the sea-level, the hills of which are formed of sandstones which contain immense coal-fields. According to a description of them, just published by D. D. Veth in the *Deutsche Geographische Blätter*, these coal-fields may contain altogether no less than 300 millions of tons of good coal. The northern, or Parambahan part of them contains two main beds of coal, having an average thickness of thirty-three feet and occupying a surface of about three square kilometres, that is, about 20 millions of tons of good coal; but the rocks are rather disturbed, and therefore the extraction of coal would be difficult. The middle, or Singalut part, situated on the right bank of the Ombilin River, contains about 80 millions of tons of coal, and consists of seven thin beds of coal, which have altogether an average thickness of 16 feet, But the best coal-field is the southern, or Sungei-Durian part. situated on the left bank of the Ombilin River, which contains about 200 millions of tons of good coal. The beds of coal are three, having a thickness of 20, 7, and 7 feet, separated from one another by sheets of sandstone 50 to 70 feet thick. As to the quality of the coal, thirteen tons having been extracted and brought to Padang, it was found that as fuel for steam-engines this coal is not below that of Cardiff or Newcastle, but that it would not be as good as these two in the production of lighting gas or for iron furnaces. As to the transport of this coal to the sea-coast, it would necessitate the construction of a railway 65 or even 100 miles long.

THE Danzig *Naturforschende Gesellschaft*, which numbers now no less than 398 members, has just issued a new volume of its *Proceedings* (new series, vol. v., fascicules 1 and 2). It contains, besides the minutes of meetings of the Anthropological, Physical, Chemical, and Medical Sections, much valuable information, especially as to the botany and zoology of Prussia. The *pièce de resistance* of this volume is an essay at a topographical flora of West Prussia, by H. von Klinggraeff, being a *résumé* of the author's own researches and of what is known on the flora of this province. The author finds that there are in this province no less than 1218 species of Phanerogams, 44 species of cellular Cryptogams, 363 species of mosses, 18 of Characeæ, and 276 species of lichens, and he takes into account only the true inhabitants of the province. As to the lower Cryptogams, the figures are but provisional ones, as the algæ and mushrooms of the province are but incompletely known. We notice also in this volume papers on the freshwater molluscs of the neighbourhoods of Danzig, by E. Schumann; on the Ichneumonids of Western and Eastern Prussia, by C. Brischke; the *Reports* on the third meeting of the Botanical and Zoological Society of Western Prussia, containing a series of catalogues of plants found during botanical excursions; an interesting paper by C. Brischke, which deals with a rather neglected question, namely, with the Phytophags which the author has observed and cultivated in the neighbourhood of Danzig; a paper on the bronze-basin of Steinwage, by Dr. Fröling; and on the Cenoman fossils which are found in the diluvium near Danzig, by Dr. Kiezow.

THE St. Petersburg Naturalist's Society intend to offer various prizes for botanical papers, and to couple with them the name of the late Dr. Schleiden, who was a member of the St. Petersburg Academy and Russian State counsellor.

ON July 21 the meeting of Polish Naturalists and Physicians took place at Cracow. Some 500 members attended the meeting.

ACCORDING to the latest investigations the *Phylloxera vastatrix* has spread enormously upon the peninsula of Istria, particularly in the neighbourhood of Pirano. The plague threatens to infect the vineyards of the Karst, of Friaul, and of Carniola.

WE learn from a circular, issued by the Director of the St. Petersburg Central Physical Observatory, that all the Arctic meteorological stations will soon be opened, and that about the autumn of 1882 we will have observations from these stations for a whole year. The following, we may remind our readers, are the stations to be established:—At Upernivik, by Denmark; in Northern Finnmarken, by Norway; on the Jan Mayen Island, and, if possible, on the western coast of Grönland, by Austria-Hungary; on Spitzbergen, by Sweden; on Novaya-Zemlya (already opened a year ago) and at the mouth of Lena River, by Russia; on Point Barrow and in Lady Franklin's Bay, by the United States. Sites have already been taken by the United States and Norway to open new stations. It is to be hoped that meteorological stations will be opened, according to the wish of the International Conference at Bern, also in Antarctic regions, namely, on South Georgia, by Germany, and at Cape Horn, by France; whilst the Netherlands expect to establish a station further in the Arctic region, namely, at Dickson Haven in Siberia. The International Conference which will be opened at St. Petersburg will establish the method of observation to be adopted at all these stations.

AN International Exhibition is planned for 1883 at Shanghai.

THE additions to the Zoological Society's Gardens during the past week include two Common Marmosets (*Hapale jacchus*) from South-East Brazil, presented by the Lord W. G. Cecil; two Common Squirrels (*Sciurus vulgaris*), British, presented by Mr. C. B. Barber; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Douglas; two Common Jays (*Garrulus glandarius*), British, presented by Mr. Arthur F. Astlay; a Common Cuckoo (*Cuculus canorus*), British, presented by Mr. Harry Morrisson; a Surucucu Snake (*Lachesis mutus*) from Pernambuco, presented by Mr. C. A. Craven; two Common Boas (*Boa constrictor*) from South America, presented by Mr. G. H. Hawtayne; a Common Adder (*Vipera berus*), British, presented by Mr. J. Snow; two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, four Common Widgeons (*Mareca penelope*), an Osprey (*Pandion haliætus*), European, purchased; a Guinea Baboon (*Cynocephalus sphinx*) from West Africa, received in exchange. Amongst the additions to the Insectarium during the same time are imagos of *Antheræa yama-mai*, bred from eggs, and larvæ of the Lobster Moth (*Stauropus fagi*), Pebble and Swallow Prominent Moths (*Notodonta ziczac* and *dictæa*) and Purple Thorn Moth (*Selenia illustraria*). Numerous Ant-Lions (*Myrmeleo formicarius*) are also now emerging in the perfect state from their burrows in the sand.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

WHAT then are those precise difficulties to which reference has been made?

The number of them is considerable, and they have arisen from careful study extending over many different fields of work.

1. We most conveniently begin by noticing those suggested in the work of comparing the lines of the different elementary bodies with the Fraunhoferian lines; work done chiefly by Kirchhoff, Angström, Thalèn and others. Kirchhoff was not long before he found that to say that each substance had a spectrum entirely and specially belonging to that particular substance was not true. He says,[2] "If we compare the spectra of the different metals with each other, several of the bright lines appear to coincide." Now Kirchhoff was working with Bunsen as his collaborateur, and therefore this was not said lightly, as we may imagine. Similarly Ångström, who was working with the assistance of the Professor of Chemistry at Upsala, was driven to exactly the same conclusion. He says[3]—

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 301.

[2] "Researches on the Solar Spectrum." Roscoe's translation. Part I. p. 10.

[3] "Recherches sur le Spectre Solaire," p. 36.

I translate his words—"Of all the bodies iron has certainly produced the greater number of lines in the solar spectrum. Some of these seem to be common with those of calcium." Thalèn carried on this work, and if one compares the magnificent tables, which we owe to his untiring skill and industry, one is perfectly astonished to find the number of coincidences which he has so carefully tabulated.

2. There was another kind of work, a newer kind of work, going on. Observers began to give particular attention to the bright lines of flames, and the lines thickened in spots. And here I may limit myself to the general statement that the divergence between the spectra of the different substances as observed in the sun and in our laboratories was very much intensified as facts were accumulated. Very many of the lines observed in flames were lines with no terrestrial equivalents, and the spot-spectrum often contained lines much thickened, which were either not represented at all, or only feebly among the Fraunhofer lines.

3. Next, among all the metalloids known to chemists only one of them—or one substance classed as such, hydrogen—was present in the solar atmosphere, and that in overwhelming quantity; whereas the efforts of Ångström, Kirchhoff, and others could not trace such substances as oxygen, chlorine, silicon and other common metalloidal constituents of the earth's crust.

4. Then again, the layer which was produced by what was taken to be gaseous magnesium round the sun, a layer indicated by the brightest member of the *b* group, was always higher—always gave us longer lines—than that other layer which was brought under our ken by the bright line D seen in the spectrum of sodium.

Here was a distinct inversion of the chemical order. The atomic weight of sodium being 23, and of magnesium being 24, the sodium ought to have been higher than the magnesium; but the contrary was the fact, and that fact still remains after twelve years of observation.

5. As the work of tabulating the lines went on, and the more complex outpourings of vapours from the sun's interior were studied, it was found that the lines of iron, calcium, and so forth revealed to us were by no means the brightest lines—by no means the most important, or most prominent lines, but lines which really we had very great difficulty in recognising as characteristic of any particular spectrum. There they certainly were, however, mapped as very fine lines by the most industrious observers. Similarly with the spots, there was an absolute inversion of the thickness of the lines of any one substance in the spot. Surely there was a great screw loose here.

6. Closely allied to these observations we had another extraordinary fact. We could quite understand why in a spot the change of refrangibility of the magnesium lines when there was a storm going on in the sun should be different from the change of refrangibility of, say, the iron lines. The natural explanation was, of course, this: you have the magnesium gas going at one rate, the iron gas going at another rate, and that is all there is to be said about it. But it was soon found that the differences which could be sharply seen between the spectrum of a particular mass of magnesium vapour and a particular mass of iron vapour extended to the iron vapour itself. There were just as many variations in the refrangibility of the lines of iron itself, for instance, as there were between the lines of iron and other substances: that is to say, we had in the one case magnesium going at one rate and iron going at another rate; but when we came to deal with the iron lines alone we found one

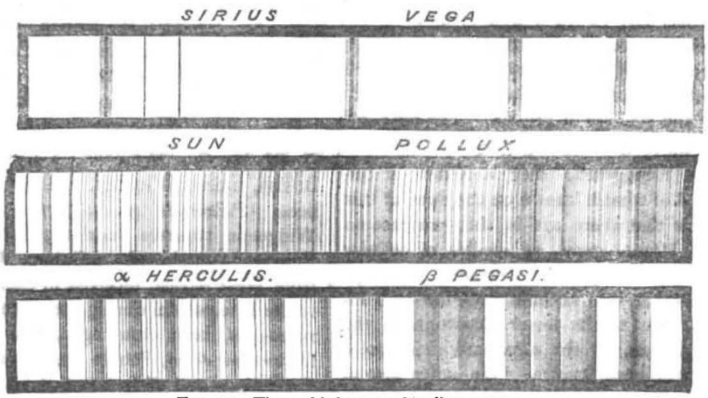


FIG. 27.—Three chief types of stellar spectra.

iron line told us the iron vapour was going at one rate, and another iron line told us that same iron vapour was going at another rate. It will be seen at once that there was a great difficulty in that.

7. Further. The lines on which these observations of the relative motions of the vapour depended were found to go in sets. In a spot, for instance, we would generally see movement indicated by one set of iron lines, whereas in a prominence we would always see a different set—a set in a different part of the spectrum altogether—registering this movement for us. Here again was considerable food for thought.

That was stated very roundly a good many years ago—in 1869. I will read what was then written on this subject:[1] "Alterations of wave-length have been detected in the sodium, magnesium, and iron lines of the spot's spectrum. In the case of the last substance the lines in which the alteration was detected were not those observed when iron, if we accept them to be due to iron alone, is ejected into the chromosphere."

That caveat with regard to iron arose from the fact that of the 460 lines recorded by Kirchhoff in 1869 only three lines of iron had been seen bright in the solar prominences.

8. Then came a point which has been very slightly alluded to already. How came it that the total chemical composition of this atmosphere of the sun, which we were taught to look upon as the exemplar of what must have once happened to our own planet, varied so enormously from the composition of the crust of our earth? No oxygen in it, no silicon, no fluorine; whereas we get abundance of titanium, nickel, and so on. It was difficult to imagine a stronger difference to exist between any two masses of matter than the chemical constitution of the incandescent sun, and of the earth, which is now cooling.

9. There was still another point of view very soon forced upon solar observers by the magnificent success which had attended the labours of Dr. Huggins, Secchi, and other observers in recording the spectra of stars. It was a most interesting inquiry naturally to see whether the stars gave spectra quite like each other, and if it should happen that they did not give spectra like each other, then the points of difference would be sure to give us some excellent working suggestions.

Now what are the facts? Here are three typical stellar spectra (Fig. 27), which show us at once that there is a very considerable difference in the phenomena. In the upper part of this diagram we have a star remarkable for the fewness of lines in its spectrum. From one end of the spectrum to the other there are not above half-a-dozen prominent lines. In the next part however we have a star which is remarkably like our own sun, both as regards the number of lines and their arrangement. In the lower part of the diagram, on the other hand, we have a star in which we get flutings instead of lines; so that we get not only a difference of degree, but a fundamental spectroscopic difference of kind. Now there is a circumstance connected with that first star with the simple spectrum very striking to any one in the habit of observing the sun, and it is this: those lines visible in the star, which, be it remembered, had been independently determined to be hotter than our sun, are precisely those lines, and none other, which we see bright on the disk of the sun itself. I have emphasised the fact that

[1] *Proc.* Roy. Soc., vol. xviii. p. 74.

we have independent evidence that the star with very few lines is hotter than our sun. It is also clear that the other star with the fluted spectrum is a star much cooler than our sun, because it was one of those red stars, the light of which is exceedingly feeble, which, on grounds independent altogether of spectroscopic evidence, are supposed to be stars in the last stage of visible cooling.

So much then for some of the earlier observations on the coincidence of metallic lines in the sun, with observations on the lines themselves in different portions of the sun's atmosphere.

10. We now come to another part of the work where we also find difficulties. Ångström, in that exceedingly important memoir which accompanies his Atlas, states:[1] "In increasing successively the temperature I have found that the lines of the spectra vary in intensity in an exceedingly complicated way, and consequently new lines even may present themselves if the temperature is raised sufficiently high." Kirchhoff, on his part, had seen phenomena very similar to those thus touched upon by Ångström, but his explanation was a different one. He did not agree that the temperature upon which Ångström laid such strong stress was really the cause at work. He attributed those changes rather to the mass and the thickness of the vapours experimented upon—nay, he went further: at a time when scarcely any facts were at his command he broached a famous theorem which went

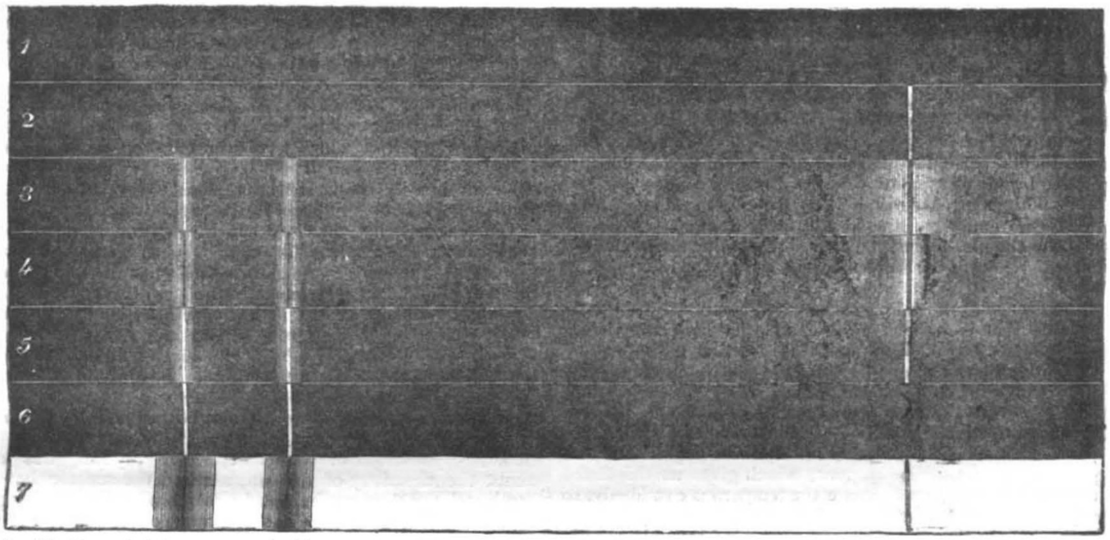


FIG. 28.—The blue end of the spectrum of calcium under different conditions. 1. Calcium combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen. 2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current. 3. The spectrum of metallic calcium in the electric arc with a small number of cells. 4. The same when the number of cells is increased. 5. The spectrum when a coil and small jar are employed. 6. The spectrum when a large coil and large jar are used. 7. The absorption of the calcium vapour in the sun.

to prove this; and yet what had Kirchhoff himself done? how had he traversed his own theory? He states that his observations were made by means of a coil using iron poles one millimetre in thickness. Now the thickness of a short spark taken from iron poles one millimetre in thickness would probably be two millimetres. Next Kirchhoff allocated the region where the absorption which produces the reversal of the iron lines took place at a considerable height in the atmosphere of the sun, and he expected the atmosphere of the sun to be an enormous mass represented by the old drawings of coronas, so that on Kirchhoff's view the thickness of the iron vapour which reversed the iron spectrum must have been, at a moderate estimate, 10,000 miles, and yet he said that the spectrum of that, and of the light given by the coil were absolutely identical; that is to say, that the *fact* was that the variation of thickness from two millimetres to 10,000 miles made no difference. That was on the one hand; on the other hand he gave us his *theorem*, showing that a slight variation of thickness would produce all the changes which Ångström and others had observed up to that time, and which we have observed since in much greater number.

A diagram (Fig. 28) will show the sort of changes to which Ångström referred, changes which have been observed by every

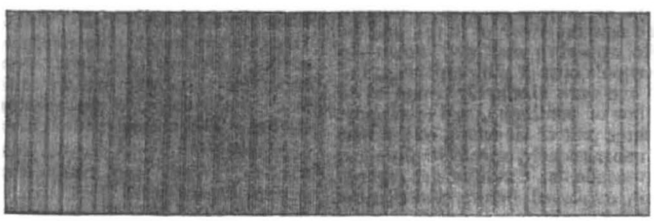


FIG 29.—Fluted spectrum of iodine.

new worker who has taken up the subject. It represents the variations which take place in the spectrum of calcium in the photographic region. At a particular temperature we get a spectrum of calcium which contains no lines whatever in the blue, but when we increase that temperature—the temperature of a Bunsen burner is sometimes sufficient to produce it—we get a line in the blue. When we pass from a Bunsen burner to an electric lamp we get this blue line intensified and reversed, and at the same time we get two new lines in the violet. Using a still higher temperature in the arc, we thin the blue line, and at the expense of that line, so to speak, we thicken the two in the violet, so that the latter equal the blue line in thickness and intensity. Passing to a large induction coil with a small jar we make the violet lines very much more prominent, and using a larger induction coil and the largest jar we can get, we practically abolish the blue line and get the violet lines alone. Now we have simply produced these effects by varying the temperature, and this diagram enables me to point out one of the things to which reference will have to be made subsequently. The thicknesses of the calcium lines in the spectrum of the sun are also given. The two lines in the violet are really H and K. The

[1] "Recherches sur le Spectre Solaire," pp. 38, 39.

other line—the all-important one at low temperatures—is feeble and unimportant. So that both on the solar evidence and on the evidence of all these spectra, whatever the explanation may be, there is the undoubted fact that fundamental changes of intensity in the lines are produced by some cause or other, and if Kirchhoff's statement about the matching of lines is true for one temperature it is false for all the others.

11. In my reference to stellar spectra I mentioned the word "fluted" spectrum. Before Kirchhoff had published his first paper two very eminent Germans—Plücker and Hittorf—were working at spectrum analysis at Bonn, and they found that in the case of a great many simple substances what are called fluted spectra were to be observed as well as line spectra.

The accompanying diagram (Fig. 29) of the fluted spectrum of iodine will show the difference between these fluted spectra and the line spectra, on which we have been exclusively occupied up to the present.

We observe that the chief novelty is an absolute rhythm in the spectrum; instead of lines irregularly distributed over the spectrum, we have groups which are beautifully rhythmic in their structure. The next diagram (Fig. 30) shows us the radiation spectrum of a particular molecular grouping of carbon vapour, that also is beautifully rhythmic; the rhythm of each of the elementary flutings exactly resembling that of the iodine.

These observations were among the first to suggest the idea that the same chemical element could have two completely distinct spectra. They were eminently suggestive, for if two, why not many?

In my reference to the "long and short" method of observation I stated that it enabled us to note what happens when a known compound body is decomposed. With ordinary compounds, such as chloride of calcium and so on, one can watch the precise moment at which the compound is broken up—when the calcium begins to come out; and we can then determine the relative amount of dissociation by the number and thickness of the lines of calcium which are produced. Similarly with regard to these flutings we can take iodine vapour, which gives us this fluted spectrum, and we can then increase the temperature suddenly, so that we no longer get the fluted spectrum at all, or we may increase it so gently that the lines of iodine come out one by one in exactly the same way that the lines of calcium came out from the chloride of calcium. We end by destroying the compound of calcium in the one case, and by destroying the fluted spectrum in the other, leaving, as the result in both cases, the bright lines of the constituents—in the one case calcium and chlorine; in the other case iodine itself. I have by no means exhausted the list of difficulties which were gradually presented to us when we considered that both in the sun and in our laboratories spectrum analysis brought before us the results of unique, absolutely similar "chemical atoms." Not only were there differences, but the differences worked in different ways, whether we passed from low to high temperatures in laboratory work, or from the general spectrum or the flame spectrum in the sun.

But I have said enough for my present purpose; details on the points I have referred to and on others must be gone into afterwards.

How then was one to attempt to grapple with these difficulties? Was it the time to found new theories? or to rest and be thankful? Was it not better to appeal to what was known—to proceed in accordance with Newton's laws of philosophising, and start no new principle unless one were absolutely bound to do so: to appeal in fact to the law of continuity, and to suppose that the explanation of a very large part at all events, of this new matter, lay in the fact that, all unconsciously, spectroscopists had been working under more transcendental conditions as regards temperature than had ever been employed before, and that the natural result was that this higher temperature had done for the matter on which they had experimented exactly what all lower temperatures had been found to do. That is to say, that they had been broken up. In other words, it lent great probability to the view that when we subjected, say iron—because it is a good thing to keep to one specific substance—to one of these transcendental temperatures, we were no longer dealing with the spectrum of iron, but with the spectrum of the constituents of iron revealed to us by a temperature at which no experiments had been made before.

And one was the more struck by the probability of this being at all events an approximation to the truth by those stellar spectra to which I have referred, and by the knowledge we possessed, that in the case of a star of the simplest spectrum we were dealing with the highest possible temperature. So the idea was thrown out that these stars were really simpler in their structure; that their immense temperature had not allowed a complex evolution of higher complex forms of chemical matter to take place; and that we had there the primordial germs of matter, so to speak, or at all events something nearer to the beginning of things than anything that we had in this cool planet of ours, or anything that we were likely to find easily here, in consequence of the various difficulties which harass every kind of experimentation. It was imagined that we might picture to ourselves a sort of celestial dissociation in the heavenly bodies which would place those stars, the spectra of which have been seen, in a different order; that the first star with lines should be a star of the simplest spectrum, the next star with lines should be that which mostly resembled our sun, and that the last in order should be that one in which the lined spectrum had utterly disappeared in favour of the fluted spectrum. If this were granted for the stars, why not attach all this to the sun? Because, as has already been mentioned, all these lines which were seen in the spectra of the hottest stars were precisely those lines which were seen most intense in the hottest parts of the sun; and it did really seem as if in that way we could eventually sooner or later—most likely later, for Art is very long—get some light on the subject.

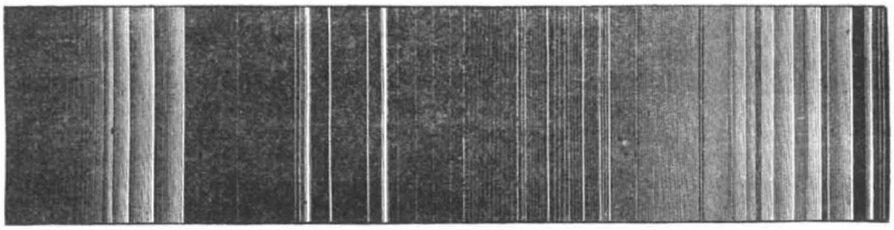
I at once say that this idea which was thrown out in the year 1873 on spectroscopic evidence had been anticipated by the foremost philosopher amongst English chemists of his time; I mean the late Sir Benjamin Brodie.[1] From considerations of a perfectly different kind he had come to the conclusion that our chemical philosophy was not anything like so firmly based as was generally imagined, and that, given a higher temperature, the elementary bodies would cease to be elementary—that the adjective "elementary" applied to them was merely the measure of our inability to dissociate them; and to watch the progress of dissociation when we got them at a temperature at our command. By a stroke of genius he, before anything was known about the chemistry of the sun, went to the sun for that transcendental temperature he was in search of; thus showing that he had an absolutely pure and accurate conception of the whole thing as I believe it to be—but that is anticipating matters. He suggested that the constituents of our elementary bodies might be found in the hottest parts of the solar atmosphere existing as independent forms. The whole merit of that conception therefore is due to Sir Benjamin Brodie, and dates from the year 1867.

Now we can easily understand, seeing that much of the spectroscopic work which had been done up to 1874 had had for its object the connecting—intermingling, so to speak—of solar, stellar, and terrestrial chemistry, that it was not a pleasant thing to find that the path seemed about to be such a very rugged one—that we seemed after all not to be in the light, but in the dark, and the very practical question was, what was to be done? Would it have been wise to have considered, *then*, the whole question of the dissociation of elementary bodies? I think it would not have been wise; the data were insufficient. The true thing to be done was, I think, to endeavour to accumulate a vast number of new facts and then to see what would happen when a sufficiently long base of facts had been obtained. What did we want? We chiefly wanted to settle those questions of the variations of spectra seen in our laboratories, and the variations observed when we passed from the spectrum, say of iron on the earth, to the spectrum of iron in solar spots and storms. The coincidence of lines of different bodies which had been referred to by Ångström and Kirchhoff also required investigation. What more ready means of doing that—what more perfect means were there than those placed at our disposal by photography? Photography has no personal equation, it has no inducement to cook a result either in one direction or the other, and it moreover has this excellent thing about it, that the results can be multiplied a thousandfold and can be recorded in an absolutely easy and safe manner. There were other reasons why photography should be introduced. We see at once that it it was quite easy to introduce the process of purification of the spectra to which I have already drawn attention, by merely comparing a series of photographs; the A, B, C of my diagram (Fig. 26) being represented, say, by iron, cobalt, and nickel, or any other substances. Again, it was quite possible by the use of the electric lamp to very considerably increase the

[1] "Ideal Chemistry." Lecture delivered to the Chemical Society in 1867, republished 1880. (Macmillan).

dispersion which Angström had employed; so that, if impurities had been suggested, there was now a method which has not yet been challenged of getting rid of them. If the dispersion was then insufficient there was nothing to prevent it being made very much more considerable, because a perfect photograph will bear a very considerable amount of magnification.

The diagram (Fig. 31) will show the method of photography that was adopted in this work, and by which the various photographs thrown on the screen were taken. The object was to compare the light of the sun with the light of the vapour in the electric arc of any particular substance that we wished to observe. By means of a heliostat and lens an image of the sun was thrown exactly between the poles of an electric lamp, and the rays diverging from it were collected by a second lens and again brought to a focus, this time on the slit of the spectroscope. The slit was provided with two slides, by means of which either its upper or lower half could be exposed, while the other half was covered. If we wished to take the solar spectrum first, the poles were separated so that they might not obstruct the sunlight; the image of the sun was allowed to fall on one-half of the slit, and the plate was exposed. That half of the slit was then covered up and the other half opened (the sunlight being cut off), and the substance volatilised in the electric arc so that its image fell on the open part of the slit. The plate was again exposed, and so the two spectra were obtained, one above the other. In this way then we had, first of all, a spectrum of the sun compared with the spectrum of the particular substance we wished to map.



Ultra-violet fluting. Blue fluting.

FIG. 30.—Carbon flutings, contrasted with the line-spectra of calcium, iron, aluminium, and other impurities of the poles.

After that we had the long and short lines in the same substance photographed on another plate. After that we had all the substances which might exist as impurities in the first substance—that is to say, all the chemical elements photographed with their lines—their long and short lines, in precisely the same manner; and finally we had a comparison of the substances we wished to photograph, say iron, with a spectrum of every other substance which might contain these impurities. It will be seen therefore that an enormous number of photographs had to be taken. As a matter of fact three or four thousand photographs have been taken, and a very considerable amount of time (about four years) was consumed in that way.

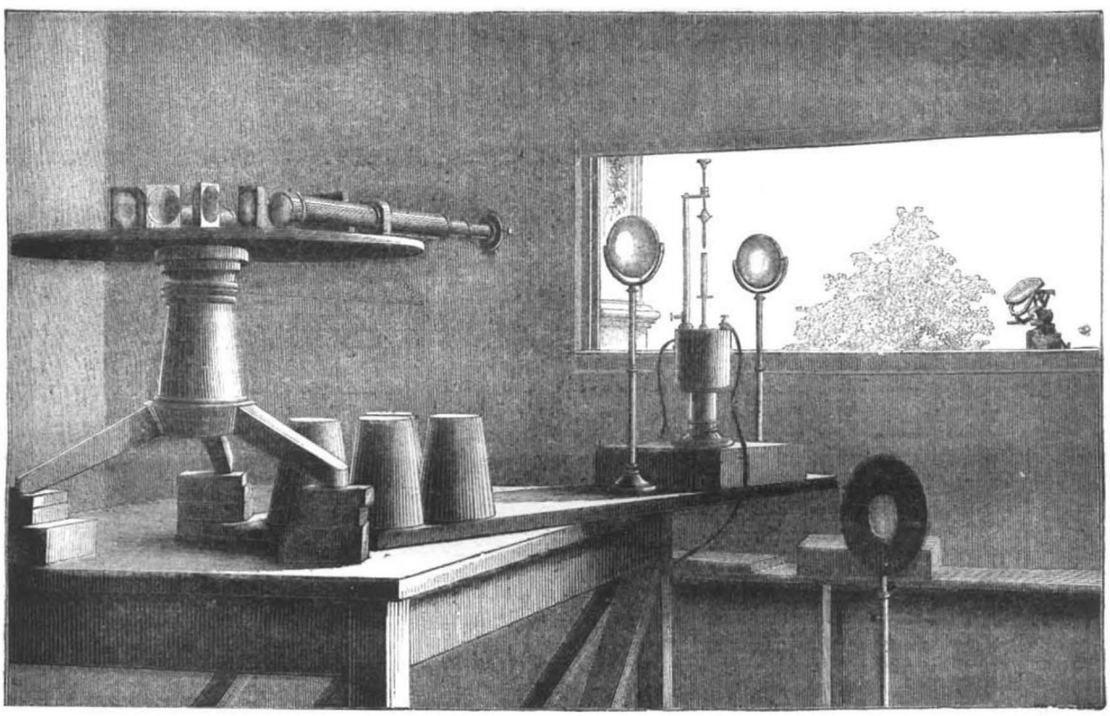


FIG. 31.—Arrangement for photographically determining the coincidence of solar and metallic lines.

But it may be said, "Surely if you are going to limit yourself to photography, you will only be dealing with a very small part of the spectrum." My reply to that is that already in the year 1875, when a part of this work had been carried on, other laboratory work had given us reason to believe that what was then being done in photography at the blue end of the spectrum would be done by photography in every other portion, for in fact a spectroscopic study of the behaviour of bodies at low temperature, to which I hope I shall have time to refer, had led several to believe—at all events had led me to believe—that what one got in the text-books about actinism and so on was but a very rough approximation to the truth. We had been taking as the functions of light what were really the functions of the

FINAL REDUCTION—IRON.

Intensity in Sun.	Wave length and length of line.	Coincidences with Short Lines.
1	39 0600 / 2	U 3, Zr 5, Yt 4
3	0622 / 4	Va 4
2	0920 / 3	Va 2, Ba 3
3	1010 / 4	Va 4, Pt 3
2	1648 / 2	Co 3
2	1755 / 3	Mn 3, Ce 4
2	1835 / 4	Os 2
1	2700 / 1	Va 2
1	2950 / 1	Mo 3
3	3023 / 4	Ce 4
5	3435 / 4	U 2
3	3475 / 2	Ba 2, Rh 2
3	3628 / 3	Ta 3
2	3975 / 2	Co 3
3	4026 / 3	Va 5
3	4422 / 4	Mo 3
3	4720 / 2	Yt 5, Th 3
2	5012 / 2	Di 2
2	5160 / 2	Ce 3, Ru 3
2	5210 / 3	W 3
2	5423 / 4	U 3, Mo 3, W 4
3	6215 / 3	Yt 5, Ce 3, Di 2
2	6571 / 2	Zr 2
3	6662 / 2	Th 1
1	7555 / 3	Os 2, Ta 4, Cr 2
3	7578 / 4	Di 2
2	7685 / 2	Va 4
2	8083 / 2	Ti 1
1	8320 / 1	Cr 3
3	9520 / 3	Ru 3
2	9750 / 2	Mo 3

bodies which received it, and it was therefore quite easy to imagine, and one was justified in hoping that as the work went on we should find, that what one particular kind of substance would do for the blue rays another particular kind of substance would do for the red rays and for the green rays, and so on. Capt. Abney in his lectures will show you that the spectroscope was no bad guide in that matter, and, thanks to his valuable researches, we are now able to photograph as well, if not better, at the extreme red end of the spectrum than we did at that time—years ago now—in the blue.

Well, then, four years were consumed in the accumulation of these facts. I do not now intend to call attention to the whole of them, but I will take some instances, directing special attention to what happened with regard to the spectrum of iron. This[1] is the final map produced up to a certain point. We have first the solar spectrum; below this are mapped all the lines of iron observed on one of the photographs which we obtained, including of course all impurities; and then follow the spectra of manganese, cobalt, nickel, chromium, uranium, cerium, and so on through the whole story. When that work had been completed in that manner we had to get rid of the impurities by the process which I have already explained, and at last we got what is called a purified spectrum, in which, along the horizon labelled iron we had only those lines left which we could not by any application of the principle which has been explained be shown to be due to the admixture of any other substance whatever. What then was the total result? The accompanying table (p. 320) will show the sort of corner in which we found ourselves after all this work had been accomplished. It gives the list of the iron lines which, after making every allowance for the existence of impurities, were found to coincide with lines in other substances.

It will be seen, for instance, that the two short lines 390600 and 395423 coincided, the first with short lines in uranium, zirconium, and yttrium, the second with short lines in uranium, molybdenum, and tungsten. Similarly there are two short-line coincidences with zirconium, and no less than six with vanadium, and so on. The total gives the coincidence of the lines of all the elements under the conditions that I have drawn attention to. So that the sum total of this really very laborious inquiry with regard to iron was that in the region between 39 and 40, the region including H and K on that map, where, before the introduction of photography, scarcely any iron lines had been seen, and where only five solar lines I think had been mapped, photography gave us a total of nearly 300 lines in the solar spectrum, and it gave us sixty-two lines of iron.

Of those sixty-two lines of iron only eighteen went straight; by which I mean that the remainder had short-line coincidences with the lines of other substances. So that the idea first thrown out by Kirchhoff, Ångström, and Thalèn of the possibility of the coincidence of lines among the metallic elements was enormously intensified. It will be seen that the thing is absolutely reversed in the case of iron, and it might be the case also in other substances. The fact of a line not being coincident with a line in another substance was the exception, and not the rule. The ratio in the case of iron being as 44 to 18.

It is amusing in the light of recent criticisms to go back to the old observations and to see with what pertinacity for the first two years we stuck to the possibility that the solar line or the iron line we were dealing with was a double line, and then, after we had to give that idea up, as the coincidences became of three, four, five, and sixfold complexity, we came to the conclusion that we were dealing with a common impurity. That of course was a point we could not settle until we had gone through all the chemical elements which were known to us, and it was going through so many substances which took up so much time.

But there was another question which became striking, in this excessively minute anatomy of even a very small portion of the solar spectrum, for I should say that the small range of the spectrum represented here forms a portion of a map which, when completed, will be the sixteenth of a mile long, so that after all we were dealing with an excessively small portion of the total work which had to be done. Having there mapped that small region, where without photography it would have been difficult to see any lines at all, we got in almost twenty cases from one end to the other, instances in which there was absolutely no relationship at all between the brightness of the iron line on our photographs and the darkness of the corresponding solar line.

These were carefully noted as "anomalous reversals," a term we coined in the laboratory at the time, and which we still use, although the word anomalous always suggests a very large amount of ignorance.

In more ways than one, then, this work landed us in rather worse confusion than we were in before. What we had to face was (1) the variation in intensity as we passed from earth to sun, a variation so great that in some cases terrestrial lines were missing in the sun, and in others feeble terrestrial lines were greatly intensified; and (2) the coincidence of lines in several spectra. That is, here and there along the spectrum we found the lines massed as it were even if the coincidence was but apparent, and it really did

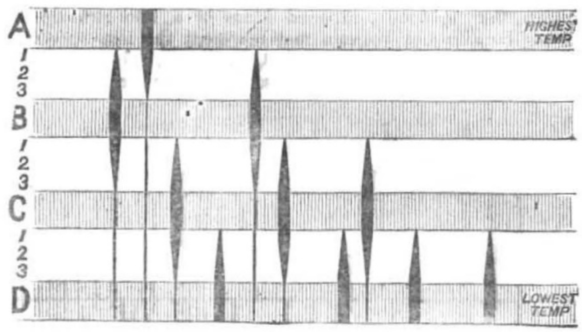


FIG. 32.[1]

seem time to consider what the effect would be, supposing that a dissociation was really going on under our eyes without our knowing or imagining anything about it. Why, it may be said, did you pitch on dissociation? For the reason that the startling results really after all contained nothing that was new—nothing that was novel about them the least in the world, if we regarded them with an absolutely unbiased and receptive mind. Dissociation would undoubtedly account for all the variations of intensity observed on passing from one temperature to another, as already exemplified in the case of the calcium lines, and moreover the short common lines, should they turn out to be truly common, which we were getting in the case of all substances, might be simply the equivalents of those short common lines of calcium which for years past we had watched coming out of the salts of calcium when decomposition was taking place. No new theory was necessary. The appeal to the law of continuity, as I said before, was really open to us, and it seemed to be our duty to appeal to it, and it was also easy to see, before really one has inquired into the matter, that if nature had built up the inorganic world in the way we now know she has built up the organic world, that precisely these facts and none other would be those she would present to us.

"Let us assume a series of furnaces A–D, of which A is the hottest (Fig. 32).

"Let us further assume that in A there exists a substance α, by itself competent to form a componnd body β by union with itself, or with something else when the temperature is lowered.

"Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be

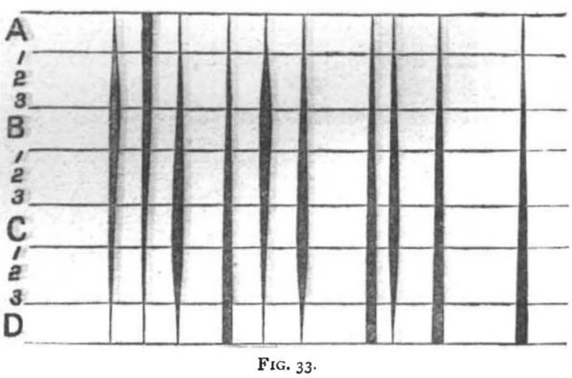


FIG. 33.

the only one visible in B, as the spectrum of the assumed elementary body α would be the only one visible in A.

"A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

[1] This map is too large and detailed to reproduce here.

[1] The figures between the hypothetical spectra point to the gradual change in the interstices of the lines as the spectrum is observed near the temperature of each of the furnaces.

"Now if into the furnace A we throw some of this doubly-compounded body γ, we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β; finally α will exist alone, and the spectrum will be reduced to one of the utmost simplicity.

"This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ, and δ all higher, that is, more compound forms of α, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

"The same reasoning therefore which shows how variation in intensity can most naturally explain the short line coincidences—lines which I have termed basic, for the line of α strong in A is *basic* in B, C, and D, the lines of β strong in B are *basic* in C and D, and so on.

"I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into α, is far below that stage, although higher than B.

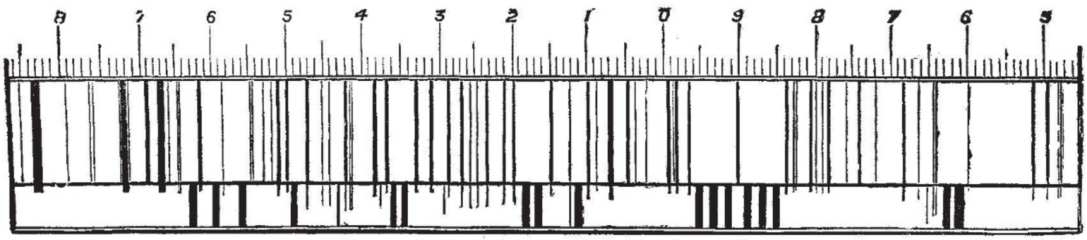


FIG. 34.—Spectrum of sun-spot observed at Greenwich.

"It will be seen from this diagram (Fig. 33) that then the only difference in the spectra of the bodies existing in the four furnaces would consist in the relative thicknesses of the lines. The spectrum of the substances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; *each line would in turn be basic in the whole series of furnaces* instead of in one or two only."

We are therefore completely justified in asking whether these are not the differences in intensities of lines to which Kirchhoff and Ångström have referred, and it is quite easy to see that if we change the temperature of the furnaces in such a manner as to produce the strongest lines, owing to the greatest quantity of the vapour given off at any temperature, that the long lines produced at these different temperatures would vary, and the longest line produced in furnace D would not be the same therefore as the longest line produced in furnace A, so that in that way we can imagine a transcendental temperature giving a very long line to a particular substance, and that substance may exist highly compounded in another substance, and yet at a lower temperature it may only appear as an exceedingly short feeble line. The result of this reasoning was, in short, to explain at once variations of intensity of the short feeble lines which were common to so many of the so-called elementary bodies.

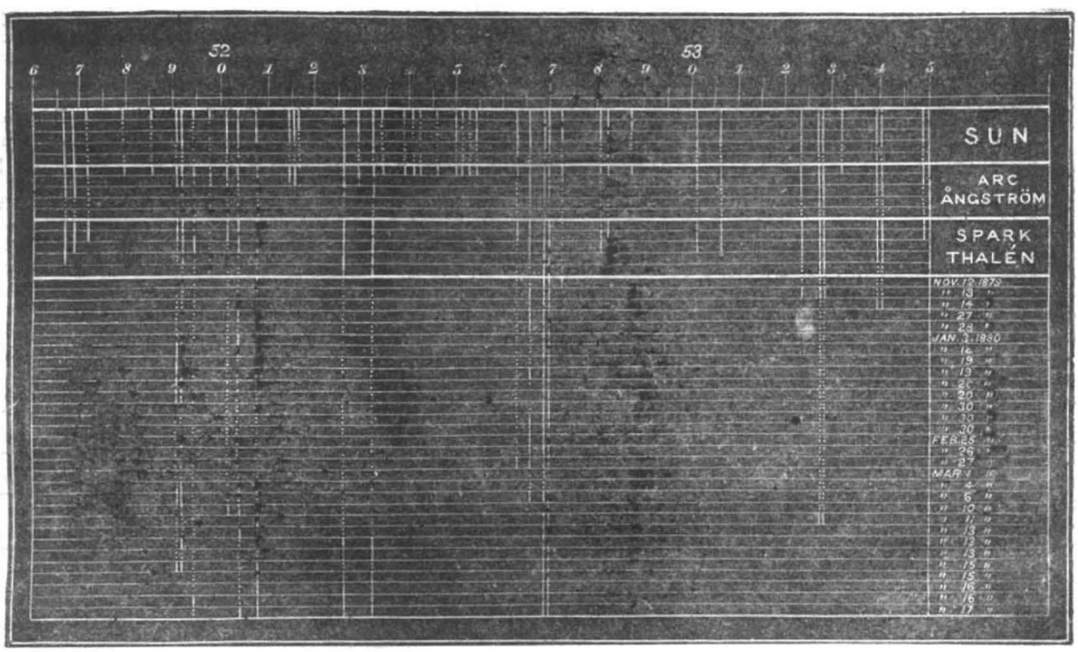


FIG. 35.—Portion of a large map showing the lines most affected in 100 sun-spots observed at South Kensington.

I am particularly anxious to point out that there is absolutely nothing new in these views. We have simply taken as our exemplar the behaviour of a known compound body, and then pushed the reasoning three or four stages further. We have gone just the safest possible way, by the easiest possible stages, from the known to the unknown.

I have now to refer, one by one, to the various tests which have been applied to these considerations, and I should now like to bring the first considerable test under notice. I shall show on a subsequent occasion the various laboratory methods that we possess of determining whether short lines are really the product of high temperature. I shall at once draw your attention to the fact that the short lines may be due, not merely to the work of high temperature, being thus truly produced by the temperature which we are employing, but they may be also the indications of excessively complex groupings which are just dying at the temperature we are using at the time. So that if it may be permitted to coin terms I should like to call some of the short lines hot-short lines, and others cold-short lines. We shall see the reason by and by.

Now if this order of things is in any way as I have stated it, the first test that we have to employ is one of excessive simplicity. The differences between terrestrial and solar spectra indicate that if the view be correct differences should be seen in the spectra of the same substances observed in different parts of the sun.

We should now have a very distinct notion of the enormous difference of temperature between the highest and lowest reaches of the solar atmosphere. The lowest region of the solar atmosphere that we can get at must be far hotter than the highest part we can get at, at all events in times of eclipses; the lines that we should see therefore in the hottest region of the sun should bring us very near to the effects of this transcendental

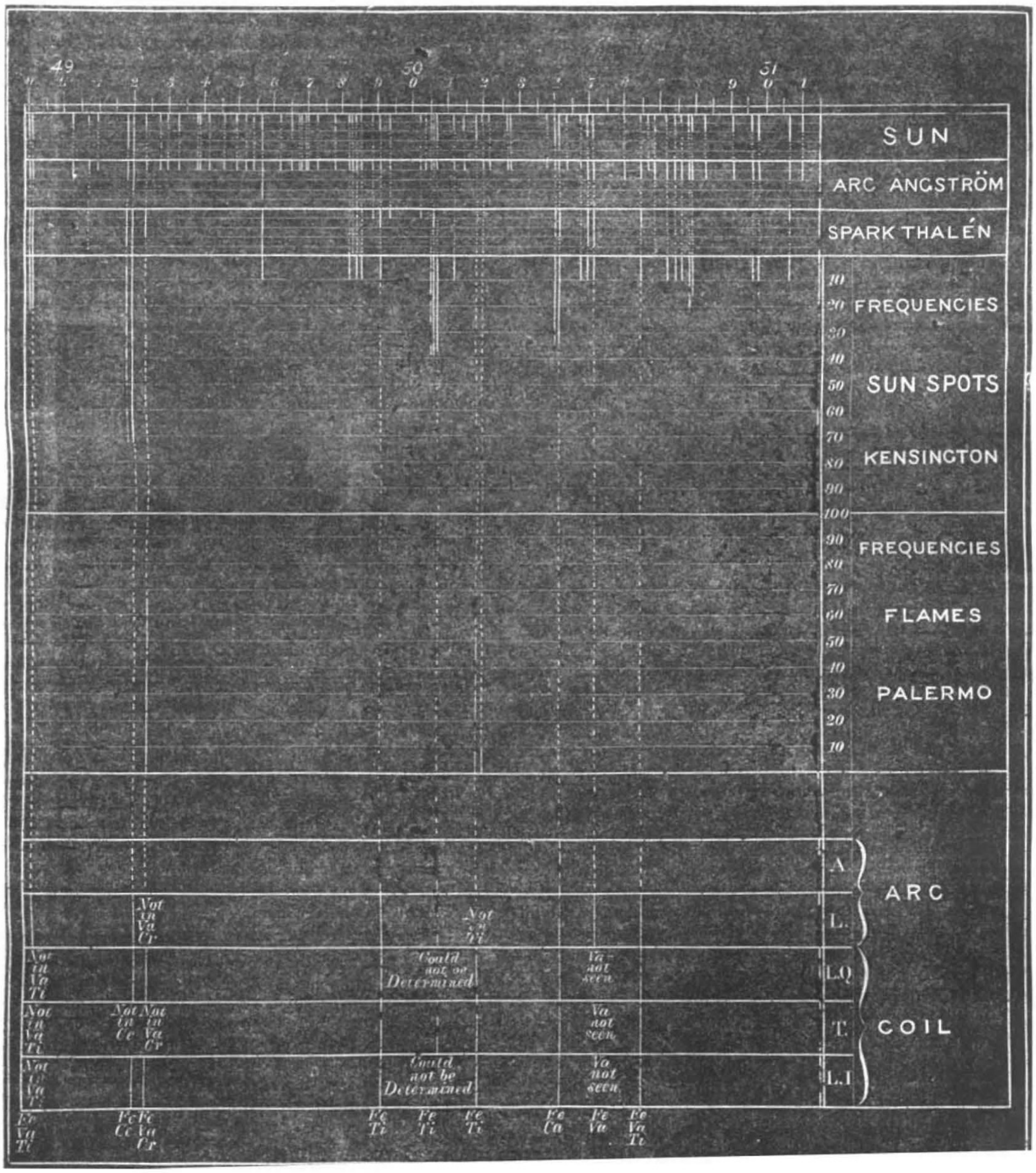


FIG. 36.

temperature to which I have referred, and the spectrum of iron seen in this way should bring us in presence of the result of the highest temperature.

Let us take then the flames as giving us the spectrum of the hottest part of the sun. Where are we to find the record of the coolest part? Now to get to this point we have had naturally to dismiss all the observations which have been made of the lines visible in solar prominences, of the lines thickened in solar spots and the like, because we know that in these prominences and spots we really are dealing with phenomena local to particular and highly heated regions.

Dealing with the whole solar spectrum we know that we are dealing with the whole of the solar atmosphere, however great, however high that atmosphere must be. Therefore we know that the solar atmospheric spectrum, the Fraunhofer spectrum, cannot by any possibility give us what is going on in any particular region—it must naturally be the summation of what is going on in every region where any absorption of any kind whatever is visible. Therefore as the spectra of prominences and of storms may be stated to be the spectra of the hottest regions of the sun that we can get at in our inquiries. The lines in the solar spectrum affected neither in spots nor flames give us an approach to the cool spectrum we are in search of. We might expect if differences were observable that we should get something like this—

Lines special to prominences	= hottest.
Lines special to spots...	= medium.
Lines affected neither in spots nor storms..	= coolest.

How have these views been tested. The first attempt made to get light out of this inquiry was one which simply dealt with a long catalogue of lines observed by Prof. Young in the memorable expedition of his to Mount Sherman, where, at the height of between 8000 and 9000 feet, with perfect weather and admirable instrumental appliances, about a month was employed in getting such a catalogue of lines as had never been got before. But it was found that, although the result of this inquiry was absolutely in harmony with these views, still after all one wanted more facts. Therefore we have endeavoured to get some of the facts here. And the way in which they have been collected is as follows:—During the last two years the spectra of 100 sun-spots have been observed in the observatory here—observed in a new fashion, and for a good reason I think. In this changeable climate it does not do to do as we began by doing—to attempt to observe all the lines acted upon in a solar spot. The excessive complication, and the intense variation of a spot-spectrum from the ordinary solar spectrum, cannot be better shown than by throwing on the screen the spectrum of one of the sun-spots lately observed at Greenwich.

The figure (Fig. 34) shows a limited part of the solar spectrum, and the lines thickened in the spot-spectrum. It will be seen therefore that to tabulate the existence and thickness and intensities of these lines over the whole of the solar spectrum would be a work which it would be difficult to accomplish in a single day, even if the day were absolutely fine. So that was given up in favour of a limited inquiry over a small part of the solar spectrum; limited further by this, that we only get the twelve lines most affected in each spot on each day. In this way we insure a considerable number of absolutely comparable observations, and we can more easily compare the spot results with those which had been obtained in the observation of the brightest lines in prominences, because when we begin to observe lines in the solar prominences one naturally begins by observing the brightest lines first. So that by observing the darkest lines first in the case of spots, one has a fairer comparison.

A diagram (Fig. 35) will show the result of our observations of 100 spots over a very limited part of the solar spectrum. We will begin by the individual observations. We have at the top the iron lines recorded among the Fraunhofer lines; below we have the iron lines recorded as iron lines by Ångström, who used an electric arc. Lower down we have the iron lines recorded by Thalèn, who used the electric spark. It will be seen that there is a very considerable difference in the spectrum of iron as viewed by means of the spark and by means of the arc, and that there is an equal difference between the spectrum of iron in the sun, that is to say, in the whole sun, determined by the Fraunhofer lines, and the spectrum of either the arc or the spark. It is also to be noted that the solar spectrum is more like the spectrum of the arc than the spectrum of the spark.

Since the relative intensities in all these cases are represented by the length of the lines, we have here an opportunity of observing and discussing the accuracy of Kirchhoff's statement that the iron lines in the sun correspond absolutely in intensity with the lines of iron seen in a light source here. It is necessary first of all to see which light source he fixes on, whether the arc or the spark. When this has been done it is found that the statement is really true with regard to neither.

That however is a digression; to proceed with the diagram, descending from this general spectrum of iron which we get by the absorption of the whole atmosphere of the sun independently of the hottest region and the coldest region—descending from the general to the particular—and taking that particular part of the solar atmosphere where the spots produce their phenomena, let us see what are the results in the case of the spots? We have in the vertical lines a record of the lines which are affected in each spot, and each of the spaces included between the horizontal lines represents a particular spot, the date being given on the right hand side; and these 100 lines which we have here represent the phenomena produced by 100 spots. The diagram is a small portion of the larger map. Now the wonderful thing that one is at once struck with is the absolute and complete irregularity of the whole result. There is no continuity among any of these lines. A careful inspection of the diagram shows us that, speaking in a general way, each of these lines is seen in one spot or another absolutely without the other. We have an *inversion* in the intensities of the lines when passing from spot to spot. Whenever we get a line intensified by Thalen, we miss it in the spots, and, as a rule, what happens is that the spectrum of the spot is not only simpler than the spectrum of the arc, but simpler than the spectrum of the spark.

Now the importance of these statements depends on other statements which we can bring to confront with them. The next diagram shows the observations of 100 prominences observed between the years 1872 and 1876. (The diagram was thrown on the screen.) Prominences exist in a region of the solar atmosphere not very far from that occupied by the spots, but we have already seen that whereas the spots are produced by a downrush of cool material, prominences are produced by an uprush of hot material. Let us see therefore if any change is produced in the phenomena; whether we shall have exactly the same lines from the flames, or the prominences, as we have from the spots; whether we shall get the same information or no.

Here are the facts with respect to Tacchini's observations:— We begin as before with the whole absorption of the sun, Ångström's map, and Thalèn's map. I think you will see a very considerable change; the iron lines (for we are only dealing with iron) most prominent in the prominences are vastly different from the iron lines most thickened in the spots. The difference is shown in the annexed diagram (Fig. 36), which represents those individual observations both of spots and flames treated in a certain way with reference to the discussion. I will at once explain to you what that certain way is. We have, as before, the three data to begin with, and we have treated the sun-spot observations so that the lengths of the lines will represent the number of times they have been seen in 100 sun-spots; the line at wave-length 4919·5, for instance, has been seen seventy-two times; *that* line, in fact, has been seen more than any other; the one at 5005·0 some forty times, and so on; very many lines having been seen less than ten times. In another part of the same diagram we have summarised the individual results obtained from Tacchini's observation of prominences in exactly the same way. The line 5017·5 was seen in 66 prominences out of 100. But why I am particularly anxious to show this diagram is this, that it brings out the perfectly natural fact—for it is the natural fact—that over this region of the spectrum, at all events, no iron lines affected in the spots are visible in the prominences. If we assume that the region occupied by prominences is hotter than the region occupied by spots, that higher region ought to do this work, and it ought to be a work of simplification. Therefore I say it is a perfectly natural result, and not one to be wondered at, that in the spectra of the flames there is no line coincident with any of the lines seen frequently widened in the spots.

Now we have these three solar spectra here which we can compare one with the other. First of all we have the iron spectrum of the sun taken as a whole. Then we have next the spectrum of spots, which we know to be hotter than the sun taken as a whole. Then we have the spectrum of flames, which we know to be hotter than the spots. It will be seen that the story, as it runs from the top of the diagram downwards, is a story of greater simplicity, as it ought to be, and it was explained in the diagram which I exhibited before I began to show these results of absolute hard facts. It will be seen that the simplicity brought about by the reduction of lines actually seen as to number, is accompanied by the appearance of new lines (produced by the transcendental temperatures) in these regions. This first discussion of a large number of spectra and of spots, as compared with storms, is, I submit, in absolute harmony with the view of the dissociation of the elementary bodies by the solar temperature suggested by Sir Benjamin Brodie in 1867, and therefore I may further add that to me, at all events, it is absolutely inexplicable on any other view.

J. NORMAN LOCKYER

(*To be continued.*)

INTERNATIONAL MEDICAL CONGRESS

THIS Congress, which opened by an informal reception at the College of Physicians on Tuesday, has so far been a real success. It has brought together something