detected in any degree make it a phenomenon of relative insignificance in the interpretation of clinical or other studies of H.F. currents". Further, the authors state in conclusion (p. 472): "It is possible, however, to say from the work now reported, that at least there is no biologic action on the substances mentioned under the described conditions of exposure to ultra-high frequency field". We understand that Prof. Szymanowski did not mean to cast doubt on the reliability of the results in question, and are in full agreement with him that further investigation of these interesting effects is very desirable.

(3) Prof. Szymanowski further directs our attention to an interesting fact which had escaped our notice, namely, that in 1896 D'Arsonval and Charrin reported attenuation of diphtheria toxin by radiation of frequency 2×10^5 cycles per second, also without any dangerous temperature elevation. It is thus possible that such effects may be associated with high-frequency currents in general, rather than with ultra-short waves only.

(4) Prof. Szymanowski agrees with us that the overwhelming majority of the facts are consistent with a purely thermal explanation of the effects, and adduces some further evidence. For example, he has found[6], in a study of the lethal time of mice, an almost exact parallelism with the law of heating of electrolytes at these frequencies, and similar results, on *Drosophila*, were afterwards obtained by Malov[7]. Further, in work not yet published, he has shown that Pflomm and Liebesney's effect on the heart-beat of the frog, previously claimed as a clear case of a 'specific' effect, can be almost exactly reproduced by a very slow temperature elevation. We may mention that Hill and Taylor[8] have also shown that this effect is thermal in origin.

Prof. Szymanowski concludes by remarking that in spite of the meagreness of the positive evidence at the present time, it seems worth while to continue to look for it "by impartial and well controlled investigations". With this we entirely concur. The primary purpose of our article was to direct attention to the fact that scarcely any of the experimental work on which belief in specific effects has been based has satisfied both these conditions.

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The Half-Drill Strip System Agricultural Experiments

"STUDENT'S" letter in NATURE of December 5 shows that he has overlooked the purpose of the paper he criticizes, although it was set out in the first sentence of the summary as follows:

"This enquiry was carried out to test the truth of the opinion expressed by 'Student' that randomization achieves its object 'usually at the expense of increasing the variability when compared with balanced arrangements', and that one of the means available to experimenters of reducing the error is by adopting 'a regular balanced arrangement'."

The quotations in this sentence are from "Student's" paper "Co-operation in Large-Scale Experiments", read before the Royal Statistical Society.

"Student" does not deny that the arrangement examined, and found to be extremely misleading, is, in fact, "a regular balanced arrangement". He even tells us that the method of calculating the error was that formerly used for half-drill strip experiments; though following his, "Student's", criticisms in 1923, "it has been customary" to use other methods of calculation for estimating the error.

It is not noticed, apparently, by "Student" that Dr. Barbacki and I criticize systematic arrangements in general on the ground that "the experimenter has an arbitrary choice between several widely different estimates". The two we show to be misleading correspond with the two random arrangements which we also test.

"Student" writes: "Had Prof. Fisher and Dr. Barbacki calculated the error on that basis" [as now advocated by "Student"] "they would have found a standard error of 2·37 per cent". This, then, it appears, is the standard error which "Student" thinks is appropriate for the systematic design discussed; yet we find that, on the same land, a random arrangement gives a standard error of less than 0·7 per cent. If "Student's" estimate is right, the randomized experiment is worth as much as the average of eleven such systematic experiments. This is, of course, a different ground of criticism from that on which I have habitually advocated randomization, for I cannot think that "Student's" new estimate is less arbitrary than the others. However, on his own result, what becomes of the claim that randomization tends to *increase* the error, or that experimenters can usefully try to diminish it by adopting regular balanced arrangements?

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Hypoglycæmic Action of Histone Insulinate

BY mixing a solution of thymus histone with another of crystalline insulin, at pH 7–7·2, a precipitate is produced containing most of the insulin previously present in solution.

When administered to normal or pancreatectomized dogs, the histone insulinate gives a more prolonged hypoglycæmia than the pure crystalline insulin. The level and duration of this hypoglycæmia are practically identical with that obtained by injection of the same amount of protamine (clupein) insulinate, prepared also from crystalline insulin[1].

As this crystalline insulin contains zinc, it is not improbable that this metal has a favourable influence in the prolongation of the hypoglycæmia, specially in view of the experiments of Fisher and Scott[2].

The detailed account of this work will be published in full shortly.

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