A SYMPOSIUM ON PROBABILITY: PART III

REMARKS ON INDUCTION AND TRUTH

(1) *General Remarks on the Symposium on Probability.*

Reading the contributions to the present symposium on probability, I find myself in agreement on many fundamental points with the views of Ernest Nagel,[1] Felix Kaufmann,[2] and Donald Williams.[3] This agreement holds not only for their general empiricist attitude, which is shared more or less by all participants in the symposium, but also, more specifically, for the view that the frequency concept of probability alone is not sufficient, that another concept of probability is essential for scientific method, and that this is a logical concept basic for testing a hypothesis on given evidence and hence for non-demonstrative inference. It seems to me that the most decisive division among the authors in this symposium concerns the question of the existence and function of this logical concept of probability, or, in other words, of the possibility and nature of inductive logic, in the sense of the logical theory of confirmation and non-demonstrative inference.[4,5] Inductive logic as a theory not contained in the theory of frequencies is rejected by Hans Reichenbach[6] and Richard von Mises.[7,8] However, there is one important difference between the positions of these two authors. Reichenbach saw, quite early, the necessity for a theory of induction and has discussed it in detail in many publications. What distinguishes his position from that which I share with the authors earlier mentioned is only the special character of his theory of induction: he identifies the basic concept of this theory, the concept of "weight", with the frequency concept

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[1] E. Nagel, "Probability and Non-Demonstrative Inference," this journal, Vol. V (1945), pp. 485-607.

[2] Felix Kaufmann, "Scientific Procedure and Probability," this journal, Vol. VI (1945), pp. 47-66.

[3] Donald Williams, "On the Derivation of Probabilities from Frequencies," this journal, Vol. V (1945), pp. 449-484; "The Challenging Situation in the Philosophy of Probability," this journal, Vol. VI (1945), pp. 67-86.

[4] R. Carnap, "The Two Concepts of Probability," this journal, Vol. V (1945), pp. 513-532.

[5] R. Carnap, "On Inductive Logic," *Philosophy of Science*, Vol. XII (1945), pp. 72-97. (This paper appeared simultaneously with Part I of the Symposium on Probability; it was not known to the other authors at the time they wrote their contributions for Parts I and II.)

[6] H. Reichenbach, "Reply to Donald C. Williams' Criticisms of the Frequency Theory of Probability," this journal, Vol. V (1945), pp. 508-512.

[7] R.von Mises, "Comments on D. Williams' Paper," this journal, Vol. VI (1945), pp. 45 f.

[8] R.von Mises, "Comments on Donald Williams' Reply," this journal, present number.

of probability. On the other hand, von Mises denies the necessity and even the possibility of an exact, scientific, and objective (i.e., not merely psychological) theory of confirmation or non-demonstrative inference or, in my terminology, of probability$_1$.[9]

[9] I should like to take the opportunity for clarifying some points in which von Mises in his second contribution (see footnote 8) has misunderstood my position.

(1) I have proposed the terms 'explicandum' and 'explicatum' merely an convenient short designations of two concepts very frequently used by scientists, including von Mises, as well as by philosophers, in discussions of the methodology of science. To give an outstanding example, von Mises' "theory of probability" proposes the concept of the limit of relative frequency in a sequence with random distribution (called by him "probability") as an exact substitute for the customary but inexact concept of the relative frequency in the long run (sometimes called "probability"). Thus, in my terminology, he proposes the first concept as an explicatum for the second as an explicandum. I am surprised to see that von Mises regards my concepts of explicandum and explicatum as "somehow metaphysical." I assume, however, that he agrees with me that his own theory, although based on an explication, is not of a metaphysical but of a genuinely scientific nature. (Incidentally, on the question to which part of the scientific realm von Mises' theory belongs, I cannot agree with his view. Here, as in earlier publications, von Mises has stated that his theory of probability is empirical, is a branch of the natural science like physics. However, his theorems, although referring to mass phenomena, are quite obviously purely analytic; the prods of these theorems (in distinction to examples of application) make use only of logico-mathematical methods in addition to his definition of 'probability', and not of any observational results concerning mass phenomena. Therefore his theory belongs to pure mathematics, not to physics. This point has been discussed in detail and completely clarified by F. Waismann on pages 239 f. of his article "Logische Analyse des Wahrscheinlichkeitsbegriffs" in *Erkenntis*, Vol. I, 1930, pp. 228-248.)

(2) My distinction between probability$_1$ and probability$_2$ is not characterized accurately by saying that the second concept applies to mass phenomena or games of chance while the first is the degree of confirmation for a single event. Actually, probability$_1$ or degree of confirmation is not restricted to single events but is applied to sentences of all forms, as explained in my earlier paper. In fact, most of the more important applications of this concept are to mass phenomena, to statistical sentences concerning frequencies in samples or in a whole population. (See the examples of theorems concerning degree of confirmation in my second paper cited in footnote 5, §§9, 10, 12,13.) The fundamental difference is rather this: 'probability$_2$' designates an empirical function, via., relative frequency, while 'probability$_1$' designates a certain logical relation between sentences; these sentences, in turn, may or may not refer to frequencies.

(3) Von Mises wonders whether my earlier view that every (true) sentence is either logically true (analytic, tautologous) or empirically true is now abandoned in the case of a (true) sentence stating the value of probability$_1$ or degree of confirmation of a hypothesis h with respect to given evidence e (e.g., "$c(h, e) = q$"). I still maintain this view. Sentences of the kind described are analytic, as I have clearly stated in my earlier paper, cited in footnote 4, (pp.522 and 526). What distinguishes statements in inductive logic from these in deductive logic is only the fact that the first contain the concept of degree of confirmation and are based on the definition of this concept, while the latter are independent of it.

In spite of the basic agreement with Nagel, Kaufmann, and Williams, there are still, of course, a number of points on which our opinions differ. It is tempting to discuss all of these problems, and I believe that, because of the basic agreement, a discussion of any of them could be fruitful. However, I will restrict myself in this paper to the discussion of two points; they seem to me especially important, and the previous discussion has cleared the ground sufficiently to make a further step towards clarification possible. In his excellent summary of the symposium, Kaufmann has given a clear outline of the various positions and their differences. In the course of his explanation of my view, he has discussed two points on which his views differ from mine. They concern the nature of inductive inference and the nature and legitimacy of the concept of truth. In the subsequent two sections I shall discuss these two points in turn.

Kaufmann has explained his views concerning the nature and the aim of the method of empirical science in the paper mentioned,[2] in earlier papers,[10] and above all in his latest book,[11] whose first half gives a detailed analysis of the methodology of empirical science in general. I find myself to a great extent in agreement with his general views on these problems. When Kaufmann states, correctly, that my present conception of logic as a theory based on analysis of meaning is closer to his position than my earlier view, then I may reciprocate by expressing my gratification in discovering that his position on the methodology of empirical science is now much closer to my position and that of empiricists in general than it was previously. I would even go as far as to classify his present views in this field as a variant of empiricism. Whether this is entirely justified depends chiefly upon one point, the nature of the "rules of scientific procedure." If I understand Kaufmann's conception of these rules correctly, they are meant to constitute the definition of "correct scientific procedure in accepting a sentence"; therefore I suppose that statements based upon these rules are meant as analytic and hence do not violate the principle of empiricism. Nagel, on the other hand, suspects an element of the synthetic *a priori* is these rules and therefore characterizes Kaufmann's position as aprioristic and Kantian. I don't think that this characterization is correct; but I agree with Nagel that further clarification is here required.[12]

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[10] Felix Kaufmann, "The Logical Rules of Scientific Procedure," this journal, Vol. II (1942), pp. 457-471; "Verification, Meaning and Truth," this journal, Vol. IV (1944), pp. 267-284

[11] Felix Kaufmann, *Methodology of the Social Sciences*, London and New York, 1944.

[12] For the interesting discussion between Kaufmann and Nagel, which took as its basis one of Kaufmann's papers (the second one mentioned in footnote 10), see this journal, Vol. V (1945, pp. 50-68. (Nagel), pp. 69-74 (Kaufmann), pp. 75-79 (Nagel), pp. 350-353 (Kaufmann).

(*2*) *On the Nature of Inductive Logic.*

Deductive and inductive (i.e., non-demonstrative) procedures seem to me to be fundamentally analogous. Therefore I regard it see justified to speak in both cases of "logic", distinguishing the two theories as deductive and inductive logic. Kaufmann, on the other hand, sees a fundamental difference between the two procedures. This is our first important point of divergence.

The analogy between the two fields as I see it will perhaps become more apparent by the following representation of examples in two parallel columns. (I insert sometimes "(K: +]" or "(K:—]" in order to indicate that I understand Kaufmann to agree or to disagree, respectively, with my statements; a question mark indicates that I am not sure whether my interpretation of Kaufmann's view is correct.)

Deductive Logic	*Inductive Logic*
The subsequent statements in deductive logic refer to these example sentences: *Premise i*: "All men are mortal, and Socrates is a man." *Conclusion j*: "Socrates is mortal."	The subsequent statements in inductive logic refer to these example sentences: *Evidence* (or premise) *e*: "The number of inhabitants of Chicago is three million; two million of these have black hair; *b* is an inhabitant of Chicago." *Hypothesis* (or conclusion) *h:* "*b* has black hair."
The following is an example of an elementary statement in deductive logic: *D1*. "*i* L-implies *j* (in E)." ('L-implication' means logical implication or entailment. E is here either the English language or a semantical language system based on English.)	The following is an example of an elementary statement in inductive logic: *I1*. "The degree of confirmation of the hypothesis *h* with respect to the evidence *e* (in E) is 2/3."
D2. The statement D1 can be established by a logical analysis of the meanings of the sentences *i* and *j* [K:+], provided the definition of 'L-implication' i given.	*I2*. The statement Il can be established by a logical analysis of the meanings of the sentences *e* and *h*, provided the definition of 'degree of confirmation' is given. [K:—?]
D3. D1 i a complete statement. We need not add to it any reference to specific deductive rules (e.g., the modus Barbara) [K:+], because these rules are merely "technical devices which aid us in realizing" that D1 and similar statements hold [K:+; the quotation is from Kaufmann]. However, the definition of 'L-implication' is, of course, presupposed for establishing Dl.	*I3*. I1 is a complete statement. We need not add to it any reference to specific inductive rules (e.g., for I1, the role of the direct inductive inference[13]) [K:—], because these rules are merely technical devices which aid us in realization that I1 and similar statements hold [K:—]. However, the definition of 'degree of confirmation' is, of course, presupposed for establishing I1.
The following is a consequence of D2.	The following is a consequence I2.

[13]See my second paper (cited in footnote 5), §9.

Deductive Logic

D4. The question whether the premise *i* is known (well established, highly confirmed, accepted), is irrelevant for D1 [K:+]. This question becomes relevant only in the *application* of D1 (See D6 and D7).

D5 follows from D1:

D5. "If *i* is true, then *j* is true." [K:+?].

D6 and D7 are consequences of D1 conearning *applications* to possible knowledge situations. D6 represents the theoretical application, (that is, the result refers again to the knowledge situation); D7 represents the practical application (that is, the result refers to a decision).

D6. "If *i* is *known* (accepted, well-established) by the person *X* at the time *t*, then *j* is likewise."[K:+?] [Here, "to know" is understood in a wide sense, including not only items of *X*'s explicit knowledge, i.e., those which he is able to declare explicitly, but also those which are implicitly contained in *X*'s explicit knowledge.]

D7. If *i* is known by *X* at *t*, then a decision of *X* at *t* based on the assumption *j* is rationally justified."

Inductive Logic

I4. The question whether the premise (evidence) *e* is known (well established, highly confirmed, accepted), is irrelevant for I1 [K:—]. This question becomes relevant only in the *application* of I1 (see I6 and I7).

There is here no analogue to D5. From I1 and "*e* is true" nothing can be inferred.

I6 and I7 are consequences of I1 concerning *applications* to possible knowledge situations. I6 represents the theoretical application, I7 the practical application.

I6. "If *e and nothing else* is *known* by *X* at *t*, then *h* is confirmed by *X* at *t* to the degree 2/3." [Here, the term 'confirmed' does not mean the logical (semantical) concept of degree of confirmation occurring in Dl but a corresponding pragmatical concept; the latter is, however, not identical with the concept of degree of (actual) belief but means rather the degree of belief justified by the observational knowledge of *X* at *t*.] The phrase "and nothing else" in I6 is essential. The requirement that the premise (evidence) *e* represent the *total* (observational) knowledge of *X* at *t* (or at least as much of it as is relevant for *h*) is often overlooked. It marks an important difference between inductive and deductive procedure; not a purely logical but a methodological difference (i.e., one concerning application).

I7. "If *e* and nothing else is known by *X* at *t*, then a decision of *X* at *t* based on the assumption of the degree of certainty 2/3 for *h* is rationally justified (for example, the decision to accept a wager on *h* at odds not higher than 2:1)."

I shall now discuss Kaufmann's[14] views concerning the difference between inductive and deductive procedure by applying them to the preceding

[14] The quotations from Kaufmann are taken from Part II of his paper cited in footnote 2.

example statements. In contrast to I4, Kaufmann remarks: "we do not—strictly speaking—infer from the propositions which represent the 'evidence' but rather from the statement that these propositions belong to the body established knowledge." The only argument he gives in support of this view is the following: "If it were not required that inductive grounds be elements of the body of knowledge established at the time at which the inference is made, then we should be able to confirm (warrant inductively) any assertion whatsoever, just as we can deduce any proposition from some other propositions." The requirement here mentioned is indeed valid; however, it concerns not the purely logical statement I1 but the statements of applications I6 and I7. Thus the situation is analogous to that in deductive logic, where likewise the reference to the knowledge of *X* does not occur in the purely logical statement D1 but only in the statements of application D6 and D7. Thus, with respect to D1, I agree with Kaufmann when he rejects the view "that reference is made in the process of deduction to established empirical knowledge." And, considering the difference between D1 and its usual application, as for example in D6, I agree further when he continues: 'But this is not the case, even though deductive inferences, in science as well as in daily life, are usually drawn from valid propositions. The decisive point is that it is irrelevant for a deductive inference whether the premises are valid." Quite so. The same holds, however, for inductive logic. It is true that inductive inferences are usually drawn, in science as well as in daily life, from valid (known, well-established) premises (as in I6). But this holds only for the usual application. The decisive point is that for the correctness of the inductive inference itself (for example, I1) it is irrelevant whether the premises (in I1, the evidence *e*) are true or not and, if they are true, whether their truth is known or not. Kaufmann's view that inductive inference, in contradistinction to deductive inference, "is essentially concerned with issues of validity," seems to me due to a failure to make in inductive logic the distinction between the logical relation itself and its application to given knowledge situations which he makes so clearly in deductive logic. Kaufmann regards the sentence "*h* may be inductively inferred from *e*" as merely an elliptical formulation for: "If *e* is an element of the body of knowledge established at the time at which the inference is made, then it is correct to accept *h* into this body." Taking instead of these two sentences my slightly different formulations I1 and I6, I regard them as analogous to D1 and D6 in this respect: I1 is not elliptical but complete, I6 is not more explicit than I1 but rather represents a special case of application.

Kaufmann sees a fundamental difference between deductive and inductive logic in still another respect. According to his view, the complete formulation of the inductive relation between two sentences must explicitly refer to some "presupposed rules of induction." Thus he rejects I3 although he agrees with D3. Here are two possible interpretations of Kaufmann's view. (i) Perhaps he means merely that the definition of 'degree of confirmation' is presupposed. On this point I do, of course, agree with him. But in this respect there is no difference between deductive and inductive logic, because any statement in any field presupposes the definitions of the terms occurring in it. (ii) Since, however, Kaufmann insists upon a difference between inductive and deductive logic, I assume that he means that not only the definition is presupposed but, instead or in addition, specific rules of induction. If he means this, I cannot agree with him. I think that, none a definition of degree of confirmation is laid down, no further rules need to be given in order to establish statements of the form I1. I have shown this by constructing a definition of a function c*, representing the degree of confirmation and then proving theorems of two kinds: (1) specific statements attributing to c* a particular *numerical value for two given sentences *e* and *h* (like I1), and (2) general statements from which those of form (1) follow as special instances.[15] The proofs of these theorems use—aside from customary deductive procedures—only the definition of c* but not any inductive postulates or rules. Therefore, the theorems cannot contain any references to such rules. Kaufmann's view here is based upon the belief that "in contrast to deductive inference it [inductive inference] does not reveal an internal relation between the propositions connected by the rules." It seems to me, however, that an elementary statement of inductive logic (as, for example, I1) expresses a purely logical relation between the two sentences involved in the same way that an elementary statement of deductive logic does (for example, D1). The relation is in both eases purely logical in the sense that it depends merely upon the meanings of the sentences or, more exactly speaking, upon their ranges. The deductive relation consists in a complete inclusion of one range in the other; the inductive relation consists in a partial inclusion.[16]

Another point on which I differ from Kaufmann concerns his distinction between accepted and unaccepted propositions. Perhaps this divergence is not fundamental and we might come to an agreement. When I read in Kaufmann's early publications his analysis of the scientific procedure and, in particular, of the examination of propositions and of their subsequent acceptance or rejection, I found myself on the whole in agreement with his

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[15] The definition and a few examples of general theorems an given in my second paper (cited in footnote 5).

[16] For an explanation of this partial inclusion nee my second paper (footnote 5), pp. 74 f. There reference is made to Waismann (see above, footnote 9), who was the first to see this situation clearly.

views. I thought that his simple distinction between acceptance and rejection was an intentional over-simplification, meant as a first step in a schematization of the procedure. In his present paper, however, it becomes clear that it was meant literally: "We do draw a sharp line of demarcation between accepted propositions and unaccepted propositions." In contrast to this, I maintain the conception rejected by Kaufmann "that we distinguish in scientific procedure between more or less firmly established propositions, and that it would therefore be arbitrary to draw a sharp line of demarcation between accepted and unaccepted propositions." It seems to me obvious that good scientists proceed in this way, and I fail to see compelling reasons for not doing so. Suppose that we ask a historian whether Napoleon did a certain thing on a certain day, or a geographer whether at a certain spot in the interior of Africa there is a lake, or a physicist at a certain time in 1939 or 1940 whether the barium appearing in a certain experiment is actually a product of the fission of a uranium nucleus. In each of these or similar cases the answer may very well be something like this: "At the present moment, the evidence available suggests this assumption; on the other hand, there are also some reasons for doubt; therefore we cannot, for the time being, either simply accept this proposition or declare it as completely unknown, let alone reject it; the situation is rather this, that we ascribe to the proposition a certain moderate degree of confirmation (or plausibility, probability, credibility, acceptability)." In cases of the kinds mentioned, the scientist will presumably not specify the degree in numerical terms but he might he willing to specify it qualitatively by comparison with other assumptions . According to Kaufmann's conception, the "sharp line of demarcation" is drawn "by distinguishing the status of propositions which makes them eligible for the function of grounds in an inductive inference from the status which excludes them from this function." Kaufmann does not reject the distinction between more or less firmly established propositions and admits that it is essential in an analysis of scientific procedure. He believes, however, that this distinction presupposes a sharp dichotomy between accepted and unaccepted propositions. Now it is true that in the simplest form of an application of an inductive procedure to a given knowledge situation we take an evidence the "known" or "well-established" results of observations. It is customary to describe the procedure in this way, and I myself used formulations of this kind above (in the examples I6 and I7). I think, however, that these formulations should be regarded merely as convenient simplifications and that there is actually no sharp line between two classes of sentences describing the results of observations which *X* has made, those which are "well-established" and those which are not. Suppose that *X* has made a certain observation and thereupon states a sentence *S* describing the result of this observation; suppose, further, that he regards *S* as fairly well but not very well established. Then it may happen that in determining the degree of confirmation of a certain hypothesis h_1 he includes *S* into his evidence, while at the same time for another hypothesis h_2 he does not include it, perhaps because he wants to be more cautious in this case and *S* does not seem to him sufficiently reliable for this purpose. Thus, in a situation of this kind, we cannot simply speak of "acceptance" or "non-acceptance" of *S* by *X* at the time in question. When we speak here of "inclusion" of *S* by *X* into the evidence for h_1 and "non-inclusion" for h_2, this is again an oversimplification, but one customary in practically all discussions of the application of both deductive and inductive logic to knowledge situations. Instead of saying that *X* does or does not know (or accept) *S* at the given time, or that he does or does not use *S* as a premise for a deductive or inductive inference, a more refined formulation might say instead that *X* attributes to *S* a certain "initial weight." Inductive logic would then have the task of determining the "derivative weight" of a hypothesis with respect to a class of evidence sentences for which the "initial weights" are given. Inductive logic would become much more complicated in this form; and it seems that so far no attempts in this direction have been made.[17] The customary simpler form is convenient and seems sufficient for many purposes. This point is one among many on which our logical methods deviate from the actual procedure of scientists. They must deviate because they are based on simplification and schematization. We should certainly not give up schematization; it is very useful and even indispensable. But we should always be aware of what we are doing.

(3) On the Concept of Truth.

The second point on which I cannot agree with Kaufmann is his discussion of the concept of truth. It seems to me that this discussion is based on an old misconception: the neglect of the distinction between truth and knowledge of truth (or verification). This misconception is widespread; and I discussed it on previous occasions.[18] Perhaps the following analysis will help towards a clarification.

Let us consider the following four sentences:

(1) "The substance in this vessel is alcohol."

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[17] The problem of weighted evidence has been indicated by Olaf Helmer and Paul Oppenheim in "A Syntactical Definition of Probability and of Degree of Confirmation," *Journal of Symbolic Logic*, Vol. 10 (1945), pp. 25-60, see p. 59; further by Carl G. Hempel and P. Oppenheim in "A Definition of 'Degree of Confirmation,' "*Philosophy of Science*, Vol. XII (1945), pp. 98-115, see pp. 114 f.

[18] See my earlier paper cited in footnote 4, p. 531, and the references there in footnote 21.

(2) "The sentence 'the substance in this vessel is alcohol' is true."
(3) "*X* knows (at the present moment) that the substance in this vessel is alcohol."
(4) "*X* knows that the sentence 'the substance in this vessel is alcohol' is true."

First a remark concerning the interpretation of the term 'to know' as it occurs in (3) and (4), and generally as it is applied with respect to synthetic propositions concerning physical things. In which of the following two senses (*a*) and (*b*) should it be understood?

(*a*) It is meant in the sense of *perfect knowledge*, that is, knowledge which cannot possibly be refuted or even weakened by any future experience.

(*b*) It is meant in the sense of *imperfect knowledge*, that is, knowledge which has only a certain degree of certainty, not absolute certainty, and which therefore may possibly be refuted or weakened by future experience. (This is meant as a theoretical possibility; if the degree of certainty is sufficiently high we may, for all practical purposes, disregard the possibility of a future refutation.)

I am in agreement with Kaufmann and with practically everybody else that sentences of the kind (3) should always be understood in the sense (*b*), not (*a*). For the following discussion I presuppose this interpretation of the sentences (3) and (4).

Now the decisive point for our whole problem is this: *the sentences* (*1*) *and* (*2*) *are logically equivalent*; in other words, they entail each other; they are merely different formulations for the same factual content; nobody may accept the one and reject the other; if used as communications, both sentences convey the same information though in different form. The difference in form is indeed important; the two sentences belong to two quite different parts of the language. (In my terminology, (1) belongs to the object part of the language, (2) to its meta-part, and, more specifically, to its semantical part.) This difference in form, however, does not prevent their logical equivalence. The fact that this equivalence has been overlooked by many authors (e.g., C. S. Peirce[19] John Dewey,[19] Reichenbach,[20] and Neurath[21]) seems to be the source of many misunderstandings in current discussions on the concept of truth. It must be admitted that any statement of the logical equivalence of two sentences in English can only be made with certain qualifications, because of the ambiguity of ordinary words, here the word 'true'. The equivalence holds certainly if 'true' is

[19] See John Dewey, *Logic: The Theory of Inquiry*, 1938, p. 345, footnote 6, with quotations from Peirce.

[20] Hans Reichenbach, *Experience and Prediction*, 1938; see §§22, 35.

[21] Otto Neurath, "Universal Jargon and Terminology," *Proceedings Aristotelian Society*, 1940-1941, pp. 127-148; see especially pp. 138 f.

understood in the sense of the semantical concept of truth.[22] I believe with Tarski that this is also the sense in which the word 'true' is mostly used both in everyday life and in science.[23] However, this is a psychological or historical question, which we need not here examine further. In this discussion, at any rate, I use the word 'true' in the semantical sense.

The sentences (1) and (3) obviously do not say the same. This leads to the important result, which is rather obvious but often overlooked, that *the sentences* (*2*) *and* (*3*) *have different contents*. (3) and (4) are logically equivalent since (1) and (2) are. It follows that (2) and (4) have different contents. (It is now clear that a certain terminological possibility considered by Kaufmann cannot be accepted. "If we constantly bear in mind that the acceptance of any proposition may be reversed," in other words, that we have always to use interpretation (*b*), not (*a*), "then we might instead call an accepted proposition a true proposition." This usage, however, would be quite misleading because it would blur the fundamental distinction between (2) and (3). I can certainly not agree with Kaufmann's opinion that "this would be in conformity with fairly well established usage." It would indeed "link the terms 'knowledge' and 'truth' with each other"; but it is precisely this linkage or identification that seems to me the source of all the trouble.)

Kaufmann comes to the conclusion that my conception, although in agreement with "the traditional view", "is incompatible with the principle of inquiry which rules out the invariable truth of synthetic propositions. It is impossible for an empirical procedure to confirm to any degree something which is excluded by a general (constitutive) principle of empirical procedure. *Knowledge of invariable truth of synthetic propositions* (*whether perfect or imperfect*) *is unobtainable, not because of limitations of human knowledge, but because the conception of such knowledge involves a contradiction in terms*." This reasoning seems to me based on the wrong identification of truth with perfect knowledge, hence, in the example, the identification of (2) with (3) in interpretation (*a*). The principles of scientific procedure do indeed rule out perfect knowledge but not truth. They cannot rule out (2), because this says nothing else than sentence (1), which, I suppose, will be acknowledged by all of us as empirically meaningful. When Kaufmann declares that even imperfect knowledge of truth is un-

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[22] For this point and the subsequent discussion compare Alfred Tarski, "The Semantic Conception of Truth, and the Foundations of Semantics," this journal, Vol. IV (1944), pp. 341-376, where a number of common misunderstandings are cleared up. Compare also my *Introduction to Semantics*, 1942; see p. 26: "We use the term ['true'] here in such a sense that *to assert that a sentence is true means the same as to assert the sentence itself*."

[23] Arne Ness has expressed doubts in this respect; but he has admitted that in 90% of the cases examined by him the persons questioned reacted in the sense of the equivalence. See Tarski, *op. cit.*, p. 360, with reference to Ness.

obtainable, then this means that even imperfect knowledge of (2) is unobtainable and hence that an event as described in (4), even in interpretation (*b*), cannot occur. However, as soon as the event (3) occurs (now always assuming interpretation (*b*)), which nobody regards an impossible, the event (4) thereby occurs too; for the sentences (3) and (4) describe merely in different words one and the same event, a certain state of knowledge of the person *X*.

Let us represent in a slightly different way the objection raised against the concept of truth, in order to examine the presupposition underlying its chief argument. The objection concerns the concept of truth in its semantical sense; Kaufmann uses here the term "invariable truth" because truth in this sense in independent of person and state of knowledge, and hence of time. (Incidentally, the word "invariable" is not quite appropriate; it would be more correct to say instead that truth is a "time-independent" or "non-temporal" concept.. The volume of a body *b* may or may not change in the course of time; hence we may say that it is variable or that it is invariable. The sentence "the volume of *b* at the time *t* is *v*" is meaningful but without the phrase "at the time *t*" it would be incomplete. On the other hand, the formulation "the sentence *S* is true at the time *t*" is meaningless; when the phrase "at the time *t*" is omitted we obtain a complete statement.. Therefore, to speak of change or non-change, of variability or invariability of truth, is not quite correct.) Now Kaufmann, Reichenbach,[24] Neurath,[25] and other authors are of the opinion that the semantical concept of truth, at least in its application to synthetic sentences concerning physical things, ought to be abandoned because it can never be decided with absolute certainty for any given sentence whether it is true or not. I agree that this can never be decided. But is the inference valid which leads from this result to the conclusion that the concept of truth is inadmissible? It seems that this inference presupposes the following major premise *P*: "A term (predicate) must be rejected if it is such that we can

[24] Reichenbach, *op. cit.*., footnote 20, p.188: "Thus there are left no propositions at all which can be absolutely verified. The predicate of truth-value of a proposition, therefore [!], is a mere fictive quality, its place is in an ideal world of science only, whereas actual science cannot make use of it. Actual science instead employs throughout the predicate of weight."

[25] I agree with Neurath when he rejects the possibility of absolutely certain knowledge, for example, in his criticism of Schlick, who believed that the knowledge of certain basic sentences ("Konstatierungen") was absolutely certain. See Neurath, "Radikaler Physikalismus und 'Wirkliche Welt,'" *Erkenntnis*, Vol. IV (1934), pp. 346-362. But I cannot agree with him when he proceeds from this view to the rejection of the concept of truth. In the paper mentioned earlier (in footnote 21) he says (pp. 138 f.): "In accordance with our traditional language we may say that some statements are accepted at a certain time by a certain person and not accepted by the same person at another time, but we cannot say some statements are true today but not tomorrow; 'true' and 'false' are 'absolute' terms, which we avoid."

never decide with absolute certainty for any given instance whether or not the term applies." The argumentation by the authors would be valid if this principle *P* were presupposed, and I do not see how they reach the conclusion without this presupposition. However, I think that the authors do not actually believe in the principle *P*. In any case, it can easily be seen that the acceptance of *P* would lead to absurd consequences. For instance, we can never decide with absolute certainty whether a given substance is alcohol or not; thus, according to the principle *P*, the term "alcohol" would have to be rejected. And the same holds obviously for every term of the physical language. Thus I suppose that we all agree that instead of *P* the following weaker principle *P** must be used; this is indeed one of the principles of empiricism or of scientific inquiry: "A term (predicate) is a legitimate scientific term (has cognitive content, is empirically meaningful) if and only if a sentence applying the term to a given instance can possibly be confirmed to at least some degree." "Possibly" means here "if certain specifiable observations occur"; "to some degree" is not meant as necessarily implying a numerical evaluation. *P** is a simplified formulation of the "requirement of confirmability"[26] which, I think, is essentially in agreement with Reichenbach's "first principle of the probability theory of meaning,"[27] both being liberalized versions of the older requirement of verifiability as stated by C. S. Peirce, Wittgenstein, and others.[28] Now, according to *P**, 'alcohol' is a legitimate scientific term, because the sentence (1) can be confirmed to some degree if certain observations are made. But the same observations would confirm (2) to the same degree because it is logically equivalent to (1). Therefore, according to *P**, 'true' is likewise a legitimate scientific term.

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[26]Compare my "Testability and Meaning," *Philosophy of Science*, Vol. III (1936), pp. 419-471, and Vol. IV (1937), pp. 1-40; see Vol. IV, p. 34.

[27] See Reichenbach, *op. cit.*, footnote 20, §7; he formulated this principle first in 1936.

[28] See the references in Reichenbach, *op. cit.*, footnote 20, p. 49.