have a discrepancy of 700 years, and a clear confounding of Zozimus of Alexandria with his namesake of Panapolis. Suidas attributes chemical works to the former, but we must remember that the word χημεία does not occur before the eleventh century, A.D. The director of the Bibliothèque Nationale,* in a recent letter for which we have to thank him, writes as follows:—"La Bibliothèque Nationale ne renferme aucun manuscrit grec de Zosime de Panapolis qui puisse attribué a une époque antérieure au XIII. Siécle. Le plus ancien de ceux qu'elle possède ne remonte pas plus loin que cette date." Everything tends to prove that the MSS. were not only written, but composed at a period posterior to the fifth century. The fanciful titles of some of them show us that their authors adopted any name they pleased; thus we have "the Epistle of Isis, queen of Egypt, and wife of Osiris on the sacred art, addressed to her son Horus," in which we find a solemn oath dictated to Isis by the angel Amnaël, who swears by Mercury and Anubis, by Tartarus, the Furies, and Cerberus, and by the dragon Kerkouroboros. The whole thing is plainly a blending of eastern and western thought: personages of Egyptian, Greek, and Roman mythology, with angels of the Talmud, and genii of Arabic lore. We are glad to find that M. Hoefer breaks freely away from the too confident Olaus Borrichius, as to the authenticity of Hermes Trismegistus. He admits that the books which bear his name are spurious, and concludes that their author, "vivait probablement à l'époque critique du Christianisme triomphant et du paganisme à l'agonie." But if we take this as the time of Constantine the Great, we must venture to attach a later date to these writings.

We recently had an opportunity of examining the MS. in the Bibliothèque Nationale, attributed to Zozimus and to the fifth century; a MS. which, from its frequent mention in both ancient and modern works on the history of chemistry, possesses special interest. It is entitled "Zozimus on Chemical Instruments and furnaces, and on the Holy Water" (Ζωσίμου περὶ ὀργάνων καὶ καμίνων καὶ περὶ του θείου ὕδατος), and it is a well-preserved MS. of the thirteenth century, written on vellum. The few drawings which it contains are asserted to have been taken by the author

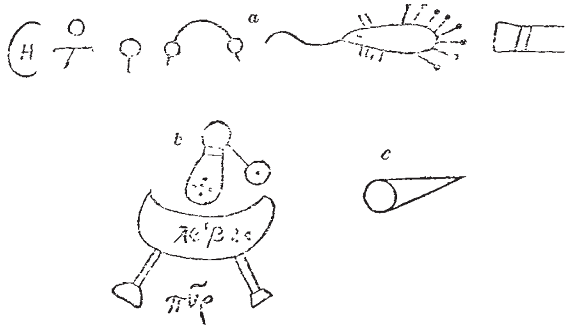

FIG. 7.—An Alembic, and Symbols from Greek MSS. on Alchemy.

from a temple at Memphis. The Alembic (*b* in the accompanying woodcut, Fig. 7) is copied from this MS., in which also the line of symbols (*a*) is found. These symbols occurred in almost every Greek MS. on alchemy which we examined, but we could find no clue to the curious porcupine-like animal. The symbol *c* is clearly of astronomical origin, and is not often met with in later works. The MSS. are for the most part devoid of figures, and not so full of symbols as later alchemical treatises.

We have endeavoured to prove (α) that no reliable date can be assigned to existing Greek MSS. on alchemy, and (β) that the accepted date is too early. Even if we could prove that a man named Zozimus, living in the fourth century, wrote treatises on alchemy, we could not use the existing MSS. for any exact purpose connected with the history of science with safety; for, since we have no such MS. earlier than the tenth or eleventh centuries, it would be quite impossible to determine whether additions had been made during transcription. The facts are simply these:—There exist in various parts of the world Greek MSS. on alchemy, none of which are older than the tenth century. Many of these bear the names of mythical personages of Egyptian mythology, some of ancient Greek philosophers, some of people who are supposed to have lived in the fourth or fifth century, A.D. When we remember that no ancient writer makes mention of alchemy or chemistry, that the word χημεία is first used in the eleventh century, and when we further bear in mind the condition of the intellectual world in the fourth and fifth centuries, we think we may well admit that further evidence is necessary before we can assert that alchemy arose in the fourth century. Indeed we are of opinion that, in spite of all that has been written on the subject, there is no good evidence to prove that alchemy and chemistry did not originate in Arabia not long prior to the eighth century, A.D.

G. F. RODWELL

* This library has so often changed its name of late, that we think it necessary to mention that we mean the library in the Rue Richelieu, which is called by old writers the *Bibliothèque du Roi* sometimes the *Bibliothèque Royale*, lately the *Bibliothèque Impériale*, still more lately the *Bibliothèque Communale*, now the *Bibliothèque Nationale*.. Juncker in his *Conspectus Chemiæ*, in speaking of various writers on alchemy cites "Zozimus Panapolites celeberrimus et magni cognomen adeptus, cujus varia scripta exstant in Bibliotheca Regia Parisiensi."

ON THE ECLIPSE EXPEDITION, 1871*

II.

I MUST now state very briefly some of the results of our work; and first, the certain results.

We were able to make out the structure of the corona. We know all about the corona so far as the structure of its lower brighter strata, that portion, viz., which I referred to in my lecture last year as being visible both before and after totality, is concerned. You may define it as consisting of cool prominences; that is to say, if you examine a prominence any day without waiting for an eclipse, and then go to an eclipse and examine the lower portion of the corona, you will find the same phenomena, minus the brightness. You find the delicate thread-like filaments which you are now all so familiar with in prominences—filaments which were first thrown on a screen in this theatre; the cloudy light masses, the mottling, the nebulous structure, are all absolutely produced in the corona, as far as I could see it with a telescope with an aperture of 6¼ inches; and I may add that the portion some five minutes round the sun reminded me forcibly in parts of the nebula of Orion, and of that surrounding η Argus, as depicted by Sir John Herschel in his Cape observations.

We have shown that the idea that we did not get hydrogen above 10 seconds above the sun is erroneous; for we obtained evidence that hydrogen exists to a height of 8 or 10 minutes at least above the sun; and I need not tell you the extreme importance of this determination. One of the proofs we have of that lies in this diagram, showing the observations made by Prof. Respighi, armed with an instrument the principle of which I hope you are now familiar with.

Just after the sun disappeared Prof. Respighi employed this prism to determine the materials of which the prominences which were then being eclipsed were composed; and he got the prominences shaped out in red, yellow, in blue, and in violet light; a background of impure spectrum filling the field, and then as the moon swept over the prominences these images become invisible; he saw the impure spectrum and the yellow and violet rings gradually die out, and then three bright and broad *rings* painted in red, green, and blue, gradually form in the field of view of his instrument; and as long as the more brilliant prominences were invisible on both sides of the sun he saw these magnificent rings, which threw him in a state of ecstacy. And well they might.

These rings were formed by C and F, which shows us that hydrogen extends at least 7 minutes high, for *had we not been dealing with hydrogen we should have got a yellow ring as well*, because the substance which underlies the hydrogen is more brilliant than the hydrogen itself, and in addition to the red ring and the blue ring, which indicate the spectrum of hydrogen, he saw a bright green ring, much more brilliant than the others, built up by the unknown substance which gives us the Kirchhoff line, 1474.

Now at the time that Prof. Respighi was observing these beautiful rings by means of a single prism and a telescope of some four inches aperture, some 300 miles away from him—he was at Poodocottah and I was at Bekul—I had arranged the train of prisms which you see here so that the light of the sun should enter the first prism, and after leaving the last one should

* A Lecture delivered at the Royal Institution of Great Britain, Monday, March 22, 1872, by J. Norman Lockyer, F.R.S. (concluded from p. 58).

enter my eye. And what I saw is shown, side by side with Respighi's observations, in this diagram, in which I have separated the rings somewhat, so that there should be less confusion that in the actual observation. Here is Prof. Respighi's first observation. He gets indications of C, D^3, F, and the hydrogen line near G. He was observing the very lowest, brightest region of all, and therefore 1474 was obliterated by the brightness of the continuous spectrum; but as the eclipse went on D^3 was entirely obliterated, and afterwards he got C and F building up rings together with 1474, which was not represented in the lower regions of the prominence—not because it was not there, but because, as I have already insisted, of the extreme brilliancy of the background. Now my observation was made intermediately as it were between the two observations of Prof. Respighi's. Let me show the observations together.

Respighi ...	C D^3		F G	Prominences at beginning of eclipse.
Lockyer ...	C	1474	F G	Corona at 80 seconds from commencement.
Respighi ...	C	1474	F	Corona at mid eclipse.

Note that I had no object-glass to collect light, but that I had more prisms to disperse it; so that with me the rings were not so high as those observed by Respighi, because I had not so much light to work with: but such as they were I saw them better because the continuous spectrum was more dispersed, and because, with my dispersion, the rings—the images of the corona—therefore did not so much overlap. Hence doubtless Respighi missed the violet ring which I saw, so faint, however, that both that and 1474 were almost invisible, while C shot out with marvellous brilliancy, and D^3 was absent.

These observations thus tend to show, therefore, that instead of the element—the line of which corresponds with 1474—existing alone just above the prominences, the hydrogen accompanies it to what may be termed a great height above the more intensely heated lower levels of the chromosphere, including the prominences in which the lower vapours are thrown a greater height. With a spectroscope of small dispersion attached to the largest mirror of smallest focus which I could obtain in England, the gaseous nature of the spectrum, as indicated by its *structure*, that is, bands of light and darker intervals as distinguished from a continuous spectrum properly so called, was also rendered evident.

These are results of the highest importance, which alone are worth all the anxiety and labour connected with the expedition.

But there is more behind.

The photographic operations (part of the expense of which was borne by Lord Lindsay) were most satisfactory, and the solar corona was photographed to a greater height than it was observed by the spectroscope, and with details which were not observed in the spectroscope.

Mr. Davis was fortunate enough to take an admirable series of five photographs at Bekul, and Captain Hogg also obtained some at Jaffna; but I am sorry to say the latter lack somewhat in detail.

I have prepared two lamps, because I am anxious to exhibit the photographs two at a time, that you may compare one with the other. [This was done.] You see that so far as the camera goes—and mark this well—the corona was almost changeless during the whole period of totality; this is true, not only for one place, but for all the places at which it was photographed.

I now exhibit two other photographs—one taken at Jaffna and the other at Ootacamund. Actinically the corona was the same and practically changeless at all the stations. You see that, though not so obvious as in the other case, there is the same similarity.

Before I leave the actinic corona, I am anxious to show you an image of it, taken during the American eclipse of 1869 in a camera exposed to the sun during the whole of the totality; to a certain extent in our recent photographs we have reproduced what was photographed in 1869.

The solar nature of most, if not all, of the corona recorded on the plates is established by the fact that the plates, taken in different places, and both at the beginning and end of totality, closely resemble each other, and much of the exterior detailed structure is a continuation of that observed in the inner portion independently determined by the spectroscope to belong to the sun.

While both in the prism and the $6\frac{1}{4}$ inch equatorial the corona seemed to form pretty regular rings round the dark moon, of different heights, according to the amount of light utilised by the instrument, on the photographic plates, the corona, which, as I have before stated, exceeds the limits actually seen in the instrument I have named, has a very irregular, somewhat stellate outline, most marked breaks or rifts (*ignored by the spectroscope*), occurring near the sun's poles, a fact perhaps connected with the other fact that the most active and most brilliant prominences rarely occur there.

From the photographs in which the corona is depicted actinically we pass to the drawings in which it is depicted visually. I would first call attention to two drawings made by Mr. Holiday, who formed part of the expedition, and in whose eye every one who knows him will have every confidence.

First there is a drawing made at the commencement of the totality, and then a drawing made at the end. There is a wonderful difference between the drawings; the corona is in them very much more extensive than is represented actinically on our plates.

Here is another drawing, made by Capt. Tupman, in which again we have something absolutely different from the photographs and from Mr. Holiday's sketches, inasmuch as we get an infinite number of dark lines extending down to the moon, and a greater extension than in the photographs, though in radial places the shape of the actinic corona and some of its details are shown.

Now the corona, as it appeared to me with the naked eye, was nothing but an assemblage of bright and dark lines, it lacked all the structure of the photographs, and apppeared larger; and I have asked myself whether these lines do not in some way depend on the size of the telescope, or the absence of a telescope. It seems as if observations of the corona with the naked eye, or with a telescope of small power, may give us such lines; but that when we use a telescope of large power, it will give, close to the moon, the structure to which I have referred, and abolish the exterior structure altogether, leaving a ring round the dark body of the moon such as Prof. Respighi and myself saw in our prisms, and in the 6-inch telescope, in which the light was reduced by high magnification so as to bring the corona to a definite ring some five minutes high, while Prof. Respighi, using a 4-inch telescope and less magnifying power, brought the corona down to a ring something like 7 minutes high.

And here we have an important connection between spectroscopic and telescopic work. If we employ a telescope in which the light is small or is reduced by high magnification, we bring the corona to a definite ring, and perhaps here we have the origin of the "ring-formed" coronas.

Many instances of changing rays, like those seen by Plantamour in 1860, were recorded by observers in whom I have every confidence. One observer noted that the rays revolved and disappeared over the rifts.

We have next to deal with the polariscopic observations.

Mr. Lewis, in sweeping round the corona at a distance of 6′ or 7′ from the sun's limb, using a pair of compensating quartz wedges as an analyser, which remained parallel to itself while the telescope swept round, observed the bands gradually change in intensity, then disappear, bands of a complementary character afterwards appearing, thereby indicating radial polarisation.

Dr. Thomson at Bekul saw strong traces of atmospheric, but none of radial polarisation, with a Savart. With the same class of instrument the result obtained by myself was precisely similar; while on turning in the Biquartz, at the top and bottom of the image of the corona, *i.e.*, near the sun's equator, faint traces of radial polarisation were perceptible for a short distance from the moon's limb. Captain Tupman, who observed with the polariscope after totality, announces strong radial polarisation extending to a very considerable distance from the dark moon.

Leaving the extreme outside of the corona as a question to be determined at some future time—and it can well wait—let us come to the base of the corona, and deal with the region to which I have already referred, close to the sun.

What was the general conclusion at which we arrived on this important point? Before I state it, let me tell you the instrumental conditions of the inquiry. We can use such a spectroscope as the one with which you are all familiar, and so arrange matters that the slit shall be carried by a clock, so that it may follow accurately the edge of the moon; but if the least variation in the rate of motion takes place, the observation is rendered almost valueless. But if we employ a spectroscope, in which we sum up the light—do not localise the light, but throw it together—it does not matter whether your clock goes well or not, you are certain to have a result worthy of credit. But if you employ such an instrument as Prof. Respighi employed, and abolish the slit altogether, the weight of any observations made with such conditions is very great.

Captain Maclear, who was observing with me at Bekul, has undoubtedly shown that when the light of our atmosphere is cut off by the interposition of the dark moon, we see very many more bright lines than we do when this is not the case, the lines being of unequal height.

Mr. Pringle, also at Bekul, showed that, at the end of totality, many lines flashed into one of these instruments, carried under these difficult conditions.

Captain Fyers, the Surveyor-General of Ceylon, observing with a spectroscope of the second kind, saw something like a reversal of all the lines at the beginning, but nothing of the kind at the end.

Mr. Fergusson, observing with a similar instrument, saw reversal neither at the beginning nor the end.

Mr. Moseley, whose observations are of great weight, says that at the beginning of the eclipse he did not see this reversal of lines. Whether it was visible at the end he could not tell, because at the close the slit had travelled off the edge of the moon.

Prof. Respighi, using no slit whatever, and being under the best conditions for seeing the reversal of the lines, certainly did not see it at the beginning, but he considers he saw it at the end, though about this he is doubtful.

From the foregoing general statement of the observations made on the eclipse of last year, it will be seen that knowledge has been very greatly advanced, and that most important data have been obtained to aid in the discussion of former observations. Further, many of the questions raised by the recent observations make it imperatively necessary that future eclipses should be carefully observed, as periodic changes in the corona may then possibly be found to occur. In these observations the instruments above described should be considered normal, and they should be added to as much as possible.

I had intended, if time had permitted me, to point out how much better we are prepared for the observation of an eclipse now than we were when we went to India, and how a system of photograph record should be introduced into the spectroscopic and polariscopic work; but time will not allow me to do more than suggest this interesting topic. I am anxious, however, that you should allow me one minute more to say how very grateful we feel for the assistance rendered by all we met, to which assistance so much of our success must be ascribed. I wish thus publicly to express the extreme gratitude of every one of our Expedition to the authorities in India and in Ceylon for the assistance we received from them, and our sorrow that Admiral Cockburn, a warm and well-known friend to Science, who placed his flagship at the disposal of the expedition, and the Viceroy, whose influence in our favour was felt in every region of India whither our parties went, and to whom we gave up our ship, are now, alas! beyond the expression of our thanks. We are also anxious to express our obligations to the directors and officers of the Peninsular and Oriental Company for the magnificent way in which they aided us. If they had not assisted us as they did, Science would have gained very much less than she has done from the observations of the last eclipse.

SCIENTIFIC SERIALS

THE *Journal of the Quekett Microscopical Club* for October 1872, contains but three papers, of which the first is a short one by Dr. Guy, F.R.S., on the "Hand Illuminator Microscope," which is followed by a more elaborate communication of considerable length, by Mr. M. C. Cooke, on "Old Nettle Stems and their Micro-fungi," in which twenty-seven species of fungi are enumerated and described which develop themselves on the old stems of the common nettle.—C. H. Peck, of Albany, U.S., communicates an article on the disease of plum and cherry trees in the United States known as "black knot," and his observations on the structure and growth of the *Sphæria morbosa* (Schweinitz) which accompanies, or causes, these gouty excrescences. The record of the proceedings of the club completes the contents of the present number.

Bulletin de l'Académie Royale de Belgique, No. 7. This number contains a paper, by M. P. J. Van Beneden, on the fossil whales of Antwerp, in which he describes several new types, among others, one (named Cetotherium) characterised chiefly by the articular condyle on the inferior maxillary, and forming a transition-type between the Balænoptera and the Cetodonts. Four species of Cetotherium are described. G. Dewalque gives a description, with plate, of a new fossil sponge, met with in the Eifel system; a species of the Astræospongium of Roemer, so named from the six-rayed star forms composing it. A new mode of estimating the advantage of binocular vision over monocular, as regards the brightness or clearness of objects, is proposed by H. Valerius. He employs Foucault's photometer, which consists of a long box, having a glass disc fixed in one end of it, and a pasteboard diaphragm in the direction of the axis of the box, moveable to or from the disc with screws. Lights are placed on either side of the diaphragm, which thus forms shadows on the disc, and the diaphragm is so adjusted that the shadow from each light occupies half of the disc. The lights having been so adjusted that the disc seems uniformly lighted, their relative intensities are as the squares of the distances separating them from the disc. M. Valerius uses, for his purpose, a prismatic tube, through which he observes the disc of the photometer. It contains a vertical screen which conceals one-half of the disc from one of the eyes. Suppose the disc to be receiving equal quantities of light from the two sources, the observer, on looking through the tube, finds that the half-disc seen with only one eye, appears less illuminated than the other. The equality is restored by moving one of the lights, and the distance of the motion is measured.—This paper is followed by one on formulæ in Ballistics, by J. M. De Tilly.—In the literature department, Baron Kervyn de Lettenhove gives an interesting account of certain documents which he examined at Hatfield House, bearing on the later history of Mary Queen of Scots. He discusses the celebrated casket letters, two of which are preserved at Hatfield, and are considered by him to be translations from the Scotch text. The letters are given in lithograph.—E. Varenbergh communicates an account of a journey made by three Flemish gentlemen to Nuremberg in the thirteenth century; an exact statement being made of the expenses incurred in travelling. One or two minor articles complete the number.

Poggendorff's Annalen der Physik und Chemie.—No. 7 (1872) commences with a paper of careful research, by H. Knoblauch, on the passage of heat-rays through inclined diathermanous plates. The rays, polarised by a Nicol's prism, were caused to pass horizontally to the plate, which was moveable about a vertical axis, and, passing through it, affected a thermopile. Two things determine the passage of radiant heat through inclined plates—the nature of the ray's polarisation, and the absorptivity of the substance composing the plate. These two influences are fully investigated and their effects described.—A continued account, by Hagenbach, of researches on Fluorescence is followed by a somewhat mathematical paper by Ketteler (also a continuation), on the influence of astronomical motion on optical phenomena. Dr. Stoletow discusses at some length the "Function of Magnetisation" of soft iron, and a description is given by G. Vom Rath, of the meteoric stones which fell at Ibbenbühren in 1870. W. Beetz, in a short note, contests the assertion of Zöllner, that an electric current is generated in the flowing of water, pointing out that, in the experiments made, the electric phenomena probably arose from the actual formation of a voltaic element consisting of two different metals (of tap and pipe), and the water, so that the same thing might be observed though the water was at rest. Zöllner's theory of terrestrial magnetism connects itself with the observation in question, as he supposes the flowing liquid masses in the earth's interior generate electric currents by their motion. This number contains, in addition, two contributions on the structure of hailstones, and one or two other short notes.

No. 8 contains the concluding part of Herr Hagenbach's researches on Fluorescence. His experiments, made with a great variety of substances, confirm Stokes's laws. He considers that all the rays are capable of exciting fluorescence. The maxima of the fluorescence varied from 7 (in chlorophyll) downwards. The spectrum of the fluorescent light varied also for different substances, but no necessary connection was apparent between the "intermittence" in the fluorescence of the ordinary spectrum, and that in the fluorescence spectrum. Change of solvent often displaced the maxima. He points out the similarity between phosphorescence and fluorescence, and thinks these are phenomena differing not in kind, but only in degree.—In the next paper