upwards of half a mile, but the actual length of wire which was submerged, and through which the current passed, was upwards of a mile. The spark was about ¼ inch in length, bluish white in colour, and very striking and interesting in effect. It was placed in the focus of a holophotalised parabolic reflector.

At the same time in order to ascertain if by means of a single battery under the charge of one keeper a succession of flashes could be produced and a string of isolated dangers illuminated, the light was sub-divided first into two separate flashes and afterwards into six different sparks. The separate lights were quite satisfactory, though they were not as might have been expected of the same power as the original single one. But as the separate sparks were very close to each other this cannot, I presume, be held to be a proper sub-division of the light.

In 1867, at the British Association at Dundee, I suggested that the "effect of the light might be also increased without using additional cells if the same current could be again utilised so as to generate a second spark in the same focus. This was proposed to be done by 'using additional coils' for the same focus or separate sparks in the foci of separate reflectors.[1] I also added that the "time is perhaps not far distant when the beacons and buoys in such a navigation as the entrance to Liverpool may be lit up by submarine conduction from a central station on either shore, while the whole management may be trusted to the charge of one or two light-keepers." [2]

I may add that similar trials were made with Wilde's electromagnetic machine, which gave a light of much greater volume and power. The electrodes employed in all these experiments were made of platinum, but several other metals were experimented with, and of all that were tried bismuth was found to give the brightest light.[3]

A committee of the Scottish Society of Arts, consisting of Dr. Ferguson, convener, Dr. Lees, and the late Dr. Strethill Wright reported in the following terms:—"The peculiar character of the light, which is flickering, though continuous, is well marked and would be easily understood. So far as Mr. Stevenson's experiments go, they seem to prove the practicability of his proposal, and your committee do not anticipate any serious obstacle to its realisation."

THOMAS STEVENSON

Edinburgh

[1] British Association Reports, 1867.

[2] *Ibid.*

[3] "Proposals for the Illumination of Beacons and Buoys," pp. 14-15. (Edinburgh: A. and C. Black.)

UNDERGROUND TEMPERATURE[4]

DR. STAPFF has continued his observations of the temperature in the St. Gothard Tunnel, and has contributed to the Swiss Natural History Society a paper[5] of fifty-six quarto pages, embodying the results.

The following is his description (pp. 26, 27), of the mode of observing the temperature of the rocks in the tunnel:—

"The exact determination of the temperature of the rocks in the tunnel formerly occasioned a notable expenditure of time and money. At first thermometers about a metre long (made by J. Goldschmid, of Zurich) were employed for this purpose; their tubes being cemented into a wooden cylinder, so that only the bulb (surrounded by a perforated steel cap) projected below, and the scale (extending from 15° to 30° C.) above. Tallow was poured round the wooden cylinder, and the whole thermometer was then thrust into a bore-hole a metre deep, so that only the scale projected, from which readings were taken from time to time until the temperature became constant. The final reading had to be corrected not only for rise of zero but also for the temperature of the quicksilver in the thermometer tube which extends from the opening to the bottom of the bore-hole. Another very notable correction was required for the more or less oblique position of the thermometer; for the hydrostatic pressure of the quicksilver presses out the glass bulb so far that without change of temperature the long thermometer reads from 0°·4 to 1°·0 less in the vertical than in the horizontal position.

"After about from three to ten days, the reading of a thermometer luted into a bore-hole ceased to alter.

"Separate trials with thermometers of similar construction, but different length, showed, moreover, that, after months, the temperature of the rock at about a metre deep was still unchanged. This is obviously owing to the small difference of temperature between the rock and the surrounding air.

"From the observations at No. 8 and No. 15, in Table III., it is seen that the temperature at the bottom of the bore-hole was sometimes a little lower and sometimes a little higher than nearer its mouth.

"This mode of observing gave correct results, but was laborious and costly, not only on account of the necessity of making special bore-holes for the purpose, but because almost every experiment cost a thermometer. The projecting end was often maliciously broken off, and on account of the swelling of the wooden case it almost never happened that at the end of an experiment a thermometer was drawn out again uninjured.

"Hermann and Pfister remedied this latter evil by surrounding the thermometer-tube from the bulb to the scale, with a glass case, and this with a steel jacket. This arrangement, however, involves not only conduction through the steel, but also continual interchange of heat by currents of air in the glass case, from the mouth to the bottom of the hole. For these reasons the observations made with these thermometers could not be employed without intricate corrections.

"Later I tried a Thomson's maximum thermometer,[1] kindly placed at my disposal by Prof. Everett, which (after previous strong cooling) was left for several days at the bottom of the bore-hole, closed air-tight. The results agreed with those obtained by other methods; but who can guarantee that the higher temperature prevailing in a newly-bored hole is always just so much depressed by the cold mass of the thermometer and its copper case, that the rock-temperature alone determines the final indication of the maximum thermometer?

"This consideration induced me to employ for rock-temperature observations (and they also serve for air and water observations), the above-mentioned short thermometers with insulated bulbs, the first of which Prof. Everett caused to be made by Negretti and Zambra for this express purpose. These thermometers, inclosed in a metal box provided with a handle, are thrust to the bottom of the bore-hole, which is at least a metre deep. To the handle is fastened a strong cord reaching to the mouth of the hole, by which it can be drawn out again at the end of the trial. The bore-hole, from the thermometer to the mouth, is stopped with greased rag or other similar material, as air-tight as possible. After two or three days, the thermometers have usually assumed the temperature of the surrounding rock, that is to say, their reading has ceased to alter. The insulation of the quicksilver prevents alterations during the drawing-out and reading of the thermometer. The correctness of the result is in no way prejudiced by sediment from the boring which may yet remain in the hole. The pouring in of some water may even be useful in accelerating the experiment. Wet bore-holes with standing-water are, however, to be avoided, because rock-temperature and water-temperature are not identical.

"In the manner last described, at every available opportunity, that is to say, when the work of the tunnel is from any cause compelled to cease for a few days, rock-temperature observations are now instituted in bore-holes ready to our hand. The observations are simple, give exact results if taken with proper precaution and sufficient duration of the experiment, and cause no further expense, since the thermometers, being sunk in the rock, are secured against wanton injury, and there are always bore-holes available."

Dr. Stapff further states by letter that the two original thermometers supplied by Negretti and Zambra having been broken, he has had others made, in which he has introduced the improvement of hermetically sealing the outer glass case, instead of closing it with a waxed cork, which gradually admitted moisture.

In the Report for 1876 an account was given of the observations of Herr Dunker in a bore about 4,000 feet deep at Sperenberg, and allusion was made to the undue weight which had been attached by some writers to the empirical formula in which Herr Dunker sums up his observations; a formula which indicates a retarded rate of increase, and, if extended to greater depths, leads to the conclusion that the temperature reaches its maximum at the depth of about a mile.

A discussion has been carried on in Germany on this subject,[2]

[4] Eleventh Report of the British Association Underground Temperature Committee, by Prof. Everett.

[5] "Studien über die Wärmevertheilung im Gotthard," 1 Theil. "Der schweizerischen naturforschenden Gesellschaft zu ihrer sechzigsten Jahresversammlung in Bex gewidmet," von F. M. Stapff. Bern, 1877.

[1] It was one of the protected Negretti maximum thermometers constructed for the Committee.

[2] See papers by Mohr, Heinrich (two papers), Dunker, and Hottenroth, in the "Neues Jahrbuch" for 1875, 1876, and 1877; by Brauns, in the "Zeitschrift für die gesammten Naturwissenschaften," 1874, p. 483; and by Hann, in the "Zeitschrift der österreichischen Gesellschaft für Meteorologie," 1878, p. 17.

chiefly in the "Neues Jahrbuch für Mineralogie," &c., and the best authorities seem to be unanimous in rejecting the hypothesis of a retarded rate of increase in the earth's surface as unwarranted, either by the Sperenberg observations or any others. Herr Dunker himself concurs in this opinion. Dr. Stapff also, though some of his own empirical formulæ indicate a retarded rate of increase, writes to Prof. Everett in the following terms:—"As to my formulas, I beg you to remember that they are not constructed for expressing laws of Nature. They simply are made for facilitating the view over a heap of figures and data of observation. And generally I beg you to be sure that those formulas, in my mind, cannot express any law for the increase of warmth at greater depths than those in which the tunnel observations were made. The formulas give good means for eliminating empirically some of the influences of the shape of surface which occur in the profile of the mountain."

Mr. W. Galloway, one of H.M. Inspectors of Mines, has taken observations in Fowler's Colliery, Pontypridd, South Wales. The shaft is 846 feet deep, and the air current down it amounts to between 20,000 and 30,000 cubic feet per minute.

In order to determine the normal temperature of the coal, a hole $1\frac{1}{2}$ inch in diameter was bored in the side of a narrow place that was being rapidly driven in the solid coal. The hole was bored in the very face, to the depth of 4 feet. The thermometer (one of the committee's slow-action non-registering instruments) was placed at the inner end; then a wooden cylinder of nearly the same diameter as the bore-hole, and 9 inches long, was pushed in until it came in contact with the copper case of the thermometer; and lastly a wooden plug, wrapped round with cloth, was driven firmly into the mouth of the hole. The thermometer was at 58° F. when it was put into the hole, and after remaining there from 2 P.M. on August 25, 1876, to 3.45 P.M. on the following day, it stood at 62°·7. There was no water whatever in the hole, and the depth below the surface of the ground was 855 feet.

The circumstances of this observation seem to preclude any considerable disturbance of the normal temperature; and combining it with the mean annual temperature at the surface, which is said to be 51°·5, we have an increase of 11°·2 F. in 855 feet; which is at the rate of 1° F. for 76 feet.

Two other observations were taken in other parts of the mine. They are not directly available for the purposes of the Committee, but were intended to test the influence of air-currents on the temperature of the coal; and they show variations of 2° or 3°, according to the season of the year.

Observations are being taken for the Committee by Mr. G. F. Deacon, Borough Engineer of Liverpool, in a bore which has attained the depth of 1,004 feet, in connection with the Liverpool Waterworks at Bootle.

The temperature at this depth is 58°·1. The observation nearest the surface was at the depth of 226 feet, the temperature at this depth being 52°. We have here a difference of 6°·1 in 778 feet, which is at the rate of 1° for 128 feet, and the same rate is approximately maintained throughout the descent. For instance, at 750 feet, the temperature was 56°, which gives 1° for 131 feet by comparison with the depth of 226 feet, and 1° for 121 feet by comparison with the bottom.

The bore is 24 inches in diameter, and the observations were taken with a protected Phillips's maximum thermometer every Monday morning. The operation of boring was continued up to twelve o'clock on Saturday night, and was not resumed till the temperature had been taken on the following Monday. The time that the thermometer remained at the bottom was not less than a quarter of an hour, and was sometimes half an hour.

The rock-formation consists of the pebble beds of the Bunter or lower trias, and most of it is described as hard, close-grained, and compact. The speed of boring is indicated by the dates of the observations at 226 and 1,004 feet, the former being November 12, 1877, and the latter August 12, 1878. A month was lost by the jamming of the drilling tool, in May and June, 1878, when a depth of about 890 feet had been attained.

The depth from the surface of the ground to the surface of the water in the bore has gradually decreased from 66 feet, when the bore was at 318 feet, to 52 feet, when the bore was at 800 feet, and to 51·1 feet, at the present depth. It would thus appear that the inflow of water from below has increased with the depth attained. There is a slow percolation from the upper part of the water-column to an underground reservoir near at hand, the top of the water-column being considerably higher than the top of the water in the reservoir. Mr. Deacon remarks that the slow upward flow which supplies the water for this gradual discharge is favourable to the accuracy of the observations (which have always been taken at the bottom), by checking the tendency of the colder and heavier upper water to descend and mix with the lower. As bearing on the subject of the disturbance of temperature by the stirring of the water in boring, as well as by the generation of heat in the concussions of the tool, it may be mentioned that the last observation before the month's interruption by the jamming of the tool was 57°·5, at 886 feet, and the first observation after the extraction of the tool was 57°·0, at 898·6 feet, the former being on May 20, and the latter on July 1. The smallness of the difference between these two temperatures seems to indicate smallness of disturbance by the action of the tool.

It appears from these various circumstances that the observations are entitled to considerable weight, and that the rate of increase of temperature downwards at Liverpool is exceptionally slow. It will be remembered that the rate found by Mr. Fairbairn, at Dukinfield Colliery, in the adjacent county (Cheshire), was also very slow, though not nearly so slow as that indicated by these Liverpool observations.—(See our Report in the volume for 1870).

Mr. E. Wethered, of Weston, near Bath, has also commenced observations in a colliery in that neighbourhood. Mr. J. Merrivale, of Nedderton, near Morpeth, has received a thermometer for observations in a colliery. Mr. J. T. Boot, of Hucknall, near Mansfield, has received a second thermometer (in place of a broken one) for observations in a deep bore, and Mr. Rowland Gascoigne, of the same town, has received one for a similar purpose.

In the eleven years which have elapsed since the appointment of this Committee a large amount of useful work has been done, by methods of observation not requiring any elaborate or expensive appliances, or any special training on the part of the observers.

Two difficulties are encountered in investigating underground temperature. We have to contrive instruments which shall truly indicate the temperature at the point of observation, and we have further to insure that this temperature shall be the same at the time of observation as it was before the locality was artificially disturbed.

As regards the first of these difficulties the Committee have been completely successful, and have largely increased the resources at the command of observers.

But in regard to the second difficulty the same amount of success has not been attained. The circulation of water in bore-holes and of air in mines are disturbing elements difficult to deal with. Even such firm plugging as was employed to isolate portions of the water-column in the great bore at Sperenberg cannot altogether remove the error arising from convective disturbance; for the long-continued presence of water at a temperature different from that proper to the depth affects the temperature of the surrounding rocks, and the temporary isolation of a short column would not abolish this source of error, even if the plugs themselves were impervious to conduction and convection.

After the experience which has now been gained of rough and ready methods, it is time to consider the propriety of resorting to a more special method, which has been more than once suggested, but has hitherto been postponed on account of the additional labour and skill which would be requisite for carrying it out.

There can be no doubt that the surest way to bring any point of a boring to its original temperature is to fill up the bore, and reduce it as nearly as possible to its original condition. Several instruments have been contrived which, when buried in the earth, with wires coming from them to the surface, admit of having their temperature observed by electrical means.

One of these is Siemens' resistance thermometer, another is Wheatstone's telegraphic thermometer, of which a description will be found in the Report of the Dundee Meeting of the British Association; another is Becquerel's thermo-electric apparatus, which has been employed by its inventor and his son and grandson for some forty years. It is described in the following terms in the first Report of this Committee (1868):—

"The thermo-electric method might also be followed with great advantage. Two wires, one of iron and the other of copper, insulated by gutta-percha or some other covering, as in submarine cables, and connected at their ends, might be let down so as to bring their lower junction to the point where the temperature is to be taken, their upper junction being immersed in a basin of water, and the circuit completed through a galvanometer. The temperature of the water in the basin might then be altered till the galvanometer gave zero indication."

Sir Wm. Thomson now adds the recommendation that, in carrying out this method, the two wires, each well covered with gutta-percha, should be twisted together; that the wires should be stout and as homogeneous as possible throughout, and that a piece of stout copper tube should be attached to the lower junction, this tube being uncovered and in close contact with the earth all round, its purpose being to insure that the junction takes the proper temperature.

It would probably be desirable, in filling up the bore, to mix clay with the original material to render it watertight, for it would be impossible to render the filling of the bore as compact as the surrounding rock.

Several pairs of wires would be buried in the same bore, with their lower junctions at different carefully-measured depths.

The upper junctions would be kept in a room provided with a steady table for a mirror-galvanometer.

THE RAINFALL OF THE WORLD[1]

1. THE pamphlet referred to below embodies the outline of an attempt to bring into harmony the disconnected, and in some cases apparently irreconcilable results that have hitherto attended comparisons of terrestrial rainfall and sun-spot variations. It relates, therefore, to the entire rainfall system of the globe.

2. The plan by which it is thought this object will be best attained is one which divides the world into a number of rainfall zones where either *à priori* considerations or actual experience would lead us to expect typical changes in the effects of a recurring secular variation in solar radiated heat upon the rainfall; it being immaterial as far as regards the practical advantages secured by this method of hyeto-graphical subdivision, whether the solar radiation be ultimately found to vary directly or inversely with the sun-spots.

3. The way in which typical changes may arise in different parts of the earth from the effects of an assumed recurring secular change in solar radiated heat, is shown by a reference to the general scheme of atmospheric circulation in conjunction with the two leading factors of variability, viz., season and latitude.

4. A consideration of these points leads the author to divide the world into five zones, which either theoretically might, or are actually known to, involve some typical change in the secular variation of the rainfall either of one season or the whole year.

5. Partly to illustrate this mode of subdivision by applying a reasonable working hypothesis, and partly in the absence of absolutely conclusive evidence in its favour, by exhibiting the harmony of existing facts with the conditions theoretically deduced from it, to promote its ultimate adoption, the theory of the inverse variation of solar radiated heat with the sun-spots is assumed throughout.

6. It is also shown in the Introduction that we have a good deal of evidence in favour of the same theory, both *à priori*, from a consideration of the principle of conservation of energy as applied to the sun, as well as indirect, from the results of thermometrical observations.

7. In applying this hypothesis to determine the rainfall variation, account is mainly taken of the direct relation between wind velocity and temperature, the secular changes in solar radiation being assumed to cause *similar* effective secular changes in the velocity of the larger atmospheric convection currents.

8. An induction from Messrs. Blanford and Eliot's theory of cyclone-generation is then made use of, in combination with the preceding hypothesis, from which it appears that while, owing to the diminished solar temperature, evaporation might be lessened in the tropics at the epoch of maximum sun-spot, the diminished carrying power of the wind (by which the prevalence of cyclones at this epoch would be accounted for, according to Blanford and Eliot's theory) might allow of greater precipitation near the place of evaporation, and therefore of a generally heavier rainfall in these regions. At the opposite epoch, on the other hand, the increased velocity of the wind would probably cause a wider distribution of tropical vapour, and therefore in combination with the direct effects of the assumed increase in solar radiation at the same epoch give rise to a deficiency of rain in parts, more especially those in which the local conditions normally tend to produce aridity.

9. These hypothetical results are then shown to approximately agree with the actual results of observations recorded in these regions.

10. It is next shown that the effects of the assumed secular change in the velocity of the anti-trade (the prevailing wind of the temperate zone) should differ considerably from those in the case of the monsoons and trades of the tropics, an increased velocity in the case of the anti-trade causing a greater quantity of tropical vapour to be conveyed to the temperate regions, and consequently a greater degree of humidity to ensue there. When, therefore, the direct effects of the assumed increase of solar heat at such an epoch are at a minimum, that is to say, in the *winter*, the relative humidity, and consequently the rainfall, should be *increased*. It is also evident that such an effect should be most conspicuously felt in those regions where rain falls *only* in the winter, and is due to the descent of the anti-trade.

11. The occurrence of this inverse variation in the zone of winter rains, which in the case of the Mediterranean stations (*Zeitschrift für Meteorologie*, Band viii. No. 6), had hitherto been deemed unfavourable to Messrs. Lockyer and Meldrum's generalisation regarding the direct variation of terrestrial rainfall with the sun-spots, is also shown to be visible in the winter rainfall of Northern India, and the rainfalls of Jerusalem and California, thereby affording some preliminary support to the notion that it holds over a still wider extent of the globe where the rain falls mostly during the winter.

12. The attempt is then made to show that while the direct effects of the secular change in the sun's heat over extra-tropical continents may, during the summer, operate so far as to destroy the indirect effects produced by the corresponding variations in the strength of the anti-trade, and as Dr. Hahn has shown in the case of the summer rainfalls of several stations in Central Europe, actually cause a *direct* variation with the sun-spots, there are, as there should be, in accordance with the hypothesis, some preliminary indications of an *inverse* variation of that proportion of the total which falls during the winter months alone, even in those places where the rain falls throughout the year. This fact, then, would imply that a change of season causes a change of type in the character of the variation, so that in order to render the variations distinctly apparent we should compare the winter and summer falls separately. It may also be inferred that the quality of the variation in the total annual fall will depend on the preponderance of the summer or winter falls respectively, which fact may help to account for the numerous anomalies noticed by those who have hitherto compared the total annual falls of places in the temperate zone with sun-spots.

13. It is finally inferred in the appendix, as a direct result of the hypothesis assumed throughout, that the winter gales of the temperate zone and the cyclones of the tropics should bear a complementary relation to each other, the former being most frequent about the time of minimum, and the latter about that of maximum, sun-spot. Some evidence in favour of this notion was recently communicated to NATURE by Mr. S. A. Hill (vol. xviii. p. 616).

14. The pamphlet is intended by the author to be considered as merely tentative, and not by any means conclusive. It is the method of division into zones and the separate comparison of seasonal falls, rather than the accordance of data with theoretical deductions, to which he desires to give prominence, and which he thinks may be of some assistance to other workers in the same field.

[1] The Rainfall of the World in Connection with the Eleven-Year Period of Sun-spots. With an Introduction and Appendix. By E. D. Archibald, Professor of Mathematics in the Patna College. (Calcutta and London: Thacker and Co. 1878.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE Cambridge Mathematical Tripos list was published on the 24th. This year the list contains 91 names. There are 28 classed as Wranglers, 33 as Senior Optimes, 29 as Junior Optimes, and 1 Ægrotat. In 1878 the list contained 94 names, 31 being Wranglers, 30 Senior Optimes, 29 Junior Optimes, and 4 Ægrotant. The first three Wranglers are Mr. A. J. Campbell Allen, of St. Peter's, Mr. George Walker, of Queen's, and Mr. Carl Pearson, King's. Mr. Campbell Allen, of St. Peter's College, the Senior Wrangler, is a native of Belfast, and was born in 1856. He received his elementary education at the