318.

ON A QUESTION IN THE THEORY OF PROBABILITIES.

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The question referred to is that discussed in the paper 121; the remarks on that paper in the Notes and References to volume II. are in a great measure to the same effect as the present and next papers, 318 and 319, the existence of which I had entirely overlooked. In the first part (dated 2, Stone Buildings, W.C., March 1862) of the present paper 318, after referring to the two modes of statement which may be called the *Causation* statement and the *Concomitance* statement, I reproduce nearly as in the Notes and References first my own solution as completed by Dedekind, next Boole's solution of the problem, involving his logical probabilities; and the paper is then continued as follows.

The foregoing paper was submitted to Prof. Boole, who, in a letter dated March 26, 1862, writes:

"The observations which have occurred to me after studying your paper are the following.

1st. I think that your solution is correct under conditions partly expressed and partly implied. The one to which you direct attention is the assumed independence of the causes denoted by A and B. Now I am not sure that I can state precisely what the others are; but one at least appears to me to be the assumed independence of the events of which the probabilities according to your hypothesis are $\alpha\lambda$, $\beta\mu$. Assuming the independence of the causes as to *happening*, I do not think that you are entitled *on that ground* to assume their independence as to *acting;* because, to confine our observations to common experience, we often notice that states of things apparently independent as to their occurrence, may, when concurring, aid or hinder each other in such a manner that the one may be more or less likely to act 'efficiently' in the presence of the other than in its absence. I use the language of your own hypothesis of *efficient* action.

2ndly. When I say that I think your solution correct under certain conditions, I ought to add that it appears to me that such conditions ought to be stated as part of the original data, and that they ought to be of such a kind that they can be established by experience in the same way as the other data are. For instance, if experience, as embodied in a sufficiently long series of statistical records, establish that

$$\text{Prob. } A = \alpha, \quad \text{Prob. } B = \beta,$$

the very same experience may, by establishing also that

$$\text{Prob. } AB = \alpha\beta,$$

whence in conjunction with the former it follows that

$$\text{Prob. } AB' = \alpha\beta', \quad \text{Prob. } A'B = \alpha'\beta, \quad \text{Prob. } A'B' = \alpha'\beta',$$

enable us to pronounce that A and B are in the long run, as to happening or not happening, in the position of mutually independent events.

3rdly. I think it may be shown to demonstration, from the nature of the result, that the solution you have obtained does not apply simply and generally to the problem under the single modification of the assumption that A and B are independent. The completed data under this assumption are

$$\text{Prob. } A = \alpha, \quad \text{Prob. } B = \beta, \quad \text{Prob. } AB = \alpha\beta,$$
$$\text{Prob. } AE = \alpha p, \quad \text{Prob. } BE = \beta q.$$

You may deduce all these from your Table of Probabilities of 'compound events' given in your paper. Now you may easily satisfy yourself that the sole necessary and sufficient conditions for the *consistency* of these data are the following:

$$\left.\begin{array}{ll} (1) & \alpha p' + \beta q \overline{\gtrless} \alpha\beta, \\ (2) & \alpha p + \beta q' \overline{\gtrless} \alpha\beta, \\ (3) & \left.\begin{array}{c} \alpha \\ \beta \\ p \\ q \end{array}\right\} \begin{array}{l} \overline{\lessgtr} 1, \\ \\ \overline{\gtrless} 0. \end{array} \end{array}\right\} \quad (M).$$

But your solution requires the following conditions to be satisfied, viz.,

$$q - \alpha p \overline{\gtrless} 0, \quad p - \beta q \overline{\gtrless} 0,$$

together with the system (3). Now (1) and (2) are expressible in the form

$$\beta(q - \alpha p) + \alpha\beta' p' \overline{\gtrless} 0,$$

$$\alpha(p - \beta q) + \beta\alpha' q' \overline{\gtrless} 0;$$

from which you will see that your conditions are *narrower* than those which the data are really subject to. If your conditions are satisfied, the data will be consistent; but the converse of this proposition does not hold.

4thly. You remark that my solution of the problem, in which the independence of A and B is not assumed, but in which the probabilities are otherwise the same as in yours, is only applicable when

$$\alpha' + \alpha p \gtreqless \beta q, \quad \beta' + \beta q \gtreqless \alpha p;$$

but you do not appear to have noticed that these are actually the conditions of *consistency in the data.* Unless these are satisfied, the data cannot possibly be furnished by experience.

5thly. You remark that I have solved the problem under what you call the '*concomitance*' statement, and not the '*causation*' statement. I think that every problem stated in the 'causation' form admits, if capable of scientific treatment, of reduction to the 'concomitance' form. I admit it would have been better, in stating my problem, not to have employed the word 'cause' at all. But the introduction of the hypothesis of the independence of A and B does not affect the *nature* of the problem.

6thly. The x, s, &c., about the interpretation of which you inquire, are the probabilities of ideal events in an ideal problem connected by a formal relation with the real one. I should fully concede that the auxiliary probabilities which are employed in my method always refer to an ideal problem; but it is one, the form of which, as given by the calculus of logic, is not arbitrary. Nor does its connexion with the real problem appear to me arbitrary. It involves an extension, but as it seems to me, a perfectly scientific extension, of the principles of the ordinary theory of probabilities. On this subject, however, I have but little to add to what I have said, *Transactions of the Royal Society of Edinburgh*, vol. XXI. part 4, "On the Application of the Theory of Probabilities &c."

7thly. The problem, as stated by me, and then modified by the simple introduction of the hypothesis of the independence of A and B, must admit of solution by my method; and that solution ought to impose no restriction beyond the conditions of possible experience noted in (M).

I should be extremely glad if mathematicians would examine the analytical questions connected with the application of my method. There can, I think, after the partial proofs which I have given, exist no doubt that the conditions of applicability of the solutions are always identical with the conditions of consistency in the data, i.e. with what I have called, in the paper above referred to, the conditions of possible experience. The proof of the general proposition would involve the showing that a certain functional determinant consists solely of positive terms, with some connected theorems which appear to me to be of considerable analytical interest.

8thly. I certainly think your paper deserving of publication. If you think proper to add any or the whole of my remarks, you can do so, with of course any comments of your own."

I remark upon Prof. Boole's observations:

1st. I do assume that the causes A and B are absolutely independent of, and uninfluenced by each other; viz. not only the probability of A acting, but also the probability of its acting efficiently, are each of them the same whether B does not act, or acts inefficiently, or acts efficiently; and the like for B.

2ndly. I do assume that the same experience which establishes

$$\text{Prob. } A = \alpha, \quad \text{Prob. } B = \beta,$$

would in the long run establish

$$\text{Prob. } AB = \alpha\beta\,;$$

if it does not, *cadit quæstio*, the causes are not independent.

3rdly. I assume not only

$$\text{Prob. } A = \alpha, \quad \text{Prob. } B = \beta, \quad \text{Prob. } AB = \alpha\beta,$$

but also as 1st above stated; and I consider that, inasmuch as the result of the investigation is to show that the conditions $q - \alpha p \nless 0$, $p - \beta q \nless 0$ are necessary and sufficient conditions, it is also a result of the investigation that these are the conditions of *consistency among the data*, viz. the conditions in order that the data may be consistent with the above assumptions as to the independence of the causes. It is clear that since, as just stated, I do assume something beyond the last-mentioned three equations, the conditions of consistency *ought* to be *narrower* than those in Prof. Boole's 3rdly.

4thly. I had not overlooked, but I ought to have stated, that Prof. Boole's conditions were actually the conditions of consistency in the data.

5thly. I contend that the conception of A and B as *causes* does alter the *nature* of the problem. For when A and B are conceived of as causes, there is a definite notion of the efficient or inefficient action of A or B; and in particular when they both act, one of them, say A, may act inefficiently. But according to the concomitance statement, then either there is no such notion as that of the efficient or non-efficient happening of A or B (I believe this to be so), *or else* the only notion of efficient or inefficient happening is happening in concomitance or in non-concomitance with E; but in this view, if A, B, E all happen, then A and B each of them happens efficiently. The argument is to me conclusive as to the diversity of the two problems.

6thly. I do not in anywise assert, or even suppose, that the ideal problem is arbitrary, or that its connexion with the real problem is arbitrary. I simply do not know what the ideal problem is; I do not know the point of view, or the assumed mental state of knowledge or ignorance according to which x, y, s, t are the probabilities of A, B, AE, BE. It is to be borne in mind that x, y, s, t are, in Prof. Boole's solution, determined as numerical quantities included between the limits 0 and 1, i.e. as quantities which are or may be actual probabilities. What I desiderate is, that Prof. Boole should give for his auxiliary quantities x, y, s, t such an explanation of the meaning as I have given for my auxiliary quantities λ, μ. I do not find any such explanation in the memoir referred to.

7thly and 8thly. No remark is necessary.

March 29, 1862.

Prof. Boole, in his reply, dated April 2, writes, "No such explanation as you desiderate of the interpretation of the auxiliary quantities in my method of solution is possible; because they are not of the nature of additional data, and their introduction does not limit the problem as any hypotheses which are of that nature do. I do not see any difficulty whatever in the conception of the ideal problem."

We thus join issue as follows: Prof. Boole says that there is no difficulty in understanding, I say that I do not understand, the *rationale* of his solution.

It may be remarked that the question may be, not to find any actual probability whatever, but only to find a Boolian probability or probabilities. Thus the equations (L), p. 356, omitting the last member, which alone involves u, determine in terms of the data α, β, αp, βq the Boolian probabilities x, y, s, t of the events A, B, AE, BE.

In a subsequent hastily-written letter, Prof. Boole gives an explanation of the equations (L), which appears to me little more than a translation of these equations into ordinary language.

April 16, 1862.