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Confirmation and Relevance

Item: One of the earliest surprises to emerge from Carnap's precise and systematic study of confirmation was the untenability of the initially plausible Wittgenstein confirmation function c†. Carnap's objection rested on the fact that c† precludes "learning from experience" because it fails to incorporate suitable relevance relations. Carnap's alternative confirmation function c* was offered as a distinct improvement because it does sustain the desired relevance relations.[1]

Item: On somewhat similar grounds, it has been argued that the heuristic appeal of the concept of partial entailment, which is often used to explain the basic idea behind inductive logic, rests upon a confusion of relevance with nonrelevance relations. Once this confusion is cleared up, it seems, the apparent value of the analogy between full and partial entailment vanishes.[2]

Item: In a careful discussion, based upon his detailed analysis of relevance, Carnap showed convincingly that Hempel's classic conditions of adequacy for any explication of the concept of confirmation are vitiated by another confusion of relevance with nonrelevance relations.[3]

Item: A famous controversy, in which Popper charges that Carnap's theory of confirmation contains a logical inconsistency, revolves around the same issue. As a result of this controversy, Carnap acknowledged in the preface to the second edition of *Logical Foundations of Probability*

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[1] Rudolf Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950), sec. 110A.

[2] Wesley C. Salmon, "Partial Entailment as a Basis for Inductive Logic," in Nicholas Rescher, ed., *Essays in Honor of Carl G. Hempel* (Dordrecht: Reidel, 1969), and "Carnap's Inductive Logic," *Journal of Philosophy*, 64 (1967), 725–39.

[3] Carnap, *Logical Foundations*, secs. 86–88.

that the first edition had been unclear with regard to this very distinction between relevance and nonrelevance concepts.[4]

Item: A new account of statistical explanation, based upon relations of relevance, has recently been proposed as an improvement over Hempel's well-known account, which is based upon relations of high degree of confirmation.[5]

Item: The problem of whether inductive logic can embody rules of acceptance — i.e., whether there are such things as inductive inferences in the usual sense — has been a source of deep concern to inductive logicians since the publication of Carnap's *Logical Foundations of Probability* (1950). Risto Hilpinen has proposed a rule of inductive inference which, he claims, avoids the "lottery paradox," thus overcoming the chief obstacle to the admission of rules of acceptance. Hilpinen's rule achieves this feat by incorporating a suitable combination of relevance and high confirmation requirements.[6]

The foregoing enumeration of issues is designed to show the crucial importance of the relations between confirmation and relevance. Yet, in spite of the fact that many important technical results have been available at least since the publication of Carnap's *Logical Foundations of Probability* in 1950, it seems to me that their consequences for the concept of confirmation have not been widely acknowledged or appreciated. In the first three sections of this paper, I shall summarize some of the most important facts, mainly to make them available in a single concise and relatively nontechnical presentation. All the material of these sections is taken from the published literature, much of it from the latter chapters of Carnap's book. In section 4, in response to questions raised by Adolf Grünbaum,[7] I shall apply some of these considerations to the Duhemian problems, where, to the best of my knowledge, they have not previously been brought to bear. In section 5, I shall attempt to pinpoint the source of some apparent difficulties with relevance concepts, and in the final section, I shall try to draw some morals from these results. All in all, I believe that many of the facts are shocking and counterintuitive

[4] Karl R. Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), appendix 9; Carnap, *Logical Foundations*, preface to the 2nd ed., 1962.

[5] Wesley C. Salmon, *Statistical Explanation and Statistical Relevance* (Pittsburgh: University of Pittsburgh Press, 1971).

[6] Risto Hilpinen, *Rules of Acceptance and Inductive Logic*, in *Acta Philosophica Fennica*, XXII (Amsterdam: North-Holland, 1968).

[7] In private conversation. At the same time he reported that much of his stimulus for raising these questions resulted from discussions with Professor Laurens Laudan.

and that they have considerable bearing upon current ideas about confirmation.

1. Carnap and Hempel

As Carnap pointed out in *Logical Foundations of Probability*, the concept of confirmation is radically ambiguous. If we say, for example, that the special theory of relativity has been confirmed by experimental evidence, we might have either of two quite distinct meanings in mind. On the one hand, we may intend to say that the special theory has become an accepted part of scientific knowledge and that it is very nearly certain in the light of its supporting evidence. If we admit that scientific hypotheses can have numerical degrees of confirmation, the sentence, on this construal, says that the degree of confirmation of the special theory on the available evidence is high. On the other hand, the same sentence might be used to make a very different statement. It may be taken to mean that some particular evidence — e.g., observations on the lifetimes of mesons — renders the special theory more acceptable or better founded than it was in the absence of this evidence. If numerical degrees of confirmation are again admitted, this latter construal of the sentence amounts to the claim that the special theory has a higher degree of confirmation on the basis of the new evidence than it had on the basis of the previous evidence alone.

The discrepancy between these two meanings is made obvious by the fact that a hypothesis h, which has a rather low degree of confirmation on prior evidence e, may have its degree of confirmation raised by an item of positive evidence i without attaining a high degree of confirmation on the augmented body of evidence $e.i$. In other words, a hypothesis may be confirmed (in the second sense) without being confirmed (in the first sense). Of course, we may believe that hypotheses can achieve high degrees of confirmation by the accumulation of many positive instances, but that is an entirely different matter. It is initially conceivable that a hypothesis with a low degree of confirmation might have its degree of confirmation increased repeatedly by positive instances, but in such a way that the confirmation approaches ¼ (say) rather than 1. Thus, it may be possible for hypotheses to be repeatedly confirmed (in the second sense) without ever getting confirmed (in the first sense). It can also work the other way. A hypothesis h that already has a high degree of confirmation on evidence e may still have a high degree of confirmation on evidence e.*i*., even though the addition of evidence *i* does not raise the degree of confirmation of *h*. In this case, *h* is confirmed (in the first sense) without being confirmed (in the second sense) on the basis of additional evidence *i*.

If we continue to speak in terms of numerical degrees of confirmation, as I shall do throughout this paper, we can formulate the distinction between these two senses of the term "confirm" clearly and concisely. For uniformity of formulation, let us assume some background evidence e (which may, upon occasion, be the tautological evidence t) as well as some additional evidence *i* on the basis of which degrees of confirmation are to be assessed. We can define "confirm" in the first (absolute; non-relevance) sense as follows:

D1. Hypothesis *h* is confirmed (in the absolute sense) by evidence *i* in the presence of background evidence $e =_{df} c(h, e.i) > b$, where *b* is some chosen number, presumably close to 1.

This concept is absolute in that it makes no reference to the degree of confirmation of *h* on any other body of evidence.[8] The second (relevance) sense of "confirm" can be defined as follows:

D2. Hypothesis *h* is confirmed (in the relevance sense) by evidence *i* in the presence of background evidence $e =_{df} c(h,e.i) > c(h,e)$.

This is a relevance concept because it embodies a relation of change in degree of confirmation. Indeed, Carnap's main discussion of this distinction follows his technical treatment of relevance relations, and the second concept of confirmation is explicitly recognized as being identical with the concept of positive relevance.[9]

It is in this context that Carnap offers a detailed critical discussion of Hempel's criteria of adequacy for an explication of confirmation.[10] Car-

[8] The term "absolute probability" is sometimes used to refer to probabilities that are not relative or conditional. E.g., Carnap's null confirmation $c_0(h)$ is an absolute probability, as contrasted with $c(h,e)$ in which the degree of confirmation of *h* is relative to, or conditional upon, e. The distinction I am making between the concepts defined in D1 and D2 is quite different. It is a distinction between two different types of confirmation, where one is a conditional probability and the other is a relevance relation defined in terms of conditional probabilities. In this paper, I shall not use the concept of absolute probability at all; in place of null confirmation I shall always use the confirmation $c(h,t)$ on tautological evidence, which is equivalent to the null confirmation, but which is a conditional or relative probability.

[9] Carnap, *Logical Foundations*, sec. 86.

[10] These conditions of adequacy are presented in Carl G. Hempel, "Studies in the

nap shows conclusively, I believe, that Hempel has conflated the two concepts of confirmation, with the result that he has adopted an indefensible set of conditions of adequacy. As Carnap says, he is dealing with two explicanda, not with a single unambiguous one. The incipient confusion is signaled by his characterization of

> the quest for general objective criteria determining (A) whether, and — if possible — even (B) to what degree, a hypothesis h may be said to be corroborated by a given body of evidence e. . . . The two parts of this . . . problem can be related in somewhat more precise terms as follows:
>
> (A) To give precise definitions of the two nonquantitative relational concepts of confirmation and disconfirmation; *i.e.* to define the meaning of the phrases 'e confirms h' and 'e disconfirms h'. (When e neither confirms nor disconfirms h, we shall say that e is neutral, or irrelevant, with respect to h.)
>
> (B) (1) To lay down criteria defining a metrical concept "degree of confirmation of h with respect to e," whose values are real numbers . . .[11]

The parenthetical remark under (A) makes it particularly clear that a relevance concept of confirmation is involved there, while a nonrelevance concept of confirmation is obviously involved in (B).

The difficulties start to show up when Hempel begins laying down conditions of adequacy for the concept of confirmation (A) (as opposed to degree of confirmation (B)). According to the very first condition "entailment is a special case of confirmation." This condition states:

> H-8.1 Entailment Condition. Any sentence which is entailed by an observation report is confirmed by it.[12]

If we are concerned with the absolute concept of confirmation, this condition is impeccable, for $c(h,e) = 1$ if e entails h. It is not acceptable, however, as a criterion of adequacy for a relevance concept of confirmation. For suppose our hypothesis h is "$(\exists x)Fx$" while evidence e is "Fa" and evidence i is "Fb." In this case, i entails h, but i does not confirm h in the relevance sense, for $c(h,e.i) = 1 = c(h,e)$.

Carnap offers further arguments to show that the following condition has a similar defect:

Logic of Confirmation," *Mind*, 54 (1945), 1–26, 97–121. Reprinted, with a 1964 postscript, in Carl G. Hempel, *Aspects of Scientific Explanation* (New York: Free Press, 1965). Page references in succeeding notes will be to the reprinted version.

[11] Hempel, *Aspects of Scientific Explanation*, p. 6. Hempel's capital letters "*H*" and "*E*" have been changed to lowercase for uniformity with Carnap's notation.

[12] *Ibid.*, p. 31. Following Carnap, an "H" is attached to the numbers of Hempel's conditions.

H-8.3 Consistency Condition. Every logically consistent observation report is logically compatible with the class of all the hypotheses which it confirms.[13]

This condition, like the entailment condition, is suitable for the absolute concept of confirmation, but not for the relevance concept. For, although no two incompatible hypotheses can have high degrees of confirmation on the same body of evidence, an observation report can be positively relevant to a number of different and incompatible hypotheses, provided that none of them has too high a prior degree of confirmation on the background evidence e. This happens typically when a given observation is compatible with a number of incompatible hypotheses — when, for example, a given bit of quantitative data fits several possible curves.

The remaining condition Hempel wished to adopt is as follows:

H-8.2 Consequence Condition. If an observation report confirms every one of a class k of sentences, then it also confirms any sentence which is a logical consequence of k.[14]

It will suffice to look at two conditions that follow from it:[15]

H-8.21 Special Consequence Condition. If an observation report confirms a hypothesis h, then it also confirms every consequence of h.

H-8.22 Equivalence Condition. If an observation report confirms a hypothesis h, then it also confirms every hypothesis that is logically equivalent with h.

The equivalence condition must hold for both concepts of confirmation. Within the formal calculus of probability (which Carnap's concept of degree of confirmation satisfies) we can show that, if h is equivalent to h', then $c(h,e) = c(h',e)$, for any evidence e whatever. Thus, if h has a high degree of confirmation on $e.i$, h' does also. Likewise, if i increases the degree of confirmation of h, it will also increase the degree of confirmation of h'.

The special consequence condition is easily shown to be valid for the nonrelevance concept of confirmation. If h entails k, then $c(k,e) \geqq c(h,e)$; hence, if $c(h,e.i) > b$, then $c(k,e.i) > b$. But here, I think, our intuitions mislead us most seductively. It turns out, as Carnap has shown with great clarity, that the special consequence condition fails for the

[13] *Ibid.*, p. 33.
[14] *Ibid.*, p. 31.
[15] *Ibid.*

relevance concept of confirmation. It is entirely possible for i to be positively relevant to h without being positively relevant to some logical consequence k. We shall return in section 3 to a more detailed discussion of this fact.

The net result of the confusion of the two different concepts is that obviously correct statements about confirmation relations of one type are laid down as conditions of adequacy for explications of concepts of the other type, where, upon examination, they turn out to be clearly unsatisfactory. Carnap showed how the entailment condition could be modified to make it acceptable as a condition of adequacy.[16] As long as we are dealing with the relevance concept of confirmation, it looks as if the consistency condition should simply be abandoned. The equivalence condition appears to be acceptable as it stands. The special consequence condition, surprisingly enough, cannot be retained.

Hempel tried to lay down conditions for a nonquantitative concept of confirmation, and we have seen some of the troubles he encountered. After careful investigation of this problem, Carnap came to the conclusion that it is best to establish a quantitative concept of degree of confirmation and then to make the definition of the two nonquantitative concepts dependent upon it, as we have done in D1 and D2 above.[17] Given a quantitative concept and the two definitions, there is no need for conditions of adequacy like those advanced by Hempel. The nonquantitative concepts of confirmation are fully determined by those definitions, but we may, if we wish, see what general conditions such as H-8.1, H-8.2, H-8.21, H-8.22, H-8.3 are satisfied. In a 1964 postcript to the earlier article, Hempel expresses general agreement with this approach of Carnap.[18] Yet, he does so with such equanimity that I wonder whether he, as well as many others, recognize the profound and far-reaching consequences of the fact that the relevance concept of confirmation fails to satisfy the special consequence condition and other closely related conditions (which will be discussed in section 3).

2. Carnap and Popper

Once the fundamental ambiguity of the term "confirm" has been pointed out, we might suppose that reasonably well-informed authors

[16] Carnap, *Logical Foundations*, p. 473.
[17] *Ibid.*, p. 467.
[18] Hempel, *Aspects of Scientific Explanation*, p. 50.

could easily avoid confusing the two senses. Ironically, even Carnap himself did not remain entirely free from this fault. In the preface to the second edition of *Logical Foundations of Probability* (1962), he acknowledges that the first edition was not completely unambiguous. In the new preface, he attempts to straighten out the difficulty.

In the first edition, Carnap had distinguished a triplet of confirmation concepts:[19]

1. Classificatory — e confirms h.
2. Comparative — e confirms h more than e' confirms h'.
3. Quantitative — the degree of confirmation of h on e is u

In the second edition, he sees the need for two such triplets of concepts.[20] For this purpose, he begins by distinguishing what he calls "concepts of firmness" and "concepts of increase of firmness." The concept of confirmation we defined above in D1, which was called an "absolute" concept, falls under the heading "concepts of firmness." The concept of confirmation we defined in D2, and called a "relevance" concept, falls under the heading "concepts of increase of firmness." Under each of these headings, Carnap sets out a triplet of classificatory, comparative, and quantitative concepts:

I. Three Concepts of Firmness

I-1. h is firm on the basis of e.	$c(h,e) > b$, where b is some fixed number.
I-2. h is firmer on e than is h' on e'.	$c(h,e) > c(h',e')$.
I-3. The degree of firmness of h on e is u.	$c(h,e) = u$.

To deal with the concepts of increase of firmness, Carnap introduces a simple relevance measure $D(h,i) =_{df} c(h,i) - c(h,t)$. This is a measure of what might be called "initial relevance," where the tautological evidence t serves as background evidence. The second triplet of concepts is given as follows:

II. Three Concepts of Increase of Firmness

II-1. h is made firmer by i. $D(h,i) > 0$.

II-2. The increase in firmness of h due to i is greater than the increase of firmness of h' due to i'. $D(h,i) > D(h',i')$.

[19] Carnap, *Logical Foundations*, sec. 8.

[20] Carnap, *Logical Foundations*, 2nd ed., pp. xv–xvi.

II-3. The amount of increase of firmness of h due to i is w.

$$D(h,i) = w.$$

Given the foregoing arrays of concepts, any temptation we might have had to identify the absolute (nonrelevance) concept of confirmation with the original classificatory concept, and to identify the relevance concept of confirmation with the original comparative concept, while distinguishing both from the original quantitative concept of degree of confirmation, can be seen to be quite mistaken. What we defined above in D1 as the absolute concept of confirmation is clearly seen to coincide with Carnap's new classificatory concept I-1, while our relevance concept of confirmation defined in D2 obviously coincides with Carnap's other new classificatory concept II-1. Carnap's familiar concept of degree of confirmation (probability$_1$) is obviously his quantitative concept of firmness I-3, while his new quantitative concept II-3 coincides with the concept of degree of relevance. Although we shall not have much occasion to deal with the comparative concepts, it is perhaps worth mentioning that the new comparative concept II-2 has an important use. When we compare the strengths of different tests of the same hypothesis we frequently have occasion to say that a test that yields evidence i is better than a test that yields evidence i'; sometimes, at least, this means that i is more relevant to h than is i' — i.e., the finding i would increase the degree of confirmation of h by a greater amount than would the finding i'.

It is useful to have a more general measure of relevance than the measure D of initial relevance. We therefore define the relevance of evidence i to hypothesis h on (in the presence of) background evidence e as follows:[21]

D3. $R(i,h,e) = c(h,e.i) - c(h,e)$.

Then we can say:

D4. i is positively relevant to h on e $=_{df} R(i,h,e) > 0$.
i is negatively relevant to h on e $=_{df} R(i,h,e) < 0$.
i is irrelevant to h on e $=_{df} R(i,h,e) = 0$.

[21] Carnap introduces the simple initial relevance measure D for temporary heuristic purposes in the preface to the second edition. In ch. 6 he discusses both our relevance measure R and Keynes's relevance quotient $c(h,e.i)/c(h,e)$, but for his own technical purposes he adopts a more complicated relevance measure $r(i,h,e)$. For purposes of this paper, I prefer the simpler and more intuitive measure R, which serves as well as Carnap's measure r in the present context. Since this measure differs from that used by Carnap in ch. 6 of *Logical Foundations*, I use a capital "R" to distinguish it from Carnap's lowercase symbol.

Hence, the classificatory concept of confirmation in the relevance sense can be defined by the condition $R(i,h,e) > 0$. Using these relevance concepts, we can define a set of concepts of confirmation in the relevance sense as follows:

D5. *i* confirms *h* given e $=_{df}$ *i* is positively relevant to *h* on e.
i disconfirms *h* given e $=_{df}$ *i* is negatively relevant to *h* on e.
i neither confirms nor disconfirms *h* given e $=_{df}$ *i* is irrelevant to *h* on e.

Having delineated his two triplets of concepts, Carnap then acknowledges that his original triplet in the first edition was a mixture of concepts of the two types; in particular, he had adopted the classificatory concept of increase of firmness II-1, along with the comparative and quantitative concepts of firmness I-2 and I-3. This fact, plus some misleading informal remarks about these concepts, led to serious confusion.[22]

As Carnap further acknowledges, Popper called attention to these difficulties, but he fell victim to them as well.[23] By equivocating on the admittedly ambiguous concept of confirmation, he claimed to have derived a contradiction within Carnap's formal theory of probability$_1$. He offers the following example:

> Consider the next throw with a homogeneous die. Let x be the statement 'six will turn up'; let y be its negation, that is to say, let $y = \bar{x}$; and let z be the information 'an even number will turn up'.
>
> We have the following absolute probabilities:
>
> $p(x) = 1/6$; $p(y) = 5/6$; $p(z) = 1/2$.
>
> Moreover, we have the following relative probabilities:
>
> $p(x,z) = 1/3$; $p(y,z) = 2/3$.
>
> We see that x is supported by the information z, for z raises the probability of x from 1/6 to $2/6 = 1/3$. We also see that y is undermined by z, for z lowers the probability of y by the same amount from 5/6 to $4/6 = 2/3$. Nevertheless, we have $p(x,z) < p(y,z)$.[24]

From this example, Popper quite correctly draws the conclusion that there are statements x, y, and z such that z confirms x, z disconfirms y, and y has a higher degree of confirmation on z than x has. As Popper points out quite clearly, this result would be logically inconsistent *if* we were to take the term "confirm" in its nonrelevance sense. It would be self-contradictory to say,

[22] Carnap, *Logical Foundations*, 2nd ed., pp. xvii–xix.
[23] *Ibid.*, p. xix, fn.
[24] Popper, *The Logic of Scientific Discovery*, p. 390.

> The degree of confirmation of x on z is high, the degree of confirmation of y on z is not high, and the degree of confirmation of y on z is higher than the degree of confirmation of x on z.

The example, of course, justifies no such statement; there is no way to pick the number *b* employed by Carnap in his definition of confirmation in the (firmness) sense I-1 according to which we could say that the degree of confirmation of x on z is greater than *b* and the degree of confirmation of y on z is not greater than *b*. The proper formulation of the situation is:

> The evidence z is positively relevant to x, the evidence z is negatively relevant to y, and the degree of confirmation of x on z is less than the degree of confirmation of y on z.

There is nothing even slightly paradoxical about this statement.

Popper's example shows clearly the danger of equivocating between the two concepts of confirmation, but it certainly does not show any inherent defect in Carnap's system of inductive logic, for this system contains both degree of confirmation $c(h,e)$ and degree of relevance $r(i,h,e)$. The latter is clearly and unambiguously defined in terms of the former, and there are no grounds for confusing them.[25] The example shows, however, the importance of exercising great care in our use of English language expressions in talking about these exact concepts.

3. The Vagaries of Relevance

It can be soundly urged, I believe, that the verb "to confirm" is used more frequently in its relevance sense than in the absolute sense. When we say that a test confirmed a hypothesis, we would normally be taken to mean that the result was positively relevant to the hypothesis. When we say that positive instances are confirming instances, it seems that we are characterizing confirming evidence as evidence that is positively relevant to the hypothesis in question. If we say that several investigators independently confirmed some hypothesis, it would seem sensible to understand that each of them had found positively relevant evidence. There is no need to belabor this point. Let us simply assert that the term "confirm" is often used in its relevance sense, and we wish to investigate some of the properties of this concept. In other words, let us agree for now to use the term "confirm" solely in its relevance sense

[25] Carnap, *Logical Foundations*, sec. 67.

(unless some explicit qualification indicates otherwise), and see what we will be committed to.

It would be easy to suppose that, once we are clear on the two senses of the term *confirm* and once we resolve to use it in only one of these senses in a given context, it would be a simple matter to tidy up our usage enough to avoid such nasty equivocations as we have already discussed. This is not the case, I fear. For, as Carnap has shown by means of simple examples and elegant arguments, the relevance concept of confirmation has some highly counterintuitive properties.

Suppose, for instance, that two scientists are interested in the same hypothesis *h*, and they go off to their separate laboratories to perform tests of that hypothesis. The tests yield two positive results, *i* and *j*. Each of the evidence statements is positively relevant to *h* in the presence of common background information e. Each scientist happily reports his positive finding to the other. Can they now safely conclude that the net result of both tests is a confirmation of *h*? The answer, amazingly, is no! As Carnap has shown, two separate items of evidence can each be positively relevant to a hypothesis, while their conjunction is negative to that very same hypothesis. He offers the following example:[26]

Example 1. Let the prior evidence e contain the following information. Ten chess players participate in a chess tournament in New York City; some of them are local people, some from out of town; some are junior players, some are seniors; some are men (M), some are women (W). Their distribution is known to be as follows:

	Local players	Out-of-towners
Juniors	M, W, W	M, M
Seniors	M, M	W, W, W

Table 1

Furthermore, the evidence e is supposed to be such that on its basis each of the ten players has an equal chance of becoming the winner, hence 1/10 . . . It is assumed that in each case [of evidence that certain players have been eliminated] the remaining players have equal chances of winning.

Let *h* be the hypothesis that a man wins. Let *i* be the evidence that a local player wins; let *j* be the evidence that a junior wins. Using the equiprobability information embodied in the background evidence e, we can read the following values directly from table 1:

[26] *Ibid.*, pp. 382–83. Somewhat paraphrased for brevity.

$$
\begin{array}{llll}
c(h,e) = 1/2 & c(h,e.i) = 3/5 & R(i,h,e) & = 1/10 \\
 & c(h,e.j) = 3/5 & R(j,h,e) & = 1/10 \\
 & c(h,e.i.j) = 1/3 & R(i.j,h,e) & = -1/6
\end{array}
$$

Thus, i and j are each positively relevant to h, while the conjunction $i.j$ is negatively relevant to h. In other words, i confirms h and j confirms h but $i.j$ disconfirms h.

The setup of example 1 can be used to show that a given piece of evidence may confirm each of two hypotheses individually, while that same evidence disconfirms their conjunction.[27]

Example 2. Let the evidence e be the same as in example 1. Let h be the hypothesis that a local player wins; let k be the hypothesis that a junior wins. Let i be evidence stating that a man wins. The following values can be read directly from table 1:

$$
\begin{array}{lll}
c(h,e) = 1/2 & c(h,e.i) = 3/5 & R(i,h,e) = 1/10 \\
c(k,e) = 1/2 & c(k,e.i) = 3/5 & R(i,k,e) = 1/10 \\
c(h.k,e) = 3/10 & c(h.k,e.i) = 1/5 & R(i,h.k,e) = -1/10
\end{array}
$$

Thus, i confirms h and i confirms k, but i disconfirms $h.k$.

In the light of this possibility it might transpire that a scientist has evidence that supports the hypothesis that there is gravitational attraction between any pair of bodies when at least one is of astronomical dimensions and the hypothesis of gravitational attraction between bodies when both are of terrestrial dimensions, but which disconfirms the law of universal gravitation! In the next section we shall see that this possibility has interesting philosophical consequences.

A further use of the same situation enables us to show that evidence can be positive to each of two hypotheses, and nevertheless negative to their disjunction.[28]

Example 3. Let the evidence e be the same as in example 1. Let h be the hypothesis that an out-of-towner wins; let k be the hypothesis that a senior wins. Let i be evidence stating that a woman wins. The following values can be read directly from table 1:

$$
\begin{array}{lll}
c(h,e) = 1/2 & c(h,e.i) = 3/5 & R(i,h,e) = 1/10 \\
c(k,e) = 1/2 & c(k,e.i) = 3/5 & R(i,k,e) = 1/10 \\
c(h \vee k,e) = 7/10 & c(h \vee k,e.i) = 3/5 & R(i,h \vee k,e) = -1/10
\end{array}
$$

Thus, i confirms h and i confirms k, but i nevertheless disconfirms $h \vee k$.

[27] *Ibid.*, pp. 394–95.
[28] *Ibid.*, p. 384.

Imagine the following situation:[29] a medical researcher finds evidence confirming the hypothesis that Jones is suffering from viral pneumonia and also confirming the hypothesis that Jones is suffering from bacterial pneumonia — yet this very same evidence disconfirms the hypothesis that Jones has pneumonia! It is difficult to entertain such a state of affairs, even as an abstract possibility.

These three examples illustrate a few members of a large series of severely counterintuitive situations that can be realized:

i. Each of two evidence statements may confirm a hypothesis, while their conjunction disconfirms it. (Example 1.)

ii. Each of two evidence statements may confirm a hypothesis, while their disjunction disconfirms it. (Example 2a, Carnap, *Logical Foundations*, p. 384.)

iii. A piece of evidence may confirm each of two hypotheses, while it disconfirms their conjunction. (Example 2.)

iv. A piece of evidence may confirm each of two hypotheses, while it disconfirms their disjunction. (Example 3.)

This list may be continued by systematically interchanging positive relevance (confirmation) and negative relevance (disconfirmation) throughout the preceding statements. Moreover, a large number of similar possibilities obtain if irrelevance is combined with positive or negative relevance. Carnap presents a systematic inventory of all of these possible relevance situations.[30]

In section 1, we mentioned that Hempel's special consequence condition does not hold for the relevance concept of confirmation. This fact immediately becomes apparent upon examination of statement iv above. Since h entails $h \vee k$, and since i may confirm h while disconfirming $h \vee k$, we have an instance in which evidence confirms a statement but fails to confirm one of its logical consequences. Statement ii, incidentally, shows that the converse consequence condition, which Hempel discusses but does not adopt,[31] also fails for the relevance concept of confirmation. Since $h.k$ entails h, and since i may confirm h without confirming $h.k$, we have an instance in which evidence confirms a hypothesis without confirming at least one statement from which that hypothesis follows. The failure of the special consequence condition and the converse consequence condition appears very mild when compared with the much

[29] This example is adapted from *ibid.*, p. 367.
[30] *Ibid.*, secs. 69, 71.

stronger results i–iv, and analogous ones. While one might, without feeling too queasy, give up the special consequence condition — the converse consequence condition being unsatisfactory on the basis of much more immediate and obvious considerations — it is much harder to swallow possibilities like i–iv without severe indigestion.

4. Duhem and Relevance

According to a crude account of scientific method, the testing of a hypothesis consists in deducing observational consequences and seeing whether or not the facts are as predicted. If the prediction is fulfilled we have a positive instance; if the prediction is false the result is a negative instance. There is a basic asymmetry between verification and falsification. If, from hypothesis *h*, an observational consequence o can be deduced, then the occurrence of a fact o′ that is incompatible with o (o′ entails ∼o) enables us to infer the falsity of *h* by good old modus tollens. If, however, we find the derived observational prediction fulfilled, we still cannot deduce the truth of *h*, for to do so would involve the fallacy of affirming the consequent.

There are many grounds for charging that the foregoing account is a gross oversimplification. One of the most familiar, which was emphasized by Duhem, points out that hypotheses are seldom, if ever, tested in isolation; instead, auxiliary hypotheses *a* are normally required as additional premises to make it possible to deduce observational consequences from the hypothesis *h* that is being tested. Hence, evidence o′ (incompatible with o) does not entail the falsity of *h*, but only the falsity of the conjunction *h.a*. There are no deductive grounds on which we can say that *h* rather than *a* is the member of the conjunction that has been falsified by the negative outcome. To whatever extent auxiliary hypotheses are invoked in the deduction of the observational consequence, to that extent the alleged deductive asymmetry of verification and falsification is untenable.

At this point, a clear warning should be flashed, for we recall the strange things that happen when conjunctions of hypotheses are considered. Example 2 of the previous section showed that evidence that disconfirms a conjunction *h.a* can nevertheless separately confirm each of the conjuncts. Is it possible that a *negative* result of a test of the hy-

[31] Hempel, *Aspects of Scientific Explanation*, pp. 32–33.

pothesis h, in which auxiliary hypotheses a were also involved, could result in the *confirmation* of the hypothesis of interest h *and* in a *confirmation* of the auxiliary hypotheses a as well?

It might be objected, at this point, that in the Duhemian situation o′ is not merely negatively relevant to $h.a$; rather,

(1) o′ entails $\sim(h.a)$.

This objection, though not quite accurate, raises a crucial question. Its inaccuracy lies in the fact that h and a together do not normally entail o′; in the usual situation some initial conditions are required along with the main hypothesis and the auxiliary hypotheses. If this is the case, condition (1) does not obtain. We can deal with this trivial objection to (1), however, by saying that, since the initial conditions are established by observation, they are to be taken as part of the background evidence e which figures in all of our previous investigations of relevance relations. Thus, we can assert that, in the presence of background evidence e, o can be derived from $h.a$. This allows us to reinstate condition (1).

Unfortunately, condition (1) is of no help to us. Consider the following situation:

Example 4. The evidence e contains the same equiprobability assumptions as the evidence in example 1, except for the fact that the distribution of players is as indicated in the following table:

	Local players	Out-of-towners
Juniors	W	M, M
Seniors	M, M	M, W, W, W, W

Table 2

Let h be the hypothesis that a local player wins; let k be the hypothesis that a junior wins. Let i be evidence stating that a man wins. In this case, condition (1) is satisfied; the evidence i is logically incompatible with the conjunction $h.k$. The following values can be read directly from the table:

$$c(h,e) = 0.3 \qquad c(h,e.i) = 0.4 \qquad R(i,h,e) = 0.1$$
$$c(k,e) = 0.3 \qquad c(k,e.i) = 0.4 \qquad R(i,k,e) = 0.1$$
$$c(h.k,e) = 0.1 \qquad c(h.k,e.i) = 0 \qquad R(i,h.k,e) = -0.1$$

This example shows that evidence i, even though it conclusively refutes the conjunction $h.k$, nevertheless confirms both h and k taken individually.

Here is the situation. Scientist Smith comes home late at night after a hard day at the lab. "How did your work go today, dear?" asks his wife.

"Well, you know the Smith hypothesis, h_s, on which I have staked my entire scientific reputation? And you know the experiment I was running today to test my hypothesis? Well, the result was negative."

"Oh, dear, what a shame! Now you have to give up your hypothesis and watch your entire reputation go down the drain!"

"Not at all. In order to carry out the test, I had to make use of some auxiliary hypotheses."

"Oh, what a relief — saved by Duhem! Your hypothesis wasn't refuted after all," says the philosophical Mrs. Smith.

"Better than that," Smith continues, "I actually confirmed the Smith hypothesis."

"Why that's wonderful, dear," replies Mrs. Smith, "you found you could reject an auxiliary hypothesis, and that in so doing, you could establish the fact that the test actually confirmed your hypothesis? How ingenious!"

"No," Smith continues, "it's even better. I found I had confirmed the auxiliary hypotheses as well!"

This is the Duhemian thesis reinforced with a vengeance. Not only does a negative test result fail to refute the test hypothesis conclusively — it may end up confirming both the test hypothesis and the auxiliary hypotheses as well.

It is very tempting to suppose that much of the difficulty might be averted if only we could have sufficient confidence in our auxiliary hypotheses. If a medical researcher has a hypothesis about a disease which entails the presence of a certain microorganism in the blood of our favorite victim Jones, it would be outrageous for him to call into question the laws of optics as applied to microscopes as a way of explaining failure to find the bacterium. If the auxiliary hypotheses are well enough established beforehand, we seem to know where to lay the blame when our observational predictions go wrong. The question is how to establish the auxiliary hypotheses in the first place, for if the Duhemian is right, no hypotheses are ever tested in isolation. To test any hypothesis, according to this view, it is necessary to utilize auxiliary hypotheses; consequently, to establish our auxiliary hypotheses a for use in the tests of h, we would need some other auxiliary hypotheses a′ to carry out the tests of a. A vicious regress threatens.

A more contextual approach might be tried.[32] Suppose that *a* has been used repeatedly as an auxiliary hypothesis in the *successful* testing of other hypotheses *j*, *k*, *l*, etc. Suppose, that is, that the conjunctions *j.a*, *k.a*, *l.a*, etc., have been tested and repeatedly confirmed — i.e., all test results have been positively relevant instances. Can we say that *a* has been highly confirmed as a result of all of these successes? Initially, we might have been tempted to draw the affirmative conclusion, but by now we know better. Examples similar to those of the previous section can easily be found to show that evidence positively relevant to a conjunction of two hypotheses can nevertheless be negative to each conjunct.[33] It is therefore logically possible for each confirmation of a conjunction containing *a* to constitute a disconfirmation of *a* — and indeed a disconfirmation of the other conjunct as well in each such case.

There is one important restriction that applies to the case in which new observational evidence refutes a conjunction of two hypotheses, namely, hypotheses that are incompatible on evidence *e.i* can have, at most, probabilities that add to one. If *e.i* entails $\sim (h.k)$

$$c(h,e.i) + c(k,e.i) \leq 1.$$

Since we are interested in the case in which *i* is positively relevant to both *h* and *k*, these hypotheses must also satisfy the condition

$$c(h,e) + c(k,e) < 1.$$

We have here, incidentally, what remains of the asymmetry between confirmation and refutation. If evidence *i* refutes the conjunction *h.k*, that fact places an upper limit on the sum of the probabilities of *h* and *k* relative to *e.i*. If, however, evidence *i* confirms a conjunction *h.k* while disconfirming each of the conjuncts, there is no lower bound (other than zero) on the sum of their degrees of confirmation on *i*.

In this connection, let us recall our ingenious scientist Smith, who turned a refuting test result into a positive confirmation of both his pet hypothesis h_S and his auxiliary hypotheses *a*. We see that he must have been working with a test hypothesis or auxiliaries (or both) which had rather low probabilities. We might well question the legitimacy of using hypotheses with degrees of confirmation appreciably less than one as

[32] This paper was motivated by Grünbaum's questions concerning this approach. See his "Falsifiability and Rationality" to be published in a volume edited by Joseph J. Kockelmans (proceedings of an international conference held at Pennsylvania State University).

[33] Carnap, *Logical Foundations*, pp. 394–95, 3b, is just such an example.

auxiliary hypotheses. If Smith's auxiliaries a had decent degrees of confirmation, his own hypothesis h_s must have been quite improbable. His clever wife might have made some choice remarks about his staking an entire reputation on so improbable a hypothesis. But I should not get carried away with dramatic license. If we eliminate all the unnecessary remarks about staking his reputation on h_s, and regard it rather as a hypothesis he finds interesting, then its initial improbability may be no ground for objection. Perhaps every interesting general scientific hypothesis starts its career with a very low prior probability. Knowing, as we do, that a positively relevant instance may disconfirm both our test hypothesis and our auxiliaries, while a negative instance may confirm them both, there remains a serious, and as yet unanswered, question how any hypothesis ever can become either reasonably well confirmed or reasonably conclusively disconfirmed (in the absolute sense). It obviously is still an open question how we could ever get any well-confirmed hypotheses to serve as auxiliaries for the purpose of testing other hypotheses.

Suppose, nevertheless, that we have a hypothesis h to test and some auxiliaries a that will be employed in conducting the test and that somehow we have ascertained that a has a higher prior confirmation than h on the initial evidence e:

$$c(a,e) > c(h,e).$$

Suppose, further, that as the result of the test we obtain negative evidence o′ which refutes the conjunction $h.a$, but which confirms both h and a. Thus, o′ entails $\sim(h.a)$ and

$$c(h,e.o') > c(h,e) \qquad c(a,e.o') > c(a,e).$$

We have already seen that this can happen (example 4). But now we ask the further question, is it possible that the posterior confirmation of h is greater than the posterior confirmation of a? In other words, can the negative evidence o′ confirm both conjuncts and do so in a way that reverses the relation between h and a? A simple example will show that the answer is affirmative.

Example 5. The Department of History and Philosophy of Science at Polly Tech had two openings, one in history of science and the other in philosophy of science. Among the 1000 applicants for the position in history, 100 were women. Among the 2000 applicants for the position in philosophy, 100 were women. Let h be the hypothesis that the history job was filled by a woman; let k be the hypothesis that the philosophy job was filled by a woman. Since both selections were made by the use of a fair lottery device belonging to the inductive logician in the department,

$$c(h,e) = .1$$
$$c(k,e) = .05$$
$$c(h,e) > c(k,e).$$

Let i be the evidence that the two new appointees were discovered engaging in heterosexual intercourse with each other in the office of the historian. It follows at once that

$$c(h.k,e.i) = 0$$
$$c(h,e.i) + c(k,e.i) = 1$$

i.e., one appointee was a woman and the other a man, but we do not know which is which. Since it is considerably more probable, let us assume, that the office used was that of the male celebrant, we assign the values

$$c(h,e.i) = .2 \qquad c(k,e.i) = .8$$

with the result that

$$c(h,e.i) < c(k,e.i).$$

This illustrates the possibility of a reversal of the comparative relation between the test hypothesis and auxiliaries as a result of refuting evidence. It shows that a's initial superiority to h is no assurance that it will still be so subsequent to the refuting evidence. If, prior to the negative test result, we had to choose between h and a, we would have preferred a, but after the negative outcome, h is preferable to a.

There is one significant constraint that must be fulfilled if this reversal is to occur in the stated circumstances. If our auxiliary hypotheses a are initially better confirmed than our test hypothesis h, and if the conjunction $h.a$ is refuted by evidence o′ that is positively relevant to both h and a, and if the posterior confirmation of h is greater than the posterior confirmation of a, then the prior confirmation of a must have been less than 1/2. For,

$$c(h,e.o') + c(a,e.o') \leq 1$$

and

$$c(h,e.o') > c(a,e.o').$$

Hence,

$$c(a,e.o') < 1/2.$$

Moreover,

$$c(a,e) < c(a,e.o').$$

Therefore,

$$c(a,e) < 1/2.$$

It follows that if *a* is initially more probable than *h* and also initially more probable than its own negation $\sim a$, then it is impossible for a refuting instance *o'* which confirms both *h* and *a* to render *h* more probable than *a*. Perhaps that is some comfort. If our auxiliaries are more probable than not, and if they are better established before the test than our test hypothesis *h*, then a refuting test outcome which confirms both *h* and *a* cannot make *h* preferable to *a*.

But this is not really the tough case. The most serious problem is whether a refutation of the conjunction *h.a* can render *h* more probable than *a* by being positively relevant to *h* and negatively relevant to *a*, even when *a* is initially *much* more highly confirmed than *h*. You will readily conclude that this is possible; after all of the weird outcomes we have discussed, this situation seems quite prosaic. Consider the following example:

Example 6. Let

e = Brown is an adult American male
h = Brown is a Roman Catholic
k = Brown is married

and suppose the following degrees of confirmation to obtain:

$$c(h,e) = .3$$
$$c(k,e) = .8$$
$$c(h.k,e) = .2.$$

Let *i* be the information that Brown is a priest — that is, an ordained clergyman of the Roman Catholic, Episcopal, or Eastern Orthodox church. Clearly, *i* refutes the conjunction *h.k*, so

$$c(h.k,e.i) = 0.$$

Since the overwhelming majority of priests in America are Roman Catholic, let us assume that

$$c(h,e.i) = .9$$

and since some, but not all, non-Roman Catholic priests marry, let

$$c(k,e.i) = .05.$$

We see that i is strongly relevant to both h and k; in particular, it is positively relevant to h and negatively relevant to k. Moreover, while k has a much higher degree of confirmation than h relative to the prior evidence e, h has a much higher degree of confirmation than k on the posterior evidence $e.i$. Thus, the refuting evidence serves to reverse the preferability relation between h and k.

It might be helpful to look at this situation diagrammatically and to think of it in terms of class ratios or frequencies. Since class ratios satisfy the mathematical calculus of probabilities, they provide a useful device for establishing the possibility of certain probability relations. With our background evidence e let us associate a reference class A, and with our hypotheses h and k let us associate two overlapping subclasses B and C respectively. With our additional evidence i let us associate a further subclass D of A. More precisely, let

$$e = x \in A, h = x \in B, k = x \in C, i = x \in D.$$

Since we are interested in the case in which the prior confirmation of k is high and the prior confirmation of h is somewhat lower, we want most of A to be included in C and somewhat less of A to be included in B. Moreover, since our hypotheses h and k should not be mutually exclusive on the prior evidence e alone, B and C must overlap. However, neither B nor C can be a subset of the other; they must be mutually exclusive within D, since h and k are mutually incompatible on additional evidence i. Moreover, because we are not considering cases in which $e.i$ entails either h or k alone, the intersections of D with B and C must both be nonnull. We incorporate all of these features in Figure 1. In order to achieve the desired result — that is, to have the posterior confirmation of h greater than the posterior confirmation of k — it is only necessary to draw D so that its intersection with B is larger than its intersection with C. This is obviously an unproblematic condition to fulfill. Indeed, there is no difficulty in arranging it so that the proportion of D occupied by its intersection with B is larger than the proportion of A occupied by its intersection with C. When this condition obtains, we can not only say that the evidence i has made the posterior confirmation of h greater than the posterior confirmation of k (thus

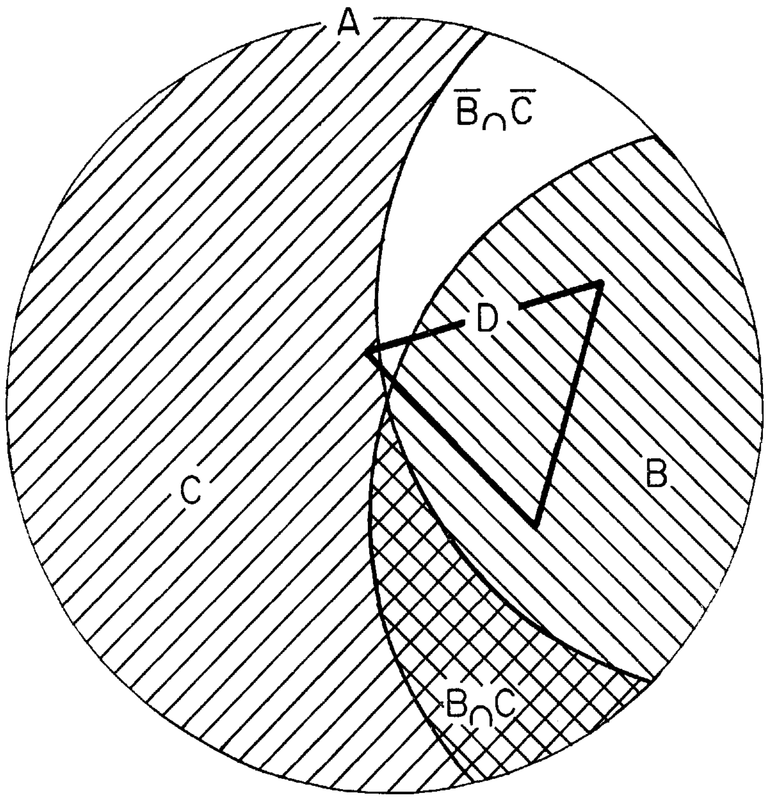

Figure 1

reversing their preferability standing), but also that the posterior confirmation of h is greater than the prior confirmation of k. Translated into the Duhemian situation, this means that not only can the refuting evidence o' disconfirm the initially highly probable auxiliary hypotheses a, but it can also confirm the test hypothesis h to the extent that its posterior confirmation makes it more certain than were the auxiliaries before the evidence o' turned up. This set of relationships is easily seen to be satisfied by example 6 if we let

$A =$ American men	$B =$ Roman Catholics
$C =$ married men	$D =$ priests.

It is evident from Figure 1 that C can be extremely probable relative to A without presenting any serious obstacle to the foregoing outcome, provided D is very much smaller than A. There seems little ground for assurance that a refuting experimental result will generally leave the auxiliaries intact, rather than drastically disconfirming them and radically elevating the degree of confirmation of the test hypothesis. This state of affairs can apparently arise with distressing ease, and there are no evident constraints that need to be fulfilled in order for it to happen. There seems to be no basis for confidence that it does not happen frequently in real scientific situations. If this is so, then whenever our auxiliaries have an initial degree of confirmation that falls ever so slightly short of certainty, we cannot legitimately infer with confidence that the test hypothesis h, rather than the auxiliaries, is disconfirmed by the refuting instance. Thus, no matter how high the initial probability of the auxiliaries a (provided it is somewhat less than certainty), it is still possible for a finding that entails the falsity of the conjunction $h.a$ to constitute a confirmation for either the one or the other. We certainly cannot say that the negative finding disconfirms h rather than a on the ground that a is more probable initially than h.

A parallel remark can be made about an instance that confirms the conjunction $h.a$. Such an instance might disconfirm either conjunct, and we have no way of saying which. In view of these dismal facts, we may well repeat, with even greater emphasis, the question posed earlier: how can any hypothesis (including those we need as auxiliary hypotheses for the testing of other hypotheses) ever be reasonably well confirmed or disconfirmed (in the absolute sense)?

The (to my mind) shocking possibilities that we have been surveying arise as consequences of the fact that we have been construing "confirm" in the relevance sense. What happens, I believe, is that we acknowledge with Carnap and Hempel that the classificatory concept of confirmation is best understood, in most contexts, as a concept of positive relevance defined on the basis of some quantitative degree of confirmation function. Then, in contexts such as the discussion of the Duhemian thesis, we proceed to talk casually about confirmation, forgetting that our intuitive notions are apt to be very seriously misleading. The old ambiguity of the absolute and the relevance sense of confirmation infects our intuitions, with the result that all kinds of unwarranted suppositions insinuate themselves. We find it extraordinarily difficult to keep a firm mental grasp upon such strange possibilities as we have seen in this section and the previous one.

5. Analysis of the Anomalies

There is, of course, a striking contrast between the "hypotheses" and "evidence" involved in our contrived examples, on the one hand, and the genuine hypotheses and evidence to be found in actual scientific practice, on the other. This observation might easily lead to the charge that the foregoing discussion is not pertinent to the logic of actual scientific confirmation, as opposed to the theory of confirmation constructed by Carnap on highly artificial and oversimplified languages. This irrelevance is demonstrated by the fact, so the objection might continue, that the kinds of problems and difficulties we have been discussing simply do not arise when real scientists test serious scientific hypotheses.

This objection, it seems to me, is wide of the mark. I am prepared to grant that such weird possibilities as we discussed in previous sections do not arise in scientific practice; at least, I have no concrete cases from the current or past history of science to offer as examples of them. This is, however, a statement of the problem rather than a solution. Carnap has provided a number of examples that, on the surface at least, seem to make a shambles of confirmation; why do they not also make a shambles of science itself? There can be no question that, for example, one statement can confirm each of two other statements separately while at the same time disconfirming their disjunction or conjunction. If that sort of phenomenon never occurs in actual scientific testing, it must be because we know something more about our evidence and hypotheses than merely that the evidence confirms the hypotheses. The problem is to determine the additional factors in the actual situation that block the strange results we can construct in the artificial case. In this section, I shall try to give some indications of what seems to be involved.

The crux of the situation seems to be the fact that we have said very little when we have stated merely that a hypothesis h has been confirmed by evidence i. This statement means, of course, that i raises the degree of confirmation of h, but that *in itself* provides very little information. It is by virtue of this paucity of content that we can go on and say that this same evidence i confirms hypothesis k as well, without being justified in saying anything about the effect of i upon the disjunction or the conjunction of h with k.

This state of affairs seems strange to intuitions that have been thoroughly conditioned on the extensional relations of truth-functional logic.

Probabilities are not extensional in the same way. Given the truth-values of h and k we can immediately ascertain the truth-values of the disjunction and the conjunction. The degrees of confirmation ("probability values") of h and k do not, however, determine the degree of confirmation of either the disjunction or the conjunction. This fact follows immediately from the addition and multiplication rules of the probability calculus:

$$(2) \qquad c(h \vee k,e) = c(h,e) + c(k,e) - c(h.k,e)$$
$$(3) \qquad c(h.k,e) = c(h,e) \times c(k,h.e) = c(k,e) \times c(h,k.e).$$

To determine the probability of the disjunction, we need, in addition to the values of the probabilities of the disjuncts, the probability of the conjunction. The disjunctive probability is the sum of the probabilities of the two disjuncts if they are mutually incompatible in the presence of evidence e, in which case $c(h.k,e) = 0$.[34] The probability of the conjunction, in turn, depends upon the probability of one of the conjuncts alone and the conditional probability of the other conjunct given the first.[35] If

$$(4) \qquad c(k,h.e) = c(k,e)$$

the multiplication rule assumes the special form

$$(5) \qquad c(h.k,e) = c(h,e) \times c(k,e)$$

in which case the probability of the conjunction is simply the product of the probabilities of the two conjuncts. When condition (4) is fulfilled, h and k are said to be independent of one another.[36] Independence, as thus defined, is obviously a relevance concept, for (4) is equivalent to the statement that h is irrelevant to k, i.e, $R(h,k,e) = 0$.

We can now see why strange things happen with regard to confirmation in the relevance sense. If the hypotheses h and k are mutually exclusive in the presence of e (and a fortiori in the presence of e.i), then

[34] The condition $c(h.k,e) = 0$ is obviously sufficient to make the probability of the disjunction equal to the sum of the probabilities of the disjuncts, and this is a weaker condition than e entails $\sim(h.k)$. Since the difference between these conditions has no particular import for the discussion of this paper, I shall, in effect, ignore it.

[35] Because of the commutativity of conjunction, it does not matter whether the probability of h conditional only on e or the probability of k conditional only on e is taken. This is shown by the double equality in formula (3).

[36] Independence is a symmetric relation; if h is independent of k then k will be independent of h.

$$(6) \qquad c(h \vee k,e) = c(h,e) + c(k,e)$$
$$(7) \qquad c(h \vee k,e.i) = c(h,e.i) + c(k,e.i)$$

so that if

$$(8) \qquad c(h,e.i) > c(h,e) \text{ and } c(k,e.i) > c(k,e)$$

it follows immediately that

$$(9) \qquad c(h \vee k,e.i) > c(h \vee k,e).$$

Hence, in this special case, if i confirms h and i confirms k, then i must confirm their disjunction. This results from the fact that the relation between h and k is the same in the presence of $e.i$ as it is in the presence of e alone.[37]

If, however, h and k are not mutually exclusive on evidence e we must use the general formulas

$$(10) \qquad c(h \vee k,e) = c(h,e) + c(k,e) - c(h.k,e)$$
$$(11) \qquad c(h \vee k,e.i) = c(h,e.i) + c(k,e.i) - c(h.k,e.i).$$

Now, if it should happen that the evidence i drastically alters the relevance of h to k in just the right way our apparently anomalous results can arise. For then, as we shall see in a moment by way of a concrete (fictitious) example, even though condition (8) obtains — i.e., i confirms h and i confirms k — condition (9) may fail. Thus, if

$$(12) \qquad c(h.k,e.i) > c(h.k,e)$$

it may happen that

$$(13) \qquad c(h \vee k,e.i) < c(h \vee k,e)$$

i.e., i disconfirms $h \vee k$. Let us see how this works.

Example 7. Suppose h says that poor old Jones has bacterial pneumonia, and k says that he has viral pneumonia. I am assuming that these are the only varieties of pneumonia, so that $h \vee k$ says simply that he has pneumonia. Let evidence e contain the results of a superficial diagnosis as well as standard medical background knowledge about the disease, on the basis of which we can establish degrees of confirmation for h, k, $h.k$, and $h \vee k$. Suppose, moreover, that the probability on e that Jones has both viral and bacterial pneumonia is quite low, that is, that people do not often get them both simultaneously. For the sake of definiteness, let us

[37] To secure this result it is not necessary that $c(h.k,e) = c(h.k,e.i) = 0$; it is sufficient to have $c(h.k,e) = c(h.k,e.i)$, though obviously this condition is not necessary either.

introduce some numerical values. Suppose that on the basis of the superficial diagnosis it is 98 percent certain that Jones has one or the other form of pneumonia, but the diagnosis leaves it entirely uncertain which type he has. Suppose, moreover, that on the basis of e there is only a 2 percent chance that he has both. We have the following values:

$$c(h,e) = .50 \qquad c(k,e) = .50$$
$$c(h \vee k,e) = .98 \qquad c(h.k,e) = .02.$$

These values satisfy the addition formula (2). Suppose, now, that there is a certain test which indicates quite reliably those rare cases in which the subject has both forms of pneumonia. Let *i* be the statement that this test was administered to Jones with a positive result, and let this result make it 89 percent certain that Jones has both types. Assume, moreover, that the test rarely yields a positive result if the patient has only one form of pneumonia (i.e., when the positive result occurs for a patient who does not have both types, he usually has neither type). In particular, let

$$c(h,e.i) = .90, \quad c(k,e.i) = .90, \quad c(h.k,e.i) = .89$$

from which it follows that

$$c(h \vee k,e.i) = .91 < c(h \vee k,e) = .98.$$

The test result *i* thus confirms the hypothesis that Jones has bacterial pneumonia and the hypothesis that Jones has viral pneumonia, but it disconfirms the hypothesis that Jones has pneumonia!

It achieves this feat by greatly increasing the probability that he has both. This increase brings about a sort of clustering together of cases of viral and bacterial pneumonia, concomitantly decreasing the proportion

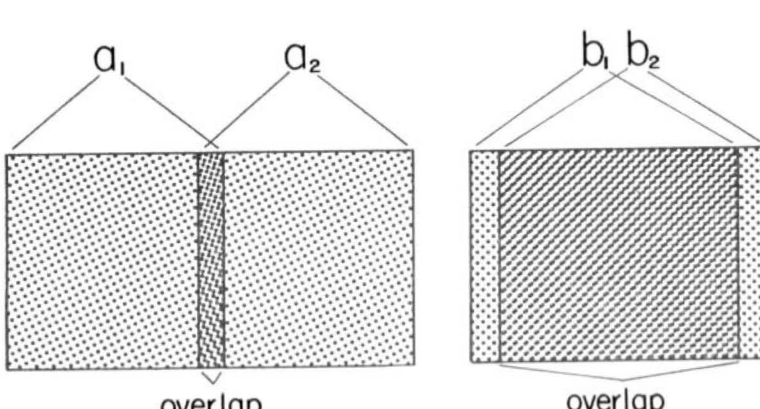

Figure 2

of people with only one of the two types. The effect is easily seen diagrammatically in Figure 2. Even though the rectangles in 2-b are larger than those in 2-a, those in 2-b cover a smaller total area on account of their very much greater degree of overlap. Taking the rectangles to represent the number of cases of each type, we see graphically how the probability of each type of pneumonia can increase simultaneously with a decrease in the overall probability of pneumonia. The evidence i has significantly altered the relevance relation between h and k. Using the multiplication formula (3), we can establish that

$$c(k,h.e) = .04 \qquad c(k,h.e.i) \cong .99$$
$$R(h,k,e) = -0.46 \qquad R(h,k,e.i) \cong .09$$

In the presence of e alone, h is negatively relevant to k; in the presence of i as well, h becomes positively relevant to k. There is nothing outlandish in such changes of relevance in the light of additional evidence. This case thus exemplifies condition (13) by satisfying condition (12).

A similar analysis enables us to understand how an item of evidence can confirm each of two hypotheses, while disconfirming — indeed, even while conclusively refuting — their conjunction. If hypotheses h and k are independent of each other in the presence of evidence e.i and also in the presence of e alone, the following relations obtain:

(14) $c(h.k,e) = c(h,e) \times c(k,e)$
(15) $c(h.k,e.i) = c(h,e.i) \times c(k,e.i)$

so that if

(16) $c(h,e.i) > c(h,e)$ and $c(k,e.i) > c(k,e)$

it follows immediately that

(17) $c(h.k,e.i) > c(h.k,e)$.

Hence, in this special case, if i confirms h and i confirms k, then i must confirm $h.k$.

A different situation obtains if h and k are not independent on both e and e.i; in that case we must use the general formulas

(18) $c(h.k,e) = c(h,e) \times c(k,h.e)$
(19) $c(h.k,e.i) = c(h,e.i) \times c(k,h.e.i)$.

Even given that condition (16) still obtains, so that

(20) $c(k,e.i) > c(k,e)$

it is still possible that

$$(21) \qquad c(k,h.e.i) < c(k,h.e)$$ [38]

which makes it possible, in turn, that

$$(22) \qquad c(h.k,e.i) < c(h.k,e).$$

Since, according to (20) and (21),

$$(23) \qquad c(k,e.i) - c(k,e) = R(i,k,e) > 0$$
$$(24) \qquad c(k,h.e.i) - c(k,h.e) = R(i,k,h.e) < 0$$

the possibility of *i* confirming each of two hypotheses while disconfirming their conjunction depends upon the ability of *h* to make a difference in the relevance of *i* to *k*. We said above, however, that the occurrence of the strange confirmation phenomena depends upon the possibility of a change in the relevance of the hypotheses to one another in the light of new evidence. These characterizations are, however, equivalent to one another, for the change in relevance of *i* to *k* brought about by *h* is equal to the change in relevance of *h* to *k* brought about by *i*, that is,

$$(25) \qquad R(h,k,e) - R(h,k,e.i) = R(i,k,e) - R(i,k,h.e).$$ [39]

We can therefore still maintain that the apparently anomalous confirma-

[38] To establish the compatibility of (20) and (21), perhaps a simple example, in addition to the one about to be given in the text, will be helpful. Let

e = X is a man. $\qquad$ i = X is American.
h = X is very wealthy. $\qquad$ k = X vacations in America.

Under this interpretation, relation (20) asserts: It is more probable that an American man vacations in America than it is that a man (regardless of nationality) vacations in America. Under the same interpretation, relation (21) asserts: It is less probable that a very wealthy American man will vacation in America than it is that a very wealthy man (regardless of nationality) will vacation in America. The interpretation of formula (20) seems like an obviously true statement; the interpretation of (21) seems likely to be true owing to the apparent tendency of the very wealthy to vacation abroad. There is, in any case, no contradiction in assuming that every very wealthy American man vacations on the French Riviera, while every very wealthy man from any other country vacations in America.

[39] This equality can easily be shown by writing out the relevance terms according to their definitions as follows:

$$R(h,k,e) =_{df} c(k,h.e) - c(k,e)$$
$$R(h,k,e.i) =_{df} c(k.h,e.i) - c(k,e.i)$$
$$R(h,k,e) - R(h,k,e.i) = c(k,h.e) - c(k,e) - c(k,h.e.i) + c(k,e.i) \qquad (*).$$
$$R(i,k,e) =_{df} c(k,e.i) - c(k,e)$$
$$R(i,k,h.e) =_{df} c(k,h.e.i) - c(k,h.e)$$
$$R(i,k,e) - R(i,k,h.e) = c(k,e.i) - c(k,e) - c(k,h.e.i) + c(k,h.e) \qquad (**).$$

The right-hand sides of equations (*) and (**) obviously differ only in the arrangement of terms.

tion situation arises from the ability of new evidence to change relations of relevance between the hypotheses, as was suggested by our initial examination of the general addition and multiplication rules (2) and (3).

Let us illustrate the conjunctive situation with another concrete (though fictitious) example.

Example 8. Suppose that the evidence e tells us that two radioactive atoms A and B decay, each ejecting a particle, and that the probability in each case is 0.7 that it is an alpha particle, 0.2 that it is a negative electron, and 0.1 that it is a positive electron (positron). Assume that the two emissions are independent of one another. Let h be the statement that atom A emits a negative electron; let k be the statement that atom B emits a negative electron. We have the following probabilities:

$$c(h,e) = .2, \quad c(k,e) = .2, \quad c(h.k,e) = .04.$$

Let i be the observation that the two particles approach one another and annihilate upon meeting. Since this occurrence requires the presence of one positive and one negative electron, i entails $\sim(h.k)$. At the same time, since a negative electron must have been present, and since it is just as probable that it was emitted by atom A as atom B, we have

$$c(h,e.i) = .5 \quad \text{and} \quad c(k,e.i) = .5.$$

Hence, evidence i, which refutes the conjunction of the two hypotheses, confirms each one of them.[40]

This occurs because the evidence i makes the hypotheses h and k, which were independent of one another on evidence e alone, into mutually exclusive and exhaustive alternatives, i.e.,

$$c(k,h.e) - c(k,e) = R(h,k,e) = 0$$
$$c(k,h.e.i) - c(k,e.i) = R(h,k,e.i) = -.5.$$

Hypotheses that were totally irrelevant to each other in the absence of evidence i become very strongly relevant in the presence of i. Again, there is nothing especially astonishing about such a change in relevance as a result of new evidence.

Since, as we have seen, all the trouble seems to arise out of a change in the relevance of one hypothesis to the other as a result of new evidence, the most immediate suggestion might be to choose hypotheses h and k whose mutual relevance relations will not change in the light of

[40] This case constitutes a counterexample to the Hempel consistency condition H-8.3 discussed in sec. 2 above.

the new evidence *i*. We have noted that this constancy of relevance is guaranteed if we begin with hypotheses that are mutually exclusive on evidence e; they remain mutually exclusive on any augmented evidence e.*i*. But when we use the conjunction of a test hypothesis *h* with auxiliary hypotheses *a* in order to attempt a test of *h*, we certainly do not want *h* and *a* to be mutually exclusive — that is, we do not want to be in the position of knowing that we must reject our test hypothesis *h* if we are prepared to accept the auxiliaries *a*, even without the addition of any new evidence *i*. It would be more reasonable to insist that the auxiliary hypotheses *a* should themselves be neutral (irrelevant, independent) to the test hypothesis *h*. If that condition is satisfied, we can accept the auxiliary hypotheses *a* and still keep an entirely open mind regarding *h*. We cannot, however, demand that *h* and *a* remain irrelevant to one another after the new evidence *i* has been obtained. The interesting test situation is that in which, given e, *h*.*a* entails some observational consequence o. If a result o′ occurs which is incompatible with o, then our hypotheses *h* and *a*, which may have been independent in the presence of e alone, are mutually exclusive in the light of the new evidence o′. Thus, the very design of that kind of test requires hypotheses whose mutual relevance relations are bound to change in the face of new evidence. Several of our examples (5 — positions at Polly Tech; 6 — celibacy among priests; 8 — electron-positron annihilation) show exactly what can happen when new evidence renders independent hypotheses mutually incompatible.

6. Conclusions

The crude hypothetico-deductive account of scientific inference, according to which hypotheses are confirmed by deducing observational consequences which are then verified by observation, is widely recognized nowadays as an oversimplification (even leaving aside the Duhemian objections). One can hardly improve upon Russell's classic example. From the hypothesis, "Pigs have wings," in conjunction with the observed initial condition, "Pigs are good to eat," we can deduce the consequence, "Some winged things are good to eat." Upon observing that such winged creatures as ducks and turkeys are good to eat, we have a hypothetico-deductive confirmation of the hypothesis, "Pigs have wings."[41] I am inclined to agree with a wide variety of authors who hold that something akin to a Bayesian schema must be involved in the confirmation of scientific hypotheses. If this is correct, it is entirely possible to have positive hypothetico-deductive test results that do not confirm the hypothesis (i.e., that do not add anything to its degree of confirmation on prior evidence). To emphasize this point, Reichenbach aptly described the crude hypothetico-deductive inference as an instance of "the fallacy of incomplete schematization."[42] Recognition of the basic inadequacy of the hypothetico-deductive schema does no violence to the logic of science; it only shows that the methods of science are more complex than this oversimplified schema.

Acknowledging the fact that positive hypothetico-deductive instances may not be confirming instances, I have been discussing the logic of confirmation—that is, I have been investigating the conclusions that can be drawn from the knowledge that this or that evidence confirms this or that hypothesis. By and large, we have found this logic to be poverty-ridden. Although evidence i confirms hypotheses h and k, we have found that we cannot infer that i confirms $h.k$. Evidence i may in fact confirm $h.k$, but to draw that conclusion from the given premises would be another instance of the fallacy of incomplete schematization. Indeed, our investigations have revealed exactly what is missing in the inference. In addition to knowing that i is positively relevant to h and positively relevant to k, we must know what bearing i has on the relevance of h to k. If this is known quantitatively, and if the degrees of relevance of i to h and to k are also known quantitatively, we can ascertain the relevance of i to $h.k$ and to $h \vee k$. Without this quantitative knowledge, we cannot say much of anything. The moral is simple: even if we base our qualitative concept of confirmation (in the relevance sense) upon a quantitative concept of degree of confirmation, the resulting qualitative concept is not very serviceable. It is too crude a concept, and it doesn't carry enough information to be useful. In order to make any substantial headway in understanding the logic of evidential support of scientific hypotheses, we must be prepared to work with at least crude estimates of quantitative values of degree of confirmation

[41] Bertrand Russell, "Dewey's New 'Logic,'" in Paul Arthur Schilpp, ed., *The Philosophy of John Dewey* (New York: Tudor, 1939), p. 149.

[42] Hans Reichenbach, *The Theory of Probability* (Berkeley and Los Angeles: University of California Press, 1949), p. 96.

and degree of relevance. Then, in contexts such as the discussion of the Duhemian problem, we must bring the more sophisticated concepts to bear if we hope to achieve greater clarity and avoid logical fallacies. In detailing the shortcomings of the qualitative concept of confirmation, we have, in a way, shown that this sort of confirmation theory is a shambles, but we have done no more violence to the logic of science than to show that its embodies more powerful concepts.

If we are willing, as Carnap has done, to regard degree of confirmation (in the nonrelevance sense) as a probability — that is, as a numerical functor that satisfies the probability calculus — then we can bring the structure of the quantitative probability concept to bear on problems of confirmation. With this apparatus, which gives us the power of Bayes's theorem, we can aspire to a much fuller understanding of relations of confirmation (in both the absolute and the relevance senses).

We can also provide an answer to many who have claimed that confirmation is not a probability concept. Confirmation in the relevance sense is admittedly not a probability; as we have insisted, it is not to be identified with high probability. A quantitative concept of degree of relevance can nevertheless be defined in terms of a concept of degree of confirmation. Degree of relevance, as thus defined, is not a probability; it obviously can take on negative values, which degree of probability cannot do. It is a probability concept, however, in the sense that it is explicitly defined in terms of degree of confirmation which is construed as a probability concept. Thus, even though degree of confirmation in the relevance sense cannot be construed as degree of probability, this fact is no basis for concluding that the concept of probability is an inadequate or inappropriate tool for studying the logic of evidential relations between scientific hypotheses and observational evidence. Moreover, it provides no basis whatever for rejecting the notion that high probabilities as well as high content are what we want our scientific hypotheses eventually to achieve on the basis of experimental testing.