Letters to the Editor

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NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 540.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

The Law of Error

DR. J. NEYMAN, in his review[1] of Karl Pearson's "Grammar of Science", which was republished on my suggestion, quotes a passage from my recent paper[2] on the law of errors as "a remarkable illustration of the confusion of the perceptual and the conceptual spheres of thought". The whole of my work on probability is based on the recognition of the distinction between description and inference, the neglect of which is responsible for much confusion in current statistical and physical theory. Inference, in my opinion, begins at an even earlier stage than Pearson states in the "Grammar". In the passage quoted it should be clear that I am speaking wholly in the inferential sphere. An actual set of observations is necessarily discrete and cannot be described by any continuous law of error. But it may provide means of saying which of several continuous laws is the more probable on the data.

Dr. Neyman says that observations are irrelevant to the truth of a mathematical theorem. I agree. But a theorem that rests on the postulate that an error is the resultant of many comparable and independent components is not wholly mathematical, and can apply only to errors that do satisfy those conditions, and the observed distribution of errors is relevant to whether those conditions are satisfied. For this reason I think that many elaborate experimental investigations to test, for example, the binomial and χ^2 distributions, are misinterpreted. They do not test whether the distributions would hold in the conditions postulated in their proofs; they test whether those conditions have been satisfied in the design of the experiment. But in the case of the mathematical 'proof' of the normal law of error, it is not shown, or even true, that the law holds for all possible errors *even if the conditions postulated in the proof are satisfied*.

On the other hand, the law of error, whatever it is, is a description of a distribution of chance, not of any set of observational facts. It is the exception, not the rule, for a set of observations to be sufficiently numerous even to distinguish between the normal and triangular distributions of chance, and if any law is asserted from some set that is sufficiently numerous, and then the method of combining the data that it implies is applied to another set that is not sufficiently numerous, inductive inference is used. So also is the hypothesis that the actual discrete observations have been derived from *some* continuous law of chance, and that the discreteness is due only to the fact that the number of observations is finite.

Considering that I have been insisting on the distinction between description and inference for nearly twenty years, I think that I might have been spared the accusation of confusing them.

I think that the time has come also for a protest against the statement that continues to be made in statistical writings that a prior probability is a frequency. So far as I am aware, the principle of inverse probability was stated by Bayes eighty years before the first statements of any frequency definition, by Leslie Ellis and Cournot. A frequency definition was certainly not used by Bayes or Laplace, and Wrinch and I showed in 1919 that it would not suffice as a basis even for direct methods. What is done in direct methods is that an inference from a hypothesis to the observations (which can be made on the hypothesis that chances exist, without the circumlocution of a frequency definition to provide an unsatisfactory justification) is converted at the end by a verbal argument into an inference from the observations to the hypothesis; what the principle of inverse probability does is to replace this verbal argument by a symbolic statement. To convert either into a prediction of a long run frequency involves a use of Bernoulli's theorem, the conditions for the applicability of which need very careful statement, which they scarcely ever receive.

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[1] NATURE, **142**, 229 (1938).
[2] *Phil. Trans. Roy. Soc.*, A, **237**, 231–271 (1938).

Effects of Be-D Radiations upon *Vicia Faba*

THE retarding action of neutron rays upon the roots of wheat seedlings has been reported by R. E. Zirkle, P. C. Aebersold and E. R. Dempster[1], and recently by R. E. Zirkle and I. Lampe[2], but the daily growth of individual seedlings after irradiation has not been reported yet. In the present experiments, the lengths of individual roots of *Vicia Faba*, which were exposed to radiations produced by bombarding a beryllium target with 2·8 Mev. deuterons from the cyclotron of this laboratory, were measured day by day after the exposure.

Three days before irradiation, the seeds were submerged in distilled water for one day and then planted in sawdust saturated with sterilized tap-water, in a dark thermostat at 30° C., where they were allowed to remain exactly for two days before irradiation. Then, only those individuals the primary roots of which were from 15 mm. to 25 mm. long were selected for the experiments. For irradiation, two of these seedlings were planted in a small glass box filled with sawdust, and were placed in the dark observation chamber of the cyclotron, where they were exposed to the beryllium-deuteron radiation for one hour at a distance of 6·5 cm. from the beryllium target, the deuteron current being 10 microamperes. During the same period two more seedlings, planted in another glass box, were placed for control in a dark chamber which was kept distant