SOFT X-RAYS; A NOTE OF INTERPRETATION.

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SYNOPSIS.

Discussion of Earlier Paper.—Some of the results described in an earlier paper are discussed and are shown to be capable of interpretations different from those given by Miss Laird.

Additional Facts.—Certain experimental details and results which did not appear in the original paper are given here as further evidence for the soundness of the conclusions therein derived.

A NOTE[1] by Miss Elizabeth Laird, appearing in the April number of this journal raises several questions concerning the results described in my paper on soft X-rays.[2]

Miss Laird perhaps did not read my paper with the utmost care since she says, "No statement is made of the vacuum obtained," notwithstanding my paragraph on page 237 in which occurs the following sentence: "The three-forked brass tube, T, connects each of the three compartments of the main apparatus directly to a charcoal-liquid-air bulb through a glass stopcock with a bore one cm. in diameter." This statement, I thought, would be sufficient to convey to the ordinary reader the fact that a liquid-air vacuum was maintained during the experiments in question and did not consider it necessary to put in my original paper the following reassuring details: Before taking each set of readings the large stopcock L, Fig. 1, was closed and the cocoanut charcoal in the liquid-air-charcoal bulb, B, was re-vitalized by pumping the bulb (while the bulb was heated by means of a Bunsen burner) with an oil pump capable of giving a pressure of 0.001 mm. After pumping for about an hour the valve, M, was closed to reduce the distillation of mercury vapor from the McLeod gauge into the charcoal bulb. Then the small stopcock, s, was closed and a two-liter flask with liquid-air was applied to the charcoal bulb. The tubing connecting the charcoal bulb to the main apparatus was about 12 mm. in diameter and only about 30 cm. long.

Again, Miss Laird says, "My apparatus was more favorable as the airtight window kept out ions and positive rays." By this and

[1] PHYSICAL REVIEW, 15, p. 293, 1920.

[2] PHYSICAL REVIEW, 14, p. 234, 1919.

by repeated emphasis upon her "airtight window" she gives the impression that the window of my apparatus was not airtight and that airtightness was necessary. Had Miss Laird read my paper with more care she would have found in it the following facts: (*a*) that my apparatus was so constructed that the window could be made successively airtight

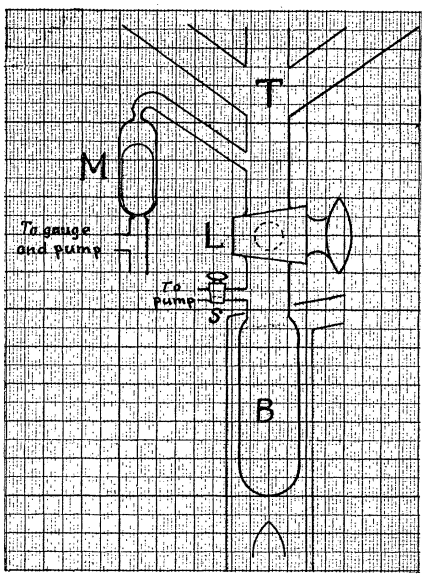

Fig. 1.

yet open to radiation, open to everything, or closed to everything; (*b*) that it was used in these three ways; and (*c*) that airtightness of the window was shown to be unnecessary.

In questioning the effectiveness of my electrostatic trap Miss Laird says, "Nothing is said as to whether the field was reversed or not," and brings in the evidence that the field in the electrostatic trap was not radial. As a matter of fact, in some of my preliminary experiments the field was reversed; furthermore the plates of the trap were connected to the terminals of a battery the middle point of which was to earth; this could be done because both of the plates were insulated. It was found that so long as the field was sufficiently strong the trap was effective irrespective of the way it was obtained. Consequently it was not considered necessary to insulate both plates in the final apparatus. Had not this result been obvious from elementary theoretical considerations and had I not developed a strong reaction against padding scientific papers with unnecessary facts and formulas, I certainly would have

incorporated the foregoing information in my paper. As to the distribution of the field, a little consideration will show that the effectiveness of the electrostatic trap depends not upon whether the field is radial or not but upon whether it is at right-angles to the velocity of the charged particles or not. This condition was fulfilled by my apparatus. As a further criticism Miss Laird says that "light is not thus trapped" by my electrostatic trap. She seems to forget that the only kind of "light" which could produce the radioelectric effects measured was the radiation due to bombardment by cathode-electrons and that it was the function of the electrostatic trap to permit the free passage of that "light" and not to trap it even if it could.

On page 294 Miss Laird says, "Dadourian's second argument rests on his attempts to measure the velocity of his photoelectrons. In Fig. 4 he shows a curve obtained by the retarded field method at 390 volts on the discharge tube. At this potential the presence of an X-radiation is not doubted, though it may be accompanied by light and other effects. I have previously shown a curve (Fig. 6, Curve II.) at 550 volts not intersecting the axis until 20 volts retarding potential is reached, as compared with his 5 volts." Here Miss Laird gives the impression that I had obtained doubtful evidence for the production of radioelectrons of only 5-volt velocity while she had obtained electrons having velocities corresponding to 20 volts. But here it must be remembered that the curve (I. of Fig. 4) which she selects for comparison represents the results of a preliminary experiment, that while it crosses the potential axis at -5 volts there is another curve in the same Fig. 4 which intersects the potential axis at -27 volts, and that in Fig. 5 there is a curve which represents results obtained under more favorable experimental conditions and which does not intersect the potential axis at all.

Referring to the effect of the field of the outer gauze upon the field of the inner gauze of my radioelectric chamber Miss Laird says, "Since in Dadourian's experiments $+5$ volts sufficed to hold back the great majority of photoelectrons, the use of $+200$ volts on the second gauze makes it highly probable that the effect of this was to counteract the assumed retarding field." There is no need to wander in the realm of probability in this connection. A glance at curve III. of Fig. 4 shows that the effect which Miss Laird would explain away by means of the second gauze is just as prominent in results obtained without the second gauze. Furthermore a comparison of the two curves of Fig. 5 shows that the effect in question is increased considerably by lamp-blacking the gauzes in spite of the $+200$ on the second gauze. Miss Laird's appeal in this connection to Von der Bijl's work with amplifiers does

not alter the situation. In amplifiers the grid is made of fine parallel wires whose distance apart is about equal to their distance from the plate and the source of electrons is very near the grid wires, while in my experiments gauzes with 1 mm. meshes were placed more than 3 mm. apart; the effective distance of the source of electrons from the inner cage being over 20 mm. Considering the relative dimensions involved and applying the inverse square law I find that the infiltration effect of the field of the "plate" into the "grid" should be in my apparatus less than one tenth of one per cent. of the effect in the amplifiers used in wireless work.

Miss Laird also finds one contradiction in my paper. She says, " It may be noted that the curves of Fig. 3 show apparently a greater proportion of the effect measured transmitted by celloidin at 80 volts than at 380. Contrary to what is given in Fig. 8." The part of the curves corresponding to 80 volts from which Miss Laird computes the proportion transmitted by the celloidin could not be used for that purpose because the corresponding readings were very small and consequently involved relatively large errors of observation. In the figure these readings appear measurable because curve I. was plotted on a very large scale in order to make its initial point coincide with that of the corresponding curve for 380 volts. It is stated in my paper that the curves were plotted on different scales. With her experience with cathode voltages below 200 she of course knows that the curves of Fig. 3 could not be used for comparing the transparency of the celloidin window for different cathode voltages.

The main object of Miss Laird's note is to question that soft X-rays are produced by cathode potentials below 200 volts. By different methods H. Dember,[1] R. Whiddington,[2] and Sir J. J. Thomson[3] obtained evidence for the production of soft X-rays with as low cathode potentials as 18.7, 128, and 10 volts, respectively. In a previous paper[4] Miss Laird disposed of these men and their evidence with the statement: " Various experiments make the results of previous observations of a Röntgen radiation produced by cathode rays of less than 200-volt velocity appear doubtful." Miss Laird now admits that some kind of radiation must be produced by the impact of electrons moving with less than 200 volt velocity but denies that this radiation can be called soft X-rays. She says, " It is not questioned that electrons of less than 200-volt velocity produce radiation by impact, but there appears as yet no proof that

[1] H. Dember, Verh. Deutch. Phys. Ges., 15, p. 560, 1913.
[2] R. Whiddington, Camb. Phil. Soc., 17, p. 144, 1913.
[3] J. J. Thomson, Phil. Mag., 28, p. 620, 1914.
[4] E. R. Laird, Ann. der Phys., 46, 605, 1915.

such radiation lies in the region designated as belonging to soft X-rays." Here the discussion is brought to a question of definition of the term soft X-rays. Miss Laird does not state the limits of "the region designated as belonging to soft X-rays," for no such limits had been designated until for reasons of expediency I defined the term "soft X-rays" in my paper as radiations, produced by the impact of electrons, which fall between the shortest known ultra-violet rays and the longest waves of characteristic X-radiation measured at the time my paper was written.