NOTES AND COMMENTS

Degree of Confirmation

1. The purpose of this note is to propose and to discuss a definition, in terms of probabilities, of *the degree to which a statement x is confirmed by a statement y.* I shall take this to be identical with *the degree to which a statement y confirms a statement x.* I shall denote this degree by the symbol ' $C(x, y)$ ', to be pronounced ' the *degree of confirmation of x by y* '. In particular cases, x may be a hypothesis, h; and y may be some empirical evidence, e, in favour of h, or against h, or neutral with respect to h. But $C(x, y)$ will be applicable to less typical cases also.

The definition is to be in terms of probabilities. I shall make use of both, $P(x, y)$, i.e. the (relative) probability of x given y, and $P(x)$, i.e. the (absolute) probability of x.[1] But any one of these two would be sufficient.

2. It is often assumed that the degree of confirmation of x by y must be the same as the (relative) probability of x given y, i.e. that $C(x, y) = P(x, y)$. My first task is to show the inadequacy of this view.

3. Consider two contingent statements, x and y. From the point of view of the confirmation of x by y, there will be two extreme cases: the complete support of x by y or the establishment of x by y, when x follows from y; and the complete undermining or refutation or disestablishment of x by y, when $\bar{x}$ follows from y. A third case of special importance is that of mutual independence or irrelevance, characterised by $P(xy) = P(x)P(y)$. Its value of $C(x, y)$ will lie below establishment and above disestablishment.

[1] ' $P(x)$ ' may be defined, in terms of relative probability, by the definiens ' $P(x, \overline{z\bar{z}})$ ' or, more simply, ' $P(x, \overline{x\bar{x}})$ '. (I use throughout ' xy ' to denote the conjunction of x and y, and ' $\bar{x}$ ' to denote the negation of x.) Since we have, generally, $P(x, y\overline{z\bar{z}}) = P(x, y)$, and $P(x, yz) = P(xy, z)/P(y, z)$, we obtain $P(x, y) = P(xy)/P(y)$—a serviceable formula for defining relative probability in terms of absolute probability. (See my note in *Mind*, 1938, **47**, 275, f., where I identified absolute probability with what I called ' logical probability ' in my *Logik der Forschung*, Vienna, 1935, esp. sects. 34 f. and 83, since the term ' logical probability ' is better used for the ' logical interpretation ' of both $P(x)$ and $P(x, y)$, as opposed to their ' statistical interpretation ' which may be ignored here.)

Between these three special cases—establishment, independence, and disestablishment—there will be intermediate cases : *partial support* (when y entails part of the content of x) ; for example, if our contingent y follows from x but not vice versa, then it is itself part of the content of x and thus entails part of the content of x, supporting x ; and *partial undermining* of x by y (when y partially supports $\bar{x}$) ; for example, if y follows from $\bar{x}$. We shall say, then, that y supports x, or undermines x, whenever $P(xy)$ or $P(\bar{x}y)$, respectively, exceed their values for independence. (The three cases—support, undermining, independence—are easily seen to be exhaustive and exclusive on this definition.)

4. Consider now the conjecture that there are three statements, x_1, x_2, and y, such that (i) x_1 and x_2 are each independent of y (or undermined by y) while (ii) y supports their conjunction x_1x_2. Obviously, we should have to say in such a case that y confirms x_1x_2 to a higher degree than it confirms either x_1 or x_2 ; in symbols,

$$C(x_1, y) < C(x_1x_2, y) > C(x_2, y) \tag{4.1}$$

But this would be incompatible with the view that $C(x, y)$ is a probability, i.e. with

$$C(x, y) = P(x, y) \tag{4.2}$$

since for probabilities we have the generally valid formula

$$P(x_1, y) \geqslant P(x_1x_2, y) \leqslant P(x_2, y) \tag{4.3}$$

which, in the presence of (4.1) contradicts (4.2). Thus we should have to drop (4.2). But in view of $0 \leqslant P(x, y) \leqslant 1$, (4.3) is an immediate consequence of the general multiplication principle for probabilities. Thus we should have to discard such a principle for the degree of confirmation. Moreover, it appears that we should have to drop the special addition principle also. For a consequence of this principle is, since $P(x, y) \geqslant 0$,

$$P(x_1x_2 \textit{ or } x_1\bar{x}_2, y) \geqslant P(x_1x_2, y) \tag{4.4}$$

But for $C(x, y)$, this could not remain valid, considering that the alternative, x_1x_2 *or* $x_1\bar{x}_2$, is equivalent to x_1, so that we obtain by substitution on the left-hand side of (4.1) :

$$C(x_1x_2 \textit{ or } x_1\bar{x}_2, y) < C(x_1x_2, y) \tag{4.5}$$

In the presence of (4.4), (4.5) contradicts (4.2).[1]

[1] In his *Logical Foundations of Probability*, Chigago, 1950, p. 285, Carnap uses the multiplication and addition principles as '*conventions on adequacy*' *for the degree of confirmation*. The only argument he offers in favour of the adequacy of these

5. These results depend upon the conjecture that statements x_1, x_2, and y exist such that (i) x_1 and x_2 are each independent of y (or undermined by y) while (ii) y supports x_1x_2. I shall prove this conjecture by an example.[1]

Take coloured counters, called '*a*', '*b*', . . . , with four exclusive and equally probable properties, blue, green, red, and yellow. Let x_1 be the statement '*a* is blue or green'; x_2 = '*a* is blue or red'; y = '*a* is blue or yellow'. Then all our conditions are satisfied. (That y supports x_1x_2 is obvious: y follows from x_1x_2, and its presence raises the probability of x_1x_2 to twice the value it has in the absence of y.)

6. But we may even construct a more striking example to show the inadequacy of identifying $C(x, y)$ and $P(x, y)$. We choose x_1 so that it is strongly supported by y, and x_2 so that it is strongly undermined by y. Thus we shall have to demand that $C(x_1, y) > C(x_2, y)$. But x_1 and x_2 can be so chosen that $P(x_1, y) < P(x_2, y)$. The example is this: take x_1 = '*a* is blue'; x_2 = '*a* is not red'; and y = '*a* is not yellow'. Then $P(x_1) = \frac{1}{4}$; $P(x_2) = \frac{3}{4}$; and $\frac{1}{3} = P(x_1, y) < P(x_2, y) = \frac{2}{3}$. That y supports x_1 and undermines x_2 is clear from these figures, and also from the fact that y follows from x_1 and also from $\bar{x}_2$.

7. Why have $C(x, y)$ and $P(x, y)$ been confounded so persistently? Why has it been ignored that it is absurd to say that some evidence y of which x is completely independent can yet strongly 'confirm' x? And that y can strongly 'confirm' x, even if y undermines x? And this even if y is the total available evidence? I do not know the answer to this question, but some suggestions may be helpful. There is first the powerful tendency to think that whatever may be called the 'likelihood' or 'probability' of a hypothesis must be a probability in the sense of the calculus of probabilities. In order to disentangle the various issues here involved, I distinguished twenty years ago what I then called the 'degree of confirmation' from both, the logical and the

principles is that 'they are generally accepted in practically all modern theories of probability$_1$', i.e. our $P(x, y)$ which Carnap identifies with the 'degree of confirmation'. But the very term 'degree of confirmation' ('*Grad der Bewährung*') was introduced by me in sections 82 f. of my *Logik der Forschung* (a book to which Carnap sometimes refers), in order to show that both logical and statistical probability are *inadequate* to serve for a degree of confirmation, since confirmability must increase with testability, and thus with (absolute) logical improbability and content. (See below.)

[1] The example satisfies (i) for *independence* rather than *undermining*. (To obtain one for undermining, add amber as a fifth colour, and put y = '*a* is amber or blue or yellow'.)

statistical probability. But unfortunately, the term ' degree of confirmation ' was soon used by others as a new name for (logical) probability; perhaps under the influence of the mistaken view that science, unable to attain certainty, must aim at a kind of ' *Ersatz* '—at the highest attainable probability.

Another suggestion is this. It seems that the phrase ' the degree of confirmation of x by y ' was never turned round into ' the degree to which y confirms x ', or ' *the power of y to support x* ' ; for in this form it would have been quite obvious that, in a case in which y supports x_1 and undermines x_2, $C(x_1, y) > C(x_2, y)$ is absurd—although $P(x_1, y) > P(x_2, y)$ may be quite in order, indicating, in such a case, that we had $P(x_1) > P(x_2)$ to start with. Furthermore, there seems to be a tendency to confuse measures *of* increase or decrease with the measures *that* increase and decrease (as shown by the history of the concepts of velocity, acceleration, and force). But the power of y to support x, it will be seen, is essentially a *measure of the increase or decrease* due to y, in the probability of x. (See also 9 (vii), below.)

8. It will perhaps be said, in reply to all this, that it is in any case legitimate to call $P(x, y)$ by any name, and also by the name ' degree of confirmation '. But the question before us is not a verbal one.

The degree of confirmation of a hypothesis x by empirical evidence y is supposed to be used for estimating the degree to which x is *backed by experience*. But $P(x, y)$ cannot serve this purpose, since $P(x_1, y)$ may be higher than $P(x_2, y)$ even though x_1 is undermined by y and x_2 supported by y, and since this is due to the fact that $P(x, y)$ depends very strongly upon $P(x)$, i.e. the absolute probability of x, which has nothing whatever to do with the empirical evidence.

Furthermore, the degree of confirmation is supposed to have an influence upon the question whether we should *accept*, or *choose*, a certain hypothesis x, if only tentatively ; a high degree of confirmation is supposed to characterise a hypothesis as ' good ' (or ' acceptable '), while a disconfirmed hypothesis is supposed to be ' bad '. But $P(x, y)$ cannot help here. *Science does not aim, primarily, at high probabilities. It aims at a high informative content, well backed by experience. But a hypothesis may be very probable simply because it tells us nothing, or very little.* A high degree of probability is therefore not an indication of ' goodness '—it may be merely a symptom of low informative content. On the other hand, $C(x, y)$ must, and can, be so defined that only hypotheses with a high informative content can reach high degrees of confirmation. The *confirmability* of x (i.e. the maximum degree of confirmation which a statement x can reach) should increase with $C(x)$, i.e. the measure of the content of x, which is equal to $P(\bar{x})$, and therefore to the *degree of testability* of x. Thus, while $P(\overline{x\bar{x}}, y) = 1$, $C(\overline{x\bar{x}}, y)$ should be zero.

9. A definition of $C(x, y)$ that satisfies all these and other *desiderata* indicated in my *Logik der Forschung*, and stronger ones besides, may be based upon $E(x, y)$, i.e. a non-additive measure of the *explanatory power of x with respect to y*, designed so as to have -1 and $+1$ as its lower and upper bounds. It is defined as follows.

Let x be consistent,[1] and $P(y) \neq 0$; then we define,

$$E(x, y) = \frac{P(y, x) - P(y)}{P(y, x) + P(y)} \qquad (9.1)$$

$E(x, y)$ may also be interpreted as a non-additive measure of the dependence of y upon x, or the support given to y by x (and vice versa). It satisfies the most important of our *desiderata*, but not all: for example, it violates (viii, c) below, and satisfies (iii) and (iv) only approximately in special cases. To remedy these defects, I propose to define $C(x, y)$ as follows.

Let x be consistent and $P(y) \neq 0$; then we define,

$$C(x, y) = E(x, y)(1 + P(x)P(x, y)) \qquad (9.2)$$

This is less simple than, for example, $E(x, y)(1 + P(xy))$, which satisfies most of our *desiderata* but violates (iv), while for $C(x, y)$ the theorem holds that it satisfies all of the following *desiderata*:

(i) $C(x, y) \gtreqless 0$ respectively if and only if y supports x, or is independent of x, or undermines x.

(ii) $-1 = C(\bar{y}, y) \leqslant C(x, y) \leqslant C(x, x) \leqslant 1$

(iii) $0 \leqslant C(x, x) = C(x) = P(\bar{x}) \leqslant 1$

Note that $C(x)$, and therefore $C(x, x)$, is an additive measure of the content of x, definable by $P(\bar{x})$, i.e. the absolute probability of x to be false, or the *a priori* likelihood of x to be *refuted*. Thus *confirmability* equals *refutability or testability*.[2]

[1] This condition may be dropped if we accept the general convention that $P(x, y) = 1$ whenever y is inconsistent.

[2] See section 83 of my *L.d.F.*, which bears the title 'Confirmability, Testability, Logical Probability'. (Before 'logical', 'absolute' should be inserted, in agreement with the terminology of my note in *Mind*, loc. cit.)

(iv) If y entails x, then $C(x, y) = C(x, x) = C(x)$

(v) If y entails $\bar{x}$, then $C(x, y) = C(\bar{y}, y) = -1$

(vi) Let x have a high content—so that $C(x, y)$ approaches $E(x, y)$—and let y support x. (We may, for example, take y to be the total available empirical evidence.) *Then for any given* y, $C(x, y)$ *increases with the power of* x *to explain* y (i.e. to explain more and more of the content of y), and therefore with the *scientific interest* of x.

(vii) If $C(x) = C(y)$ then $C(x, u) \gtreqless C(y, w)$ whenever $P(x, u) \gtreqless P(y, w)$.

(viii) If x entails y, then: (*a*) $C(x, y) \geqslant 0$; (*b*) for any given x, $C(x, y)$ and $C(y)$ increase together; and (*c*) for any given y, $C(x, y)$ and $P(x)$ increase together.[1]

(ix) If $\bar{x}$ is consistent and entails y, then: (*a*) $C(x, y) \leqslant 0$; (*b*) for any given x, $C(x, y)$ and $P(y)$ increase together; and (*c*) for any given y, $C(x, y)$ and $P(x)$ increase together.

10. All our considerations, without exception, may be relativised with respect to some initial information z; adding at the appropriate places phrases like 'in the presence of z, assuming $P(z, \overline{z\bar{z}}) \neq 0$'. The relativised definition of the degree of confirmation becomes:

$$C(x, y, z) = E(x, y, z)(1 + P(x, z)P(x, yz)) \qquad (10.1)$$

where

$$E(x, y, z) = \frac{P(y, xz) - P(y, z)}{P(y, xz) + P(y, z)} \qquad (10.2)$$

$E(x, y, z)$ is the explanatory power of x with respect to y, in the presence of z.[2]

11. There are, I believe, some intuitive *desiderata* which cannot be satisfied by any formal definition. For example, a theory is the better confirmed the more ingenious our unsuccessful attempts at its refutation have been. My definition incorporates something of this idea—

[1] (vii) and (viii) contain the only important desiderata which are satisfied by $P(x, y)$.

[2] Let x_1 be Einstein's gravitational theory; x_2 Newton's; y the (interpreted) empirical evidence available today, including 'accepted' laws (it does not matter if none or one or both of the theories in question is included, provided our conditions for y are satisfied); and z a part of y, for example, a selection from the evidence available one year ago. Since we may assume that x_1 explains more of y than x_2, we obtain $C(x_1, y, z) \geqslant C(x_2, y, z)$ for every z, and $C(x_1, y, z) > C(x_2, y, z)$ for any suitable z containing some of the relevant initial conditions. This follows from (vi)—even if we have to assume that $P(x_1, yz) = P(x_2, yz) = P(x_1) = P(x_2) = 0$.

if not as much as can be formalised. But one cannot completely formalise the idea[1] of a sincere and ingenious attempt.

The particular way in which $C(x, y)$ is here defined I consider unimportant. What may be important are the *desiderata*, and the fact they can be satisfied together.

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[1] There are many ways of getting nearer to this idea. For example, we may put a premium on crucial experiments by defining

$$C_{a,\,b}(h) = (C(h, e_b)\prod_{i=1}^{n} C(h, c_i, e_a))^{1/(n+1)}$$

where $c_1, c_2, \ldots,$ is the sequence of experiments made between the moments of time, t_a and t_b. We have $t_a < t_1 \leqslant t_i \leqslant t_n = t_b$. e_a and e_b are the total evidence (which may include laws) *accepted* at t_a and t_b. We postulate $P(c_i, e_b) = 1$ and (to ensure that only new experiments are counted) $P(c_i, e_a) \neq 1$ and $P(c_i, Uc_j) \neq 1$. (' Uc_j' *is the spatio-temporal universalisation of* c_j.)

A Note on a Suggested Modification of Newton's Corpuscular Theory of Light to Reconcile it with Foucault's Experiment of 1850

IT WILL be remembered that Newton's theory assumed that, when a ray of light passes into a denser medium, the perpendicular component of its incident velocity is accelerated owing to an attractive force acting perpendicularly to the refracting surface, and consequently the component parallel to the surface remains constant. This gives the result, in contradiction with both the wave theory and Foucault's experiment of 1850 (proving that the velocity of light in water is less than in air) that the actual velocity will be greater in the medium of refraction.

The view was expressed by the late Alexander Wood—in the course of a discussion of the rôle of crucial experiments in physics[1] —that in order to reconcile Newton's theory, it would have been sufficient to introduce the following assumption as a way out: namely, that when the light goes into a denser medium *the perpendicular component of the incident velocity remains unchanged*, while the parallel component is diminished by action of a frictional kind. This assumption would yield the consequence that the light will travel

[1] Alexander Wood, *In Pursuit of Truth*, London, 1927, 47-48