LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Resonance of Sodium Vapour in a Magnetic Field.

IT is well known from the observations of Wiedmann and Schmidt, Wood, Dunoyer, and others that dilute sodium vapour contained in a glass bulb emits resonance radiation when a soda flame is focussed upon it. During the last few days I have examined the effect on the resonance radiation of putting the resonating vapour in a magnetic field.

If the exciting flame contains very little soda, the resonance radiation is diminished by the field.

If, on the other hand, the flame is rich in soda, the field greatly increases the resonance radiation.

It is too early to put forward more than a tentative suggestion towards the explanation of these effects. Each sodium line emitted by the resonating vapour is broadened by the Zeeman effect. The flame poor in salt gives a narrow exciting line, and magnetic broadening throws a part of the resonating line off the exciting line, thus diminishing the light.

Adding more salt to the flame makes each exciting line broader, and (it is provisionally assumed) reverses the middle of it. Thus magnetic broadening of the resonance line tends to bring the brightest parts of the exciting line into action, and increases the light.

It may be remarked that with an intermediate condition of the flame a moderate field would produce the kind of effect last referred to, while a very strong field would separate the side components so far as to throw them beyond the limits of the exciting line. An effect of this kind has been observed, though unfortunately the condition of the exciting flame at the time was not noted. The current was switched on, and as the field increased (this takes a perceptible time) the resonance radiation increased and then diminished again. On turning the current off, the light again passed through a maximum. The greatest strength of field used in this experiment was about 14,000 units. Brightening can be distinctly observed with 1000 units, when a well-salted flame is used.

I have not been able to find that any previous observations have been made on the resonance of sodium vapour in a magnetic field. Observations were made on mercury vapour by Malinowski (*Phys. Zeits.*, September, 1913). The present experiments were suggested by some made in this laboratory by Mr. F. S. Philipps on mercury vapour (see NATURE, December 4, 1913). His observations were independent of Malinowski's.

R. J. STRUTT.

Imperial College, South Kensington, March 9.

The Spectra of Hydrogen and Helium.

DR. BOHR'S letter in NATURE of March 4, although giving an interesting discussion of some aspects of this problem, does not meet the particular point which my letter was designed to raise. This point was solely that since combination series must be expected from the "4686" series in any circumstances, and since the lines so calculated occupy the positions in which lines have been found by Evans, they cannot be used to discriminate between theories of the origin of spectra, for we cannot prove that the observed lines are not these combination lines. It is true that Bohr's theory involves the combination principle, but so also does that of Ritz, who originated the principle.

My letter (NATURE, February 11, p. 642) took up this purely negative attitude, and was not intended as a criticism of the theory. It did not even advocate a hydrogen origin for the lines. Fowler's view, that the "4686" series is a 4N series analogous to that in magnesium, was, in fact, stated to have more evidence in its favour. Whether the origin be really hydrogen or helium is not actually relevant to my argument. Even if the origin is really helium, it was pointed out by Fowler in his Bakerian lecture that his results do not formally imply Bohr's theory. Since that time, the writer has published a proof that the theory cannot explain 4N series in general, for such elements as magnesium. It can only deal with helium, and the formal analogy between helium and magnesium would weaken, rather than strengthen, the theory.

The greater part of Dr. Bohr's letter does not bear on my original point, for he is seeking to discriminate between a hydrogen and helium origin, and between his view and Rydberg's, not directly by Evans's experiments, but by other considerations. At the risk of going further from the point at issue, I feel that some remarks on these considerations are necessary.

The references to Rau's experiments on voltages necessary to produce series are interesting, and if they have been interpreted correctly—there is some doubt of this—they show that the chemical origins of the series are those stated by Dr. Bohr, and by Stark and others. They show also that the electrons in Bohr's model atoms have the proper angular momenta. There are other reasons for believing that the relation of the atom to Planck's h is contained in the angular momenta, and such atoms were treated by the writer some years ago, but with a different kind of emission. Nevertheless, Rau's experiments have nothing to do with the mechanism of spectral production, and cannot support any theory of the mechanism of radiation. For the radiation problem is quite superposed on any specification of the steady configurations of non-radiating atoms.

The remarks concerning Rydberg's view proceed throughout on the supposition that the usual constant μ—Rydberg's phase—is *zero* in these series. No such case is known elsewhere in the whole range of spectra. It is quite easy to fit the "4686" series into a formula exhibiting it as a principal series of hydrogen, if this constant μ is not arbitrarily chosen as zero. There are other arrangements of the disputed series as hydrogen series which are formally possible, but their description would occupy too much space here. A full account of the whole problem will be published shortly, so that I propose to discontinue the present discussion with this letter. Meanwhile a protest must be urged against Dr. Bohr's conviction that the spectrum of atomic hydrogen consists solely of the Balmer, Ritz, and Schumann series. For MM. Fabry and Buisson have shown that a very large number of lines in the "secondary" spectrum are due to atoms of hydrogen. A correct model of the hydrogen atom must account for more emission spectra than have yet been deduced by Bohr's theory. Finally, I must again state explicitly that my present purpose is not to call the theory into question. My only concern is to show that no decisive factor has yet entered, and that judgment between theories must at least be suspended for the present. The importance to physics in general of the whole question of spectral emission is so great that a hasty decision must not be made. And the fact remains that all the present experimental results are explicable in widely different ways. The test mentioned at the end of my previous letter still appears to be an obvious crucial one.

J. W. NICHOLSON.

University of London, King's College, March 5.