travels were much impeded by a revolution among the natives and the prolonged drought. Nevertheless four cases, containing collections of seeds, plants, reptiles, insects, and mollusks, have arrived at Frankfort, and Dr. Kobelt has obtained valuable results concerning the geographical distribution of mollusks. With regard to the revolution among the Arabs it appears that they are of opinion that the fifty years during which the Prophet has permitted the French to hold Algiers are now over. Dr. Kobelt has left for Spain, where he will continue his researches.

WE are informed that Mr. J. M. Schuver, the adventurous Dutch traveller, who not long ago started on his formidable journey from Cairo to the Cape, is not at Famaka, on the southern frontier of Fazokl, as has been stated, but has established his head-quarters for the present a considerable distance to the south, and actually in the Galla country. A quantity of stores have lately been sent from London to Fazokl for him by way of Suakim, and it is Mr. Schuver's intention to return to Fazokl for them in November next, before proceeding on his southward journey. In the meantime he has established a dromedary post between his camp in the Galla country and Khartum.

IN the July number of *Petermann's Mittheilungen* Lieut. Kreitner describes at considerable length the observation made by him while in company with Count Szechenyi, journeying from Sayang in Yunnan to Bhamo in Burmah; a useful map accompanies the paper. Dr. Junker continues his letters describing his travels in the Niam-Niam country, concluding with some important observations on a visit he paid to some of the Monbuttu tribes. Dr. Radde concludes the narrative of his journey to Talysh, Aderbéjan, and Savulan.

WE have received from Perthes of Gotha parts 23 to 26 of the new edition of Stieler's Hand-Atlas. This edition has continued to appear with praiseworthy regularity, and will be completed in other six parts.

AMONG the papers in No. 20 of the *Bulletin* of the Lyons Geographical Society are the following:—The Economic Unity of the Globe, by Prof. C. Stewart Merritt; the South Pole, by M. E. Chambeyrin; the Slave Coast, by Dr. Chappet; South Africa, a lecture by the Rev. M. Coillaird, the missionary who succeeded Serpa Pinto; Lake Fucino, by M. Math. Desgrands.

THE U.S. steamer *Alliance*, in search of the *Jeanette* expedition, arrived at Hammerfest on its way to the Siberian Arctic Seas on the 24th inst.

THE Egyptian Geographical Society does not often issue a *Bulletin*, but when it does the number usually contains some good matter, often drawn from the archives of the General Staff, the chief of which is President of the Society. The number just published contains, among other matter, a paper on Cape Guardafui by Col. J. Graves of the Staff, and another on the country between the coast and the lofty plateau of Abyssinia by Gen. Stone Pasha.

COMMANDANT TITRE, who was formerly at the head of the Survey Department in Algiers, has lately published a large map of Algeria, which embodies all the most recent topographical information.

## SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

WE have next to consider another method, which enables us to determine the motions of the solar gases. It has been already noticed that it is easy to see the prominences rushing with extreme velocity upwards in radial lines from the photosphere, and that while they are thus being carried up by some violent motion of ejection from below, they are twisted out of the radial line, now to the right, and now to the left, by what we are justified in describing as winds in the atmosphere of the sun. Those were the mere visual phenomena which were incidentally observable the moment a method was obtained of viewing the forms of prominences as well as the bright lines produced by the vapours of which they were built up, and they afforded us an opportunity of getting an insight into solar meteorology.

It was soon however perfectly clear that there was another method, in some respects a much better method, of doing this work. When we consider how it happens that we get any phenomena visible in our universe at all, we are driven to the conclusion that it depends on the fact that bodies in a state of agitation reflect, so to speak, their own state of agitation on the ether, and that the ether carries those vibrations, those agitations to our eyes. So that if we can assume, as we must assume, that the sun with its gases, consisting of hydrogen, magnesium, &c., was communicating its vibrations to the ether, and the ether was communicating in its turn its vibrations to us, it was obvious we had there an opportunity of testing a view which had been put forward by Doppler a good many years ago, to the effect that the light from a moving light source is not the same in all its qualities as light from a fixed one.

The colours which we see in the spectrum are exactly analogous to the notes which we hear in a piano when we go from one end of the scale to the other. Doppler imagined the equivalent of a piano going away from or coming towards the listener with considerable velocity — a velocity comparable, in fact, to the velocity of sound through the air. It is perfectly clear that under these circumstances we should no longer get true concert pitch, for the reason that the note which gives us a certain tone, because it produces in the air so many waves per second, will change its tone if the source of the note is coming to us. Take, for instance, a tuning-fork giving concert C, and imagine it rapidly coming to us: the waves of sound will be crushed together, we shall have more waves in a second falling on the ear, and we shall get a higher note. If we imagine, on the other hand, the tuning-fork is going away from us, the notes will be paid out at longer intervals, so to speak, and we shall get a lower note. In neither case shall we continue to have concert C. A very familiar instance where we do get this change of pitch due to change of motion, is produced in these days of very rapid railway travelling. Any of us who have been at a country railway station when the express is coming by will know that as the train approaches us the note of its whistle is at one pitch, and as it goes from us after passing it changes and gets lower, according to the velocity of the train. A familiar experimental illustration of this principle is to attach a whistle to the end of a long india-rubber tube. If then a person sounds the whistle by blowing through the open end of the tube, and while still blowing whirls it round rapidly in a vertical plane in which an observer is standing, that observer will note that when the whistle is approaching him in one part of the curve, and the waves are therefore being crushed together, the note will appear higher than when it is receding from him in the opposite part of the curve, where the waves are being, as it were, pulled asunder. Now apply that to the light of the sun. The long notes of light are red, and the short notes are blue, and if we sharpen or shorten any light note in any part of the spectrum we shall give that light a tendency to go towards the blue, and if we lengthen or flatten it we shall give it a tendency to go towards the red, so that, for instance, if a mass of magnesium gas giving the line or note in the green indicated by "*b*" is approaching us with a velocity comparable to the velocity of light, the line will change its position in the spectrum towards the blue; and if we are careful to note the exact amount of change of refrangibility as it is called, we shall have then an absolute method of determining the rate of motion of that mass of gas. This will help us in more ways than one. Suppose we observe the gas at the limb of the sun, we shall then, if we get any change of refrangibility, be justified in calling it a solar wind, because the motion thus indicated would be very nearly parallel to the surface of the sun; but if on the disk of the sun itself—take a spot, for instance, in the very middle of the disk—we get any change of wave-length such as I have referred to, it is perfectly clear that we shall no longer be dealing with what we can justly call a wind, it will really be an upward or downward current. So that this principle enables us at the limb of the sun to determine the velocity of solar winds, and at the centre of the sun to determine the velocity of those up-rushes or down-rushes, in fact, those convection currents to which Prof. Stokes has already directed attention.

The accompanying drawings (Fig. 16) were made when the sun was in a considerable state of agitation in the year 1872. They give us one of the lines of hydrogen, and indicate, I think, amply this kind of phenomenon. We have in the first figure on a large scale the "F" line of hydrogen, the line in the green at the edge of the sun. The slit—the perfectly straight slit—has been worked round the limb in search of a prominence, and it has found one. But the slit is no longer shown us as a perfectly straight line, it is in fact a very irregular one; and further than this it branches

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[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150) Revised from shorthand notes. Continued from p. 274.

at a certain height. The line of hydrogen has really divided into two lines of hydrogen, so that there we get, according to the principle just laid down, an indication of the fact that the hydrogen up to a certain height was very nearly at rest, and that beyond part of it was torn away, the line being deflected towards the blue, indicating that it is approaching us. Now the other Fraunhofer lines in the diagram may be looked upon as so many milestones which enable us to measure by the deflection the number of miles traversed by the gas in one second; for these deflections are nothing more or less than alterations of wave-length, and, thanks to Ångström's map, we can measure distances along the spectrum in $\frac{1}{100000000}$ mm., and we know that an alteration of $\frac{1}{10000000}$ mm. in the wave-length of the F line towards the violet means a velocity of thirty-eight miles a second towards the eye; and that a similar alteration towards the red means a similar velocity from the eye; so that carrying the part of the line which has the greatest deflection from the normal down to the dots, we find that the velocity of the solar wind under observation at that time was something like 114 miles per second.

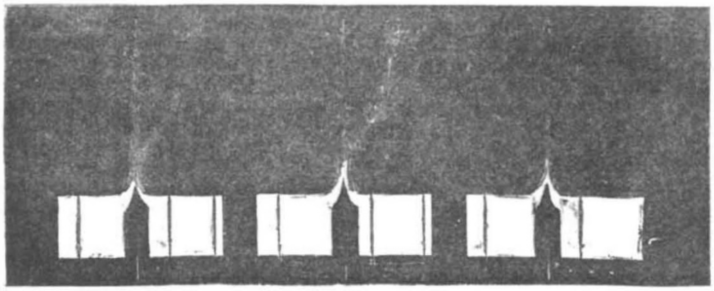

FIG. 16.—Alterations of wave-length in prominences. The dots show $\frac{1}{10000000}$ mm.

In the second figure this same prominence is seen a short time afterwards. The tremendous rush of hydrogen has descended somewhat nearer the sun, and bringing that in the same way down to our milestones, we can give that velocity at something like fifty miles per second. The wind velocities measured in this way have amounted to 140 miles a second. The phenomena of convection currents give us velocities which very often amount to forty or sixty miles a second.

This method enables us to determine a matter which a few years ago we could not have determined in any other way. I refer to the fact that the motions of the solar winds are to a very large extent cyclonic. These various effects have been produced by varying the position of the slit a very little indeed over a small prominence.

In the first of the accompanying diagrams it will be seen that the hydrogen line indicates by its change of refrangibility that the gas is receding from us, that the waves are being lengthened out, and that they therefore have approached towards the less refrangible end of the spectrum. In the third diagram we see that in that part of the prominence the rays were being deflected towards the violet; that is to say, they were approaching us. In the middle of the prominence we get indications that they were both receding and approaching, as shown in the second diagram. Now if anybody in the moon had as good a method as this of measuring an earthly cyclone, he would see exactly this sort of thing—the part of the cyclone receding from him would give a deflection in one direction, the centre of the cyclone would give him both deflections, because he would get currents going in both directions, and on the other side of the cyclone he would get a deflection in the other direction.

So obvious and so very definite did these observations at last become that a new word had to be coined to separate the forms of the prominences as seen with a widened slit from the forms which were assumed by the prominences depending on their rate of motion.

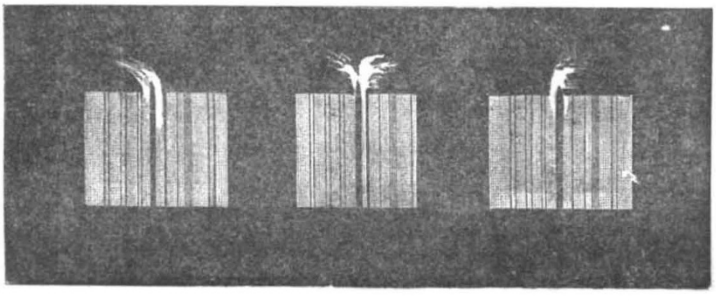

FIG. 17.—Solar cyclone. Left-hand diagram, retreating side of cyclone on slit; centre diagram, both sides on slit; right-hand diagram, advancing side on slit. The right-hand side of each diagram is the most refrangible.

Fig. 18 is a diagram of what have for this reason been called "motion forms," because such forms are really not the forms of the prominences at all—have nothing whatever to do with the shape of the prominences, but are simply produced by the various changes in the refrangibility of the light brought about by the varying motions in different parts. It is a very remarkable fact, noticed at the time, that some prominences seem to be shot up like so many smoke rings—little cyclones. And many of the strangest motion-forms are due to this cause. The velocities in the same prominence vary very much from the time it leaves the photosphere until it arrives at its greatest elevation in the sun's atmosphere, indeed the variations in any one prominence are almost as great as the variations observed between any two prominences.

There is another important fact connected with this: when the phenomena are observed close to the limb it is very often seen that the dark line on the surface of the sun is broken; in fact we got a doubling of the dark "F" line in exactly the same way as we got this doubling of the line in the prominence itself. That taught us that not only were these motions enormous in the case of vapours ejected from the sun, but that the subjacent parts of the sun itself—of the photosphere rather—felt that same influence.

The next point observed was (and this was an observation very difficult indeed to make near the limb) that whenever we got any very considerable velocity we got a new order of phenomena altogether, indicated in these two diagrams (Figs. 19 and 20).

It was found that the absorption of the hydrogen, or of the magnesium, or of the sodium, as the case might be, was enormously reduced; that for that part of the sun there was practically no absorption; but instead of absorption an excessive brilliance in that part of the spectrum where the dark line would otherwise be. In the brighter portion between the two small spots (Fig. 19) the absorption is replaced by an exceedingly brilliant radiation, so brilliant indeed that it is quite impossible to draw a diagram so as to give any idea of the intense brilliancy of some of these little spots of light which one sees in the spectroscope; they fatigue the eye enormously, although they cover such a very small portion of the field of view.

Accompanying this intense radiation there is a gradual fading away of the absorption line; it wanes, and fades, and becomes almost invisible; while, on the other hand, on the other side or in other places, instead of getting a brilliant patch of light of the same width as the "F" line, we get one many times broader. We have also the absorption deflected to the left, or red end of the spectrum, and on this side it is gradually fined or eased off, so that it is very difficult to determine exactly where this broadened, deflected "F" line actually ceased to give us absorption; whereas at the other side, where it changed its refrangibility towards the blue end of the spectrum, we have an enormous patch of light. Now the explanation of that is perfectly simple; we have at one part of the spot an enormous up-rush, an ejection of hydrogen so intensely hot that it declines naturally to absorb the light from anything behind it, because it finds nothing hotter. This gradually replaces the absorbing hydrogen which was driven down again with considerable velocity, and so changed its refrangibility towards the the red.

Enough has been said already to show that this method of studying solar phenomena *in situ* has really helped us enormously with regard to the chemical nature of the sun. We can allocate the absorption of the hydrogen, magnesium, and so on; we can see *where* they are absorbing, and in the phenomena just referred to, where they cease to absorb, we get bright lines.

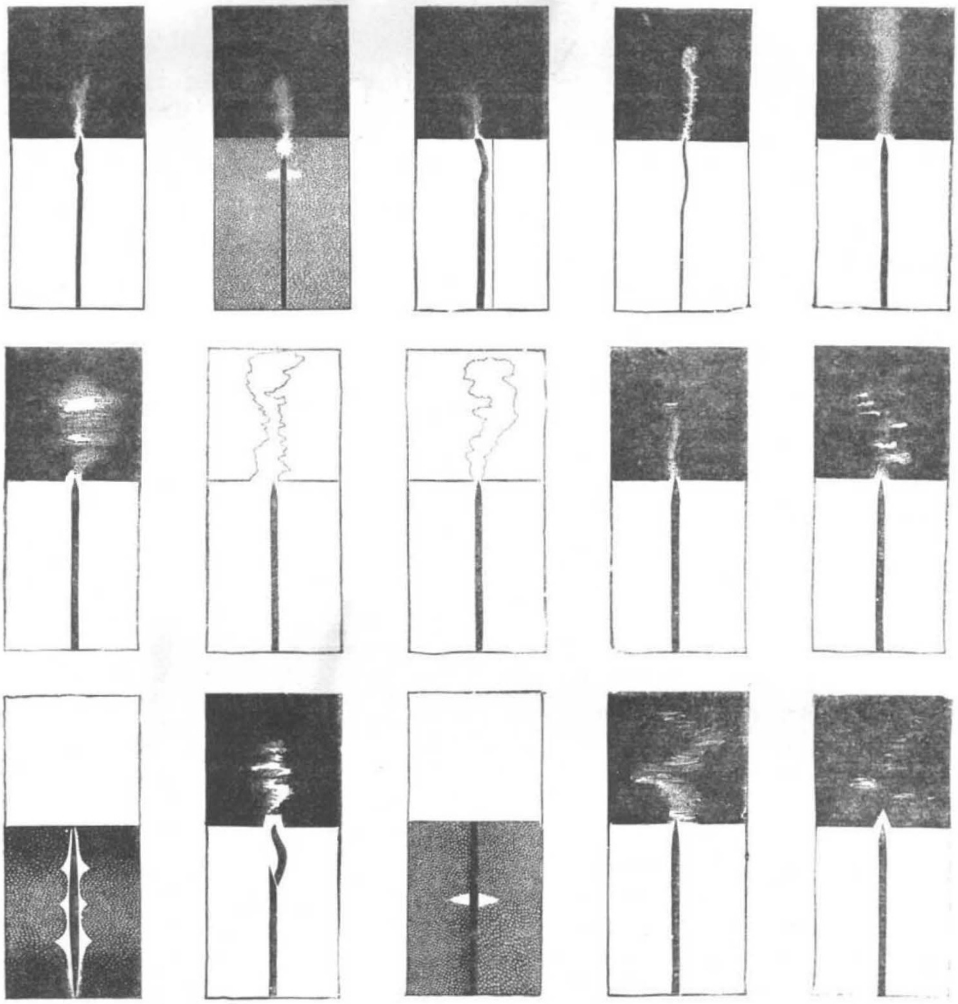

FIG. 18.—Motion-forms.

What, then, was the totality of the knowledge which had been acquired a few years ago with regard to the chemical nature of the sun's atmosphere taken as a whole—the sun's atmosphere from the upper reaches of the coronal atmosphere down to the region where, doubtless, the spot phenomena are located?

I have two little glass vessels here which ought to point what I wish to say. I have here hydrogen arranged so that I can make t luminous with a minimum of agitation. If we examined it with the spectroscope, we should find it would give the F line alone, there is nothing red about it. Now there is a region around the sun which gives us something very like that in colour, and something absolutely like it, so far as the result of spectroscopic observation is concerned. Now we have in this other little tube hydrogen in a condition to be considerably agitated, because instead of allowing it to occupy a globe, it is arranged so that the electric current has to pass through a fine capillary space in which the gas is inclosed. That is a condition which is supposed to give us the effect of high temperature. This really does give us something like what we see in the next lower solar region. This is exactly the same gas as we have in the globe, but it is treated differently, and the effect is widely different. As we pass from few encounters of molecules to many it is very much more luminous, and it is red. The level which gives such a spectrum as is got from the capillary tube is considerably lower than the one which gives us the F line alone (Fig. 21).

Is this all? By no means; going further down, as was pointed out at an early stage of the work, we get some lines seen in the spectrum of magnesium all round the sun at certain periods of the solar activity. Underneath this again we get a layer in which lines seen in the case of sodium are almost as constantly seen. Still a lower depth—practically there is no end of them—in which we get the lines of iron and other substances. There are many lower variable layers depending upon local disturbance. Tacchini, an eminent Italian observer, has studied these very carefully. We have by these observations a means of determining the fact that the solar atmosphere consists of what may be very conveniently and justly called a very considerable number of layers; and what happens with these layers is this. If the sun is quiet, or if we observe any particular part of the sun at any particular time at which it is not agitated, the layers visible at that time, few in number, are nearly concentric (Fig. 21), but the moment there is any agitation in any part of the sun the lower layer shoots up into the next layer above it; the next shoots up into the one next above that; and so on (Fig. 22). How far into the very confines of the solar atmosphere this sort of action goes we do not know, because it wants more time to observe than is afforded by an eclipse, but it is certainly known that from the very lowest layer to the upper hydrogen one the layers are made to obey this same sort of rhythmic movement, and extend over like so many shells, so many domes on every part of the sun, which is being most violently agitated at the time.

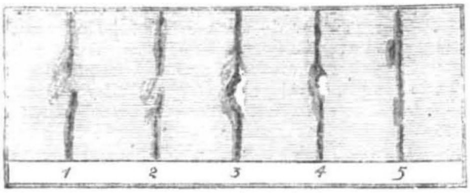

FIG. 19.—Contortions of F line on disk. 1 and 2, rapid downrush and increasing temperature; 3 and 4, uprush of bright hydrogen and downrush of cool hydrogen; 5, local downrushes associated with hydrogen at rest.

So far then we have so many shells, so to speak, so many thinnings out.

Tacchini's work shows well that observers have gone into considerable detail. I give one of his drawings (Fig. 23).

The figure shows two separate portions of the chromosphere, and below each portion is shown the height above the photosphere to which the various substances indicated by the lines given extend. Thus it will be seen that the magnesium stratum reaches the greatest elevation, next the so-called 1474 stuff, then an undetermined substance giving a line at wave-length 4923, another giving a line at 5017, then sodium, then a substance giving a line between B and C, another with a line between B and A, and finally one with a line at 5369. The two last layers were not observed in the second portion shown. It will be observed that most of the lines seen in these small prominences belong to substances with which we are totally unacquainted on this earth.

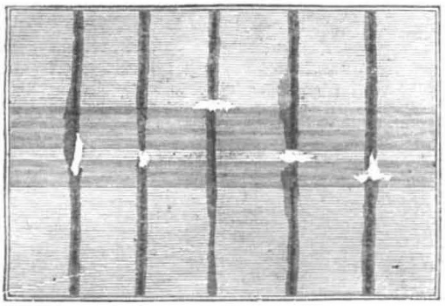

FIG. 20.—Contortions of F line on disk, in connection with spots and uprushes of bright hydrogen.

So much for the first results obtained in localising the solar chemistry. We pass from a general theory, saying that the absorption is above the sun, and that the sun consists of such and such chemical substances; we go to a very much more complete picture, in which we say that the solar absorption is built up by vapours of so and so, and so and so, corresponding to different heights, changing their forms, changing their shapes, changing their quantities at different times, some of them being more particularly visible in the bright ejections from the interior called prominences, and others again being brought to our ken in those down-currents called spots.

Attention must next be drawn to another method of observation, or rather to the same method extended to a different line of work.

Kirchhoff, when he examined the sun as a whole, compared it with the light of a light source as a whole. So far we have seen the difference in the results obtained when we pass from the question of observing the sun as a whole to that other more detailed question of observing every little bit of the sun that we can get at.

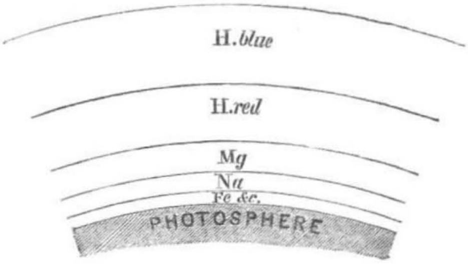

FIG. 21.—Stratification of the solar atmosphere.

Now is it worth while to do this with the light source?—that is the question. Take the case of the volatilisation of iron in an electric arc. It is obvious that light from every part of a light source placed in front of a slit must enter every part of it; and if there are any differences between the light proceeding from the upper pole or the lower pole, or from the globule of iron which is being melted, and exists in a liquid form, or from the vapours of iron which surround that liquid globule—if there are any differences in these, those differences must be absolutely lost, for the reason that light from all these parts of the very compound phenomenon we are observing will pass to every part of the slit. But if we introduce a lens between the light source and the slit of the spectroscope, if as we throw an image of the sun on the slit, so we throw an image of the light source on the slit, we ought really to bring about a very considerable difference. For instance, we ought to be able to focus the light on the slit in such a way, that if there are any differences we should see them. It is difficult for us on a small scale to see whether there are any such differences, but if in an electric lamp we so volatilise a piece of iron, and throw the image on a screen, we readily see that there are very considerable optical differences in the various parts of the image of the light

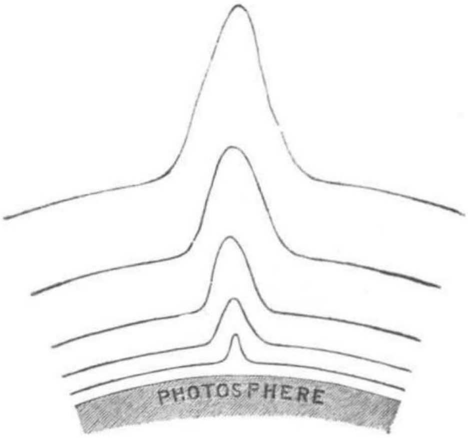

FIG. 22.—Stratification of solar atmosphere, showing the upheaval of a prominence.

source. We have the upper and lower pole, the globule of iron volatilising, and the vapour, both in the arc, properly so-called, and the accompanying flame. It is obvious that if we throw the image of the arc on the slit we can examine the vapour without getting any light from the pole. It is also obvious that if we arrange the slit horizontally while the current is passing in a vertical direction from one pole to the other, we shall be able, by moving the slit upwards, to see if there are any differences observable in the vapour, first in the region where we have intense boiling and volatilisation going on, and in the necessarily cooler region where the arc is in contact with the outer air. Photographs taken in this manner show what is really observed in the case of iron under these circumstances. Whether we use the artifice of a horizontal arc with a vertical slit, or a vertical arc with a horizontal slit, does not matter, provided we keep the slit immersed in the light of the arc, and thus reflect the light from the poles, and at the same time arrange the slit so that we can compare the light in the interior portion of the light source with the light nearer its boundaries—if we take all these precautions we shall then get in the case of every substance such a result as here exhibited (Fig. 25). We have in the centre a complete spectrum, its intensity being gradually toned down, and some of the lines being left behind as we look up and down towards the boundary where we have the spectrum of that portion of the arc which was the last to retain its luminosity in consequence of its cooling. If we take horizons from the central portion of the diagram to the point furthest distant from that central axis, we find at last the light becomes absolutely monochromatic. The iron vapour at this distance from the central axis really was only radiating to us the vibration rendered visible to us by that one line. As we get nearer and nearer the centre of agitation the spectrum becomes more complex, until at length when very near the central axis we get a great many short lines introduced, so that the spectrum at that point is most complex. This I am anxious to draw attention to with some insistence, because we shall understand at once the terms long and short lines from this diagram, and about those long and short lines there will be a great deal in the sequel.

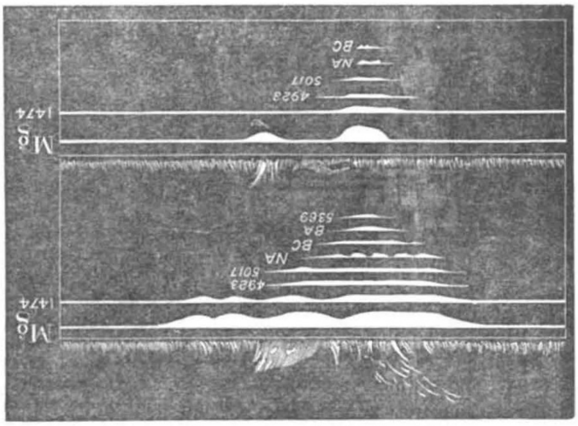

FIG. 23.—Chromosphere with jets (Tacchini).

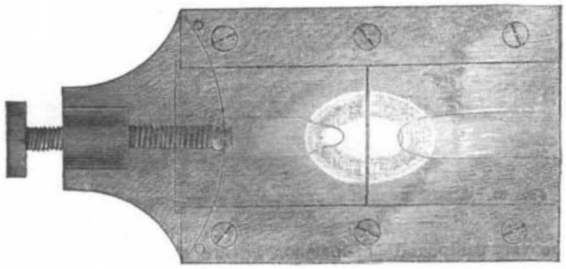

FIG. 24.—Arrangement for obtaining long and short lines. Image of the horizontal arc on slit plate of spectroscope.

The figure shows the much more simple spectrum of sodium.

In all cases under the conditions mentioned it is quite easy to obtain photographs of longs and shorts; the longest line in the middle is D, that to the left the line is the green, and we find that one line excels all the others, and reaches a greater distance from the central axis of the photograph.

An electric lamp can be arranged to show the long and short lines of sodium on a screen; the arrangement is rather a delicate one, but the point is that we have not, as in the case of the other electric lamps, vertical poles, but horizontal ones, and we have a vertical slit close to the horizontal poles in the very middle of the lamp, so that if the experiment is carried far enough we can then prove the accuracy of the statement that the line is an image of the slit, because the slit generally melts, and we see the shape of the lines varying on the screen as the melting goes on. The lines are of different lengths: the yellow is longer than the green, the green longer than the red, and so on.

Results obtained by this method have a very important bearing upon every question connected with solar spectroscopy. When these spectra were observed—the spectra of the longs and shorts, of course we had a perfectly new set of phenomena to deal with. In all preceding spectra all the lines had been practically shown of the same length, or else the lengths had represented their intensities. But here we had, in the case of each chemical substance, to deal with the remarkable fact that when that chemical substance was examined in this way, some of the lines were long and some of them were short, and the question naturally arose, how is it that some of the lines are long and some of them short? That question was an exceedingly difficult one to answer: I do not know that it has been thoroughly answered yet; but while researches were being made for the answer to this question certain general statements became possible which are of very considerable importance to us in our inquiry. Such a general statement as this, for instance, that if we take, say, some iron, observe its spectrum, and then mix some manganese with it, and observe the spectrum of the mixture: if the quantity of manganese is very small, we shall only get the longest line of manganese; if the quantity of manganese is increased, the next longest line will come in; and so on. So that if the spectrum of any specimen of iron was photographed, it was at once easy to see whether there was an impurity of manganese in that iron. If you make the admission that the spectra of iron and manganese, and so on, were the spectra of bodies not decomposable at the temperature which you were employing—if, for instance, there was a great quantity of manganese existing as impurity in the iron—you got a great many lines, and of course with the quantity of admixture the number of lines would go on increasing until you had 50 per cent. of each, when you would have the greatest number of lines of iron and the greatest number of lines of manganese you could ever get together, but in no case then would you get *all* the lines of iron, or *all* the lines of manganese.

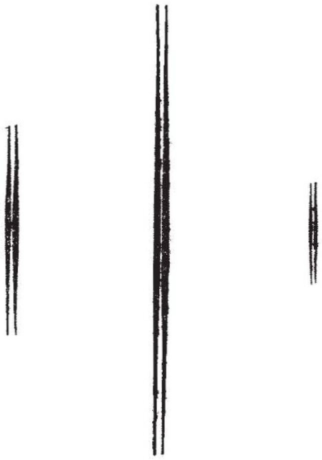

FIG. 25.—Spectrum of sodium, showing the long and short lines.

The great importance of this result was, that it enabled any spectroscopist, or any chemist who chose to take the trouble and devote the time to it, to examine as to the existence of impurities in different substances: not to determine the absolute amount of impurity, but enabling him to say that in specimen A there is a greater impurity of X than there is in specimen B, or there is a greater impurity in specimen Y of article A than there is in specimen Z, and so on. The statements were not absolute, but they were relative, and being relative they were certainly a very great advance on anything which had been done before, because until this question of longs and shorts was introduced it was almost impossible to see how to eliminate impurities.

There was another matter: it was easy to determine the behaviour of compound bodies under the action of heat by such a method. For instance, if we took the salts of calcium, or of strontium, salts which have as perfect and as complete spectra of their own as iron itself—if we heated them properly, that is to say, if we did not employ too high a temperature, and did not give them a chance of oxidising, it was exceedingly easy to see how these would behave when the heat was gradually increased, and it was then found that the longest line of the metal was always the one which showed itself first. In fact the metal always behaved as an impurity, and brought out this longest line first, in exactly the way that the smallest quantity of impurity would do. Those are small examples of the work which was done, in the one case by working at a constant temperature, and in the other case by working at varying temperatures; and you see it was possible in this way to prepare maps in which all the various impurities of one substance in another may be eliminated. A diagram will explain the way in which this new knowledge could be utilised. We have, for instance, a great number of photographs of iron, cerium, vanadium, and a great number of other chemical elements. We have compared the spectrum of each of the chemical elements with all the others, compared the lines of iron with cerium, titanium, and so on. The question now is, Given these photographs bristling with impurities—for if there were no impurities present in these photographs we should not know that our photograph was a good one—how are we to produce a map which shall be absolutely purified, in which none of these impurities shall have any effect? This diagram (Fig. 26) will show the process which was rendered possible by this long and short series of observations. We have there mapped three spectra, with their long and short lines. We have compared A with B, and we find that in the photograph which gives us A compared with B we have so many lines of the two substances. Now we say if B exists in A as an impurity, the longest line of B will be there. We look for the longest line of B, and we find it, and we put a minus sign over that line in A to show it is most probably due to an impurity of B. We then ask if there is any more B in A, and we naturally look for the next longest line of B; we find that, and we put a minus sign over that, and then we look for the next longest line, and mark that; then we look for the next one—it is not there—then there is no more of B in A. In that way, if we knew everything, we should years ago have been able to determine a spectrum of a substance A, from which all traces of the spectroscopic effects due to the presence of a substance B, had been eliminated, and we might go on with substance C, and so on, and in that way eliminate the effects of C as well as B from the substance A.

I am the more anxious to insist on this work because I shall have to show subsequently that it took a very long time to execute it; that the work is of a very rigid nature; and that, so far as I know, no other suggestion has been made with regard to obtaining pure spectra; and of course, if we wish to study the physics of the sun—especially the chemical physics of the sun—the first desideratum, as Kirchhoff saw, and as Ångström saw, and as we all see now, is to have a series of maps absolutely and completely beyond all suspicion.

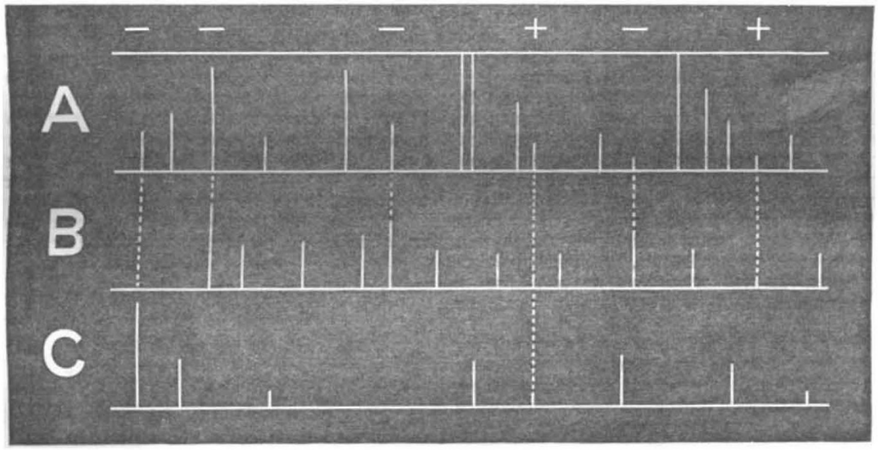

FIG. 26.—Diagram showing the process by which impurities are eliminated from spectra. The lines marked — are due to impurities of one substance in another; those marked + are common or basic lines.

There is one other question to be referred to. Was the way perfectly clear, taking the work as it stood, four or five years ago? Did our chemical theories then explain all the facts which had been gathered by many men in many lands touching this localisation of the solar chemistry? The localisation had depended on using existing maps, whether tainted with impurities or not, observing the lines in all prominences and spots. Was everything, I say, quite clear, let us say, five years ago? I shall have to show that things were by no means at all clear; that any one who took the trouble to bring together all the results which had been obtained up to that time would have found not only that there was a rift in the lute, but that there was a very big one, and that the discord which grew upon one as one went into detail either with regard to the spectrum of the spots or with regard to the spectrum of the prominences, or with regard to the general localisation of the solar layers, was really very much more remarkable than the accord, and that although, of course, an immense deal had been done towards elaborating a view of solar chemistry a great part of which would stand, still there was a great deal which required a considerable amount of attention and a great deal more which suggested that there was still a higher light to be got before we could really face the magnificent problem with which we are attempting to grapple.

J. NORMAN LOCKYER

(*To be continued.*)

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## ANCHOR ICE

IN an address recently delivered at the Annual Convention of the American Society of Civil Engineers in Montreal, Mr. James B. Francis, the President, gave, *inter alia*, the results of his observations, during forty years, of anchor ice. The following is the passage in question:—

A frequent inconvenience in the use of water-power in cold climates is that peculiar form of ice called anchor or ground ice. It adheres to stones, gravel, wood, and other substances forming the beds of streams, the channels of conduits, and orifices through which water is drawn; sometimes raising the level of water-courses many feet by its accumulation on the bed, and entirely closing small orifices through which water is drawn for industrial purposes. I have been for many years in a position to observe its effects and the conditions under which it is formed.

The essential conditions are, that the temperature of the water is at its freezing-point, and that of the air below that point; the surface of the water must be exposed to the air, and there must be a current in the water.

The ice is formed in small needles on the surface, which