6 November 1931.

Dr J.H. Gaddum,
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Hampstead,
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My dear Gaddum:

Thanks for your letter; you have, I think, taken my criticisms perfectly, so there is no point in my reiterating anything of my previous letter. With respect to the weights of Thomson's paper, which I see I ought to have ascribed to earlier work by Urban, there is nothing arbitrary, or dependent on a priori assumptions. If one examines any body of data in relation to a definite theory, such as the normal distribution of resistance or susceptibility, the theory gives a definite probability of death p at any concentration, and a definite variance n pq for the observation $\frac{p}{t}$ at that concentration; this, by a large sample approximation, is equivalent to a definite variance of the estimated susceptibility giving the weight required for fitting the straight line.

As a first approximation the weights may be calculated from the observations, but, of course, in the next step one takes the weights from the theoretical values, which is

obviously better, and so improves the fit; this is merely the ordinary device for solving a number of transcendental equations.

When you say "if one knows that positive errors of measurement are more frequent than negative etc.", I take you to mean, when one has for comparison biassed and unbiassed measurements, and in ~~each~~ such a case it is obviously good sense to use the data available as far as may be to correct the bias. My point is that one does not know it apart from observation, and it is the problem of utilising the relevant observations available which is the subject of statistical theory.

I am sending herewith three offprints and a bibliography of which you can ask me for others, if you want them, giving which also gives the reference to an old paper that I have too few of. I would not expect an identical result from χ^2 and from inverse probability, but I should expect something pretty near; I should be surprised if a result was often significant on one view but not on another. χ^2 does not depend on the assumption of normal variability for it is applied to frequencies, not to variates, but the distribution is only exact for infinitely large samples; it is, however, exceedingly reliable when the expectations are not less than 5 in a class.

Thomson wrote before degrees of freedom were heard of. You are quite right that 2 should be deducted for fitting.

Yours sincerely,