gap operated on 60-cycle current giving 120 sparks a second would enable the Tesla coil to deliver the equivalent of 2 lb. of radium.

It is clear that the outside electrode method of using vacuum tubes does not make it possible to use all of this power. However, a transfer of 2×10^{11} electrons would not puncture the glass. Estimates show that very considerable radium equivalents can be obtained even by the method of external electrodes.

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An Optical Paradox.

SURELY there is nothing peculiarly 'optical' in the 'paradox' to which Mr. Smith refers in NATURE of Feb. 25. In respect of most measurable properties (for example, length and mass) systems can be arranged in a series such that each member, though indistinguishable from its immediate neighbours, is distinguishable from those more remote. In other words, the relations usually termed 'equality' in measurement are not transitive; though $A = B$, $B = C$, it does not follow that $A = C$.

That is a fact; paradox can enter only in describing it. Mr. Smith's description appears paradoxical, because the term 'identical' that he employs usually implies transitiveness. To determine whether a relation is transitive, at least three members of its field must be compared; accordingly, if 'identical' means a transitive relation, none of his comparisons, each involving two sensations only, can establish identity. It is *not* a 'quibble' to say that two sensations, A and B, indistinguishable when compared directly, are not identical; for they may be distinguished by the classes, C_A and C_B, consisting of sensations from which they are respectively indistinguishable. C_A and C_B always contain common members; but in general they are not coextensive, and each contains members foreign to the other. A and B are truly identical only if C_A and C_B are coextensive, and A is indistinguishable, not only from B, but also from every sensation indistinguishable from B.

In theory these considerations may be 'widely ignored,' but in practice they are not. Poincaré's suggestion that the theory of errors of measurement and of the adjustment of observations should be based on them has not been widely adopted. But, as I have tried to show in Chaps. xvi. and xvii. of my "Physics," it leads to practical rules for dealing with these matters closely resembling those in general use, which are more often based on the futilities of the Gaussian Gospel. Whenever we recognise the possibility of errors and take steps to avoid them, we are in effect giving full weight to Mr. Smith's considerations. Nobody experienced in photometry would actually compare lamps through a simple unidirectional chain such as he describes.

NORMAN R. CAMPBELL.

A PARADOX, resembling that which Mr. T. Smith describes in NATURE of Feb. 25, p. 281, was stated by G. F. Stout in his "Manual of Psychology" (1915), pp. 303 to 304, for senations in general.

For a particular sensation, that of weight lifted, the paradox was abolished by the experiments of F. M. Urban (*Archiv für gesamte Psychol.*, **5**, 15, 16), for they showed that the threshold was not the definite thing that Mr. Smith assumes it to be, but that when one stimulus was kept fixed, the probability of the observer making the decision 'equal' varied with the other stimulus in a gradual manner.

Is the optical threshold unlike that for weight?

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Feb. 29.

DR. CAMPBELL seeks to demolish the paradox by applying to sensations an argument constructed to take account of the variability of our measurements of the properties, believed to be constant, of external objects. Besides differing in other important respects, the two applications are unlike in that the presence of errors in our measurements is readily demonstrable by intercomparison, whereas the view that indistinguishable sensations are not in fact equal sensations is an arbitrary assumption which it would be difficult to support by direct evidence. This view is perhaps derived from the wish that it were permissible to regard visual sensation and light stimulus as definite single-valued functions of one another.

Now there may be occasions when it is convenient to postulate an idealised system of sensations bearing such a relation to a series of stimuli; but the concept is essentially theoretical, and must not be confused with the sensations of experimental photometry. Experimentally, sensation denotes an impression individual to the observer, an impression, moreover, of so fleeting a nature that group intercomparison is impossible. The observer's verdict on his sensations at any given moment is the only one that matters: if he says that two sensations are indistinguishable, we ought either to accept that statement as final or bring forward evidence to justify our distinguishing between them.

I am inclined to think that the physicist has been handicapped in considering the photometric problem by his expectation of a close correspondence between sensation and stimulus. It is characteristic of the way in which he has grown accustomed to regard his experiments that he should, without question, ascribe the sensation he associates with the left side of the field solely to the lamp on his left, and to the lamp on his right the sensation he associates with the right side of the field. I have little doubt that he habitually presses too far the view that sensations may be isolated. In photometry, particularly when the two parts of the field are nearly or exactly matched, we ought to regard the sensation as a function of both the stimuli. On this view the argument of the paradox fails, not because the two sensations in a single observation differ, but because we are not justified in isolating the two halves of the system from one another or in assuming an unvarying connexion between the radiation on the unaltered side of the system and the sensation in the part of the field we link with it.

More generally the principle to which I wish to direct attention is that every sensation is a function of all the stimuli. The principle is illustrated by the fact that the introduction of peripheral illumination may enable us to discriminate between sensations when differentiation was previously impossible. Dr. Campbell would perhaps explain this result by saying that the introduction of the additional illumination in effect constitutes the replacement of the old comparator by a better one. That view is, I think, legitimate, but it is not consistent with the assumption of an unvarying connexion between stimulus and sensation.

T. SMITH.