begun to despair of breaking him in to civilised life.

One Sunday my sisters set off for Sunday-school, and were surprised, on nearing the church, to find "Middy" at their heels. He was told to "go home," and he was found at the house on their return. Nothing more was said on the subject, which was forgotten by the next Sunday. But when my sisters entered the school-room on that day, great was their amusement to see the little dog seated calmly as a scholar in one of the classes! He behaved quite quietly during the lessons, and then left with the children, and trotted home alone. To prevent constant repetitions of this behaviour, he had to be caught hours before school-time and shut up. He was very clever in evading capture—crept into hiding early in the day, and bolted when we were off guard. On these occasions he was certain to be found in his place at school.

It perhaps should be especially noted that "Middy" had never been to the church before, and that a whole week had elapsed between his first and second attempts.

MARY KNOTT

7, Kaga Yashiki, Tokio, Japan, January 20

Frost in Devonshire

THE Rev. A. D. Taylor, Rector of Church Stanton, a parish in Devonshire, some 900 feet above sea-level, writes me under date of the 22nd inst. :—

"We have had for three days the most wonderful rime. The trees have been covered, every twig and bud, with ice, on the average an inch at least in depth. I have measured several pieces, and have found them 1¼ to 1½ inches from base to edge. The whole place has been like fairy-land, or a silver country. To-day it has all fallen, with a continuous rushing and rattling on the bushes for four hours. The very leaves of the laurels were so frozen that you could take off each leaf a perfect *ice-leaf*—an exact reproduction in transparent ice, of about twice the thickness of this (ordinary letter) paper, of the laurel leaf—every vein and unevenness of edge distinct and clear. The children collected scores of them, and very lovely they looked. I have never seen anything of the sort which would compare with it. The people call it *rängling* (phonetic spelling), a queer word of which I never heard before."

Keen frost in an excessively moist air no doubt sufficiently explains the beautiful phenomenon itself; but can any Devonshire man explain the country people's word?

Bregner, Bournemouth, February 24 HENRY CECIL

"Pictorial Arts of Japan"

IN my review last week of Mr. Anderson's "Pictorial Arts of Japan" I inadvertently wrote the "eight Nirvanas" of Gautama instead of the "eight incidents (more properly 'features'—*ta siang*) of the Nirvana." F. V. DICKINS

University of London, Burlington Gardens, W., March 1

DISCOVERY OF A NEW ELEMENT BY CLEMENS WINKLER[1]

IN the summer of 1885 a rich silver ore was found at Himmelsfürst, near Freiberg; it was pronounced by A. Weisbach to be a new mineral, and was named *Argyrodite*. T. Richter examined its behaviour in the blow-pipe flame, and found that it consisted chiefly of sulphur and silver together with a little mercury, which latter element has never before been found at Freiberg.

[1] From the *Berichte* of the Berlin Chemical Society, No. 3.

The author has analysed the new mineral, and finds that the amount of mercury only amounts to 0·21 per cent., whilst silver is present to the extent of 73-75 per cent., and sulphur to the extent of 17-18 per cent. He also finds a very small quantity of iron, and traces of arsenic. However often and however carefully the analysis was conducted, a loss of 6-7 per cent. always remained unaccounted for. After a long and laborious search for the source of this error, Clemens Winkler has at length succeeded in establishing the presence of a new element in argyrodite. *Germanium* (symbol Ge), as the new element is called, closely resembles antimony in its properties, but can, however, be sharply distinguished from the latter. The presence of arsenic and antimony in the minerals accompanying argyrodite, and the absence of a method of sharply separating these elements from germanium, made the discovery of the new element extremely difficult.

The author, having a more detailed communication in view, confines himself to the following particulars :—

When argyrodite is heated out of contact with the air, which is best effected in a current of hydrogen, a black crystalline and moderately volatile sublimate forms, which melts to brownish-red drops, and which consists principally of germanium sulphide, together with a little mercury sulphide. Germanium sulphide dissolves readily in ammonium sulphide, and, on the addition of hydrochloric acid, is thrown down again in a pure state as a snow-white precipitate, which is immediately dissolved when treated with ammonia; the presence of arsenic or antimony colours the precipitate more or less yellow.

On heating germanium sulphide in a current of air, or on warming it with nitric acid, a white oxide is produced which is not volatile at a red heat and which is soluble in potash solution; when the alkaline solution is acidulated and submitted to the action of sulphuretted hydrogen, the characteristic white precipitate is produced.

The oxide is readily reduced by hydrogen, whilst the sulphide on account of its volatility is more difficult to reduce. The element, like arsenic, has a gray colour and moderate lustre, but is volatile only at a full red heat, and is decidedly less volatile than antimony. Its vapour condenses to small crystals recalling those of sublimed iodine; these show no tendency to melt and could not be confounded with antimony.

When germanium or its sulphide is heated in a current of chlorine it yields a white chloride which is more readily volatile than antimony chloride; its acidulated aqueous solution yields a white precipitate with sulphuretted hydrogen.

The author intends to undertake the determination of the atomic weight of germanium, even if it can be decided only approximately, as this will show whether the new element is to occupy the vacant position in the periodic system between antimony and bismuth.

THE STORY OF BIELA'S COMET[1]

II.

BRANDES, one of the two German students spoken of, was riding in an open post-waggon on the night of Dec. 6, 1798, and saw and counted hundreds of these shooting-stars or meteors. At times they came as fast as six or seven a minute. These meteors which Brandes saw that night we know now were bits from Biela's comet. In November 1833 occurred the famous star-shower, which some of you saw. The facts of that shower gave to two New Haven men, Profs. Twining and Olmsted, the clue to the true theory of the shooting-stars. From that date

[1] A Lecture delivered by Prof. H. A. Newton, on March 9, 1874, at the Sheffield Scientific School of Yale College, U.S. From the *American Journal of Science*. Continued from p. 395.

abundant, while plagioclase felspar is comparatively rare. With the rounded grains of quartz and felspar, a few examples of hornblende and other minerals, including jade, also occur.

But far greater is the number of mineral species represented in the smaller subangular and angular sand-grains. In addition to the minerals already mentioned, I have recognised several varieties of mica, augite, enstatite, tourmaline, sphene, dichroite (cordierite), zircon, fluorspar, and magnetite.

The *mud* is a much more difficult material to study the mineral characters of than the sand, owing to the extreme minuteness of its particles. It is a ver striking fact, however, that kaolin, which constitutes the predominant ingredient of clays, appears to be almost absent from these Nile-muds. Chips and flakes of quartz, felspar, mica, hornblende, and other minerals, can be readily recognised, and it is often evident that the unaltered particles of such minerals make up the greater part, if not the whole mass, of the fine-grained deposits. The mineral particles are, of course, mingled with a larger or smaller proportion of organic particles. Frustules of *Diatomaceæ* occur in these muds, as was pointed out by Ehrenberg, but unless special precautions were observed in collecting the samples it would be unsafe to draw any deductions from their presence.

The striking peculiarities of these sands and muds of the Nile-Valley appear to be capable of a simple explanation. In countries where rain falls and vegetation abounds, water charged with carbonic acid is constantly tending to break up the compound silicates; the silicates of the alkalies and the alkaline earths being decomposed and their constituents removed in solution, while the silicate of alumin becomes hydrated, and is carried away in suspension by water in the form of kaolin. In this way, the felspars and nearly all other compound silicates are affected to such an extent that in most granitic and metamorphic rocks they show evidence of extensive "kaolinization," while the clays derived from them are made up for the most part of crystalline plates of kaolin. But in a rainless country, like Northern Africa, none of these agencies will operate, and the disintegration of the solid rocks is effected by mechanical means; the most potent of these mechanical agents are the heat of the sun, causing the unequal expansion of the minerals which build up the rocks, and the force of the wind, producing constant attrition of the disjoined particles.

This being the case, it will be readily understood that the coarser sand-grains will include felspar and other minerals in a nearly unaltered condition, while in countries where the chemical agents of the atmosphere come into play, such particles would be more or less completely converted into kaolin. In the same way the mud, instead of consisting of scales of kaolin originating from chemical action, will be formed of particles of the chemically unaltered minerals reduced to the finest dust by purely mechanical agencies.

The chemical analyses which have been made of these Nile muds entirely support these conclusions. Instead of containing a considerable proportion of combined water, as do all the ordinary clays, their composition is that of a mixture of anhydrous minerals.

But there is fortunately a kind of evidence, derived from chemical analysis which is of the greatest value from its bearing on the questions we are now discussing—that, namely, which is derived from a study of the composition of the Nile-waters.

It must be remembered that the Nile is a river of a very peculiar and exceptional character. The last tributary which it receives is the Atbara, which falls into it in lat. 17° 38′ N.; from that point to its mouth, in 31° 25′ N. lat., the river does not receive a single affluent; for a distance of 1400 miles it acquires no fresh supply of water except what is brought to it by superficial torrents after heavy rains in Lower Egypt. It has been clearly demonstrated that, after receiving the Atbara, the Nile undergoes a continual diminution in volume in its course through Egypt. This is no doubt in part due to percolation of the water through the delta-deposits, and in part to the water being drawn off in canals for purposes of irrigation; but a large part of this diminution in volume must certainly be ascribed to the great evaporation which must be going on from the surface of the river during the last 1400 miles of its course.

Although we shall not be able to calculate the exact loss of the Nile by evaporation in the course of 1400 miles through one of the hottest and driest regions of the globe, yet we cannot doubt that this loss is enormous. Now the effect of this constant evaporation must be to concentrate the saline matters held in solution, and we might therefore anticipate that the waters of the Nile in Lower Egypt would contain an exceptionally high percentage of saline matters in solution.

But what are the actual facts of the case?

According to the analyses of Dr. C. Meymott Tidy, the Nile contains only a little more than one-half of the proportion of soluble materials which exists in the Thames, the Lea, the Severn, or the Shannon!

A little consideration will show, however, that this startling and seemingly anomalous result is capable of simple and easy explanation. The substances dissolved in the water of rivers is of course derived from the materials composing the rocks of the river-basin, through the action of water holding carbonic acid or other acids in solution.

Hence we are led by the study of the composition of the Nile water to the same conclusion as was reached by the study of microscopical characters of the muds and sands of the delta, that while in the rainy districts of the temperate zones the disintegration of rocks is mainly effected by chemical agencies, in the rainless areas of the tropics the same work is almost exclusively effected by mechanical forces.

The products of these two kinds of action are, however, essentially different. In the former case we have formed crystals of kaolin, which constitute the basis of all the true clays, a large quantity of lime, magnesia, iron, soda, and potash salts with silica passing into solution; while, in the latter case, the several minerals of the rock are simply reduced to fragments of varying size and form, and but little matter passes into solution.

The whole of the observations described in the present report are in entire harmony with this explanation. The comparatively unaltered condition of the felspars and other complex silicates in the sands; the absence of kaolin from the muds, and the presence of the chips and flakes of the unattacked minerals in the muds; and finally the small quantity of dissolved matter in the Nile-water, in spite of the enormous concentration it must have undergone by evaporation—all point to this same conclusion.

In the estimates which have been made of the rate of subaërial denudation in different parts of the globe, it has usually been assumed that this action is similar to what is seen taking place in our own country and in North America. But the observations detailed in this report prove that in rainless tropical districts, where little or no vegetation exists, the disintegration of rocks, though not, perhaps, less rapid than in temperate climes, is different alike in its causes and in its products.

It has often been pointed out by chemical geologists that metamorphic action could not have produced many of the schists from sedimentary rocks, for the former are rich in potash, soda, and other materials which have been dissolved out from the latter during the disintegration of the rock-masses from which they were derived. The recognition of a kind of action whereby great masses of sedimentary materials can be produced, rich in those substances which are usually removed in a state of solution, is not destitute of interest at the present time, when the question of the origin of the crystalline schists is one that presses for solution.

PARIS

Academy of Sciences, November 30.—M. Jurien de la Gravière, Vice-President, in the chair.—The Vice-President announced the death of the President, M. Henri Bouley, who died on the morning of the same day. The speaker referred in warm terms to the career of M. Bouley, his entire devotion to science, and the courage with which, although suffering from a fatal disease, he continued to the last to fulfil the duties of his office.—Obituary notices of M. Bouley: by M. Hervé Mangon, in the name of the Academy of Sciences; by M. A. Milne-Edwards, in the name of the Natural History Museum; by M. A. de Quatrefages, as Vice-President of the Acclimatisation Society; and by M. Fremy, Member of the Academy.—As a mark of respect for its late President, the public meeting of the Academy was immediately adjourned.

BERLIN

Physiological Society, October 30.—Prof. Zuntz spoke on the apnœa of the fœtus and the cause of the first respiration, setting forth the present state of the question, and then passing to consider the assertion of Prof. Preyer, who, by experiments on rabbits and guinea-pigs, sought to prove that it was not the change in the gas of the blood which was the cause of the first respiration, but a stimulus exercised

on the integument. Prof. Zuntz had quite recently, in conjunction with Dr. Cohnstein, made observations on a new-born lamb that, connected by the umbilical cord with the ewe, came into the world completely apnœic, and, notwithstanding that the most varied stimulations were exercised on the skin, continued apnœic for ten minutes long, though in all other respects these stimulations were normally responded to. Not till the placenta had detached itself did the respiration begin. This observation proved with all certainty that apnœa was dependent on the sufficient supply of oxygen, and that the first respiration was induced by a deficit of oxygen. They therefore repeated the experiments of Prof. Preyer, and came to the conclusion that under them the circulation of the blood always suffered disturbance in consequence of the pressure exerted, whereby the supply of oxygen to the fœtus was impaired, and that the fact which Prof. Preyer adduced in support of the accuracy of his view, namely, that the blood of the umbilical vein always appeared of a bright scarlet red, served exactly to disprove it. The brighter blood of the umbilical vein was, accordingly, an argument of a disturbance in the circulation of the blood, in consequence of which less arterial blood reached the fœtus, and, notwithstanding its greater saturation of oxygen, the blood was, therefore, unable, on account of its deficient quantity, to convey the requisite amount of oxygen to the whole blood. The respiratory centre in the brain thus got supplied with blood poorer in oxygen, and when a stimulation of the skin was superadded the first respiration ensued. In the case of the less excitable brain of the fœtus it was necessary that the outward stimulation should supplement the deficiency of oxygen. In the case of the normally born, however, the detachment from the placenta and the absolute want of fresh oxygen sufficed to stimulate the respiratory centre to activity. In the case of the adult, finally, with excitable brain, a slight reduction of oxygen was itself sufficient to excite respiration.—Referring to the beautiful discovery by Mr. Haycroft, of the fact that the ferment of the saliva in the leech prevented coagulation, Prof. Zuntz recommended the use of this ferment of the leech in measurements of blood-pressure, with a view to avoiding coagulation. This substance had the advantage over all other preventives of coagulation, that in no respect had it any toxic effect. Into the tube conjoining the artery of the animal examined with the manometer of the kymographion a T-tube was intercalated, and by its means a cubic centimetre of the ferment of the leech was squirted per hour into the separate fluid. This was sufficient for the marking of curves of blood-pressure for seven hours consecutively, without the least trace of coagulation.—In view of the divergence of opinions prevailing regarding the alimentary value of the peptones—some maintaining that peptone was used as an alimentary deposit in the body, while others considered that only the albumen absorbed as such was capable of being deposited, the peptones getting, on the contrary, further decomposed—Prof. Zuntz had a number of feeding experiments instituted with peptones. A somewhat long series of experiments was executed on a little dog, first with meat, then with peptone furnished from fibrine, next with albumose substances or propeptones, and, further, with lime. The experiment was arranged in such wise that the dog, along with equal quantities of fat and starch, received daily the like amount of nitrogen. The quantity of secreted nitrogen was daily determined, and thereby the deposit of nitrogen ascertained. The dog first got meat for some days, then peptones for some days, next thereafter meat again, and, following thereon, albuminose substances; this in turn was succeeded by meat days again, then lime days, and, finally, meat days anew. The deposit of nitrogen was now found to amount to—(1) with meat diet, 0·502 grammes nitrogen daily; (2) with peptone, 0·584 grammes; (3) with meat, 0·513 grammes; (4) with propeptone, 0·70 grammes; (5) with meat, 0·46 grammes; (6) with lime, − 0·5 grammes; (7) with meat, 0·48 grammes nitrogen. Meat feeding, accordingly, yielded about the same quantity of nitrogen deposit on each occasion of its being used; in the case of feeding with peptone and propeptone the nitrogen deposit was somewhat greater than in the case of meat-feeding, a result explained by the fact that all the nitrogen of meat did not belong to the albumen, but in part appertained to the nitrogenous bases, which could yield no nitrogen deposit. In the case of lime-feeding, on the other hand, a loss of nitrogen for the body was the result. Prof. Zuntz next had a further series of feeding-experiments performed with the peptones occurring in trade. The dog in question received only fat in addition to the nitrogenous nutriment. In the first days, with meat-feeding, a deposit of nitrogen, to the amount of 0·2 grammes daily, the result; under feeding with Kämmerich's peptone follow thereon, the daily deposit of nitrogen was − 0·4 grammes; th meat days, next succeeding, again yielded 0·2 grammes nitrogen in deposit, while the feeding, thereafter ensuing, with Koch's peptone again showed − 0·4 grammes nitrogen in deposit. The series was closed by meat-feeding, which produced 0·3 grammes deposit of nitrogen. The marketable peptones were therefore, notwithstanding the like supply of nitrogen, incapable of producing a deposit of albumen; on the contrary there rather occurred a loss of corporeal albumen, not so great, however, as when the like quantity of nitrogen was partaken in the form of lime. A series of experiments was finally carried out with the marketable peptones on a dog which for a considerable length of time had been fed only with rice and fat, and had thereby been very much reduced in strength. In this case the first day of feeding with Kämmerich's peptone produced a deposit of nitrogen to the amount of 0·6 grammes; in the following days this deposit was less; and soon the nitrogen showed itself at equilibrium. Under feeding with Koch's peptone, too, the animal, which was very much reduced, was maintained at equilibrium in respect of nitrogen.—Dr. Weyl communicated the results of his further investigation into the constitution of the derivatives obtained from cholestearine, which, at a meeting of the Society before the vacation, he had declared to be terpenes. He endeavoured to determine the molecular weight of those carbo-hydrates which, according to the nature of terpenes, had the composition $(C_5H_8)^n$. The vapour density, determined according to the method of Victor Meyer, showed itself in the lead bath not normal. It corresponded with the composition C_5H_8, thus indicating decidedly that a dissociation had set in during the process of heating. Other terpenes also, such as turpentine oil and camphor, yielded results which were not normal and showed a dissociation into the radical C_5H_8, a circumstance which likewise argued the terpene nature of cholestearine. Dr. Weyl was able, finally, to demonstrate the connection of cholestearine with the terpenes by showing that the latter very beautifully produced the well-known cholestearine reaction. Further experiments with a view to determining the vapour density in a vacuum would perhaps yield the molecular weight of these interesting carbohydrates.

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express a decided view on the subject (such as Mr. Burt, Sir W. Elliot, and the late Mr. Knowles), that blasting should be prohibited, at any rate in fiery mines. It was admitted that the cost of working coal would be much increased by the enforcement of the suggested prohibition, and the majority of competent witnesses examined afterwards by the Royal Commission maintained that the abolition of shot-firing in coal-getting must be attended by very formidable difficulties, and must, in fact, cause the closing of many pits.

I have shown that even the comparatively very small amount of fire-damp which may, at any rate occasionally, pervade the air in portions of mine-workings where thorough ventilation is most effectually provided for, and may escape detection, suffices to determine the production of a disastrous explosion, if, under these circumstances, a blown-out shot occurs where an accumulation of dust exists; and that it is even possible, in the complete absence of fire-damp, for a blown-out shot to give rise to an explosion in a very dusty working or mine, where the coal is of a specially inflammable and sensitive character. Such being the case, the fact cannot be ignored that last year's decision of the late Home Secretary—which raised consternation in many mining districts—to prohibit the firing of shots in any colliery within a period of three months after the existence of gas had been there reported (while the workmen were in any part of the mine), is far from affording the contemplated protection against disaster resulting from the use of explosives *in the ordinary manner*.

This most grave aspect of the question has received the anxious attention of the Commissioners, who would not have considered themselves justified in relinquishing their work until they had practically investigated, as far as in their power, any measure or suggestion appearing to afford promise of aid in furnishing definite replies to the following important questions:—

(*a*) Whether sufficiently efficient substitutes for explosives exist to warrant the assertion that their abolition need not interfere very materially with the reasonably profitable working of collieries;

(*b*) Whether, therefore, it is practicable to limit their use strictly to localities where the absence of every possible risk of explosion can be demonstrated; or

(*c*) Whether any modifications in the ordinary method of using explosives in mines can be so confidently relied upon to guard against, or overcome, certain dangers attendant upon blasting operations in collieries, that it may be practicable to clearly define and lay down certain conditions which will insure the safe use of explosives, either generally, or in all but special cases, which can be precisely defined.

As regards the first question:—The power and efficiency of recently improved mechanical appliances for bringing down coal or for driving headings or crossways, warrant the sanguine expectation that compressed air and even manual power may be, at no distant day, brought to bear so advantageously in mines where fire-damp occurs, as to render it no great hardship to dispense with the use of explosives in some of the work where at present they are considered indispensable.

The considerable and very rapid increase in volume which freshly-burned quicklime sustains when slaked, led, many years ago, to attempts to apply it to the bringing down of coal; but the idea did not assume a really practical form until Messrs. Sebastian Smith and Moore worked out a simple method of applying the lime so as to insure the effective operation of the disruptive force which it is capable of exerting, and to utilise the considerable heat, developed by the union of the lime with water, in the rapid generation and super-heating of steam in somewhat considerable quantity, thus supplementing, in an important manner, the force exerted by the expansion of the lime. The public has been made familiar, in last year's and this year's Exhibitions, with the general nature of Messrs. Smith and Moore's lime cartridges. The Commissioners witnessed their performances at Shipley Collieries soon after their successful elaboration, and the results of subsequent inquiries and experiments have convinced them that, for coal-getting, the lime process can be, to a large extent, substituted for powder, and that its employment, while securing comparative immunity from danger, is unattended by any important practical difficulties.

It has received extensive trial in many of our mining districts, and also on the Continent, and has already taken firm root in some parts of Staffordshire, Yorkshire, and Derbyshire. Its elaborators do not contend that it affords the means of dispensing with the use of explosives, or of specially powerful mechanical appliances, in the removal of stone, or even in some very hard coal; but it is certain that in many collieries, where the prevalence of fire-damp renders the use of the safety lamp imperative, the replacement of shot-firing by lime-cartridges, while unattended by any increase in the cost of getting the coal, would reduce the risk of explosions to those arising from carelessness, or from what should now become the very remote contingency of the use of unsafe or defective lamps.

The idea has been entertained that, by surrounding or covering the charge of powder in a shot-hole with some material which evolves vapour of water, or carbonic acid, when exposed to sufficient heat, these would be liberated by the firing of the shot in sufficient quantity and with sufficient rapidity to extinguish flame and sparks projected by it; but the authors of such suggestions have failed to realise the fact that the exposure of these substances to heat on the firing of a shot would be almost instantaneous, and would therefore leave, at any rate, the greater proportion practically unaltered.

It was suggested by me to the Commissioners that possibly the sudden liberation of carbonic acid, confined in the liquefied state, and placed either over or under the charge in a shot-hole, might prove effective in extinguishing flame and sparks, and a number of experiments have been made in this direction, with considerable, though not complete, success.

Dr. McNab was the first to put into practical execution the idea of using water tamping, in the form of a long cylinder filled with the liquid and placed over the powder charge; with the twofold object of extinguishing the projected flame and sparks, and of diminishing, by dispersion of the water in the immediate vicinity of the shot, the persistence of the powder smoke, which is a source of much inconvenience and loss of time. While it has been demonstrated that decided economy in time does result from the more rapid clearing of the air from smoke when the water tamping is used, many careful experiments conducted for the Commission have shown that no reliance could be placed upon the extinguishing power of water, applied in the way originally suggested by Dr. McNab.

In 1879 I suggested to the Commission a plan by which possibly the more violent explosives, of the dynamite class, might be safely and efficiently applied to the getting of coal, based upon the principle of distributing the force developed by the detonation of small charges over a considerable area through the agency of a column of water, within which the detonated charge was confined. This principle, which has since received important applications in connection with military service, appeared applicable to effect a modification of the shattering action, which renders the violent explosives inapplicable to coal-getting, when used in the ordinary manner, their effects being thus assimilated to those of powder, while the sparks and highly-heated gases projected by a blown-out shot might be effectually quenched by the water which would envelope them at the instant of their projection.

Experiments carried out at Wigan in 1880, demonstrated that the coal brought down by small charges of dynamite inclosed in water compared very favourably with the best results furnished by full powder charges, and these results have been fully confirmed by trials since carried out for the Commission in South Wales. Absolute immunity from danger of the ignition of an explosive gas-mixture by a blown-out shot of dynamite or similar explosive agent was not found to be secured by this system of blasting; such was the case, however, then the blown-out shot was projected into air containing fire-damp in proportions approaching that of an explosive mixture, and in which a very inflammable coal-dust was thickly suspended. It has also been found, in the Commissioners' experiments, that the superposition of the water-tamping, according to Dr. McNab's original plan, over a dynamite charge, appears to afford security against the ignition of a dust-laden mixture of air with a somewhat considerable proportion of gas.

Even while actively engaged in the completion of their Report, the Commissioners are still pursuing this subject experimentally, with the desire of furnishing, as far as in their power, decisive and thoroughly reliable data regarding the amount of security which appears to be afforded, by these methods of working, against the most prominent and prevalent sources of danger in connection with the use of explosives in coal mines; and—while I have been engaged upon this address—a still more simple method of applying water to counteract the dangers arising from blown-out shots has suggested itself to Mr. Galloway,—preliminary experiments with which have furnished most important results.

I have now attempted to give you an outline of the progress made within the last few years towards a thorough comprehension of the nature and causes of those dangers which most prominently direct public attention to the perils of the miner's calling—and of the advances already made, and rapidly progressing, towards the provision of the miner with really safe and efficient underground illumination, with efficient substitutes for explosives for a large proportion of the work connected with coal mining, and with safe methods of using explosive agents where these cannot be dispensed with; so safe that the terrors which have attended blasting in mines may be confidently expected speedily to fade away. I venture to think it will have demonstrated that we have made most satisfactory and important progress in all of these several directions, thanks to the labours of professional associations, of scientific and practical experts, and, I think I may also say, thanks to the exertions of the Royal Commission on Accidents in Mines.

I have been led to refer more fully than I had first intended to the work performed by the Royal Commission—the results of which, in detail, will shortly be in the hands of the public—because I felt sure that the members of the Society of Arts would take a most lively and sympathetic interest in the labours of men, who have not allowed themselves to be discouraged by unjust attacks and ignorant criticism, from endeavouring to carry to a useful termination the arduous work which they cheerfully took upon themselves.

The Commissioners have been silent while hard things have been said of them; but it were idle to deny that they have acutely felt the injustice reflected upon them by some writers in the public Press who, while posing as judges or philanthropists, have not earned for themselves, by knowledge acquired, or by work performed, the right to criticism.

Thirty years' personal experience of the work of experimental Committees has taught me that *ad interim* reports are not unfrequently worse than valueless, and this would certainly have been the case had the Commissioners attempted to make any so-called progress reports, because conclusions, or suggestions, might have been put forward in them which would have had to be afterwards recalled, or incomplete data given, which might have been misleading, and, therefore, even dangerous.

As regards the question of the unsafe nature of certain so-called safety lamps, however, I have pointed out that the Commissioners, just five years ago, reported to the Home Secretary in no hesitating terms, in the belief that their statements would have been published,—and it is no fault of theirs that the public was not informed of their strongly-expressed conclusions on this subject, but has been, on the contrary, recently told in the *Times* by a well-known mining engineer that the results of the Royal Commission's labours "have not even extended to the official condemnation of the known unsafe lamps."

The daily journals have at any rate chronicled the activity of the Commission by recording the dates and *locale* of their frequent meetings,—and have been cognisant, therefore, of the fact that their place of work was easily accessible. This being so, it is somewhat matter for surprise that the writer of very condemnatory paragraphs in an editorial article, suggested by correspondence published in the *Times* last June, should not have cared, in the first instance, to inform himself, however imperfectly, of the kind of work upon which the Commission was engaged, and to take that opportunity of seeking some little correct information on the subjects with which his graphic pen was directed to deal. Had he done so, he would scarcely have instructed the public that "a huge majority of colliery accidents arise from explosions;" that "coal mines generate an explosive gas, which, when collected in a quantity, and exposed to a flame, ignites, and blows into fragments the workings in which the vapour and flame meet;" "that every coal mine has its explosive gas," or that "often the miner has opened the door of his lamp to light up the cavern, already perhaps darkening with the heaviness of a gas-laden atmosphere." I will do him the justice to believe that he would not have felt disposed, after even very brief inquiry, to indorse as "not exaggerated" the declaration of the "strenuous and benevolent correspondent," Mr. Ellis Lever, "that the delay in the issue of the Commission's Report was "to the eternal discredit of Royal Commissions."

After all, however, it rests entirely with the public Press to decide for itself whether the ends it has in view are such as to render it desirable to seek for correct information before administering public condemnation.

But, with a public official, especially when connected with the very Department of State most directly concerned in the work of the Commission, the case is very different; and it is scarcely to be credited that the gentleman intrusted with reporting to the Home Secretary upon the circumstances attending the explosion last summer, at Clifton Hall Colliery, should not have thought it worth his while to ascertain, by inquiry, which could not but have been of immediate service to him, whether the delay in the completion of the Commissioners' Report was "unaccountable."

To this Society, which has always distinguished itself by its encouragement of earnest workers, and by its just judgment of their labours, I have ventured, as one of its members, to make these comments, which could not be uttered by me in my capacity as a member of Her Majesty's Commission, whose duty it is simply to report the results of their labours when they have, to the best of their judgment, fulfilled the duties imposed upon them.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 19.—Abstract of "Report on a Series of Specimens of the Deposits of the Nile Delta, obtained by the recent Boring Operations." By J. W. Judd, F.R.S., Professor of Geology in the Normal School of Science and Royal School of Mines. Communicated by order of the Delta Committee.

Neither of the borings made for the Royal Society, under the superintendence of the engineers attached to the army of occupation in Egypt, appears to have reached the rocky floor of the Nile-Valley, nor do the samples examined show any indication of an approach to such floor. What were at first supposed to be pebbles in one of the samples from Tantah, prove on examination to be calcareous concretions ("race," or "kunkur"). Nevertheless, these borings appear to have reached a greater depth than all previous ones in the same district with one or two exceptions. The deepest of the three borings now reported upon have been carried to 73 and 84 feet respectively.

The samples from these borings, like those examined by Mr. Horner, show that the delta-deposits all consist of admixtures, in various proportions, of blown-sand and alluvial-mud. I can find no evidence to support the suggestion made by Sir J. W. Dawson, F.R.S., from a hasty examination of the specimens, that "at a depth of 30 or 40 feet the alluvial mud rests on desert sand;" on the contrary these borings, like those of older date, show that the deposits of the Nile Valley consist of a succession of different beds in some of which sand, and in others mud, forms the predominant constituent.

The *sands*, when separated from the mud by washing, are found to be made up of two kinds of grains, the larger being perfectly rounded and polished, while the smaller, on the contrary, are often subangular or angular.

The larger and well-rounded grains may be described as microscopic pebbles; their surfaces are most exquisitely smoothed and polished, and their forms are either globular or ellipsoidal. In size they vary greatly, being occasionally as large as a small pea. They only very occasionally exhibit traces of deposits of iron-oxides upon their surfaces.

Embedding these grains in Canada balsam, and examining them by transmitted light, with the aid of the polariscope, we are in nearly all cases enabled to determine their mineral characters. The majority of the grains consist of colourless quartz, though occasionally rose-quartz, amethystine quartz, citrine, and smoky quartz also occur. This quartz exhibits unmistakable evidence of having been derived from granitic rocks; it is constantly seen to be traversed by bands of liquid- and gas-cavities, and very frequently contains numerous black hair-like inclusions (rutile?). Much more rarely we detect grains of quartz which consist of aggregates of small crystals, and are evidently derived from metamorphic rocks. With the pure quartz grains we find also a considerable number of rounded particles of red and brown jasper and of black Lydian stone, with fragments of silicified wood.

But in addition to the different varieties of quartz, particles of felspar are found in considerable abundance among these large rounded grains. What is very remarkable about these felspar-grains is the slight traces of kaolinisation which they exhibit; they are, in fact, almost as fresh and unaltered as the grains of quartz themselves. Ordinary orthoclase and microline are

[Dec. 10, 1…

…st directly concerned in the work … very different; and it is scarcely … man intrusted with reporting to … he circumstances attending the … on Hall Colliery, should not have … certain, by inquiry, which could … te service to him, whether the … ne Commissioners' Report was … ways distinguished itself by … s, and by its just by its…

I have been favoured by my friend Dr Schweinfurth of Cairo with a specimen of the filtrate of Nile water collected I presume at Cairo and from the river near the bank. It is a hard almost stony mass very much finer in texture than the ordinary Nile mud and a little lighter in colour. It is very slowly softened and diffused in water and when so diffused in proper quantity gives it precisely the appearance of the ordinary Nile water. A part of it settles rapidly and on microscopic examination is found to resemble the ordinary Nile mud in presenting unweathered particles of crystalline minerals but of extreme fineness. The remainder continues in suspension for a long time and traces of turbidity remain in the water even after it has been kept still for 36 hours. The fine sediment held in suspension after some hours is found to be an impalpable powder, aggregating in flocculi and possibly in definite forms under the

microscope. It has ~~yet~~ not yet been analysed but I have no doubt from its characters that it consists largely of kaolinic ~~soils~~ fine debris of rocks, perhaps with a small amount of organic matter. When dried on filtering paper it appears as a smooth yellowish crust and ~~[illegible]~~ curls up at ~~the~~ edges when ~~dry~~.
The large volume of this impalpable matter washed out of the Nile bed and finally carried off to sea in any proportion accounts for the comparative absence of the finer debris of crystalline rocks in ~~the~~ latter. Yet even in the very fine silt which takes longest to settle, the microscope shows some remains of undecomposed silicates.

It is to be observed however that as stated in the text, this material is all derived not from the rainless district of the Nile Valley, which affords only wind-blown desert sand but from the well-watered mountains of Abyssinia

Schwerpunkte Speeches
& other Extracts

~~Gebel Aspert~~

~~Mrs Gebel~~
~~Mersal~~

Cut of Nile above Thebes

Between Erment and Esneh not far above Thebes the Nile has cut ~~thro'~~ a ridge of limestone ~~only~~ probably nummulitic. It seems to have changed its channel from the West to the East of a mass of rock crowned with a dome a tomb or Sheikh's tomb ([illegible]) Accdg to Maspero up to Roman period ran on both sides of island opposite since that time filled up west channel and now runs on East side only — (See Guide book about this)

Maspero letter in Acady / Aug/85

Murphy on
New Nile Cutting