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

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 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 sub-aërial 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, was the result; under feeding with Kämmerich's peptone following thereon, the daily deposit of nitrogen was – 0·4 grammes; the 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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