these two mistakes and I apologise. But fortunately these regrettable errors do not affect the argument at all. Nor do they give grounds for Professor Carnap's complaint. This complaint is directed not against an argument of mine but against a mere *footnote* of eight lines (and he gives the reader no warning that the two mistakes both occurred in that footnote). The purpose of the footnote was to help the reader to find passages in *Probability* which support my contention : the contention that it is sufficient for me to discuss (iii), i.e. the ' quantitative ' concept of confirmation, since no theory (' independent of (iii) ', I ought to have added) of the other two concepts is offered. The purpose of my footnote was, no doubt, partly defeated by printing ' 492 ' instead of ' 482 ', and to a lesser extent by omitting the three dots. These two mistakes of mine are incontestable *facts*. All the rest is Professor Carnap's *interpretation*. The suggestion that I propped up my criticism by ' alleged quotations ' is absurd. (Readers of the more ' substantial discussion ' between Dr Bar-Hillel and myself will be able to judge whether my arguments are in need of such props to prevent them from collapsing.)

My reply to Professor Carnap's section (*b*) will be found, in this number, in sections (5) and (6) of my ' Adequacy and Consistency : A Second Reply to Dr Bar-Hillel '.

My reply to Professor Carnap's section (*c*) is this. Professor Carnap asserts that I confuse two formulae which he numbers (3) and (4), and he says that (3) is valid while (4) is false. But as I show, in section (8) of my ' Second Reply ', (4) follows from (3). Thus Professor Carnap's assertion is logically inconsistent ; and he is seriously in error if he believes that my argument collapses because of the alleged ' confusion ' of (3) and (4).

K. R. POPPER

Further Comments on Probability and Confirmation

A Rejoinder to Professor Popper

(1) PROFESSOR K. R. POPPER, in his reply to my comments on a note of his, does not accept my view that the disagreement between him and Professor R. Carnap on questions of logical probability and degree of confirmation is mostly a verbal one. On the contrary, in this reply he goes on to make much stronger claims than he did in the first note and charges that Carnap's theory of confirmation, as presented in his two recent books,' is partly inconsistent, and partly inadequate from the point of view of *his own* requirements, not merely from that of *my* (Popper's) requirements ' (*Reply*, p. 158).

Because of the severity of these charges and the great importance of the issues behind them—I think it is no exaggeration to state that the problems around the logic and methodology of induction occupy the central position in modern philosophy of science—and because of the fact that Carnap's works on inductive logic are not so well known among British logicians and methodologists as they deserve to be, in my opinion, it might be worth-while to dedicate more space to this discussion than I did in my very brief *Comments*.

Carnap himself will answer Popper's *Reply* in so far as it is based upon attributing to Carnap statements which he did not make. This will enable me to restrict myself to the substantive side of the issue. I shall, however, rely occasionally on Carnap's remarks.

(2) It should by now be perfectly clear that the terms 'logical probability', 'degree of confirmation', 'degree (measure) of relevance', 'degree (measure) of (evidential, factual) support' (and others), in their pre-systematic uses, cover at least two explicanda which, strongly related as they may be, are still quite different. (This ambiguity is, of course, different from that of the term 'probability' itself, exhibited and treated by Carnap as 'probability$_1$' vs. 'probability$_2$'.) A hypothesis *h* with a high initial (or absolute) logical probability will retain the same high degree of probability relative to any evidence-statement *e* that is irrelevant to it (from which *h* is independent). *h* may even retain a high probability, though not as high as before, in face of an evidence-statement *e* that is negatively relevant to it (that undermines it). All this is commonplace and probably would not cause any controversy, were it not for the fact that Carnap would, in his systematic use of 'degree of confirmation' (but also in some pre-systematic uses of this term), express the situation by saying that in both cases *h* has a high degree of confirmation on *e*, which can easily be paraphrased by saying that *h* is highly confirmed by *e*, which sounds paradoxical enough since *e* is either irrelevant to *h*, in the first case, or even negatively relevant, in the second case.

(3) Had Popper only called attention to this terminological oddity and insisted on having it revised, reserving the expression 'degree of confirmation' for a pre-systematic synonym (or systematic explicatum) of 'degree of relevance', he would have a strong point, and I, for one, would have had no objection. It even seems to be the case that Popper himself did use this term, or rather its German equivalent '*Grad der Bewährung*', in this sense and that Carnap did not notice immediately that he was deviating from this use when he began using the term himself somewhat later, thereby adding to the confusion. And it is true that Carnap, as late as in 1950, in section 41A of *Probability*, presented one of the characterisations of probability$_1$ (logical probability) in terms of strength of support, which is indeed definitely misleading. It seems that by stating there, 'To say that the probability$_1$ of *h* on *e* is high means that *e* gives strong support to the assumption of *h*, that *h* is highly confirmed by *e* . . .' (p. 164), Carnap himself fell prey, for a moment, to the ambiguity of the pre-systematic usage of 'highly confirmed', and that he (erroneously) identified 'the probability of *h* on *e* is high' with '*e* gives strong support to *h*', in this preliminary discussion, just because both locutions can somehow be replaced by 'the degree of confirmation of *h* on *e* is high', the second in ordinary language, the first in Carnap's own technical sense.

(4) Popper, however, was not satisfied by calling attention to these inadvertencies but went on to charge Carnap's systematic use of 'degree of confirmation' with inconsistency and inadequacy, as mentioned before. I intend to show that the charge of inconsistency is completely unfounded, whereas the charge of inadequacy is justified only to a very limited degree.

(5) Popper is aware of the difficulties involved in showing that a certain explication is inadequate, in view of the vagueness of the formulations in which the conditions of adequacy are generally, and necessarily, couched. He still believes that in our case the divergence from one of these conditions is so flagrant as to leave no doubt about the inadequacy. The violated condition is that of sufficient agreement with intuition, where 'sufficient' is to be interpreted as 'approximate'. The inadequacy

consists then in the fact that the initial as well as any relative value assigned by Carnap's *c*-functions to a universal hypothesis in a universe with infinitely many individuals is zero, whereas ordinary intuition would regard at least some of these hypotheses as highly confirmed, and hence insist upon having assigned to them a high degree of confirmation.

If Popper is right, then he has indeed shown that Carnap's *c*-functions are inadequate explicata. This, of course, would not at all deprive these functions of all value. There are innumerable scientific concepts that can by no means be regarded as explicata of pre-scientific notions and are nevertheless of highest importance. But Carnap's work would certainly lose much of its philosophical significance.

(6) But is Popper right ? Is a value zero for a would-be explicatum of a logical-probability function for a general law in an infinite universe really counter-intuitive ? The gist of Carnap's argument in *Probability*, section 110G, is that this is not so, at least not for *guided* intuition, though it might be so for unguided intuition. Carnap believes that he is in a position to persuade those who, for intuitive reasons, would like to assign to a ' well-confirmed ' law a high value of logical probability not to insist on this requirement, in case it should lead to technical difficulties, and to look on this inclination as a rather misguided expression of their desire to assign high probability-values to the next few instances of the law. There are some good arguments that can be brought forward in support of Carnap's attitude, and he will doubtless exhibit them in the second volume of *Probability*. I am not sure that they will convince Popper. But even if they did, it must be admitted that the qualification ' guided ' that has to be prefixed to ' intuition ' in the phrase ' sufficient agreement with intuition ' is a major change in the formulation of the adequacy conditions, and a change to the better, to my mind.

(7) Further, Popper has separate objections against Carnap's notions of instance-confirmation and qualified instance confirmation. I did not understand his first reason for the claim of inadequacy of the unqualified instance confirmation (*Reply*, p. 160). His second reason consists in asserting that Carnap's definition of this notion as well as that of the qualified instance confirmation is inconsistent, because it is hit by the paradox of confirmation (*Reply*, p. 161). I glanced, at Popper's invitation, at the two pages of *Probability* (p. 572 and p. 469) that were supposed to contain the inconsistency but could find none. Sticking to Carnap's definition for ' c_{qi} ', given in *Probability*, page 573, it is rather obvious that the values of c_{qi} will be invariant with respect to any replacement of one of its arguments by an *L*-equivalent one. What now made Popper think differently ? He seems to have been misled by the fact that the qualified instance confirmation of a law l_1 need not be the same as the qualified instance confirmation of a law l_2 which is *L*-equivalent to l_1. The point is, of course, that l_1 and l_2 are not at all arguments of the relevant function (c_{qi}). (It is true that Carnap did not mention this point explicitly, but then he promised to deal more extensively with the whole topic in vol. II of *Probability*.[1])

[1] Popper suggests a ' rectification ' of Carnap's definition of the qualified instance confirmation. He does not, however, say explicitly what he means to take as the definiendum. If he intends to retain Carnap's own definiendum, then his suggestion is an unnecessary complication, since Carnap's simpler form also fulfils the invariance requirement. However, if he intends to have ' $c_{qi}(l, e)$ ' as the definiendum, it can be shown that Popper's definition is hit by the very paradox it was aimed to avoid.

What has to be understood is that the value of the qualified instance confirmation of a law *l* having the form of a general implication, on a certain evidence *e*, is *not* the value of the degree of confirmation of *l* (on some evidence *e'*) but rather the degree of confirmation of a new instance of the consequent of *l* on an evidence which is the conjunction of *e* and the corresponding instance of the antecedent of *l*.

The same misunderstanding made Popper claim somewhat further (*Reply*, p. 161) that Carnap's two concepts of instance confirmation led to absurd consequences. The first illustration presented by Popper for this purpose consists of a universe of coin tosses, with only two predicates ' heads up ' and ' tails up '. Let the evidence *e* be ' out of twenty past tosses ten were heads up and ten tails up ', let the hypothesis *h* be ' all future tosses will be heads up'. Under these circumstances, charges Popper, everybody would agree that the hypothesis *h* has been amply refuted by the evidence, whereas Carnap assigns to this hypothesis on this evidence an unqualified instance confirmation of $\frac{1}{2}$. The truth is, of course, that Carnap too assigns to *h* a very low confirmation value—for an infinite universe just zero—whereas the confirmation value $\frac{1}{2}$ is assigned not to *h*—and this in spite of the English formulation—but rather to an instance of *h*. And this value looks very reasonable, since it seems to be fair to bet on this evidence with even odds that the next toss will result in heads up. (Popper does never mention this characterisation of degree of confirmation, i.e. as a fair betting quotient, though Carnap himself regards it clearly as a more adequate characterisation than that through evidential support.)

(8) The seemingly strongest objection of Popper's, viz. that connected with the *content-condition* and discussed in *Reply*, page 160, is based upon a simple confusion, as Carnap shows in his *Remarks*, and will therefore not be discussed here.

(9) In conclusion : Though Popper's criticism of Carnap's position seems again to be objectively unfounded, in the main, and based upon factual errors as well as misunderstandings, it forcefully shows the necessity of further discussion of the exact relationships between the systematic uses of such terms as Carnap's ' degree of confirmation ', ' relevance measure ', ' instance confirmation ', Popper's ' logical probability ', ' degree of confirmation ' or Kemeny-Oppenheim's ' degree of factual support ' and the pre-systematic uses of these terms as well as of ' measure of evidential support ', ' content ', ' acceptability ' and ' reliability '.[1] It is indeed Carnap's contention that all these pre-systematic usages—in their semantic aspects—can be explicated in terms of (regular) *c*-functions (though not, of course, always *as c*-functions). One of Popper's main aims in his recent series of notes seems to have been to challenge this contention. I believe that he failed to substantiate his objections. He did, however, succeed in calling attention to some weaknesses in current terminology, and it might perhaps be advisable to change these formulations accordingly, in addition to giving new thought to the clarification of the relationship between systematic and pre-systematic usages of the current terms in the field of probability and confirmation.

Y. Bar-Hillel

[1] I do not believe that Carnap would want to regard either his degree of confirmation or his relevance measure as explicata of the *acceptability* of a theory, though he might have done so in the past. He does, however, regard instance confirmation as a measure of the *reliability* of a law. Cf. *Probability*, p. 572 and *Reply*, p. 162 and n. 2.