Logical Reconstruction, Realism and Pure Semiotic
Author(s): Herbert Feigl
Source: *Philosophy of Science*, Vol. 17, No. 2 (Apr., 1950), pp. 186-195
Published by: The University of Chicago Press on behalf of the Philosophy of Science Association
Stable URL: http://www.jstor.org/stable/184919
Accessed: 30/09/2008 12:39

LOGICAL RECONSTRUCTION, REALISM AND PURE SEMIOTIC

HERBERT FEIGL

In this rejoinder to the critical comments elicited by my essay "Existential Hypotheses," I propose to deal first with the challenge coming from the avowedly different philosophical outlook of Professor Churchman. My other critics, Professors Frank, Hempel, Nagel and Ramsperger, on the whole, share my basic conception of the tasks of philosophy of science and epistemology, even if they dissent in one important respect or another from the special solution I suggested. But since I discern even in Professor Nagel's remarks (and possibly also between the lines of Professor Ramsperger's comments) a pragmatist or instrumentalist strain akin to the major contentions of Professor Churchman, it will be well to begin with a defense and further clarification of my underlying point of view. Only after this restatement of my platform will I undertake to defend semantic realism against the specific criticisms advanced by the last four authors.

Professor Churchman wants to know what is the use of logical reconstruction. Does it help science in its own progress? Will the methods and techniques of science benefit from metalinguistic analyses of its concepts and assertions? My answer is unhesitatingly in the affirmative. If logical reconstruction were no more than an idle parlor game invented solely for the delight of those who care to play it, I should not for a moment admit it as a legitimate task for philosophy. Ultimately I too share the value judgments of the pragmatists. But I think the history of science shows most convincingly that new levels of reflection, even when they seemed remote from direct practical application, have either indirectly or in the long run helped in the main concerns of the scientific enterprise. Modern symbolic logic at first was widely condemned as a play thing; it was considered sterile by as eminent a thinker as Poincaré. Even if its present day utilizations in the axiomatics of mathematics, probability theory, theoretical physics, biology, psychology, etc., may not be as impressive as the utilization of non-euclidean geometry or of group theory, etc., there is no doubt that we could no longer do without it. The highly important and practical results of cybernetics (of N. Wiener and others) to which Churchman refers with such high appreciation may well be mentioned as in part dependent on the developments in mathematical logic. And who can conceive of contemporary mathematical logic without making use of the reconstruction that such metalinguistic studies as pure syntax and pure semantics have made possible?—But I need not rely on parallels, analogies or vague promises for my argument. Even if these be questioned I would maintain that the complexity of the logical structure of present day science requires analysis, at least for the reason that we attain the clarity requisite for the avoidance of inconsistencies and confusion of various kinds in our thinking about science. If, for example, experimental physics is regarded as a tool for the acquisition of knowledge applied in engineering (and so ultimately, we hope, for human welfare), then theoretical physics and pure mathematics, each in turn are further tools in the same enterprise; and finally, the operational, syntactical and semantical analysis of the symbolisms used on these various levels is a further instrument, a further technique designed to help in the progress of science. Does Churchman need to be reminded that studies, e.g., in the foundations of probability and statistics (in which he himself has shown a strong interest) are of the nature of a logical reconstruction? Does he, the esteemed editor of the *Philosophy of Science* journal, need to be refreshed on the difference between statements in science and statements about science? Doesn't he too, for the sake of clarity, find it imperative to distinguish among the latter very sharply between statements concerning the psycho-socio-historical aspects of science (i.e., the context of discovery) and statements concerning the logical aspects (i.e., the context of justification[1])? If Churchman is at all willing and able to distinguish an account of the genesis or development from an account of the validating reasons of scientific knowledge claims, would he not then wish to make each of these accounts as explicit, articulate and efficient as possible? If so, then I think he will have to admit that the philosopher of science, or in any case the *logician* of science (in contradistinction to the sociologist of science) is charged with the task of ascertaining the validating grounds and the validating principles of scientific assertions. What specific forms the fulfilment of this task may take; what specific tools may prove the most useful in its pursuit, these are of course further questions regarding which there can be legitimate controversy.

Turning more specifically to the issue under discussion I would urge the following considerations upon Professor Churchman, and perhaps upon pragmatists in general:

(1) Knowledge-claims in the natural and the social sciences are legitimate only if they are based on specifiable evidence. If we are not to be bogged down with the well known troubles of a pure coherence view of confirmation we must at any given stage of science be able to quote observations that serve as evidence, at least until further observations impel us to discredit such erstwhile evidence and replace it by other observations.

(2) While I have not only admitted but indeed stressed that there is no completely sharp division between the directly observable and the indirectly confirmable, I would most definitely oppose any attempts to make more of this "fuzziness" than it warrants. A. C. Benjamin, for example, possibly under the influence of Whitehead, seems to me to have blurred important issues by the too facile device of expanding his distinction (of degree) of the "clearly given" and the "obscurely given" beyond the very narrow zone of border-line vagueness in perception.[2] The indirectness of confirmation in the case of existential hypotheses of my type B (e.g., as regards past events, electric fields, nuclear processes, etc., etc.) is radical and irremediable. Visible spectral lines are evidence for the dynamics of atoms. Inscriptions on tombstones are evidence for events in history. Only by distorting the ordinary meaning of the word "evidence"

[1] Cf. H. Reichenbach, *Experience and Prediction* (§1) for a clear statement of this difference.

[2] A. C. Benjamin, *An Introduction to the Philosophy of Science*, Macmillan, 1937, New York: esp. pp. 131–134.

beyond recognition could one reverse these relationships. In the explication of the justification of our knowledge claims we must pay attention to the (epistemic) primacy of the observable; even if, as I have stressed, this primacy becomes irrelevant in the ("realistic") account of the finished scientific theory. In which other way could one possibly state on what grounds the more highly theoretical assertions of science can be warranted? If Churchman's outlook does not embrace this basic minimum of empiricism, then I have either not even begun to understand him or he is not entitled to classify himself as a pragmatist or as an experimentalist.

(3) The term "observable," as Churchman maintains, may indeed be taken to refer to a dispositional property, and as such it involves some of the relativities which Professor Ramsperger stresses. But it must be remembered that in the (indeed customary and legitimate) sense in which both commentators use the term, it is a scientific term and therefore presupposes the frame of the scientific account of the world that enables us to speak of observers and observed objects, of organisms and their environment, of the conditions and the consequences of perceptual processes, and the like. In the context of logical reconstruction however the scientific characterization of observability must be understood as an extrasystematic, as it were, marginal, didactic or elucidatory hint. In the reconstruction in terms of pure pragmatics certain predicates are distinguished from others by purely formal features. In the earlier phase of logical positivism we used to speak of primitive or undefined predicates which served as the basis for the introduction (by explicit or contextual definitions) of derived predicates. Although we have changed our views on some basic features of the forms of reconstruction we still maintain as radically as ever the distinction between the logical analysis of scientific language from the account of the world given in terms of the scientific language. That the logical analysis must, in order to be adequate, finally disclose a certain congruence with the scientific account of knowledge, is precisely one of the major points of my essay, and I regret that it was not accepted as an olive branch by the pragmatists.

(4) To the teleological or purposive character of the scientific (as well as of the logico-analytic!) enterprise I attribute the same importance as does Churchman. All goal directed behavior depends on drives or needs (primitive or derivative) and is characterized by docility (analyzable in terms of feedback mechanisms). Far from condemning teleological concepts I would be eager to see them completely purged of their metaphysical connotations and clarified in terms of causal-statistical mechanisms. The new discipline of cybernetics strikes me as very hopeful in this connection. But again, this pertains to science (for our issue, the bio-psycho-sociology of knowledge) and not directly to the philosophy of science. Teleological considerations become relevant only if we step outside the context of logical reconstruction and ask for a (pragmatic) justification or vindication of the very principles that are presupposed in the (cognitive) justification (validation) of our knowledge claims. I have tried to disentangle this delicate and complex problem in a special essay on the meaning and the limits of justification.[3]

[3] Entitled "De Principiis non est disputandum —?" forthcoming in *Essays in Analytic Philosophy*, ed. by M. Black, Cornell University Press, 1950.

Turning to Professor Ramsperger's suggestions in favor of Objective Relativism or Contextualistic Realism, I admit that I have dismissed this view all too briefly without discussion. But even with the helpful illustrations in Ramsperger's comments (as well as the fuller presentation in his book *Philosophies of Science*) I am still unable to see how his position can obviate the issue of realism vs. phenomenalism. Granting the relativity to context of the facts of perception, I would first of all ask: how is this relativity itself confirmed? Ultimately by observations, is it not? And this for the simple reason that any statement of the dependence of perceptual fact upon the conditions of observation is in the nature of an empirical law (psychological, psycho-physical, psycho-physiological, physical, socio-psychological, etc.). Such laws can however be established only by confirming instances, and the description of these instances must, at that stage of inquiry and until further notice be taken as data with no "if" or "provided" attached to them. Availing myself of one of Ramsperger's own examples I would advance the following further considerations and questions: In order to predict what color impression a given observer will receive (and report) when exposed to light rays we must know (or assume) a good many facts about the observer as well as about the radiation. Is it not a conclusion of scientific research, rather than one of its basic epistemological presuppositions, that the color impression is dependent upon the conditions of the observer (his location, adaptation, accommodation, retinal and neurophysiological, psychological, etc., characteristics) and the conditions of the radiation (frequency, intensity, polarization, etc.)? Quite generally, do not all statements of contextuality or relativity occur *within* the frame of scientific concept formation? In this age of relativistic and quantum physics I am as fully impressed with the relational character of "reality" as is Professor Ramsperger. But I do not see how the very statement of any relational or relativistic situation can be significant (let alone fruitful) unless it is made in terms of some *invariants*. The program of scientific knowledge proceeds unmistakably from narrow and local contexts to wider and (ultimately or ideally) universal contexts. Of course we can never be sure that the basic constants (such as those of contemporary physics: c, e, m, h, etc.) are not themselves relative to as yet unrecognized contexts. But this reservation is only the indispensable "valid until further notice" clause, the warning call of caution, insisted upon by any empiricist aware of the self correcting nature of scientific research. Fully granting all this, I still maintain that any statement of relativity to context can serve in scientific explanation and prediction only if it is formulated in terms of functional relations which, at least for the time being, are regarded as invariant. I trust that I shall not be grossly misunderstood as advocating some metaphysical absolutes. Now the natural laws stating some of the more pervasive invariancies of relationships contain concepts of the hypothetical-construct type. In the above example we may make use of Maxwell's concept of the electro-magnetic field. If it were maintained that the total meaning of statements containing such concepts consists in the (infinite) set of directly verifiable statements describing observable results in observable contexts, then I would characterize this position as (contextualistic) phenomenalism. However, Ramsperger, does allow for something more; namely counter-factual conditionals. And if I may suggest a few important distinctions, Ramsperger requires counterfactuals not only of the ordinary type, i.e. those that specify what would be observed under (a) actually unrealized and (b) technically unrealizable conditions, but also, and this is notable: (c) physically unrealizable conditions. I shall not elaborate the obvious objection here that the actual procedures of scientists do not involve considerations of this last sort; for example, no atomic physicist seriously depends upon the fictional conditional concerning how he would perceive a hydrogen atom if his organism were reduced to comparable size. Rather I should urge Ramsperger to realize, that whatever he can state in the form of the last type (c) of counterfactual hypotheticals he actually deduces from a theoretical system which he *presupposes*. That this theoretical system in turn is arrived at by inductive and analogical reasoning, or in any case can be justified only by inductive logic, will be granted. But neither the meaning nor the validity of the system depends upon the mentioned fictitious conditionals.

This brings me, finally, to the searching questions and criticisms of Professors Frank, Hempel and Nagel. (Since their comments have a good deal in common I shall address myself to these three critics simultaneously.) Their major doubts concern the precise nature of the *surplus of meaning* which according to my view attaches to existential hypotheses which are *in principle* only indirectly confirmable. Much as I have tried in my essay to make myself clear on the significance of these crucial (italicized) phrases it seems I have not fully succeeded. Let me tackle first the last point once again. In the scientific account of the cognitive process (which Churchman mistakes for a philosophical account, and which Ramsperger presupposes for the formulation of his counterfactuals) we trace the adaptations of the organism (human being) to its environment (physical and social) in terms of the psychology of learning. The organism, being a spatio-temporally minute part and quite recent arrival in the vast setting of the processes of the universe, acquires habits of action and of expectation. In this task the symbolic function of language is of the greatest importance. The "mapping" of the universe is carried out by means of the reference of linguistic, or in any case rule-governed, symbols. Only some symbols (or rather individual tokens thereof) actually confront their designata within human experience. The vast majority has what some realists are fond of calling "transcendent reference." It was my concern to show that this transcendence is completely unobjectionable in contradistinction to the transcendence invoked in metaphysical speculation. The manner in which the knowing organism is embedded in the world of which it is a part simply precludes *in principle* direct experience or confrontation of all but a minute portion of that world. This "impossibility in principle" is not a logical impossibility in the sense of self-contradiction. It is a physical impossibility in the sense that it involves incompatibility with acknowledged basic features and laws of the universe. From a philosophical point of view it is important to differentiate those physical impossibilities that involve specifically the cognitive processes of organisms from those that don't. In the latter class we find for example the various types of *perpetuum mobile* of thermodynamics. But the "egocentric" and "present-moment" predicaments of epistemology involve the knowing subject (organism). As I indicated in my essay, the impossibility of a return to the past (or the impossibility of reducing one's size to atomic dimensions, or of making one's retina sensitive to radio-waves) are matters either of the basic structure or of special laws of nature. In the context of logical reconstruction we reflect these "predicaments" simply by the choice of the *basis* of reconstruction. The evidential basis thus understood can therefore never provide for sets of statements that would be equivalent to statements whose factual reference transcends the physically possible direct evidence. The counterexample contrived by Hempel is specious in that the logical equivalence of the two statements depends (according to his own presuppositions) upon the non-factual character of the statements: that the 1st of June (1949) falls on a Wednesday is either an analytic proposition and therefore irrelevant for what the example is to prove, or else factual (socio-linguistic), but then it is not *logically* equivalent with the other statement (that the 2nd of June falls on a Thursday).—Even if we allow, as Nagel does, for infinite sets of statements, capable of extension in unforeseen directions, the epistemic predicaments will "in principle" preclude strict logical equivalence of statements only indirectly confirmable with statements directly confirmable. This is my reason for ascribing a "surplus meaning" to the former.

Professor Frank's comments seem to imply a denial of this surplus meaning. Indeed, if he identifies the meaning of scientific statements with their truth conditions (this latter term understood in the sense of evidential basis) then this is precisely the phenomenalistic position I was concerned to refute. But if we take the term "truth-conditions" in its recent semantic usage, then it coincides completely with the sense in which I used the term "factual reference." In this (semantical) sense, and in this sense only, can we say of an existential hypothesis that it means precisely what it says. The intention of my essay was to avoid both the metaphysical excesses of traditional physical realism, e.g. that of M. Planck and in much of American Critical Realism as well as the reductive fallacies of phenomenalistic positivism (Mach, the Vienna circle, etc.). The slogans of operationism and of the first phase of logical positivism were: "A concept is identical with the set of operations that determine its application;" "the meaning of a statement is the method of its verification." I agree with Frank that these slogans were excellent devices for the elimination of metaphysics. But as is so frequently the case in the history of ideas, these extreme measures, this all too radical handling of Occam's razor, went too far in the other direction. Schlick's memorable essay on Positivism and Realism[4] attempted to do justice to both sides but unfortunately remained vague and vacillating just in the most crucial points. At the time of writing that essay, Schlick was reacting against his earlier realistic position. This accounts for the decidedly phenomenalistic trend in all his later work. The proper synthesis, I still maintain, could be found and formulated only in terms of pure semiotic which became fully available only after Schlick's premature death in 1936. Nevertheless, Schlick's early realism, expounded in his *Allgemeine Erkenntnislehre* of 1918 and 1925, was an admirable informal anticipation of the sort of realism toward which Carnap (ever since "Testability and

[4] Contained in *Gesammelte Aufsätze*, Gerold, Vienna, 1938.

Meaning" and his recent work on inductive logic) has been modifying his earlier positivism.

Twentieth century physics, I should like to suggest against even such an authority as Frank, does not lend more than a superficially convincing support to phenomenalistic positivism. The evolution of recent physics of course represents a powerful argument against any sort of apriorism. But it is consonant only with a sufficiently broadminded empiricism. As I indicated already in my remarks to Ramsperger's comments, there is no difficulty in combining the idea of factual reference with whatever relativities need to be taken into consideration. In regard to spacial or temporal determinations we can take a certain frame of reference for granted, and express in the coordinate language thus provided the measurable (or inferable) quantitative values of lengths, durations, masses, etc. relative to that frame of reference. Or else, we decide upon Minkowski's representation in which case the four-dimensional intervals between space-time-points (events) are among the objects of factual reference. Although I cannot possibly enter here into a discussion of quantum mechanics, I should like to anticipate at least one challenging question that arises out of the interpretation of Schrödinger's equations. I might be asked about the factual reference of the wave and the particle concepts. In agreement with the generally accepted interpretation by Max Born I would of course consider the values of ψ^2 as statistical frequencies. But I would insist that the frequencies concern micro-events which according to only indirectly confirmable existential hypotheses have *some but by no means all* the characteristics of (classical) particles in motion, collision, etc. Semantic realism as I should like to see it understood, is free from the dangers of metaphysics precisely because it does *not* prescribe anything at all about the *nature* of the designata of our theoretical constructs. It is concerned only with the most abstract and formal features of the semiotic situation. There is no danger that the wish for picturization, so strong in the older, metaphysical forms of realism, will dictate the application of the categories of commonsense to domains where they are notoriously out of place. Things are and will always be—as far as we can meaningfully talk about them—what they are confirmably *knowable* as; and it is up to the advance of science, not to logical or semiotic analysis, to tell us what things are "really" like. But it is the task of logical analysis to tell us by means of what rules of our language we describe the objects of our knowledge, and (this was our major concern) what we mean by the surplus of the *knowable* over the *known*.

The exact explication of this surplus meaning is a further task which I have indeed only sketched in outline. I readily concede that pure pragmatics has not been developed to the extent that its indispensability or fruitfulness is as obvious as is (to my mind at any rate) the value of pure syntax and pure semantics. Fortunately I can here again refer to the articles by Wilfrid Sellars (listed in the bibliography of my essay) in which the basic ideas of a pure pragmatics are set forth. The work of W. Sellars has impressed upon me the perfect analogy of all three branches of pure semiotic: syntax, semantics and pragmatics. Ironically, the general resistance against recognition of the clarifying power of these three disciplines appears to be inversely related to their philosophical importance and must be overcome one by one in the chronological order of their development. As I see it, Frank and Nagel allow for syntactical studies of the language of science and supplement them by methodological or operational analyses. But those latter analyses are still mixtures of the descriptive pragmatics as pursued in the history of science and pure pragmatics which is a formal discipline that deals with the norms of meaning, meaningfulness, verification, confirmation, verifiability and confirmability. In his concluding remarks, Hempel concedes that a purely syntactical account of science must be supplemented by a "semantical interpretation of at least some of its terms." I suspect that Hempel has here in mind only the predicates whose designata are observable thing-properties and the proper names which designate the objects of direct acquaintance. The various arguments that I adduced against this syntactical positivism and in favor of a semantic (or perhaps, as I had better call it, "pragmatic") realism simply amount to the claim that when we fully and justly explicate the way in which we use the language of science (or homologously, the language of commonsense) we cannot do without a set of designata that are in principle beyond the reach of direct experience. I maintain that a good many statements concerning theoretical constructs and hypotheses made by Frank, Hempel and Nagel are *de facto* statements in the pure pragmatics of science. Any surprise of my good friends at having, at best implicitly, utilized the metalanguage of pure pragmatics would be no better justified than the surprise some of us had some fifteen years ago when we learned from Tarski that any statements about the truth of sentences or the designation of terms (with which ordinary conversation and certainly logical discussions abound) belong to the metalanguage of semantics. (Monsieur Jourdain was surprised that he had been speaking prose all his life.)

A more serious and difficult question pertains to the probability of existential hypotheses. This a highly involved issue because neither the frequency theory nor the logical theory of probability have as yet provided a full and satisfactory account of the probability of complex scientific hypotheses. Moreover the issue between the frequency and the logical interpretations of probability is still controversial. It seems to me, however, that on either interpretation we can define inductive probability only if we have first of all clearly settled the vocabulary and the rules of the language in which both the hypotheses and their supporting evidence are formulated. The ratio of ranges which defines the degree of confirmation of an hypothesis cannot be determined unless we presuppose a definite set of particulars, predicates and relations. It was my contention that the language of science employs terms whose designata extend far beyond the scope of the phenomenal data. The temperature of a body, for example, is not to be identified with any or all the possible operational indications of that temperature. It is a state of that body to which we can refer only after the language has been sufficiently extended to include, besides the predicates needed to describe the various indications, also the states indicated. I must admit that I cannot at present furnish an accurate reconstruction of the meaning of inductive probability for existential hypotheses (Type B). But it seems obvious, especially considering the inadequacies of phenomenalistic interpretations granted by Nagel, that we cannot identify the probability of an existential hypothesis, e.g. regarding the surface temperature of the sun, with the probability of the outcome of any one (or all) of the various indirect indications and measurements that would confirm that hypothesis. Since Hempel, in criticizing my statement, refers to Carnap's article on "The two concepts of probability," I may in turn refer him to footnote 20 in that same article in which Carnap explicitly endorses my "empirical realism." The interpretation of the language in which we can meaningfully speak either of limits of statistical frequency or of ranges of propositions seems to me to be precisely the one of semantic realism.

At the risk of provoking intense controversy I might suggest an argument that goes beyond the considerations of my essay. This argument would require a good many qualifications to safeguard it against misinterpretation. Since there is no space here to do this I shall state my point quite bluntly, but would not wish to insist on either its cogency in its present form or its indispensability for my point of view. In brief, I contend that there is a specific kind of difference that *makes* a difference between Syntactical Positivism and Semantic Realism. I still maintain of course (as before) that this difference is not of the kind that we so often encounter in the case of rival scientific hypotheses or theories. Differences between rival theories, if they consist in discrepancies of their factual content and not merely in their logico-mathematical formulations (i.e. in their respective degrees of formal simplicity) can indeed be determined by empirical tests. The difference between Syntactical Positivism and Semantic Realism lies in their different semantical interpretation of one and the same theory. The kinetic theory of heat, to take a simple example, from 19th century physics, is interpreted by syntactical positivists as merely a convenient formal device designed to correlate and unify the various empirical laws of thermodynamics. A phenomenalist like Mach would admit as much as this only in his more tolerant moments. In view of the triumphant success of the molecular, atomic and quantum theories during the last eighty years, more recent phenomenalists (such as P. Frank, R. Von Mises, N. R. Campbell, H. Dingle, G. Bergmann, a.o.) do not in the least deny the fruitfulness of those "constructions." But would physicists have pursued this type of theory construction and attained their goals with such remarkable success if they had really held the phenomenalistic interpretation and not merely paid lip-service to it (as did some of them, e. g., Heisenberg and Dirac)? Now this question might be dismissed in the familiar manner as a purely psychological and historical one, concerned with the development of scientific ideas, and the heuristic efficacy of pictorial models. I hasten to assure the reader that my argument is intended in a *logical* sense, concerned with the semantic interpretation, not with the heuristic value of the picturization of theoretical systems. Here then, is what I suggest: The difference that makes a difference can be explicated by the differing inductive probabilities of concrete predictions. In the example of the kinetic theory, a consistent phenomenalist would say (and did say) that Maxwell's theorem concerning the distribution of velocities among the molecules of a gas is merely part of the mathematical model whose exclusive task is to integrate into an expedient deductive structure the various experimental laws which state the relations between such observables as pressure, volume, temperature, concentration, rate of diffusion, viscosity, etc. Since "constructs" like the mass and the velocity of individual molecules are expressly viewed (by the phenomenalists) as "nothing but" parameters in an abstract model, he could not *on this interpretation* have predicted with any appreciable probability the outcome of such experiments as that of Born and Stern. In this experiment it became possible, by a simple but most ingenious device, to measure the speeds of individual molecules. In order to derive this outcome with the high probability that physicists in general attach to such predictions[5] the macro-observable setup of the experiments must be interpreted in terms of micro-existential hypotheses. This, however, involves the abandonment of the phenomenalistic interpretation of the theory in question. I should like to ask syntactical positivists, and phenomenalists generally, to provide a plausible reconstruction of this striking feature of modern science: the high objective probability of the results of experiments of the kind mentioned. A purely syntactical interpretation of the postulates of the theories in question does not seem to me at all adequate for the explication of this feature.

My point is simply this: The customary probabilistic realism in trying to justify "transcendent" hypotheses on the basis of experimental findings has put the cart before the horse. Only after the introduction of the realistic frame can we legitimately argue inductively either from the theory to the outcome of as yet unperformed experiments; or vice versa from the results of experiments to *specific* postulates of the theory. But the presupposed introduction of the realistic frame, i. e. the semantic-realistic interpretation of the theory, is a step that can be justified only instrumentally: It furnishes the very possibility of a theory that is inductively fruitful.

Looking back to the realism—positivism controversy of two generations ago (Boltzmann and Planck vs. Mach and Ostwald) we may say that the subsequent developments in epistemology and especially in pure semiotic have enabled us to eliminate the metaphysics from realism by utilizing the positivistic warnings against picture thinking. On the other hand we have preserved the sound element of realism in the idea of the factual reference of (some of the) hypothetical constructs. A positivism freed from the confines of a narrow phenomenalism can yet retain its most vital safeguard: the confirmability criterion of meaning. The resulting synthesis, empirical realism reconstructed in terms of pure semiotic, should help in avoiding wasteful controversies in the development of science. Thus, even if such logical reconstruction bakes no bread and builds no bridges; even if in and by itself it does not yield new techniques of empirical research; it may yet fulfill a function that even pragmatists might recognize as quite useful.

University of Minnesota

[5] Similar cases in point are: the outcome of the von Laue and Bragg X-ray diffraction patterns revealing the atomic structure of crystals; the cloud chamber tracks and Geiger counter indications in many other experiments; the Stern-Gerlach results on the magnetic moments of atoms; and countless other results in recent experimental atomic and nuclear physics.—Genetics and bacteriology furnish analogous illustrations.