He shows that the work already done enables him to confirm the presence of Sr, Pb, Cd, K, Ce and U, and also that it indicates the probability of the presence of Va, Pd, Mo, In, Li, Rb, Cs, Bi, Sn, La, Gl, Yt or Er.

## IV. Note on the Bright Lines in the Spectra of Stars and Nebulæ." By J. Norman Lockyer, F.R.S. Received December 31, 1877.

Owing to absence from England in April last, I have only just become acquainted with Dr. Huggins' paper, in reply to that by Mr. Stone, on the above subject. As Mr. Stone has again directed attention to the matter, I am anxious to say that I agree with him so entirely* that two years ago I searched for indications of a large chromosphere in the case of α Lyræ and some other stars. I believe I have had glimpses of bright lines at *F* and *b*, but if this discussion had not arisen I should

* I append an extract from a lecture on the Structure of Nebulæ and Stars, which I gave at Manchester in the autumn of 1876 ("Manchester Science Lectures"), to show the perfect accord there is between us. "There are nebulæ and stars with spectra so similar that if one had the evidence of the spectroscope alone, it might be impossible to decide which was nebula and which was star. Now this may be a little startling to some of you, and therefore it is only fair I should explain it. The stars, you know, are so remote from us that in the most powerful telescopes to which spectroscopes are applied, they appear only as the finest points of light. Now these points of light, it is not absurd to imagine, may in some instances be two millions, or perhaps even three millions, of miles in real diameter. We know that our own sun, which is certainly not the largest star in the heavens, is nearly one million miles in diameter; that is to say, the true sun, the true stellar nucleus, is one million miles in diameter. Now when I dealt in my second lecture with the physical constitution of the sun, I pointed out that the sun which we see, the sun which sends us the majority of the light we receive, is but a small kernel in a gigantic nut, so that the diameter of the real sun may be, say, two million miles. Suppose then that some stars have very large coronal atmospheres; if the area of the coronal atmosphere is small compared with the area of the section of the true disc of the sun, of course we shall get an ordinary spectrum of the star; that is to say, we shall get the indications of absorption which make us class the stars apart; we shall get a continuous spectrum barred by dark lines. But suppose that the area of the coronal atmosphere is something very considerable indeed, let us assume that it has an area, say fifty times greater than the section of the kernel of the star itself; now, although each unit of surface of that coronal atmosphere may be much less luminous than an equal unit of surface of the true star at the centre, yet if the area be very large, the spectroscopic writing of that large area will become visible side by side with the dark lines due to the brilliant region in the centre where we can study absorption; other lines (bright ones) proceeding from the exterior portion of that star will be visible in the spectrum of the apparent *point* we call a star. Now it is difficult to say whether such a body as that is a star or a nebula. We may look upon it as a nebula in a certain stage of condensation; we may look upon it as a star at a certain stage of growth."

have still hesitated to mention this, as I have been hoping for an increase of optical power which would have enabled me to be quite certain on the point. My aperture (6 inches Cooke) is not adequate to put the result beyond all doubt.

As, however, the question has been raised, it is better at once to state the attempt and its result, and to ask others with greater optical power to search for the lines; taking the precaution to use the cylindrical lens close to the eye, and not to apply it to the instrument until the rays to be examined are absolutely in focus on the slit, if a slit is used. It is possible scintillation may help matters.

## V. "On the relative 'Facility of Production' of Chemical Combinations." By Sir B. C. Brodie, Bart., D.C.L., F.R.S. Received November 21, 1877.

(Plate 4.)

A circumstance which cannot fail to impress the student of chemistry is the extreme paucity of chemical substances. The combinations of which he can conceive are innumerable, but those which he can realize are few—a mere sprinkling from the sea-shore sand—and, making the fullest allowance for human incapacity, it yet appears that there are natural facilities and natural obstacles in the way of making certain compounds which are independent of our skill and power.

It is my object to show that among these is to be reckoned that relative "facility of production" of chemical substances which depends on the mathematical laws of combination which are inherent in the very nature of chemical combination and unalterable. By this I mean the relative facility with which the units of matter may be constructed and taken to pieces, owing to the relation of these units to the number and kinds of the simple weights of which they are made up; and I propose to consider in what way, if any, this unequal "facility of production" is connected with these mathematical laws. Is it true that, as a matter of theory, there arises from these laws any greater facility for the production of one compound than of another, or do all stand on the same level? and, further, does experience tally with the conclusions of theory?

In my second Memoir,* on the Calculus of Chemical Operations, I have shown that the unit of every chemical substance is to be regarded as constituted by the performance of the several operations $\alpha, \chi, \xi \ldots\ldots$ upon the unit of space, and have also shown† that the ultimate analysis of every chemical event leads to the con-

* "Phil. Trans.," vol. clxvii, part i, p. 35, 1877.

† *Loc. cit.*, p. 115.