plausible measures of informative content, some of which have been discussed recently by Carnap and myself in a common paper [1]) are, roughly speaking, inversely proportional, a ' good ' hypothesis must indeed have high degrees of both, with contradiction easily avoided through the qualifiers ' initial ' (or ' absolute ') and ' relative to the available evidence ', respectively.

Though the methodological situation may be adequately and satisfactorily described in terms of Carnap's degree of confirmation alone (informative content being definable on its basis), there is, of course, no objection to employing also the relevance terminology and saying, for instance, that good scientific hypotheses should have low initial information, hence high initial content, with empirical evidence (' experience ') being highly positively relevant to them.

Altogether, though Popper's recent polemic against the current theory of degree of confirmation seems to be unjustified and in spite of the fact that his positive proposals in this respect seem to have been effectively anticipated in Carnap's treatment of relevance, there can be no doubt that the complex position held by scientific hypotheses with regard to logical probability, informative content, and relevance of empirical evidence have been greatly clarified by Popper's remarks.

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' Content ' and ' Degree of Confirmation ' : A Reply to Dr Bar-Hillel

DR BAR-HILLEL's concluding remarks are gracious, and even encouraging. But since they do not quite match what he says in the body of his note, they are perhaps only meant as balm to my wounds. For if he is right in what he says in the body of his note then I cannot possibly have ' greatly clarified ' the ' complex position ' by my remarks. On the contrary, I must have left this position in a state of even worse confusion than it was left in by ' the current theory of confirmation ', as Dr Bar-Hillel calls it.

My note ' Degree of Confirmation ' which has given rise to Dr Bar-Hillel's comments was critical of Carnap's theory, because I had to make clear why I wanted to propose a new definition of degree of confirmation.

[1] Y. Bar-Hillel and R. Carnap, ' Semantic Information ', this *Journal*, 1953, 4, 147-157, especially 149-151

I am afraid that Dr Bar-Hillel forces me now to criticise Carnap's theory further.

For the simple answer to Dr Bar-Hillel is that *there is no ' current theory of confirmation '*.   Or, to put it more clearly, the theory proposed in Carnap's two books, *Logical Foundations of Probability*, and *The Continuum of Inductive Methods*, is partly inconsistent, and partly inadequate from the point of view of *his own* requirements, not merely from that of *my* requirements.   Moreover, the points where it fails were briefly (but, I think, sufficiently) discussed in my *Logik der Forschung*, fifteen years before the ' current ' theory was published.

I shall try to prove these charges as clearly and simply as possible.

Carnap sets out to establish a theory of confirmation.   He distinguished three kinds of concept of confirmation : [1]

> (i) the classificatory concept of confirmation ;
> (ii) the comparative concept of confirmation ;
> (iii) the quantitative or metrical concept of confirmation.

All three concepts are discussed at some length ; but in the end, only a theory of (iii) is offered.[2]   It is this concept which I shall prove to be partly inconsistent and partly inadequate, by Carnap's own standards.

A very important touchstone of the theory is, clearly, the way it treats *the degree of confirmation of a universal law*.   (In my opinion, it is the *only* important case ; I know that Carnap's opinion is different, but we need not discuss this point here.)

On page 571 of *Probability* Carnap tells us, quite correctly, that, according to his definition of degree of confirmation, *the degree of confirmation of all universal laws is zero* for a world with infinitely many individuals, and indistinguishable from zero for any world with very many individuals (especially if we know, at least, a very large lower limit).

Now this result is clearly counter-intuitive.   It attributes to the best confirmed laws such as ' sugar is soluble in water ' precisely the same ' degree of confirmation ' as to laws which are always refuted (or which

[1] R. Carnap, *Logical Foundations of Probability*, 1949 ; here for short ' *Probability* ', see especially pp. 21-23 ; by the same author, *The Continuum of Inductive Methods*, 1952 ; for short, ' *Methods* ; and ' On the Comparative Concept of Confirmation ', this *Journal*, 1953, **3**, 311 sqq. ; for short ' *Comparative* '.

[2] *Probability*, p. 492, which says of the classificatory concept, ' This concludes the discussion of the classificatory concept.   We have not found an adequate explicatum . . .'   Similarly the later *Comparative*, p. 317 (in a reply to Dr Bar-Hillel's criticism): ' The definition of the comparative concept in my book was too narrow. . . .' Then Carnap quotes, p. 318, from *Probability*, p. 467 : ' However . . . it seems doubtful whether a simple definition can be found ', and adds : ' The discussions in this article will make these doubts even stronger.'   Thus no current theory of either the classificatory or the comparative concepts is claimed to exist.

are self-contradictory). And Carnap himself admits that this result is counter-intuitive : he says that an engineer will (intuitively) call certain laws 'very reliable' ; 'well founded' ; 'amply confirmed by numerous experiences' ; and he adds himself that 'these phrases . . . are intended to say something about . . . degree of confirmation '.

But Carnap does not think that he has shown the *inadequacy* of his definition of degree of confirmation. Instead, he introduces *ad hoc* two new concepts, specially designed for the purposes of escape from the difficulty ; the concept of an *instance-confirmation* and that of a *qualified instance-confirmation*. I shall criticise these two a little later ; first I shall try to show why Carnap, by his own standards, should have discarded his definition of degree of confirmation as *inadequate*, as soon as he found a case which he himself felt to be counter-intuitive.

For we read on page 232 of *Probability* : 'How is the adequacy of a function *c* proposed as a [definition of degree of confirmation] . . . to be judged ?' The answer contains the following passage : '. . . Then we examine whether the value of $c(h, e)$, calculated on the basis of the given definition, is sufficiently in agreement with the intuitive value.' And Carnap continues : ' Since the intuitive determination of a value is in general rather vague, an approximate agreement will be regarded as sufficient.' Clearly, there is not very much hope of proving inadequacy by this vague method. Nevertheless, the intuitive inadequacy we have found is sufficiently glaring to prove inadequacy. The gap between the intuitive value for a very well confirmed law and the value zero just could not be wider.[1] If ever there can be a case in which we can reject adequacy on the basis of the test suggested by Carnap, this must be it.

But I find in *Probability* a second remark of Carnap's on adequacy which, I think, clearly implies the inadequacy not only of the definition of degree of confirmation which he favours, but of any function which satisfies the traditional laws of the calculus of probability (and therefore of all ' regular *c*-functions ' in Carnap's terminology).

The passage is connected with Dr Bar-Hillel's comments in several ways. First, I should not have noticed it without Dr Bar-Hillel's insistence that Carnap has anticipated my proof (in ' Degree of Confirmation ') of the inadequacy of any probability function to serve as a measure of degree of

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[1] In the continuation of the passage, Carnap mentions that if the inadequacy arises only in special cases ' the definition need not be entirely abandoned ; it may be that a suitable modification can be found '. But what is meant here by ' modification ' is clearly not the replacement of the definition, for these special cases, by a completely different definition, but a modification of the general definition which makes it more adequate for the special cases. (I personally should not admit that *all universal laws* form a ' special case ' ; but again, there is no need to argue this point.)

confirmation.   But Dr Bar-Hillel is right and I am very grateful for his reference : Carnap has anticipated, in essence, my examples ; he has even drawn from them the conclusion that certain classificatory confirmation-concepts are inadequate.   But he has not drawn from his examples the conclusion which I have drawn : that all probability functions are inadequate to serve as degree of confirmation.

I shall not again go over the field covered by my previous note.   But the following is an immediate consequence of his and of my examples :

Let the content of x be part of that of y, so that x follows from y.   Then the following ' *content-condition* ' (Carnap calls it ' consequence condition ') is *invalid* : ' If x follows from y, then every z that confirms y confirms x at least to the same degree as it confirms y.'

Carnap draws from the examples mentioned the correct conclusion that what I call the ' content condition ' is invalid.   But we have the following universally valid formula of the calculus of relative probabilities for every a, b, and c :

$$p(ab, c) \leqslant p(b, c)$$

now let ab be y, and let b be x, so that x follows from y, and that the content of x is contained in the content of y.   Then we see that, for every z,

$$p(y, z) \leqslant p(x, z),$$

which is, precisely, the invalid content condition which Carnap uses on the bottom of page 474 of *Probability* as an argument to show the invalidity of a confirmation concept.   But he does not say that it proves the inadequacy of all probability functions (' regular c-functions '), which I pointed out in my note—and which I had pointed out long ago in my *Logik der Forschung* where I used the invalidity of the content condition for the same purpose. (This partly answers Dr Bar-Hillel's point about contents.)

I think I have proved that on Carnap's own showing, his ' regular c-functions ' and therefore his concept of ' degree of confirmation ' are inadequate.

But I have still to say something about his two concepts of instance-confirmation ; for these are not regular c-functions at all (a point not mentioned by Carnap), that is to say, of course, not regular c-functions of their arguments.[1]   It might be thought, therefore, that they escape our argument from the invalidity of the content condition.   This, however, is not the case, as Dr Bar-Hillel will easily verify.

But this is not my main argument against the two concepts of instance-confirmation.   They are different.

(1) The concept of unqualified instance-confirmation is inadequate for at least two reasons.   The first is that in a sufficiently complex world, its

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[1] This fact may be easily overlooked by Carnap's readers, because the two concepts are defined with the help of regular c-functions.

value will be very close to zero for any complex predicate. The second is that it shares the absurdity of the (rectified) qualified instance-confirmation which will be discussed next.

(2) The qualified instance-confirmation of which Carnap says in *Probability*, page 572, that it ' seems in many cases to represent still more accurately what is vaguely meant by the reliability of a law *l* ' is, I am sorry to say, inconsistent : it is hit by the paradox of confirmation (discussed in *Probability*, page 469), as Dr Bar-Hillel will no doubt see at a glance if he compares the two pages.

This means that not even a vaguely adequate degree of confirmation for a universal law has been proposed. But, as a matter of fact, the paradox of confirmation is not so serious as it looks : there is a general method for avoiding it by symmetrisation, and it can be easily applied to our case. (It means replacing, in (15) on page 573, ' h' ' by ' $j \supset h'$ ', and ' j ' by ' $h' \supset j$ '.) But even after this rescuing operation, the rectified concept of qualified instance-confirmation and the non-qualified concepts both lead to absurd consequences.

Take a universe of coin tosses with only two predicates : ' coming up heads ' and ' coming up tails '.

Let somebody propose the hypothesis ' all tosses always come up heads ' ; let there be a sequence of tosses to test this hypothesis, with tails coming up on the average at every second toss. Then we will all say that the hypothesis has been amply refuted by the evidence. However, Carnap's unqualified, and the by me rectified qualified, instance-confirmations both give the hypothesis a confirmation value of exactly $\frac{1}{2}$.

Similarly, a hypothesis which is regularly refuted in every hundredth instance gets a confirmation value which rapidly approaches $99/100$. I need not say that both ought to have zero confirmation.

I discussed an equivalent case long ago in my *Logik der Forschung*, page 191, and briefly described how these absurd consequences may lead the probability theorist to adopt another definition which, as I there showed, leads to the probability zero for all hypotheses. I was no prophet : I should have presented the problem the other way round, showing how one can escape from the frying pan of zero probability into the fire of instance-confirmation, with its absurdly high probabilities for regularly refuted hypotheses.

Thus there is no ' current theory of confirmation ', I contend.

But what about my own theory, and Dr Bar-Hillel's claim that it has been anticipated by Carnap's relevance-measure ?

As I pointed out in my note, both my concepts, ' explanatory power ' and ' degree of confirmation ', are *measures of dependence* ; and so is every measure of ' relevance ' (cf. Keynes' *Treatise*, pp. 54, 120, 121, 146, 150). This does not mean that Carnap's measure of relevance is adequate for a

measure of degree of confirmation. (It does not satisfy most of my criteria of adequacy nor my other *desiderata*.) Nor does Carnap anywhere propose that his relevance measure should be used as one of degree of confirmation. In fact, Keynes, or rather W. E. Johnson's [1] ' coefficient of dependence ' (or ' coefficient of influence ') which I may denote by ' Co ' is mathematically much nearer to my degree of confirmation since my function E, the explanatory power, is equal to $(Co - 1)/(Co + 1)$ (provided we change the order of Keynes' first two variables). I suspect, however, that both Johnson and Keynes would have been surprised to see their ' coefficient of dependence ' championed as a measure of the explanatory power of a theory, or of the degree to which a theory is confirmed by tests.

But had I done no more than show that Carnap's ' degree of confirmation ' is inadequate while his ' relevance measure ' was an adequate measure of the acceptability [2] of a theory (which it is not), I should have done something not anticipated by ' the current theory '.

I may now sum up my reply to Dr Bar-Hillel's points 1 and 2, by way of criticising his ' *dictionary* '.

Dr Bar-Hillel's dictionary is incorrect. As to its third line (which happens to be the point at issue) : what I call ' degree of confirmation ' is not the same as Carnap's ' relevance measure'. Carnap's relevance measure approaches zero with increasing content of the hypothesis (or of any other statement involved) ; and it becomes zero for every universal law. My ' degree of confirmation ' approaches one with increasing content of the hypothesis, provided it is a successful hypothesis. These are mathematical consequences of the two definitions. The discrepancy could hardly be greater. Also, Carnap leaves no doubt that from the point of view of his theory, any non-additive confirmation function is ' entirely unacceptable ' (cf. *Continuum*, p. 85 ; see also *Probability*, pp. 369 f.) ; but my ' degree of confirmation ' is non-additive.

Lines four to six of Dr Bar-Hillel's dictionary are also incorrect, for the same reason. ' Is positively relevant to ', in Carnap's sense, approaches zero, if the content of the statements in question increases ; while ' supports ', in my sense, behaves differently : a statement may have a very

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[1] Cf. Keynes, *Treatise on Probability*, 1921, pp. 116, 150

[2] Carnap always intended (as I did) that his degree of confirmation should serve as a measure of the *acceptability* of a theory. Dr Bar-Hillel's historical comment, at the beginning of his point 1, may give the impression that the term ' degree of confirmation ' was from the beginning used by Carnap in a sense different from mine. The fact is, however, that Carnap's term ' degree of confirmation ' occurs first in ' Testability and Meaning ', *Philosophy of Science*, 1936, 3, where he used it to translate my term ' *Grad der Bewährung* ' ; cp. for example p. 427 where Carnap writes : ' Popper has explained the difficulties of such a frequency interpretation of the degree of confirmation ', etc.

high content and yet it may, for this very reason, strongly support or undermine another.

As to the remaining lines—one and two—they are correct. But they contain, precisely, the error exposed above : the content condition. Which means that, according to Carnap's standards as well as mine, ' degree of confirmation ' cannot be a probability function.

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