LETTERS TO THE EDITOR.

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Electro-Atomic Phenomena in the Magnetic Field.

I HOPE you will grant me the opportunity of making some brief observations on an article which appeared in the issue of NATURE for April 3, in which the anonymous reviewer "N. R. C." delivers a judgment of exceptional severity on one of my publications, "Electro-atomic Phenomena under the Action of the Magnetic Field," and for that reason very different from the other judgments which have come to my notice.

I shall limit myself to a few points with the aim, not of inducing the reviewer to change his opinion, but of facilitating somewhat an equitable estimate by readers of NATURE, which is so widely diffused and read by so many cultivated persons who are not occupied in any particular way with physics.

The reviewer seems unwilling to take into consideration the fact that my book was written with the sole purpose of bringing together and co-ordinating my recent very numerous new experiments, of not one of which (and this seems to me something important) has he been able to place the perfect validity in doubt. Hence he has confined himself to attacking the hypotheses proposed by me to account for the new facts. He declares that he is occupied only with the theory put forward to explain the facilitated ionisation by shock which I have demonstrated to take place through the influence of the magnetic field. He says, in fact: "This matter has been discussed less thoroughly than the theory of magnetic rays." By this he evidently intends to convey to the reader the conviction that the latter theory has been destroyed by the courteous objections of some physicists, to whom I believe I opposed exhaustive, and not less courteous, rectifications.

The theory under examination at present consists in this: Under the action of the field a gaseous molecule tends to orient itself in such a manner that the force due to the field, and acting upon a satellite electron, acts towards the outside of the atom, and therefore facilitates the liberation by shock of the electron itself. If the atom has one single satellite electron, certainly it will act in this manner, because it will be paramagnetic according to the accepted theory. And the behaviour of the air, which was the gas experimented upon by me, is paramagnetic.

Hence no one can succeed in understanding what the question propounded by the reviewer means: "How Prof. Righi arrives at a result so directly contrary (?) to that on which Langevin's theory of diamagnetism is based? According to the new theory, all atoms must be paramagnetic." As if I had declared that I had obtained my results by experimenting, not only on air, but also on diamagnetic gases, it being granted that for the latter that objection would have a serious value. It is well to direct attention to the fact that the reviewer seems to believe that a diamagnetic substance orientates itself, and in a sense opposite to a paramagnetic, in a uniform field.

It is a method of polemic unhappily sometimes adopted (although rarely in scientific questions), this of combating assertions which were not made; unfortunately, such a method can leave an unfavourable, although unjust, impression on the great majority of readers.

Furthermore, if (and this is not at present the case) there should some day be presented any facts contravening the theory of Langevin, it need not be necessary to choose between this and another theory; so long the nature of the connections, in virtue of which the tendency of the trajectory of an electron to orient itself in a given manner influences the orientation of the entire atom, remain still indeterminate.

I refrain from noticing the final hint on the typographical quality of my book. It is not clear whether this constitutes an unjust estimate of the sacrifices undergone by my country during the recent war—in which case I should protest with all my soul—or whether it is nothing but a witticism in somewhat questionable taste, intended to raise a contrast between the external aspect of the book and its contents—in which case I would leave the judgment to readers of NATURE.

AUGUSTO RIGHI.

Bologna (Italy), April, 1919.

I AM certainly sorry that Prof. Righi should regard my review as of "exceptional severity." It is true, of course, that more space is occupied in it by criticism than by the expression of approval; but this is simply due to the fact, familiar to every reviewer, that while the good features of a work can often be described in a single sentence, many sentences are usually necessary to explain why a less favourable view is taken of other features. It was not my intention to imply that the book is without value, and I cannot help thinking that Prof. Righi has—unintentionally, of course—adopted the practice which he deprecates of "combating assertions which were not made."

I am not at all "unwilling to take into consideration the fact that the book was written with the purpose of co-ordinating very numerous new experiments." I stated, and I repeat, that Prof. Righi "has recorded a large number of interesting and suggestive facts, which deserve the close attention of all students of physics." What more could I say, unless I proceeded to give an account of these facts—a task which is entirely unnecessary and obviously impossible in any reasonable space? Again, when I said that "this matter has been discussed less thoroughly . . ." I "intended to convey" nothing but a bare statement of fact which provided a reason for the choice of one portion of the work rather than another for more detailed discussion. So far as I can ascertain by an examination of the literature, there are much fewer papers dealing with Prof. Righi's theory of magnetisation than with his theory of magnetic rays. If I have overlooked some of the literature, I apologise; but if I have not, it is not my fault that one of the theories has received more attention than another.

No useful purpose would be served by a further elaboration of my criticism of Prof. Righi's theory. A reviewer is surely not only entitled, but in duty bound, to record his difference of opinion from the author on any matter which is essential to the work reviewed; in doing so he does not condemn the author, but merely invites those interested in the matter to read the work and to judge between the conflicting opinions. Prof. Righi has expressed his opinion in his book, and I have indicated mine very briefly in my review; if there is to be further discussion, it had better take place in the normal manner in technical journals.

But perhaps I may add a few words in further explanation of my reference to Prof. Langevin's theory. According to that theory, when a revolving electron is introduced into a magnetic field, the radius of the orbit is unchanged, but the angular velocity is