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TOWARDS A CRITICAL PHILOSOPHY OF SCIENCE

Up until fairly recently, and still continuing in many quarters, the received view about the philosophy of science was one rooted in the Vienna Circle/Logical Positivism/Logical Empiricism [1]. In fact, it is something of a commonplace to point out that logical positivism both established the philosophy of science as a professional specialty (with its own disciplinary identity) and in so doing left an indelible philosophical mark upon it. Indeed, as Suppes points out[2], although logical positivism was widely rejected as a general epistemology, many continued to see it as adaquate philosophy of science. Logical positivism may have died as a general epistemological or philosophical movement, therefore, but it was merely transformed into philosophy of science. Indeed, although many philosophers of science would argue that this positivist philosophy of science is inadequate, its characteristic orientation (e.g., „the logic of science") still largely dominates the field of philosophy of science.

During the heyday of logical positivism (and to a lesser extent even today), philosophy of science was identified (roughly) as what logical empiricists such as Carnap, Hempel, Reichenbach, and Feigl and those sympathetic to them (e.g., Nagel, Braithwaite, Salmon) did in the way of articles and books on the philosophy of science. It was never admitted that there were full-fledged alternative and competing philosophies of

[1] How to distinguish these three philosophical positions is difficult to determine but nothing of any importance seems to depend upon it. In the remainder of this paper I will lump all three together under the label „logical positivism", a type of generic term for those several views held in common by all three positions.

[2] F. Suppes, *The Search jor Philosophical Understanding oj Scientific Theories,* [in:] *The Structure of Scientific Theories,* 2nd Ed., F. Suppes ed. (Urbana: University of Illinois Press, 1977), p. 6.

science — since this possibility would have seemed to make philosophy of science „non-scientific" [3] — or if this was admitted, these alternatives were seen as family disputes between, say, Carnap and Reichenbach. What seemed unquestionable, however, was that philosophy of science simply was analytic philosophy of science, a point Brodbeck explicitly makes [4]. If one were to suggest there were non-analytic philosophies of science, for example, phenomenological philosophy of science or Whiteheadian philosophy of science, it would not have been difficult to detect a thinly past attitude of contempt or amusement among many philosophers of science [5].

Such a situation remains to a surprisingly large extent still true of the philosophy of science today. If one peruses the pages of the two most prestigious journals devoted explicitly and exclusively to the philosophy of science — „Philosophy of Science" and „British Journal for the Philosophy of Science" — one will look mostly in vain for articles dealing with the philosophy of science of Husserl[6], Heidegger, Dewey, White- head, Polanyi, Piaget, Lorenzen, Brunschvicg, etc.[6] This is not because no one is writing articles on these figures — they are — but rather because these journals do not publish articles of this type. In short, it we label the kind of philosophy of science done by the logical empiricists

[3] This is the impression one gets, for example, from reading the Preface in Feigl and Brodbeck's classic *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953). Indeed, one suspects that if there was a „unity of science" movement within logical positivism, there was also a „unity of philosophy of science" movement — there is or ought to be just one philosophy of science! One day in class, in response to a question about her philosophy of science, May Brodbeck replied: „This is not *my* philosophy of science: this is *the* philosophy of science". Feigl was much more tolerant of diversity.

[4] M. Brodbeck, *The Nature and Function of the Philosophy of Science,* [in:] *Readings in the Philosophy of Science,* H. Feigl & M. Brodbeck, eds. (New York: Appleton-Century Crofts, 1953), p. 5.

[6] Other journals stressing philosophy of science are less doctrinaire but also less prestigious and hence less paradigmatic: Scientia, Epistemologia, Dialectica, Zeitschrift fuer allgemeine Wissenschaftstheorie, Explorations in Knowledge, Syn- these, Zygon, Inquiry, Journal of the British Society for Phenomenology, Fundamenta Scientiae, etc.

and contemporary Anglo-American philosophers of science as „analytic", then the point I am making is that contemporary philosophy of science is rather exclusively analytic, and non-analytic philosophy of science is ignored or derided [7].

According to the received view regarding the philosophy of science, philosophy and science are sharply to be separated. Since there were only two kinds of cognitively meaningful assertions — those of logic and mathematics (on the one hand) and those of the empirical sciences (on the other) — and since there was a sharp distinction made between the analytic/synthetic, the a priori/a posteriori, and the necessary/contin- gent — the philosophy of science could either be construed as being empirical (and hence like the empirical sciences) or as logico-mathematical (and hence like logic and mathematics). The first option would have produced a „naturalistic" philosophy of science [8] — something that Otto Neurath seems to have suggested early on — or a „logistic" philosophy of science [9], one that turned philosophy (and philosophy of science) into a kind of applied logic [11]. (The turn towards logistic philosophy of science seems to have been due largely to the influence of Carnap, with Schlick's view on this question being somewhat less clear). With the influence of Russell, Frege and Bolzano being so strong on the Vienna Circle, it can hardly be surprising that under the particular influence of Carnap, the early positivists became logicists and Platonists.

According to this standard logicistic view, philosophy of science did not make any kind of empirical assertions (which was the proper task of science), but rather its propositions were analogous to (or reducible to)

[7] This same point could be put somewhat differently by saving that Anglo- American philosophers of science read virtually nothing except books and articles in English. This even includes analytic philosophy of science done on the Continent. If Stegmueller's work, for example, had not been translated into English, I doubt whether anyone would have read it. Similar remarks apply to Lorenzen's Constructivism and to the Konstanz school.

[8] For a discussion of one kind of „naturalistic" philosophy of science see R. Gie- re, *Explaining Science: a Cognitive Approach* (Chicago: University of Chicago Press, 1988) and P. Thagard, *Computational Philosophy of Science* (Cambridge, MA: M.I.T. Press, 1988), Thagard (19).

[9] The turn towards logistic philosophy of science seems to have been due largely to the influence of Carnap, with Schlick's view on this question being somewhat less clear.

those of logic and mathematics. Furthermore, since logic and mathematics were taken to be analytic, *a priori* and necessary [10], so philosophy of science was construed to be analytic, *a priori* and necessary, whereas science was synthetic, *a posteriori*, and contingent. As a second order reflective discipline, philosophy of science would never compete with science— and this meant it would make no empirical claims that might conflict with science — but would be a meta-level „analysis" of science, and analysis, being assimilated to applied logic, would possess those features of logic — its analyticity, *a priori* nature and necessity — that would make it categorically distinct from the empirical realm [11].

Thus, on this view, philosophy of science was concerned exclusively with logical analysis. In the inaugural issue of „Philosophy of Science" [12] Carnap asserted that philosophy of science deals with „the logical analysis of the concepts, propositions, proofs, theories of science, as well as... [its] ...methods". As P. H. Niddith [15] puts it:

> ...the dominant conception of the nature of philosophy among philosophers in the English-speaking world [is that] philosophical problems are not empirical, and philosophical utterances... are not empirical: their non-empirical character is bound up with their being in some special sense 'explanatory'. This conception arose by virtue of the desire not to fall into metaphysics on the one hand nor to armchair science on the other.

Furthermore, just as (applied) logic and meta-mathematics are activities to be judged by their own intrinsic (logical or formal) standards, so philosophy of science was imagined to be an activity carried out according to autonomous philosophical (i.e., logical) methods and judged by criteria of a purely philosophical (i.e., logical) kind. This meant that the philosophical correctness of a proffered philosophical analysis of, say, some scientific concept was not to be evaluated by reference to actual scientific practice or its history; this would be tantamount to confusing philosophy of science with science itself. Thus, philosophy of science was not descriptive of actual science, but rather prescriptive of it, or (as they preferred to express it) philosophy of science offers explications[13] of scientific concepts and rational reconstructions of scientific activity [147]. Furthermore, philosophy of science was radically different from the apparently related disciplines of history of science, psychology of science, and sociology of science. Nothing in these latter (empirical) sciences could be of any relevance to the philosophy of science, since this would be confusing philosophy of

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[10] Not everyone in the early Vienna Circle held this view about the radical non-empirical nature of logic. Kurt Godel, Alfred Tarski and Hans Hahn seem not to have.

[11] For example, C. Hempel *(Aspects of Scientific Explanation,* [in:] *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* [New York]: Free Press, 1965], pp. 412—415) likens the task of philosophy of science to that of proof theory in meta-mathematics.

[12] R. Carnap, *On the Character of Philosophical Problems,* Philosophy of Scien

[135] P. H. Niddith, *Introduction,* [in:] *The Philosophy of Science.* P. H. Niddith, ed. (Oxford University Press, 1968), p. 5.

[14] R. Carnap, *The Logical Foundations* of *Probability,* 2nd ed. (Chicago: University of Chicago Press, 1962), pp. 5—7.

science with science [15] and would involve a fallacy of deriving a norm from a fact.

As a consequence of their heavy reliance upon the areas of Russell, Frege and Bolzano, who were largely Platonistic in their philosophy of logic and mathematics, most of the early logical positivists were logicists about the foundations of mathematics. In their philosophy of science, this logicistic method resulted in another kind of „logicism", namely, the thesis (roughly put) that all significant philosophical questions were reducible to questions of logic. What was crucial to science, therefore, was not scientific activity itself — what we can call the entire scientific enterprise — but rather a set of scientific propositions. Secondly, it was the underlying logic of these propositions that was crucial and this meant, in keeping with the positivist's interpretation of logic, that their formal aspect and not their content was what was important. To obtain this, one had to abstract the formal aspect from the wider context of scientific thought and canonize it via some abstract model. Initially, this took the form of maintaining, as Carnap did, that what was significant about scientific knowledge could be completely captured by the logical syntax of language. Later, Carnap's restrictive proposal was broadened so as to include the semantic aspect of logic (and sometimes it was even admitted that logic had a pragmatic aspect)[16]. The result of such a philosophy of science was a philosopher's philosophy of science not a scientist's. This often resulted in a philosophy of science fiction rather than a philosophy of real science. When this occurred, the „analyses" were of little relevance to science but ones which would allow generations of graduate students to pursue dissertation topics on, say, the paradox of the ravens or the „grue-bleen" paradox (to mention just two). Anyone who has ever taught Goodman's „new riddle of induction" to scientists will real immediately the struggles involved in convincing them that this

[15] M. Brodbeck, op. cit., p. 3.

[19] For his history of Carnap's changing views about this question see *Carnap's Intellectual Autobiography*, [in:] *The Philosophy* of *Rudolph Carnap*, P. A. Schlipp, ed. (LaSalle. IL: Open Court, 1963), pp. 46—56, 53—56, 60—67.

This view of the connection between science and philosophy, togetner with the associated view of the nature of the philosophy of science, has fewer followers today than it once did (although I think the majority of philosophers of science still hold something like this received view — just look at the kind of articles they typically publish). The reasons for this change are complex and have been documented historically by several individuals. But the upshot is that it is no longer defensible to hold this view of the relation between philosophy of science and science. Philosophy of science ought to be and increasingly is a philosophy of real science: it is concerned with understanding science philosophically, and anything that can help us do this should be used. This includes the history of science, the psychology of science, and the sociology of science. In particular, philosophical analyses of science need to be checked against real science, since a constraint on any adequate philosophy of science is that it match actual cases of science at their paradigmatic best[17].

That the philosophy of science cannot be sharply separated from the history of science now seems to be widely accepted by philosophers of science. Furthermore, many of them argue that a logicist conception of the philosophy of science must give way to a more historicist conception of the philosophy of science and that a stress on formalism must be balanced by a stress on the development of scientific theories. This would involve, as I have argued elsewhere[18], seeing them as developmental entities which change according to certain developmental dynamics. Furthermore, to understand science, one must understand real scientific practice with all its complexities and messiness and this, in turn, would involve one in the sociology of science. If one is going to understand how actual science is practiced today, then a description and explanation of that practice is essential. This would include studying science as a social institution, understanding the social norms operating to reward scientific merit and creativity and punish fraud and deception[19]. To understand science we must understand how graduate students are educated, the role

[17] I am explicitly ignoring ail the philosophical problems underlying this statement. Needless to say, considerably more must be said concerning how this is supposed to work. Lacking such a conception leads to conceptual problems exemplified, for example, by R. Ackermann, *The Philosophy of Science* (New York: Pegasus, 1970).

[18] R. Kitchener, *Developmental Explanations,* Review of Metaphysics, 36 (1983), pp. 791—818; *Is Genetic Epistemology Possible?,* British Journal for the Philosophy of Science, 38 (1987), pp. 238—299; *Piaget's Theory of Knowledge: Genetic Epistemology and Scientific Reason* (New Haven: Yale University Press, 1986).

[19] See R. Merton, *The Sociology of Science* (Chicago: University of Chicago, 1973).

of textbooks in the entire scientific enterprise, the need to publish articles in order to receive tenure and promotion, the dissemination of knowledge through conferences and informal groups, the role of the Nobel Prize in directing scientific practice, the epistemological importance of the „research unit" vs. the individual scientist, etc. If these topics do not appear relevant to the understanding of science, then just consider the absolutely crucial but philosophically neglected role of funding and grants in contemporary science (and it associated political influence on science). Anyone who knows a smidgeon about current science knows the absolutely dominating role that grants play in the scientific endeavor and yet one will look in vain for a philosophical discussion of this feature of science. Instead, what one finds are discussions of, say, Popper on why scientists (should) abandon a theory when it is falsified (via *modus tollens).* True, there are numerous discussions in the sociology of science which are of questionable value (e.g., citation counting) and others (such as the „Strong Programme") which are philosophically questionable. But there are also numerous other parts which are positively valuable. Although the psychology of science is a discipline that remains to be developed, it too would go far towards helping us understand science in the modern world. I think that a more adequate rendering of the philosophy of science should be conceived in which one includes the history, sociology and psychology of science in addition to the philosophy of science.

The resistance of the positivists to such a view was philosophically anchored in the questionable assumption that one can separate (in a rigidly compartmentalized way) empirical science from philosophy. At most, however, there is a difference in degree between good science and good philosophy, since both must involve conceptual analysis and empirical facts. Furthermore, if one is going to pay attention to real science, one must know something about it and this means that a philosopher of science must not only read an occasional textbook on, say, physics, but must have considerable theoretical and practical experience in the science about which (s)he is philosophizing. The extent of this knowledge may even have to include the conducting of experiments and/or the making of observations as well as a detailed knowledge of the appropriate advanced mathematics that is closely related to the science in question.

If this is correct, then I think a more critical philosophy of science would include the philosophical perspective that science and philosophy are not sharply separated, that the philosophy of science must be rooted in a thorough knowledge of the relevant sciences and even that in order to answer a „philosophical" question, it may be appropriate to fund research of a purely „scientific" kind, say, conducting a research program

> on a particular phenomenon such as the Kervran effect. In short, a more correct philosophy of science should definitely be a scientist's philosophy of science and not a philosopher's philosophy of science.

Philosophy of science on the received view is concerned with the logic of science. It

> ...interprets science as a body of deductive or quasideductive systems of assertions: these

systems and their components are analyzed and judged by using concepts and rules (a.g., *modus tollens)* belonging to the study of formal inference and implications, or to some extension of this, e.g. the probability calculus, or are analyzed into something modelled on it, e.g. a formal theory of simplicity [20].

Thus, the scope of philosophy of science was basically that of logic — a kind of applied logic. At least, methodologically speaking, philosophy of science was pursued by means of methods basically similar to those found in logic. What then of other fields of traditional philosophy, namely epistemology, metaphysics and ethics? Clearly, epistemology was to be included, since science obviously consists (on the received view) of a set of propositions (claims) about knowledge of the natural world. Scientists not only construct theories, laws, and models, which have and underlying logic to them and whose logical analysis was the task of philosophy of science, they also make various kinds of epistemic claims about these entities and engage in epistemic activities (e.g., observation and reasoning), which are designed to support these claims. Thus, philosophy of science came to be seen as dealing with the logic, methodology and epistemology of science.

On this view, therefore, logic and epistemology have a crucial part to play in the philosophy of science but other areas of traditional philosophy have less clear ones. This is especially true of ethics and (to a lesser extent) metaphysics. As Herbert Feigl puts it:

> [Philosophy of science] may involve reflections upon problems traditionally classified as „metaphysical"... But the present trend favors a restriction of the discipline of philosophy of science to the logical analysis and clarification of the knowledge-claims of the sciences [21].

The status of the metaphysics of science in logical positivism has always been somewhat unclear. On the one hand, metaphysics (and ethics) were declared to be cognitively meaningless: „In the domain of metaphysics [Carnap says [22]] including all philosophy of value and normative theory, logical analysis yields the negative result that the alleged statements in this domain are entirely meaningless". Metaphysical statements, Carnap claimed, were simply *Lebenseinstellungen* and *Lebens- gefuehle,* subjective existential commitments and expressions of personal feeling.

As such, metaphysical statements did not belong, properly speaking, to science or

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[20] P. H. Niddith, op. cit., pp. 2—3.

[21] H. Feigl, *Philosophy of Science,* Philosophy (Englewood Cliffs, NJ: Prentice- Hall, 1964), p. 470. This same point is made by Robert J. Baum (Can *Governmental Support of Philosophy of Science Research be Justified?,* PSA 1976, vol. 1, F. Suppe & P. D. Asquith, eds. [East Lansing, MI: Philosophy of Science Association 1976], p. 293). In fact Baum is one of the few philosophers who have explicitly claimed that philosophy of science should include the epistemology, metaphysics and ethics of science. In fact, he even goes so far as to suggest that every branch of philosophy should be a part of the philosophy of science, including for example, the political philosophy of science, the aesthetics of science, etc.

[22] R. Carnap, *The Elimination of Metaphysics through Logical Analysis of Language,* [in:] *Logical Positivism,* A. J. Ayer, ed. (Glencoe, IL: Free Press, 1959), pp. 60—61. This article originally appeared in 1932.

to the philosophy of science [23], since science included only what was cognitively meaningful. Obviously, a key question here concerns the meaning of .metaphysics'. According to Carnap, metaphysical statements „transcend the limits of human knowledge" in the sense that there is no empirical method for verifying such statements. This, in turn, meant (roughly put) that from such metaphysical statements, no protocol or observation statements were deducible such that these observation statements would verify, falsify or confirm the truth of the metaphysical statement. Thus, by 'metaphysics' Carnap meant

> ...any alleged knowledge by pure thinking or by pure intuition that pretends to be able to do without experience. But the verdict equally applies to the kind of metaphysics which, starting from experience, wants to acquire knowledge about that which transcends experience by means of special inferences (e.g. the neo-vitalist thesis of the directive presence of an „entelechy" in organic processes, which supposedly cannot be understood in terms of physics: the question concerning the „essence of causality", transcending the ascertainment of certain regularities of succession; the talk about the „thing in itself" [24].

In his afterward to this article (written in 1957) Carnap says that the term metaphysics (appearing in his earlier article) refers to the field of alleged knowledge of the essence of things which transcends the realm of empirically founded, inductive science. Metaphysics, in this sense, in- eludes systems like those of Fichte, Schelling, Hegel, Bergson and Heidegger. But it does not include endeavors towards a synthesis and generalization of the results of the various sciences[25]. Indeed, following Feigl, we can call this latter type of metaphysics an *inductive metaphysics,* which designates „speculative extrapolations based on scientifically obtainable evidence" [26], e.g., cosmology and psychoanalysis. This may be risky, Feigl says, but not meaningless.

Feigl goes on to allow still another kind of metaphysics as legitimate — *categorical analysis,* which is „an investigation of the basic concepts and conceptual frames used in our knowledge of reality. This is not fundamentally different from the sort of logical analysis pursued by the positivists"[27]. Thus, an investigation into the basic concepts of, say, physics — matter, energy, space, time, causality, determinism — would presumably be an example of this kind of metaphysics. Indeed in another place Feigl explicitly calls such reflections „metaphysical" and allocates this task to the philosophy

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[28] [6] In a sense, however, Carnap's existential or emotive theory of ethics was a kind of „philosophy of science" as applied to ethics, only it was a philosophy of science in a sense broader than their own conception of the philosophy of science, since the latter was equivalent to „a philosophy of scientific knowledge" whereas the former was a philosophy of the entire scientific enterprise" (including non- epistemic domains).
Metaphysics, [in:] *A Modern Introduction to Philosophy, P.* Edwads & A. Pap, eds. (New York: Free Press, 1973), pp. 761—763 and A. J. Ayer's, *Language, Truth and Logic,* 2nd Ed. (New York: Dover, 1946), pp. 33—34.

[25] R. Carnap, op. cit., p. 80.

[26] H. Feigl, *The Wiener Kreis in America,* [in:] *The Intellectual Migration: Europe and American, 1930—1960* (Cambridge: Harvard University Press, 1969), p. 655.

[27] Ibid.

of science (even though it is overshadowed, Feigl says, by investigations into the epistemology of science)[28]. Likewise, Carnap's[29] famous distinction between internal questions and external questions would presumably be still another example of this third type of legitimate metaphysics.

In addition, therefore, to a *transcendent metaphysics*, which the positivists found objectionable, they were prepared to allow for the legitimacy of an *inductive metaphysics* [30] — or perhaps even a *hypothetico- deductive metaphysics* [31] — and Feigl's categorical analysis. Both of the latter „legitimate" types of metaphysics might also be called a *philosophy of nature,* i.e., an investigation into „the nature of nature". However,

what the philosophy of nature is supposed to include, and how it differs both from science and the philosophy of science are questions difficult to answer.

On the one hand, a legitimate philosophy of nature should not be what is often caricatured as 19th century *Naturphilosophie*, a scientifically irresponsible philosophical anticipation of science in which philosophers purport to advance some kind of transcendent or transcendental supra-scientific knowledge of nature. But, on the other hand, is the philosophy of nature anything more than a coherent synthesis of our best scientific theories and hence not different in principle from science itself? Is there something distinctive a philosopher can contribute towards the philosophy of nature? Is there perhaps a distinctive kind of philosophical knowledge of nature ?[35] If so, what is its nature ?

Secondly, how is a philosophy of nature different from philosophy of science? Although Compton [32] has argued that the philosophy of nature is different from the philosophy of science, his views seem to me to be questionable. For what he apparently means is that philosophy of science deals with the logic of inquiry, the structure of explanation and the analysis and clarification of concepts, whereas the philosophy of nature inquires into „what it is *as such* to be an event or an entity in nature, possessing certain qualities and enjoying relations and changes" [33]. Here Compton seems to be

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[28] H. Feigl, *Philosophy of Science,* p. 470.

[29] R. Carnap, *Empiricism, Semantics and Ontology,* [in:] *Carnap's Meaning and Necessity* (Chicago: University of Chicago Press, 1952), pp. 205—221.

[30] See also E. Harris, *The Foundations of Metaphysics of Science* (London: George Allen & Unwin, 1965), p. 29.

[31] A. N. Whitehead, *Process and Reality,* D. R. Griffin & D. W. Sherburne, eds. (New York: Free Press, 1978), p. 3. The term hypothetico-deductive as applied to Whitehead's metaphysics emerged in philosophical conversation with Don Crosby, while riding ski lifts one day. He is not to be held responsible for my misinterpretation. of Whitehead (although he is responsible for my bad skiing that day).

[325] E. Harris, op. cit., p. 169—172 apparently does not believe so, whereas John Compton *(Reinventing the Philosophy of Nature,* Review of Metaphysics, 33 [1980], pp. 2—28; *Understanding Science,* Dialectica, 16 [1963], pp. 155—176) does, with Ivor Leclerc *(The Necessity Today of the Philosophy of Nature,* Process Studies, 3 [1973], pp. 158—168) being somewhere in the middle.

[36] J. Compton, *Reinventing the Philosophy of Nature.*

[33] J. Compton, *Understanding Science, p.* 169.

adopting the old positivist's conception of the philosophy of science as the logic, methodology and epistemology of science. But on the rather unexciting claim I am advancing, philosophy of science should also include the ontology of science and hence philosophy of nature belongs to the philosophy of science.

Philosophy of nature seems to be largely concerned with what we can call *the ontology of nature* and with a synthetic view of nature rather than with an analytic one. But should philosophy of nature be seen as anything else than a correctly interpreted philosophy of science, in particular, as the metaphysics of science? According to Taylor[38], for example, philosophy of nature is a kind of cosmology — the investigation of the most general characteristics of external nature. As such it deals with the nature of matter, mechanism vs. teleology, space-time, evolution, etc. But, as I have already pointed out, such questions as these are paradigmatic examples of issues discussed in the philosophy of science and belong squarely in the positivistic tradition. In short, philosophy of science should include the metaphysics (or ontology) of science as a fundamental part and should not be construed merely as the epistemology of science.

Two other ways in which the logical positivists were involved in metaphysics, which I do not have time to discuss, include the realism vs. non-realism issue about the ontological status of theoretical entities and the distinctive positive metaphysics of positivism — the philosophy of logical atomism — underlying their entire philosophical program.

With regard to the first issue — scientific realism — it was (not surprisingly) those philosophers of science who have been most explicitly committed to realism, e.g., Feigl, Smart, Sellars, Harre, and Popper, who have advanced ontological claims about, say, materialism, whereas phe- nomenalists, instrumentalists and conventionalists have wanted to avoid all kinds of scientific metaphysics. Thus, if one thinks that science can advance something approaching the correct view of reality, (s)he will be attracted toward the position of realism about the ontological status of theoretical entities, whereas those who deny this will tend to be nonrealists. It is no accident that those who, like Duhem, want religion to give the ultimate view of reality would remove this ontological function from science, nor is it an accident that phenomenologists (such as Husserl and Heidegger), pragmatists (such as Dewey) and ordinary language philosophers (such as Ryle and Wittgenstein) have been anti-realists. For all of these thinkers, human experience (as lived through) is the ultimate criterion of what is real and scientific theories are ideal abstractions from this *Lebenswelt.* Classical positivism (e.g., Mach and Pearson) is a philosophical bedfellow with these other apparently radically different philosophical movements, but they were all united by a common rejection of scientific realism. Hence, technical issues in the philosophy of science concerning the ontological status of theoretical entities do make an important difference to one's view about „metaphysics and science".

That the underlying metaphysics of logical positivism was logical atomism and that this was a full-blown ontology is a point that hardly is new but for our purposes one worth reiterating [34]. This ontology was not one inspired by a careful study of science —

[34] The most consistent and relentless contemporary defense of logical atomism is Gustav Bergmann. See his *The Metaphysics of Logical Positivism,* 2nd ed. (Madison: University of

as one might expect — but rather was *a priori* in nature, motivated by purely logico-philosophi- cal considerations. Indeed it seems undeniable that logical positivists must have done some serious compartmentalization in order to adhere to a logical atomism (on the one hand) while at the same time investigating the logic of field theory, relativity theory, quantum mechanics etc. in physics. Here were philosophers of science, well-acquainted with the revolutionary developments in physics from 1905 to 1927, who simply failed to incorporate any of these „new views of reality" into their metaphysics at all; instead, they continued to adhere basically to an ontology based upon Newtonian Mechanics [35]. Why? I can only suggest two reasons. One, they were (initially) anti-realists and did not believe the new sciences were giving them a new ontology, especially when it corrected their view that their experience — interpreted positivistically — was the only reality. Second, they were conventionalists about metaphysical commitments: the issue of one metaphysical view versus another, say, idealism versus realism, was seen to be cognitively meaningless and, simply put, were personal matters of taste, subjective preferences, existential commitments, pragmatic decisions etc. This view emerges most clearly in Rudolph Carnap's „Empiricism, Semantics and Ontology" but is even present in his earlier work *The Logical Structure of the World* [il]. Carnap's philosophy of science has always had a strong commitment to conventionalism — a point still not sufficiently appreciated in contemporary discussions of logical positivism.

After having said that the logical positivists did give some role to metaphysics in the philosophy of science, I have to add quickly that I do not think this ever equalled the importance they attached to the epistemology of science and that, consequently, the place of a metaphysics of science was underestimated. This situation has largely continued throughout the philosophy of science, say, from 1950 to the present. Today, philosophy of science is still basically concerned with the logic, methodology and epistemology of science. True, there are numerous discussions of realism vs. anti-realism, but this is basically seen as an epistemological issue. There are also numerous discussions of space, time, matter, causa- lity, etc. But what seems lacking are discussions about the implications of these concepts, as contained in theories of physics, for our view of the world. This, in turn, is due largely to the fact that, typically, a single isolated concept, say, matter, is thoroughly discussed but there is a conspicuous absence of discussions about its relevance for more global questions in which an entire metaphysical view of nature is articulated under a single motif and all the various elements integrated into a coherent and unified whole. There is, in short (to use Broad's distinction) plenty of discussion of „critical" or analytic metaphysics but virtually none of speculative or synthetic metaphysics. That is precisely what is missing from the

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Wisconsin Press, 1967). On the connection between logical atomism and philosophy of science see his *Philosophy of Science* (Madison: University of Wisconsin Press, 1957).

[35] This is clearest in the case of Bergmann, who quite clearly and explicitly adopts Newtonian mechanics as his model of what science should be and constructs his philosophy of science to limn its underlying features. See his *Philosophy of Science*, Ch. 1. In this regard, contrast the work of Wolfgang Koehler, who created a Gestalt philosophy of science explicitly based upon field theory in physics. See his *Die physikalischen Gestalten in Ruhe und in stationaeren Zustand* (Erlangen: Weltkreis, 1920).

pages of „Philosophy of Science" and „British Journal for the Philosophy of Science" and that is precisely what is, by contrast, offered in a slate of recent books on „the metaphysics of contemporary physics" [36]. Insofar as logical positivism (and the type of contemporary philosophy of science inspired by it) does own up to a metaphysics, it is an *analytic metaphysics* as opposed to a *synthetic metaphysics.* As one person recently characterizes the former: „The basic idea of analytical metaphysics is that the world can be understood by breaking it down into its most fundamental parts, or „constituents" [37].

Thus, what I am suggesting is that it is not sufficient for a critical philosophy of science merely to allow a place for metaphysics in the philosophy of science; instead a more proper conception of the role of metaphysics must be sufficiently broad to include synthetic metaphysics as well as analytic metaphysics. Up to now, this has been missing form the mainstream of philosophy of science but it is a direction in which a more critical science of philosophy ought to go.

Ethics, insofar as it is interpreted in a non-naturalistic way, is cognitively meaningless according to the logical positivists. Ethical assertions make no claims to be true, they make no claims about what is the case, they are not rationally justifiable. On all these counts they are opposed to factual statements. Facts and values are thus radically different for the positivist and one ought never to commit the naturalistic fallacy of moving from an 'is' to an 'ought'.

On the „standard" positivist conception of ethics — the „emotivist", „imperative" (or what Carnap preferred to call the optative approach [38]) — ethical claims (beliefs) were really not beliefs at all but attitudes. Basically they were seen as expressions of existential commitments to a *Le- benseinstellung,* to a rule or a way of life. One could, of course, champion a way of life, but one should not be misled about its nature: at rock bottom these ethical stements were matters of personal taste not subject to rational justification; furthermore, since they had no epistemic standing and since facts and norms are categorically distinct, facts could not be relevant to their epistemic standing. Although Carnap never explicitly noted this, his metaphysical outlook embodied in his classic paper, „Empiricism, Semantics and Ontology", has considerable bearing on his theory of values.

Internal and external questions differ radically for Carnap. Internal questions can only be answered within the respective linguistic framework — either by empirical or logical means. External questions, which are questions about the linguistic framework as a whole, can neither be answered from within a system nor, in fact, answered at all. For such questions do not call for an answer to a theoretical question so much as a practical decision whether to adopt it or not. External questions thus ultimately involve matters of choice and appear to be indistinguishable from existential commitments. Insofar as they are not beliefs but rather attitudes, they appear to be precisely like value

[36] Perhaps the most widely read of these books is Fritjof Capra's *The Tao of Physics* (New York: Bantam, 1976), with a close second being Gary Kukav's *The Dancing Wu-Li Masters* (New York: Bantam, 1980). There are a score of other books devoted to similar themes.

[37] L. L. Blackman, *Introduction,* [in:] *Classics of Analytical Metaphysics* (Lanham: MD: University Press of America, 1984), p. xiii.

[38] Whereas Carnap endorsed a version of the emotive theory, H. Reichenbach

judgments in general. They are, Carnap claims, non-cognitive[45.]

Can these framework decisions have any kind of rational justification, e.g., a pragmatic justification or what Feigl calls a 'vindication'? That is to say, can Carnap be interpreted as claiming that although these are practical matters of choice they are nonetheless subject to some kind of rational constraint? For after all (we are inclined to say) some linguistic frameworks are better suited than others for achieving certain kinds of goals.

Here Carnap is uncharacteristically obscure. On the one hand, he admits that „theoretical knowledge" will influence these non-cognitive decisions. Thus, given that language is intended to be used for communi- [39] cation of factual knowledge, then "[t]he efficiency, fruitfulness, and simplicity of the use of the thing language may be among the decisive factors" [40]. The acceptance of a framework cannot be judged as being either true or false, according to Carnap, because it is not an assertion at all: „[i]t can only be judged as being more or less expedient, fruitful, conductive to the aim for which language is intended" [41]. It thus appears that alternative linguistic frameworks are to be tested by virtue of „their success or failure in practical use". Such testing, Carnap assures us, it essential for scientific progress and those with no useful function will sooner or later be eliminated [4S].

It thus appears that Carnap is offering us the kind of pragmatic justification, which Feigl calls a vindication[42], and hence there is a rational basis for a framework decision, one rooted in pragmatic success. This is really an illusion, however, for questions about pragmatic usefulness (means-end justifications) are empirical questions about what actually results in what; these facts may *motivate* decisions, Carnap claims, but they cannot rationally justify them.

> The thing language in the customary form works indeed with a high degree of efficiency for most purposes of everyday life. This is a matter of *fact*, based upon the content of our experiences. However, it would be wrong to describe this situation by saying: „The fact of the efficiency of the thing language is confirming evidence for the reality of the thing world" [43].

(The Rise of Scientific Philosophy [Berkeley: University of California Press, 1951], pp. 276—302) adopted an imperative interpretation. Herbert Feigl, by contrast, although flirting somewhat with these non-cognitivist interpretations, came to a basically cognitive interpretation of ethical statements. See his *Validation and Vindication: an Analysis of the Nature and the Limits of Ethical Arguments,* [in:] *Readings in Ethical Theory,* W. Sellars & J. Hospers, eds. (New York; Appleton-Century-Crofts, 1952), pp. 667—680 and *De Gustibus Non Disputandum...,* [in:] *Philosophical Analysis,* M. Black, ed. (Englewood Cliffs, NJ: Prentice Hall, 1950), pp. 113—147. Feigl's approach had a strong influence on P. Taylor, *Normative Discourse* (Englewood Cliffs, NJ: Prentice-Hall, 1961).

[45] R. Carnap, op. cit., p. 208, 215.

[46] Ibid., p. 208.

[41] Ibid., p. 214.

[42] H. Feigl, De *Gustibus Non Disputandum...?*

[50] R. Carnap, op. cit., p. 208, my emphasis. Carnap goes on to say (p. 208): „we should rather say instead: 'This fact makes it advisable to accept the thing language'." But, given what he apparently means — that pragmatic considerations causally motivate but don't rationally justify — this is precisely what he cannot say since facts cannot make anything advisable.

„Judgments of this kind [Carnap says] supply the motivation for the decisions of accepting or rejecting the kind of entities" [51].

It begins to appear as if Carnap is here advancing views strongly resembling those of Thomas Kuhn [44], concerning the employment of criteria for theory choice. Kuhn's ideas have been violently attacked by numerous philosopher of science upholding the banners of rationality and objectivity. But it seems difficult to avoid the conclusion that Carnap is not only a conventionalist about theory choice but also a relativist, irrationalist and subjectivist.

In Carnap's only major discussion of ethics *per se* [45], the connection between ethical attitudes and external questions is once again not explicitly discussed. Many of his comments, however, are relevant to this question. Carnap's theory of optatives, as he calls it, contains two key theses. (1) First, *the thesis of non-cognitivism:* If a statement on values or valuations is interpreted neither as factual nor as analytic, then it is non-cognitive [46]. (2) Second, *the thesis of pure optatives*: there are pure optatives, i.e., there are statements with pure optative meaning[47] logically implying no factual claims. It follows that pure optatives are clearly noncognitive. In a philosophical analysis of a value assertion, it will often turn out, Carnap claims, that there is a pure optative mixed with impure optatives and pure factual statements. But Carnap's point is that all truly ethical statements are analyzable as pure optatives (and hence without any factual consequences) and that impure optatives will contain both a factual component and a pure optative. Ethical statements, like commitments to linguistic framework, are attitudes, preferences, choices or wishes and are noncognitive. Like external questions, they call for *a* commitment, an existential leap of faith. Commitments to a framework, we will recall, cannot be rationally justified but can be causally motivated by „reasons". Presumably, therefore, the same would apply to pure optatives, i.e., one can give no reasons for them, but one can cite their causes. On this point Carnap is obscure, for (unlike his earlier discussion) he now seems to admit that there are reasons and not merely causes for an ethical attitude [48], and hence that the latter can be rationally justified. When someone advances a reason (i.e., a belief) for an attitude, (s)he is claiming the former is a valid reason for the latter, and we must investigate „whether his belief was obtained in a rational way, i.e., whether it is supported by the evidence available to [him], and... whether the belief constitutes a rational reason for the preference" [49]. In contradistinction to his earlier claims about external questions being

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[44] Th. Kuhn, *Objectivity, Value Judgments and Theory Choice,* [in:] *The Essen tial Tension* (Chicago: University of Chicago Press, 1977), pp. 320—330.

[53] R. Carnap, *Replies and Systematic Expositions,* [in:] *The Philosophy of Rudolph Carnap,* P. Schlipp, ed. (LaSalle, IL: Open Court, 1963), pp. 999—1016: „Value Judgments". For an incisive critical evaluation of Carnap's (and Reichenbach's) theory of ethics, see A. Kaplan, *Logical Empiricism and Value Judgments,* [in:] *The Philosophy of Rudolph Carnap,* pp. 827—858.

[54] R. Carnap, op. cit., p. 999.

[47] The optative meaning of a statement is that kind of meaning common to wishes, proposals, requests, demands, commands, prohibitions, preferences, etc.

[56] Ibid, p. 1006.

[57] Ibid., p. 1006.

noncognitive and in contradistinction to his claim that ethical statements are non-cognitive, one can easily interpret his above remarks in such a manner that he appears to be claiming that there are (or can be) reasons for an attitude, i.e., as claiming not only that there are causes of attitudes but also (sometimes at least) reasons. In this case (presumably), the attitude would be justified and hence, we might naturally suppose, not cognitively meaningless.

Unfortunately, this is not the case, for although Carnap admits there can be reasons for an attitude, this not sufficient to make the attitude cognitive, for any evidence or reasons on the basis of which an attitude is formed is not part of the *meaning* of the statement. On the contrary, the meaning of a statement consists of the factual implications derived or derivable from the statement: „My thesis of pure optative says merely that there are optatives which do not logically imply any factual statements; the thesis does not say anything about the *reasons* for the attitude expressed in the optative" [550]. Since an ethical statement has no subsequent factual implications, he claims, it has no cognitive meaning and is purely optative. Carnap is prepared to admit that particular facts may be a reason for one's ethical „beliefs" and that observational data, in turn, may be the reason for one's belief in these prior facts, even though not implied by these beliefs. Such observational data would constitute evidence on which a person's factual belief is founded. Carnap then goes on to say: „...the observational evidence which a person may or may not have for his belief in a statement describing a physical situation, is not part of the meaning of this statement"[51]. Thus, because ethical attitudes logically imply no observational data, they have no meaning, *even though there may be reasons for the ethical attitude.* The meaning of a proposition apparently consists only of the factual propositions it implies and not in any prior evidence in its favor. In short, there may be reasons — even good reasons — for an ethical attitude but it is nonetheless cognitively meaningless! Carnap's theory of ethics, as a metaethical theory, thus appears to be a non-cognitivist one about meaning but a cognitivist one about the logic of moral reasoning.

Carnap's treatment of ethics is puzzling in still another respect. In an earlier discussion of his views about internal and external questions, I pointed out (without exploring the point) that his views about how external questions are answered bears a strong resemblance to those of Thomas Kuhn's, namely that commitments to certain kinds of scientific theories, frameworks or norms appear to be basically existential commitments lacking any rational basis. In the present context Carnap appears to be saying that such commitments *can* have a rational basis and hence can be justified (although still cognitively meaningless). This suggests, contra his earlier discussion, that external questions — at least ethical ones — can be rationally based. But Carnap proceeds to undercut this interpretation by adopting a position, once more, strongly resembling a relativistic one. Suppose, for example, two individuals A and B disagree about certain ethical norms (or certain scientific theories or „paradigms"). It is still logically possible (Carnap suggests) for A and B to agree in all beliefs, for their reasoning to be in perfect accord with deductive and inductive canons, and yet for them to differ in their optative

[50] Ibid., p. 1007.
[59] Ibid.

attitudes.

> ...both may have exactly the same relevant evidence, apply the same valid inductive method, and thus come to exactly the same degree of credence for all relevant propositions... The difference between A and B in their decisions... is based, in this case, not on a difference in their theoretical thinking but rather on a difference in their preferences... and finally on a difference in character [6o].

Thus, decisions (preferences) are underdetermined by all the rational evidence. If we recall Kuhn's similar views about value judgments in science, we an immediately see that Carnap's position seems to be indistinguishable from it. 'All we need do is to apply the same analysis, *mutatis mutandis*, to theory choice in science to see what Carnap's view must be: two scientists can agree about all the facts and reason impeccably and yet prefer different theories. Hence Carnap appears to be a kind of *fideist* about theory choice in science. Once more, it appears that theory choice in science (as in ethics) must be matters of personal taste — „de gustibus non disputandum".

If scientists *qua* scientists make value judgments (epistemic or non- epistemic), then such judgments fail to have cognitive meaning and are matters of personal commitment. If, as I have suggested, such a view seems to follow from Carnap's reconstructed theory of values, then one might well understand why the positivists would have spent so little time on the role of ethics and norms in science. For it seems to be clear that, on this meta-normative theory, science would fail to command the kind of allegiance the positivists thought it should (since science would have nothing to say about ethics). Naturally, they would have kept quiet about this aspect of their philosophy of science — or even have repressed it — since it would have endangered their entire philosophical program. It is not surprising, therefore, that positivists systematically failed to discuss „values and science" and failed to include axiology as a branch of the philosophy of science.

I think it is arguable that the situation is not much better today and the reason for it is that most philosophers of science continue to have [52] a non-cognitivist view of ethics. One indication of this is the appalling lack of discussion of this very issue — values and science. For example, if one consults the „British Journal for the Philosophy of Science", one will find only one general discussion of this issue in 35 years! If one peruses the pages of „Philosophy of Science", the situation is not much better: in the last 15 years, there is again only one general discussion (although other articles have appeared in earlier issues). Two articles in 50 years of combined publication is hardly an impressive record, especially since it is now conceded that „values and science" is a crucial philosophical topic.

This entire situation needs to be redressed. We must sever the ties that binds non-cognitivist ethics and philosophy of science. The philosophy of science must include, as an essential part, the axiology of science.

The epistemic values of science are those values related to the acquisition of (theoretical) knowledge and concern questions of evidence and truth, e.g., simplicity, falsifiability, objectivity, replicability, testability, theoretical coherence, plausibility,

[52] Ibid., p. 1009.

power, curve-fitting, alpha level of significance, etc. In addition to epistemic, moral and aesthetic norms associated with theoretical science, the axiology of science should include normative issues as these relate to scientific experimentation and observation (e.g., the treatment of human and animal subjects under experimental conditions) and norms as they relate to applied science and technology (e.g., recombinant DNA, nuclear energy). But, as I have already suggested, to deal adequately with the ethics of science, one must also deal with the hard realities of actual scientific practice. It is clear, for example, that cheating is much more widespread in science than most people — especially scientists — believe it is or will admit. But it does no good to claim scientists ought not to do this if the social institution of science is such that cheating, in some form, seems unavoidable.

Up to now, I have been sugesting that the philosophy of science should include the epistemology, metaphysics and axiology of science. I have also suggested that such a philosophy of science cannot be sharply separated from science (on the one hand) nor from the history, psychology and sociology of science (on the other). In conclusion, I want to add the following qualification to my conception of the nature and scope of the philosophy of science: it should be a *critical* philosophy of science.

Up to now, philosophers of science have often been guilty of a version *ot scientism,* which (broadly conceived) can be characterized as an uncritical dogmatic worship of science, its methods and results, and a faith in its ability to solve all of our pressing problems. At the very least, it is

the view that the scientific way of knowing is the only way, that what is ultimately real is what is contained in our scientific world view, and that what is valuable and how we ought to live is what the latest scientific theory says. I am not saying these views are in fact wrong — although they may well be — I am only saying that there is a need to critically evaluate whether they are true or not. The one thing recent philosophers of science such as Popper and Feyerabend have taught us is the need for a critical stance and this is best served by a theoretical pluralism. In the context of the philosophy of science, this can be put by *saying we need competing philosophies of science* that engage each other in confrontation; we cannot continue to have the present situation of a monolithic analytic philosophy of science underlying the outlook of professional philosophers of science.

If one is going to be critical of science — in the sense of performing a. *critique* of it — one will not simply *describe* what it is, what its epistemology, ontology and ethics are — although this will be an important part. But neither will one *prescribe* — in a philosophically *a priori* or foundationa- list way — what it ought to be (although there will be something of a prescriptive element to it). Either extreme should be rejected as inadequate. I would favor an approach that combines features of both in such a way that ones philosophical „analyses" or accounts would be tested against science as it really is. How is it to be done is something I cannot go into here.

What I imagine such a „critique" and a critical philosophy of science to be would be one characterized as an *internal* critique of science, not an *external* one. It would have as one of its goals the *improvement* of science as a result of pointing out some of its current shortcomings. But it would also unmask certain inflated pretensions of science, ones encapsulated in what Bernie Rollin calls the *ideology* of science[53]. As recent philosophers such as Habermas have suggested, one of the tasks of critical approach to science is *ideology Critique,* and this would certainly fall within the present conception of the philosophy of science.

Perhaps the best overall summary of what a critical approach to the philosophy of science would include comes from a recent book on Heidegger's philosophy of science [54]. To ask the critical question, one might say (adapting some of Kockelmans' remarks) in:

> ...to ask the question of precisely what science is, how it is related to all the other orientations of man toward the world, what its prospects and what its limits

[53] See his forthcoming book *Animal Consciousness, Animal Pain, and Scientific Change* (Oxford: Oxford University Press, 1989).

[54] J. J. Kockelmans, *Heidegger and Science* (Washington, D.C.: Center for Advanced Research in Phenomenology and the University Press of America, 1985).

are, what kinds of contributions the sciences can make to meaningful discourse about religious, moral, aesthetic, social, political, and educational issues, and what are the areas in which, in this regard, one may not expect a positive contribution from the sciences, simply because of the fact that one appears to run into issues which lie far beyond the competence of the scientific method[55].

One can hardly find a better characterization of what I take a critical philosophy of science to be.

[5] Cf. E. McMullin's *(Recent Work in the Philosophy of Science,* New Scholasticism, XL [1966], p. 509) remark: „Seldom has a philosophical subject [philosophy of science] been as closely identified with a language [English] and a geographic location [Anglo-American countries] as philosophy of science has become in this century".

[6] It should be pointed out that this picture has changed somewhat in recent years and that one can now find discussions of phenomenology and the philosophy of science in at least one of these journals (Heelan, 1987).

[11] For a discussion of „logistic" philosophy of science see Ernan McMullin, *The Ambiguity of 'Historicism',* [in:] *Current Research in Philosophy of Science,* Peter D. Asquith and Henry E. Kyburg Jr., eds. (East Lausimg, MI: Philosophy of Science Association, 1979). ce, 1 (1934), pp. 5—19.

[17] H. Reichenbach, *Experience and Prediction* (Chicago: University of Chicago Press, 1938), p. 6.

[27] R. Carnap, op. cit., pp. 76—77. Other positivists shared this view. See M. Schlick, *Positivism and Realism,* [in:] *Logical Positivism,* A. J. Ayer, ed. (Glencoe, IL: Free Press, 1959), pp. 83—102; A. J. Ayer, *Demonstration of the Impossibility of*

[38] A. E. Taylor, *Elements of Metaphysics* (New York Barnes & Noble, 1903).

[41] R. Carnap, *The Logical Structure of the World and Pseudoproblems in Philosophy,* Rolf A. George, trans. (Berkeley: University of California Press, 1967). [Originally published in 1928].

[48] Ibid, p. 221.

[51] R. Carnap, op. cit., p. 214.

[55] Ibid., p. 3.