How (Not) to Write the History of Pragmatist Philosophy of Science?

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This survey article discusses the pragmatist tradition in twentieth century philosophy of science. Pragmatism, originating with Charles Peirce’s writings on the pragmatic maxim in the 1870s, is a background both for scientific realism and, via the views of William James and John Dewey, for the relativist and/or constructivist forms of neopragmatism that have often been seen as challenging the very ideas of scientific rationality and objectivity. The paper shows how the issue of realism arises in pragmatist philosophy of science and how some pragmatists, classical and modern, have attempted to deal with it. Various dimensions of the realism dispute are thus discussed, especially realism as contrasted to instrumentalism and realism as contrasted to relativism/constructivism. It is argued that the pragmatist tradition cannot avoid these tensions but is largely constituted by them.

1. Introduction
This paper discusses the possibility of identifying a distinctive tradition of pragmatist philosophy of science, thereby providing a survey of the ideas and problems constitutive of such a tradition. The task is challenging, because pragmatism, originating with Charles S. Peirce’s writings on the pragmatic maxim as a logical method of “making our ideas clear” in the 1870s, is a background both for Peircean forms of scientific realism and related dynamic theories of scientific progress (e.g., Isaac Levi, Ilkka Niiniluoto) and, via the pragmatist views of William James and John Dewey, for the more relativist and/or constructivist forms of neopragmatism (e.g., Richard Rorty, Hilary Putnam) that have sometimes been seen as abandoning the very ideas of scientific rationality and objectivity. In contrast to realism, these constructivist neopragmatists claim that scientific objects, or perhaps reality in general, are not “ready-made” or mind-
theory-independent but constructions based on our various scientific (and other) perspectives and social practices. The continuing debate over scientific realism and truth is, hence, crucial in this tradition—if, indeed, such a tradition is usefully identifiable at all—and it is only from the perspective of this debate, in some of its key dimensions, that I will here try to survey the history of pragmatist philosophy of science. Moreover, the simplified picture of there being two basically different pragmatisms, realistic and relativistic, or objective and subjective (cf., e.g., Haack 1998, 2004; Rescher 2000, 2005), ought to be enriched by a more nuanced historical narrative.

In addition to the realism vs. relativism (or realism vs. constructivism) controversy, the history of pragmatism in the philosophy of science can also be approached from the perspective of another opposition, viz., the one between scientific realism and instrumentalism. While James, as we will see, clearly maintained an instrumentalist interpretation of scientific theories and Dewey explicitly labeled his view “instrumentalism,” though in a sense broader than the one in which the term is used today, it is also simplistic to identify pragmatism with instrumentalism, if the latter is understood as a denial of scientific realism (that is, as the claim that theories lack truthvalues, being mere instruments for prediction and control of observable phenomena). Karl Popper, for example, characteristically ignored pragmatism, equating it with such antirealist instrumentalism, as well as with the somewhat vague idea that knowledge or truth per se is not valuable, but only practically “useful” knowledge is worth striving for—and many have followed him in adopting this negative attitude to pragmatism. Given Popper’s status as a towering figure among twentieth-century philosophers of science, and the simultaneous rise of scientific realism, such accusations cannot have failed to affect the credentials of

1. See, e.g., Popper 1972 [1963], 99, 223, 382; and 1994, 118, 154, 171–174. Because of his evolutionary epistemology, Popper may, however, have been unable to avoid some sort of pragmatist commitments (see Campbell 1974, especially 447 ff.). Lakatos 1970 was as critical as Popper: according to him, James denied “the possibility of any kind of objective knowledge” (99), and pragmatism is, in James as well as in Quine, a philosophy of mere “psychological comfort” (185). Even in contemporary discussion, pragmatism is sometimes equated with instrumentalism and/or constructive empiricism (and thus with one or another form of empiricist antirealism): see, e.g., Leplin’s 1997, 132 ff. way of contrasting the pragmatic virtues of theories with the realist’s key virtue, truth. Niiniluoto 1999 is more careful, but as a proponent of scientific realism, he does count pragmatism—at least in its typical versions—as one of the positions he opposes, if not as a downright instrumentalism, at least as an attempt to replace the correspondence theory of truth (which Popper also favored) by an epistemic “surrogate” (see 11–12, 101–103).

2. Niiniluoto’s 1999 book is one of the best examinations of both historical and systematic issues related to scientific realism.
virtually all forms of pragmatism in this field. However, pragmatism and instrumentalism, though distinguishable, do meet in Rudolf Carnap’s (1950) logical empiricism, which was “pragmatist” in the sense of treating the “external questions” about the choice of a linguistic framework as only pragmatically decidable, avoiding “metaphysical” postulations of entities.

A major event in the history of twentieth century pragmatist philosophy of science was W. V. Quine’s attack on the “dogmas of empiricism” in 1951, yielding (in Quine’s own words) “a more thorough pragmatism” than Carnap’s (and C. I. Lewis’s), as well as a “shift toward pragmatism” within a more holistic, undogmatic empiricism (see Quine 1980 [1953], 20, 46). However, as several neopragmatists and scholars of classical pragmatism have argued, Quine’s views, while grounding an influential synthesis of logical empiricism and American pragmatism, and thereby playing a crucial role in the emergence of the later “post-positivist” consensus in the philosophy of science, were actually quite far from classical pragmatism. Quine, just like the logical empiricists, philosophized about science “from a logical point of view,” drawing insufficient attention to actual scientific practices and their historical development. More genuinely pragmatist perspectives on science, often without explicit commitments to pragmatism, have emerged in, e.g., Putnam’s, Thomas Kuhn’s, Larry Laudan’s, and (more recently) Joseph Rouse’s work, as well as in the interdisciplinary discussions within science and technology studies, often examining concrete cases of scientific practice.

This, roughly, is the story I will tell in what follows. This vast amount of material cannot, obviously, be extensively reviewed in a single essay. I will try to show, through a selective discussion focusing on some of the key figures of pragmatist philosophy of science (old and new), how the issues of realism vs. relativism (constructivism) and realism vs. instrumentalism have emerged in the pragmatist tradition, and I will briefly indicate how a synthesis, “pragmatic realism,” can be (and has been) defended. I will try to demonstrate that, and how, pragmatism can be viewed as a tradition (with a relatively loose identity) whose viability partly results from its open boundaries and the frequent recurring of questions about whom to include in the tradition, and on the basis of which criteria. I will keep my discussion as tightly focused on the philosophy of science as possible, although the boundaries between the subdisciplines of philosophy are often difficult to draw sharply—especially if one has pragmatist sympathies.

We should start with a discussion of the reasons why the pragmatist

tradition has so easily been saddled with instrumentalist interpretations, at least among realist philosophers of science.

2. Instrumentalism and operationalism in James and Dewey

2.1. Peirce’s influence on later philosophy of science was, though slow, eventually so enormous that I must largely set him aside here, in order to keep my discussion within a manageable size. In any case, it is not particularly problematic, in Peirce’s case, what his role in the pragmatist tradition is. Quite clearly, without him, there would be no pragmatism at all. On the other hand, so many theorists of scientific inference, especially abduction, have drawn influence from Peirce that it would be too open-minded to include them all within pragmatism. Peircean insights in logic, semiotics, and the theory of inference can be utilized quite independently of his pragmatism. Yet, obviously the tension between scientific realism and constructivism or idealism (though, in Peirce’s case, not relativism) is already present in Peirce’s early writings on the pragmatic method, particularly in his application of this method of “making our ideas clear” to the concept of reality, which is explained as the object of the “final opinion” of the scientific community of researchers employing the scientific method. It is left unclear which comes first: the mind- and research-independent reality itself or the process of fixing the final opinion? This is, essentially, the tension between realism and idealism. The other key tension in the realism discussion, the one between scientific realism and instrumentalism, can also be traced back to Peirce, because his pragmatic conception of meaning influenced some later verificationist and logically empiricist theories, although the proponents of those theories seldom realized that Peirce was not only a logician and a philosopher of science but also a speculative metaphysician, contrary to the spirit of the Vienna Circle.

4. Peirce figures largely in, e.g., Burks’s 1977 [1963] pragmatic theory of probability (see especially 165–178), though Burks finds Peirce’s pragmatic principle of meaning “essentially the same” as the logical empiricists’ operationalism and verifiability principle of meaning (167), complaining (!) that pragmatism fails to reduce general theoretical statements to statements directly verifiable by observation (174). Given the later developments in pragmatism, it is ironic that anything like this has ever been seen as something that pragmatism ought to achieve. Suppe 1989, 112, referring to Burks, also claims Peirce to have anticipated Bridgman.

5. I am, of course, referring to Peirce’s famous essays, “The Fixation of Belief” (1877) and “How to Make Our Ideas Clear” (1878), which can be found, e.g., in Peirce 1931–58, vol. 5, or Peirce 1992–98, vol. 1. On the realism vs. idealism tension in Peirce, see Pihlström 2004.

6. Nagel 1961, 129 cites both Peirce and Dewey as advocates of the what he calls the
Despite these interpretive problems, Peirce, the founder of pragmatism, was a precursor of scientific realism—particularly of the idea that the advancement of science can be understood as a progression toward the truth, or as an increase of the truthlikeness of theories. His concept of abduction has also been employed in the debate over whether realism can be defended as the best (or only) explanation of the empirical success of science. Thus, in an important sense, Peirce is a classic of what is today known as scientific realism. The same cannot, however, be said about his friend William James. On the contrary, it is easy to see James as a precursor of instrumentalism, one of the major orientations opposed to realism in twentieth century philosophy of science. Thus, when we move from Peirce to James and Dewey, the tensions centering around the issue of scientific realism become deeper and more essential. In James’s main works, we find, for instance, the following somewhat overlapping remarks:

[A]s the sciences have developed farther, the notion has gained ground that most, perhaps all, of our laws are only approximations. The laws themselves, moreover, have grown so numerous that there is no counting them; and so many rival formulations are proposed in all the branches of science that investigators have become accustomed to the notion that no theory is absolutely a transcript of reality, but that any one of them may from some point of view be useful. Their great use is to summarize old facts and to lead to new

“instrumental view of theories.” For a sympathetic discussion of Peirce’s place in the history of veriﬁcationism, see Misak 1995. See also note 4.

7. See Niiniluoto 1999, as well as Levi 1991. Recent theorists of truthlikeness, such as Niiniluoto, need not share Peirce’s conception of truth as the ideal limit of inquiry, or the “ﬁnal opinion” of the scientiﬁc community, but the basic idea—the gradual approximation of truth through a succession of strictly speaking false theories by means of an investigation employing scientiﬁc methods—is closely similar.

8. See, for variations of this idea, some of the papers in Leplin 1984, as well as Leplin 1997 and Niiniluoto 1999; for criticism of this “explanationist” line of defending realism, see Fine 1996 [1986]. de Regt 1999, 376–379 notes, however, that the concept of abduction employed in this discussion is not Peirce’s, who eventually came to view abduction as “the First Stage of Inquiry” instead of an explanatory inference. de Regt (ibid.) also interestingly considers Peirce’s extreme scholastic realism about “real generals” as a form of scientific realism (see also Haack 1998, Pihlström 2003), and ﬁnds Peirce’s views less relevant to the contemporary challenges to realism based on the underdetermination of theory by data than has sometimes been supposed (de Regt 1999, 385–391). For earlier, still inﬂuential discussions of Peirce’s relation to scientiﬁc realism, see Almeder 1980, 1983, 1989 and Skagesstad 1981; for discussions of Peirce as a precursor of Popper’s scientiﬁc realism and pursuit of scientiﬁc objectivity, see Freeman and Skolimowski 1974 and Niiniluoto 1984.
ones. They are only a man-made language, a conceptual shorthand [...], in which we write our reports of nature [...]. (James 1975 [1907], 33.)

[W]e are witnessing a curious reversion of the common-sense way of looking at physical nature, in the philosophy of science favored by such men as Mach, Ostwald and Duhem. According to these teachers no hypothesis is truer than any other in the sense of being a more literal copy of reality. They are all but ways of talking on our part, to be compared solely from the point of view of their use. The only literally true thing is reality; and the only reality we know is, for these logicians, sensible reality, the flux of our sensations and emotions as they pass. (Ibid., 93.)

There are so many geometries, so many logics, so many physical and chemical hypotheses, so many classifications, each one of them good for so much and yet not good for everything, that the notion that even the truest formula may be a human device and not a literal transcript has dawned upon us. We hear scientific laws now treated as so much ‘conceptual shorthand,’ true so far as they are useful but no farther. Our mind has become tolerant of symbol instead of reproduction, of approximation instead of exactness, of plasticity instead of rigor. (James 1975 [1909], 40.)

Theories, then, are for James essentially “instruments, not answers to enigmas” (James 1975 [1907], 32; original emphasis). They enable us to cope with the world we live in; their purpose is not to “copy” the facts, or the world. Their “truth” lies, pragmatically, in their usefulness. Scientific ideas, in particular, “agree” with their real objects only in the more or less operationalist sense of a practical process of conduction leading from a present idea to a future one: “The term ‘energy’ doesn’t even pretend to stand for anything ‘objective.’ It is only a way of measuring the surface of phenomena so as to string their changes on a simple formula.” (Ibid., 103.)

James does talk about “approximation,” but he does not seem to have in mind the scientific realists’ view of the approximation of truth by means of successive theories increasing in verisimilitude (cf. Niiniluoto 1984, 1999). Rather, contrasting approximation with truth as a “literal copy,” he rejects the realist idea that our theories could ever represent the way things mind- and theory-independently are. James’s empiricism,

9. The purpose of this paper is not, of course, to evaluate James’s notorious “pragmatic theory of truth” (see James 1975 [1907], chap. 6) in any general way. For some discussion and references, see Pihlström 1996, 1998, and 2004.
influenced by Mach, Ostwald, Duhem, Poincaré, and others, thus led him at least close to what later became known as instrumentalism. Another major factor that led James to pursue these ideas was undoubtedly his need to acknowledge both science and religion as equally legitimate, and experientially anchored, perspectives on reality, or practices of coping with the world.

However, immediately after the second of the above-quoted passages, James points out that the view he discusses “seems to be too economical to be all-sufficient,” because “[p]rofusion, not economy, may after all be reality’s key-note” (James 1975 [1907], 93). He also rejects Berkeleyan phenomenalism on pragmatic grounds, emphasizing that “the category of trans-perceptual reality is now one of the foundations of our life” (James 1975 [1909], 43). Hence, in James we can observe, instead of a clear acceptance of instrumentalism, a tension between instrumentalist and realist ideas, pretty much as we find in his works a tension between idealist and realist viewpoints, and between scientific and religious perspectives on reality.

2.2.
Something similar seems to be true about Dewey’s pragmatic naturalism, as articulated in major works such as Experience and Nature (1989 [1929]), The Quest for Certainty (1960 [1929]), and Reconstruction in Philosophy (1957 [1948]), often labeled “instrumentalism,” “experimentalism,” or “operational thinking.” This is no place to describe even in general terms what Dewey’s views were like. It should be perceived that his position has clearly instrumentalist and more generally empiricist elements, although his talk about instrumentalism should not be simply equated with later philosophers’ of science narrower treatment of the empiricist doctrine carrying the same title. In any case, it is evident that Dewey is not innocent for the subsequent association, by Popper and others, of pragmatism and instrumentalism (in the narrower sense). When, for instance, Percy Bridgman (1960 [1927], 5, 31–32) speaks about “operational thinking” and suggests, notoriously, that the meaning of theoretical concepts is to be defined with reference to practical operations in the laboratory, one cannot help noticing a clear similarity to Dewey’s insistence—roughly at the

10. See the editors’ notes to James 1975 [1907], 153–154.
same time—on practice, operationality, and the instrumental use of theories.\textsuperscript{12}

Dewey (1960 [1929], 79) celebrates the “overtly executed operations of interaction” that are needed for obtaining scientific knowledge, thereby rejecting the gap traditionally thought to lie between knowledge and action. This seems to lead to antirealism:

There is something both ridiculous and disconcerting in the way in which men have let themselves be imposed upon, so as to infer that scientific ways of thinking of objects give the inner reality of things, and that they put a mark of spuriousness upon all other ways of thinking of them, and of perceiving and enjoying them. It is ludicrous because these scientific conceptions, like other instruments, are hand-made by man in pursuit of realization of a certain interest—that of the maximum convertibility of every object of thought into any and every other. [. . .] [W]hen the physical sciences describe objects and the world as being such and such, it is thought that the description is of reality as it exists in itself. [. . .] However, the] business of thought is not to conform to or reproduce the characters already possessed by objects but to judge them as potentialities of what they become through an indicated operation. [. . .] To think of the world in terms of mathematical formulae of space, time and motion is not to have a picture of the independent and fixed essence of the universe. It is to describe experienceable objects as material upon which certain operations are performed. (Ibid., 135–137.)

Science, then, has no more privileged relation to the real than (some) other human practices. “There are as many conceptions of knowledge as there are distinctive operations by which problematic situations are resolved” (ibid., 221). In brief, “scientific conceptions” are not “revelations of antecedent properties of real Being and existence” but “instrumentalities which direct operations of experimental observations” (ibid., 192). Natural laws, similarly, are “intellectual instrumentalities,” “formulae for

\textsuperscript{12} One of the classical figures of logical empiricism, Carl Hempel, employs an example drawn from Dewey when discussing his famous model of deductive-nomological explanation (see Hempel 1965, 235 ff.), but fails to consider pragmatism. See, however, Hempel 1992 for a brief account of a view on scientific progress that does not invoke the concept of truth: Hempel argues that the idea of science as a quest for truth is fundamentally mistaken and that a “strictly pragmatist and non-realistic” conception of scientific inquiry replaces the mistaken goal of truth by that of “epistemically optimal worldviews” (ibid., 48–51).
the prediction of the probability of an observable occurrence,” instead of being statements about “ultimate and rigid uniformities of being” (ibid., 205–206; original emphasis). A law that was supposed to “govern phenomena” ought to be understood as “a way of transacting business effectively with concrete existences” (ibid., 207). The universality typically claimed to characterize laws and theories “is not that of inherent content fixed by God or Nature, but of range of applicability” (Dewey 1957 [1948], xv). Scientific concept(ion)s and theories are tools, “open to development through use” (ibid., 145).

Similarly, in his naturalist magnum opus, Experience and Nature, Dewey (1989 [1929], 115) equates “the proper objects of science” with “nature in its instrumental characters.” The key point is to stop treating the object of science as “complete and self-sufficient”; this will only result in an insoluble problem, viz., the question concerning the relation between the “perceptual order” and the order of “inferred and logically constructed real objects,” with two incompatible kinds of knowledge and of the objects of knowledge (ibid., 116). In this context, Dewey also reminds his potential misunderstanders that his “instrumentalism” is not a theory about personal satisfaction in knowing, but about “the proper objects of science” (ibid., 126). Precisely as such, it might, however, be regarded as straightforwardly antirealistic.

Dewey’s commitment to instrumentalism is relatively obvious when he argues that the problem of the “two tables” (the so-called “Eddington tables”) is illusory, because the table we perceive and use is “the only table,” “for it alone has both individuality of form [. . .] and also includes within itself a continuum of relations or interactions brought to a focus” (Dewey 1960 [1929], 240). Is Dewey saying, then, that the scientific table, or the “scientific image” according to which the table is not a concrete, per-

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13. Dewey here seems to allow for probabilistic laws merely.
14. For Dewey’s instrumentalism about laws, see also, e.g., Dewey 1989 [1929], 121–123. Shook 2003b, 328 connects the Deweyan view of laws as revisable “working hypotheses” with the more recent position among philosophers of science, such as Ronald Giere and Nancy Cartwright, that scientific realism need not be committed to the truth of exact laws of nature.
16. Compare: “The world as we experience it is a real world. But it is not in its primary phases a world that is known, a world that is understood, and is intellectually coherent and secure. Knowing consists of operations that give experienced objects a form in which the relations, upon which the onward course of events depends, are securely experienced. It marks a transitional redirection and rearrangement of the real. It is intermediate and instrumental [. . .].” (Dewey 1960 [1929], 295.) On the importance of distinguishing between knowledge and experience in Dewey’s naturalism, see, e.g., Hildebrand 2003.
ceived, practically used object but a collection of microphysical particles, is unreal or illusory? If so, he is subscribing to the instrumentalist position, which several philosophers of science, especially following Wilfrid Sellars (1963), have powerfully called into question (cf. Tuomela 1985, Niiniluoto 1999). Or is he, rather, subscribing to idealism, or to what would today more accurately be called constructivism, when he maintains that scientific objects are not independent of inquiry (cf. also Shook 2000)?

Already in an earlier work, Dewey (1916, 30) admits that he may sound like an idealist when he holds that thinking, which “is instrumental to a control of environment,”[17] in a way constructs its objects: “[P]rocesses of reflective inquiry play a part in shaping the objects—namely, terms and propositions—which constitute the bodies of scientific knowledge. [. . .] Insofar as it is idealistic to hold that objects of knowledge in their capacity of distinctive objects of knowledge are determined by intelligence, [this view] is idealistic. It believes that faith in the constructive, the creative, competency of intelligence was the redeeming element in historic idealisms.” (Ibid., 30–31; see also 60.) Dewey’s Hegelian background is visible here, though he immediately adds that his instrumentalism (or idealism) does not postulate an entity or substance which constitutes the world but “defines thought or intelligence by function, by work done, by consequences effected” (ibid., 31). Moreover, the ultimate justification of scientific theorization, for Dewey, seems to be humanistic rather than narrowly instrumental:

Natural science loses its divorce from humanity; it becomes itself humanistic in quality. It is something to be pursued not in a technical and specialized way for what is called truth for its own sake, but with the sense of its social bearing, its intellectual indispensable-

17. See Dewey 1916, 331, for his characterization of instrumentalism as a “logical version of pragmatism,” according to which “knowing is literally something which we do.” On this intimate relation between knowledge and action, see especially Dewey 1960 [1929], passim. Given Dewey’s lifelong effort to avoid unpragmatic separations between these two, it is odd indeed to note that a relatively early French critic and further developer of pragmatism, Gaston Bachelard, has been interpreted as criticizing James’s and Dewey’s pragmatism for a model of instrumental or practical rationality in which reasoning (intellectual work) is separated from practical work (Tiles 2005, 172). Bachelard’s antireductionist emphasis on the richness and diversity of reality, to be encountered in a variety of historically developing ways, is obviously close to pragmatism, but his view that pragmatism failed to seek a “detailed understanding of particular conditions” and discuss the use of technology in experiments (ibid., 160) is narrow, if not downright false. Dewey, in particular, devoted a life’s work to such issues. Perhaps Bachelard’s major novelty in relation to pragmatism was his insistence on the processual dynamics of science, coming close to views later known as Kuhn’s.
It is technical only in the sense that it provides the technique of social and moral engineering. (Dewey 1957 [1948], 173.)

The charges of instrumentalism and idealism have their obvious justification, but it would be overhasty to judge Dewey to be simply an antirealist in his philosophy of science. For one thing, he—just like James—uses the terminology of “objects,” “concept[ion]s,” “theories,” “hypotheses,” etc., more loosely than is customary in (analytic) philosophy of science; I have, in fact, quoted extensively from his writings partly in order to bring this fact to the fore. This habit of usage makes confusions easy. For another thing, Dewey was, perhaps primarily, a naturalist, always arguing that experience and knowledge, including the production of scientific knowledge (and its objects), are natural phenomena in a natural world in which we try to cope, to settle the problematic situations we arrive at. Whatever is natural to our practices of inquiry, of settling indeterminate situations, is to be accepted rather than treated with philosophical suspicion. A philosophical skepticism about, say, the existence of unobservable theoretical entities would have been an utterly unpragmatist and unnatural attitude, from Dewey’s point of view. Rather, we should take seriously the natural practices of inquiry we engage in. It was always the actual practice of inquiry itself that Dewey appealed to when, for instance, attacking the ancient ideal of certainty and the “spectator theory of knowledge” (Dewey 1960 [1929]). It was science as it is actually conducted that he admired when we wrote: “In science the order of fixities has already passed irretrievably into an order of connections in process.” (Dewey 1957 [1948], xl.)

We may conclude, with Steven French (1989, 293–294, 298–299), that antirealist forms of empiricism, such as the logical empiricists’ instrumentalism or Bas van Fraassen’s (1980) “constructive empiricism,” are unpragmatic views because they are, above all, forms of skepticism, blocking the road of inquiry by doubting the existence of theoretical entities—which is something not actually doubted in the course of scientific inquiry itself. This is close to saying, with Quine, that sceptical doubts, if real (and not the kind of “paper doubts” mocked by Peirce), are themselves scientific doubts. A (critically) realist view of the explanatory postulations of scientific theories is thus a necessary condition for the possibility of scientific practices as we know them, and pragmatists, if anything, ought to take scientific practices seriously. Therefore, pragmatism should acknowledge our habitual scientific practices in which unob-

18. French’s response to these scientific antirealisms draws from Peirce rather than Dewey, but I believe that an essentially similar response can be made on the basis of Dewey’s naturalism, which is as anti-skeptical as Peirce’s pragmatism.
servable theoretical entities are postulated without paying much attention to the relatively uninteresting possibility of entertaining skeptical doubts about their existence. Insofar as the classical pragmatists (particularly James and Dewey) to some extent refused to do this, their instrumentalism ought to be significantly modified by the contemporary pragmatist. 19

Our preliminary conclusion at this point is that the classical pragmatists—we are specifically speaking about James and Dewey here—were definitely not scientific realists, if scientific realism is defined as the thesis that scientific theories provide us with the only true (or truthlike) picture of reality, or that the “scientific image” is ontologically prior to the “manifest image” we are acquainted with in our ordinary experience. 20 But they were scientific realists, or at least their views were compatible with scientific realism, in the sense of the rejection of unnatural skeptical doubts about the existence of theoretical entities postulated in the actual course of scientific theorization (as long as such theorization is firmly rooted in its humanly natural practical background). Of course, all pragmatists should admit that the specific postulations of unobservable entities in (current) scientific theories may turn out to be ill-founded. But, as fallibilists, we should adopt such a critical, open attitude to any human claims and ideas whatsoever. The key pragmatist move is to liberate not only science but also scientific realism—and all other philosophical interpretations of science—from foundationalist pursuits of certainty, essences, and other remnants of “first philosophy.”

3. Quine’s “more thorough pragmatism”? 21

3.1. This is an obvious place for a turn toward Quinean naturalism, according to which philosophical prescriptions, such as the strict principles of logical empiricism, should never be prior to scientific theorization itself. There is, famously, no “first philosophy” over and above science, according

19. See, however, Shook’s 2003b version of pragmatically realist philosophy of science, which rejects an “extreme” realism about entirely unobservable entities.

20. For such a strong conception of scientific realism, see Sellars 1963 and Tuomela 1985; for some criticism, see Pihlström 1996. According to Rorty 1997, 92–93, scientific realism and religious fundamentalism are “products of the same urge,” the pursuit of an absolute conception of reality. It is this pursuit that the pragmatist who follows James’s and Dewey’s pluralist insights should abandon; and one certainly need not be a Rortyan in order to be able to appreciate these insights. (On James’s defense of pluralism against monism, see James 1975 [1907], chap. 4; cf. also Putnam’s 1981, 1983, 1990, 1994, 1995 analogous rejections of absoluteness and the “God’s-Eye View.”)

21. This section is partly derived from Koskinen and Pihlström 2006.
to Quine. In pragmatist terms, this naturalization of the philosophy of science can be expressed by saying that “first-philosophical” norms of justification, rationality, theory-choice, etc., can never override the practices of inquiry that scientists themselves engage in. In “Two Dogmas of Empiricism,” Quine (1980 [1953], chap. 2) advanced a “shift toward pragmatism” and claimed to represent a more thorough pragmatism than Carnap had done in his seminal “Empiricism, Semantics and Ontology” (1950). However, Quine hardly ever commented—any more than Carnap—on any historical detail on the actual pragmatist philosophers of the American tradition. It is not clear, then, what exactly Quine’s place in the history of pragmatist philosophy of science is. If he occupies such a place at all, it is in the interesting intersection of pragmatism and logical empiricism.


23. Carnap’s 1950 distinction between internal and external questions of existence was, of course, an important milestone in pragmatism (or the synthesis of pragmatism and logical empiricism), as he held that the latter kind of questions are only pragmatically decidable ones about the utility of choosing a particular linguistic framework, such as the “world of things,” or the system of natural numbers. Carnap, clearly, was more a positivist than a pragmatist, although his famous distinction did influence, among others, Putnam’s neopragmatist “internal realism” (cf. Putnam 1981, 1983, 1990). It is also a noteworthy historical fact that Carnap engaged in a dialogue with John Dewey on meaning and the nature of philosophical problems in the first volume of Philosophy of Science in 1934 (see Shook 1998, 462). C. I. Lewis, whose views in many ways resembled Carnap’s, endorsed roughly at the same time pragmatist views of meaning and experience in contrast to strictly positivist ones (Lewis 1934).

24. In addition to Carnap’s and Lewis’s contributions, Charles Morris was instrumental in the 1930s in bringing this connection to the fore, emphasizing the complementarity and even the possibility of “convergence” of pragmatism and logical empiricism (see Morris 1937, 1938, 1979 [1937]; cf. Stadler 1997, 401, 869 ff.). Later Morris returned to the topic in his contribution to Carnap’s Schilpp volume (Morris 1963). According to Morris, it is possible to hold on to the requirement of excluding meaningless expressions—both in the logical and the “biological-empirical” senses—even if one broadens the logical empiricists’ original strongly individualist verifiability principle by reinterpreting it pragmatically, in order to make sense of social cooperation in scientific experience. In his response to Morris, Carnap 1963, 860 emphasized Morris’s and Ernest Nagel’s role in the exchange between the traditions of pragmatism and logical empiricism. Just like Morris, Nagel also stressed Peirce’s role as a precursor of “modern empiricism” already soon after the collapse of the Vienna Circle (see Nagel 1940). A paper with the same title, “Charles S. Peirce, Pioneer of Modern Empiricism,” was presented by Nagel at the Fifth International Congress for the Unity of Science at Harvard in September, 1939 (Stadler 1997, 430). Another bridge builder who might have been more influential, had he lived longer, was Frank Ramsey, whose 1927 paper, “Facts and Propositions,” bears some resemblance to Peirce’s and James’s views: Ramsey held that the meaning of a proposition lies in the activity the stating of the proposition would lead to. An even earlier connection between the two tradi-
Still, among recent surveys of pragmatism, Susan Haack’s (2004) contains a section on Quine (27–28), as does Nicholas Rescher’s introductory book (2000, 37–39). For a long time, Quine has been discussed, in general presentations of pragmatism, as a (neo)pragmatist of some kind (see, e.g., Murphy 1990). L. E. Hahn’s preface to the “Schilpp volume” dedicated to Quine described Quine’s position as “pragmatic naturalism” (Hahn and Schilpp 1986, xv). The conviction that Quine represented pragmatism or that he was a major actor in the “pragmatic turn” in twentieth century philosophy of science is, hence, widespread.

However, did the “endorsements of pragmatism” one finds in both “Two Dogmas” and “On What There Is” (Quine 1980 [1953], chap. 1–2) curiously vanish from Quine’s later work, as Christopher Hookway (1988, 50) claims? This shift of emphasis is related to Quine’s increasing tendency to describe himself as a naturalist and a “robust realist.” However, even in the early works, the use of the term “pragmatic” and related expressions is subordinated to empiricism and thereby to a treatment not of individual pragmatic purposes or interests but of scientific objectivity: the talk about “pragmatic standards” refers “to an appeal to the pursuit of efficient predictive control over experience which serves as the ‘duty’ of science. Here Quine’s empiricism intervenes to assign a meaning to the ‘pragmatic’ which puts into question the claim that the presence of pragmatic considerations in scientific growth is in tension with a realist construal of science.” (Ibid., 53.) One might, therefore, view Quine’s “pragmatism” as a descendant—philosophically, not necessarily historically—of Peirce’s objective, realistic pragmatism (cf., e.g., Haack 1998, 2004; Rescher 2000, 2005; Pihlström 2004), according to which the relevant kind of pragmatic results or conceivable practical consequences that our concepts and conceptions ought to have are, primarily or perhaps exclusively, scientific, empirical ones—in contrast to James’s and his followers’ more pluralistic, less science-oriented conception of the “practical consequences” of beliefs.

Several (neo)pragmatists and scholars of classical pragmatism have challenged the standard account of Quine as a pragmatist—with good reason,
given Quine's own explicit doubts about his place in the pragmatist tradition, and about what pragmatism even means (cf. Quine 1981). Indeed, as Haack (2004, 27) points out, “it is hard to take Quine’s reference to pragmatism [in ‘Two Dogmas’] as seriously historically intended, given that he includes Carnap, surely by any standards more positivist than pragmatist.” In order to enrich our picture, we should take a brief look at the ways in which Quine’s philosophy of science can be claimed to differ, in many cases significantly, from pragmatists’—old and new—ideas.

3.2. Hilary Putnam’s criticism of Quine is particularly relevant here, because Putnam—in good pragmatist company (e.g., James)—rejects Quinean reductive scientism. Putnam attacks Quine’s views because they actually lead, by his lights, to something very much like Richard Rorty’s irresponsibly relativist neopragmatism. From Putnam’s perspective, then, Quine’s version of pragmatism or naturalism ought to be rejected pretty much for the same reasons that the responsible pragmatist ought to reject Rorty’s neopragmatism. The key to this criticism is the claim that both Quinean scientistic naturalism and Rortyan relativism tend to lose the humanly inescapable normativity of semantic and epistemic practices. Putnam has written on Quine for decades, and obviously we cannot pay attention to everything he has to say about Quine here (see, e.g., Putnam 1990, chap. 20 and 21; 1994, chap. 12 and 18). His main argument, in any event, seems to be the one claiming that Quine is led, through his radical naturalism, to a form of meaning skepticism, and thus to global relativism and antirealism—in any case to a devastating loss of normativity, which no pragmatist should tolerate.

25. It is an open question whether Quine’s “scientism” (a word the use of which he endorsed himself, when I had the opportunity to interview him at Harvard in 1995) ought to be classified as “reductive” or, perhaps better, “eliminative.” Quine does, after all, eliminate from his scientific ontology a number of entities that many people, including most pragmatists, accept, e.g., mental, intensional, and normative ones. Such an elimination may, indeed, be seen as one of his major projects, especially in *Word and Object* (Quine 1960) and subsequent writings.

26. See Rorty 1979, 1991, and 1998. Rorty himself favors terms such as “ethnocentrism” and “antirepresentationalism,” refusing to be a “relativist.” Rortyan views do, however, represent relativism in Laudan 1990, and elsewhere. As the issue is about the very possibility of transhistorical and transcultural normativity, relatively little hangs on terminology here. In any case, we can safely ignore Rorty’s neopragmatism for the purposes of this paper, because his contributions to pragmatist philosophy of science have been scarce—unless one wants to emphasize his claim that science is in no closer contact with “reality as it is in itself” than, say, poetry. For a discussion of pragmatism paying attention to both Rorty’s and Putnam’s positions, see Pihlström 1996, chap. 4.

27. For several papers examining the prospects of pragmatic realism in relation to (pri-
Putnam is not alone in this criticism. According to Michele Marsonet (1993), Quine’s views have encouraged “scientific anti-realism.” David Weissman (2003), in turn, places Quine in the Cartesian tradition, with language replacing the self-sufficient mind, and argues that his “pragmatism” faces a tension between naturalism and idealism (see Weissman 2003, 71): is the natural or physical world mind-independently real, or is it somehow made up by the mind—or, rather, language (in particular, scientific language-use)? This tension can be perceived in the pragmatist tradition more generally (cf. Pihlström 1996); accordingly, in this sense, Quine easily qualifies as a pragmatist. On the other hand, non-pragmatist philosophers may also easily face similar difficulties in steering a middle course between (scientific) realism and antirealism, e.g., idealism, constructivism, or relativism.

Other, perhaps more clearly pragmatist, critics of Quine have formulated their complaints quite differently. For instance, John Shook (2002) argues that Quine’s “global realism” postulates experience-transcendent unobservable entities, while Dewey’s more properly pragmatic philosophy of science (see section 2 above) precludes such a realist postulation. This may or may not be accurate, depending on how realistically Quine’s ontology in general is understood (cf. Koskinen 2004), and on how exactly we construe Dewey’s contribution to these issues. Independently of the realism debate, Shook’s charge that Dewey’s naturalism is essentially broader than Quine’s scientism is a traditional one among pragmatism scholars. Shook (2002) is one of those who are prepared to view Quine’s position as a pragmatist one “only in the most marginal of senses,” that is, only in specific comparison to Carnap’s. Most Deweyan pragmatists would probably agree with Shook that Quinean empiricism is simply too narrow to cover the richness of immediate human experience, or with Leemon B. McHenry (1995) that the elements of (ontological) pragmatism in Quine are insufficient to significantly connect him with classical pragmatism. Accordingly, they would agree with Matthew Brown’s (2005) rejection of the myth—explicitly formulated, as Brown notes, by Ernest Gellner—of Quine as “the last pragmatist,” because Quine was not merely an inaccurate interpreter of pragmatism but always more clearly an empiricist (or even a positivist) than a pragmatist, admitting that it was never clear to him what it takes to be a pragmatist.28

28. For other relatively recent pragmatist criticisms of Quine, see Nevo’s 1992 defense...
I conclude, preliminarily, that if Quine can be said to have been a pragmatist at all, he was a pragmatist of a reductively scientistic type, very far even from Peirce. His role in the history of pragmatist philosophy of science is perhaps simply that he made the term “pragmatism” (like the term “ontology”) more widely used and legitimate. Moreover, Quine, like Carnap, does carry the instrumentalist heritage of logical positivism; hence, if we wish to draw a distinction between pragmatism proper and instrumentalism, understood as some kind of vulgar pragmatism (cf. the previous section), then we certainly should not include Quine among our paradigmatic pragmatists. Quine’s place in the pragmatist tradition in the philosophy of science should, then, be spoken about only with utmost care. Even so, he is an extremely important transitory figure without whom later neo-pragmatist twists in the development of twentieth century philosophy of science—even in the work of philosophers such as Putnam and Rorty—could hardly have taken the form they did.

4. Paradigms and reality: Kuhn, Putnam, and pragmatic realism

4.1. What I will try to do next is to substantiate my claim that pragmatism—especially in its Deweyan naturalist version—need not amount either to an empiricist instrumentalism opposed to scientific realism about unobservable theoretical entities and processes or to a radical relativism (constructivism) sacrificing the objectivity of the scientific method and scientific knowledge. Of course, I am not the first pragmatism scholar to make such a suggestion. In discussions of Dewey, as already noted, it has been repeatedly emphasized that Dewey’s instrumentalism is different from the one influential in and after logical empiricism, and even that Dewey’s version is compatible with scientific realism, because the Deweyan instrumentalist can accept unobservable theoretical entities while denying that theories simply mirror reality (see Morgenbesser’s “Introduction” to his 1977, xvi–xvii, xxix; cf. Tiles 1988). In James’s case, this would be more difficult to document, but something was already said in this regard in section 2 above.

In any case, both of these pragmatists denied that science—even our
best-advanced science—describes “reality as it exists in itself” (Dewey 1960 [1929], 136–137; cf. James 1975 [1907], chap. 5, 7); yet, this is not to deny the ontological status of the theoretical postulations of science.\(^{29}\) Nor is it to deny the (relative) objectivity, “humanly speaking”—to borrow a term from Putnam—of scientific theories and methods. The pragmatist does not hold, with Paul Feyerabend (1993 [1975], chap. 1), that “anything goes,” or that a pluralist proliferation of scientific practices and methods is a good thing as it stands, because scientific practices have their own in-built normativity, and normative distinctions between good and bad science are internal to those practices themselves, hence something that the pragmatist ought to take seriously. However, this does not mean that the norms of scientific methodology would be handed down to us from above; to the contrary, science is a continuing critical process, in which not only theories but the methods used to justify them are constantly open for reevaluation. Instead of celebrating pluralism and anarchy as such, the pragmatist follows Dewey in emphasizing the critical function of philosophy, its role as “the critical method for developing methods of criticism” (Dewey 1989 [1929]).

In general outline, the view these sketchy reflections yield (I hope) is the following (cf. Pihlström 1998, 85–86). The pragmatist can, and should, try to develop a pragmatic realism about scientific knowledge and its objects, including unobservable ones—even when agreeing with James and Dewey that theories are, above all, instruments for coping with the world, that is, for living in the natural world surrounding us and settling the various problematic situations life brings with it. Nothing prevents us from viewing some of our problematic situations as highly “theoretical” ones. This view amounts to a subordination of scientific realism to pragmatism, a commitment to, say, the unobservable entities a theory postulates on a pragmatic basis, from the point of view of our scientific practices. The practice of science is, simply, a realistic practice, and the pragmatist should follow it, instead of instituting external norms foreign to it.\(^{30}\)

\(^{29}\) It is often difficult for philosophers of science to appreciate the unique position pragmatism enjoys as a middle path between scientific realism and empiricist instrumentalism. Compare Tiles’s 1988, 7 words: “When pragmatists suggest that correspondence is a misleading notion to use when trying to give an account of what makes a good scientific theory, they sound to realists like confused positivists, for they insist that the role of theory is as an instrument to guide experimental and observational practice. But as they deny that we can make a sharp separation between our theoretical and observational claims they sound to positivists like muddled realists.”

\(^{30}\) Appeals to scientific practice have become more and more usual among thinkers not directly associated with pragmatism (see, e.g., Rouse 1996, 2002; as well as the reflections on Rouse’s 1996 book in Pihlström 1998, chap. 9)—not only among relativist sociologists
The instrumentalist view, according to which theories do not genuinely purport to describe reality at all but only structure and systematize the connections between observable phenomena, is extremely restrictive and, therefore, foreign to the habitual actions and deliberations standardly performed within our practices of inquiry and theory-choice. Instrumentalism and its more recent manifestations, such as constructive empiricism (van Fraassen 1980), are essentially skeptical views blocking the road of inquiry. Naturalists and pragmatists, however, do not aim at “refuting the skeptic” in the first place; consequently, they need not worry about refuting the instrumentalist, either, in any general foundationalist manner. Skeptical issues should, as Quine correctly saw, be understood as issues arising within science, if genuinely arising at all. They are not autonomous problems of a first philosophy prior to science; conceived as such, they would amount to what Peirce called “paper doubt.” Yet, the truly pragmatic naturalist also rejects Quine’s extremely restricted conception of scientific (empirical) experience as the physical stimulation of an organism’s sensory receptors. Science qua practice is ineliminably normative, and no adequate understanding of this normativity is achieved within a philosophy of science defined by Quinean restrictions.

4.2.

Now, where should we look for this kind of a reconciliation of normatively rich pragmatism and scientific realism—and the corresponding detachment of pragmatism from both radical relativism and extreme empiricist instrumentalism? Somewhat surprisingly, I propose, in the work of thinkers whose “pragmatist” thought is often associated with antirealism rather than realism: Hilary Putnam and Thomas Kuhn. Just like Putnam, whose “internal realist” or “pragmatic realist” views have often been characterized as yielding a combination of pragmatism and Kantian idealism, Kuhn can be interpreted as a naturalistically relaxed, post-Darwinian and historically concerned Kantian transcendental idealist—with as firm a commitment to a form of empirical realism as in Kant himself. In this respect, Kuhn and Putnam stand close to each other, despite their disagreements over the nature of reference and meaning (in)variance in scientific theory-change, as well as the related threat of relativism that Putnam, like many others, has perceived in Kuhn’s views. They have also been treated as equally dangerous ontological relativists, constructivists, or idealists by researchers within science and technology studies, but even among scientific realists (cf. Kukla 1994).

31. Sections 4.2 and 4.3 touch upon themes more fully discussed in Pihlström and Siitonen 2005.
more traditional scientific realists who favor a correspondence theory of truth.  

It will be useful to adopt, for a moment, a comparative approach, that is, to note briefly how Putnam and Kuhn refer to each other, especially in relation to the realism issue. Putnam’s references—from his early scientifically realist phase through his first substantial defenses of internal realism (Putnam 1981, 1983) up to, and including, his more recent treatments of the realism issue (Putnam 1994)—have been critical: together with Feyerabend and the notorious “French postmodernists,” Kuhn seems to be, for Putnam, one of those irrationalist relativists and incommensurabilists who give up the objectivity of science. According to Putnam, that objectivity must be maintained, even if metaphysically realist interpretations of it have led philosophers astray. Hence, in brief, Putnam’s reading of Kuhn does not significantly differ from the traditional line of criticism that pictures Kuhn as a relativist turning science into a subjective, irrational conversation—even though Putnam’s own views have frequently been subjected to similar anti-relativist attacks (cf. Davidson 1984, Norris 2000).  

32. See, e.g., Niiniluoto 1999 and Norris 2000; cf. also the well-known attack on conceptual relativism, as represented by Kuhn, Quine, and others, in Davidson 1984.  

33. It may be noted that, despite his frequent discussions of Putnam, especially in the essays collected in Kuhn 2000, Kuhn hardly ever refers to the earlier pragmatists.  

34. We need not take up the complex task of interpreting Putnam’s changing positions here. For a brief account of Kuhn’s and Putnam’s controversy over realism and the reference of scientific terms, see Andersen 2001, 56–60; see also Gupta 2002, chap. 6, for the suggestion that Putnam’s internal realism is a synthesis of Popper’s scientific realism and Kuhn’s “relativism”; and Niiniluoto 1999, chap. 7, for an interesting comparison between internal realism and Kuhn’s post-Structure views. Putnam’s accusations that Kuhn is a relativist are, of course, standard; this has been the usual charge among Kuhn’s critics from early on (see, e.g., Schefler 1982 [1967]; for a more recent version of the same line of argument, with a link to today’s “science wars,” see Sokal and Bricmont 2003, 67–73).  

35. Such an impression of Kuhn as a relativist “conversationalist” may have been strengthened by Rorty’s 1991, 1998 frequent rhetoric addressed to “us Kuhnians,” coupled with his insistence on there being only conversational constraints for inquiry.  

36. My discussion of Putnam and Kuhn here focuses on the issues of realism vs. idealism and realism vs. relativism, not on the issue of realism vs. instrumentalism. I take it as obvious that Putnam has never denied, any more than Kuhn, the existence of unobservable theoretical entities postulated in scientific theories, although both may have denied the independent, unconstructed, “absolute” existence of any entities (scientific and commonsense alike). Thus, Putnam writes: “Electrons exist in every sense in which chairs (or sensations) exist; electron talk is no more derived talk about sensations or ‘observable things’ than talk about sensations or chairs is derived talk about electrons. Here I am a ‘scientific realist’.” (Putnam 1994, 495.) For more on Putnam’s internal or pragmatic realism as a mediator between scientific realism and idealism or constructivism, see Pihlström 1996, chap. 4, and Pihlström 1998, chap. 3.
Kuhn’s tone of voice differs from Putnam’s. His fundamental agreement with Putnam is clearly visible in his 1991 essay, “The Road since Structure,” when he says that “the relationship between the lexicon—the shared taxonomy of a speech community—and the world the members of that community jointly inhabit” cannot be “the one Putnam has called metaphysical realism,” because “[i]nsofar as the structure of the world can be experienced and the experience communicated, it is constrained by the structure of the lexicon of the community which inhabits it” (Kuhn 2000, 101; see also ibid., 218–221, 247). Kuhn still maintains, as he did in his main work (see especially Kuhn 1970, “Postscript”), that insofar as the notion of truth has anything to do with scientific development, it cannot be extra-theoretical correspondence truth but must be “intra-theoretically” applied (Kuhn 2000, 115, 160–162, 251). This, obviously, is a view to some extent (though perhaps not entirely) shared by Putnam. “No more in the natural than in the human sciences is there some neutral, culture-independent, set of categories within which the population—whether of objects or of actions—can be described.” (Ibid., 220.) Truth, then, might be pragmatically (rather than correspondence-theoretically) characterized, whether we are talking about natural-scientific truth or interpretive, geisteswissenschaftlich truth.

Even so, Kuhn, as much as Putnam, resists any naive antirealist use of the metaphors of “construction” or “invention.” The world is, in some sense, “experientially given” and “solid” (ibid., 101), not anything we simply make up. Still, he speaks about the “world-constitutive role” of intentionality and mental representations (ibid., 103), thereby returning, with some qualifications, to his frequently attacked view that different paradigms constitute “different worlds” and that the world “changes” when a new paradigm is adopted in a scientific revolution (cf. Kuhn 1970, 111; see also ibid., 206). 38 Kuhn replaces “the one big mind-independent world about which scientists were once said to discover the truth” by “the variety of niches within which the practitioners of these various specialities practice their trade” (Kuhn 2000, 120). This conception of practice-bound “niches” suggests a strongly pluralist view of science (ibid., 119, 249). Putnam (e.g., 1994) has repeatedly embraced pluralism, in contrast

37. This can be regarded as a successor notion to Kuhn’s 1970 famous (or notorious) concept of a paradigm. I shall, however, freely speak about both paradigms and lexicons, without attempting any careful definition of these notions. Both could be translated into a more pragmatist jargon by speaking about “scientific practices.”

38. See several essays addressing this theme in Horwich 1993, and see especially Sharrock’s and Read’s 2002, 50–58 illuminating discussion. Sharrock and Read set out to show that Kuhn is, in using phrases like “world changes,” much less radical than both his friends and foes have thought he is.
to a number of recent physicalists and scientific realists who (still) subscribe to the “unity of science” program, though in a manner different from the logical positivists. Here, Putnam and Kuhn equally clearly subscribe to a kind of pragmatism. There is an irreducible plurality of scientifically acceptable ways of representing reality—of “carving up” the world by means of scientific concepts and theories—and thereby settling problematic situations; these vary according to the environment, the situations, or the Kuhnian “niches” we find ourselves in, and the various pragmatic needs and interests that govern our attempts to deal with such situations. In this sense, no theory is, to borrow James’s phrase, a “literal copy” of reality. All our theories are rooted in our needs to cope with the pragmatic, concrete situations we arrive at in the course of our natural lives.

4.3.

The plausibility of the above-quoted constructivist-sounding statements by Kuhn depends on their being interpretable from the standpoint of a Kantian construal of pragmatism (or of naturalized pragmatic realism): “By now it may be clear that the position I’m developing is a sort of post-Darwinian Kantianism,” Kuhn tells us. “Like the Kantian categories, the lexicon supplies preconditions of possible experience. But lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another.” (Kuhn 2000, 104.) The Kuhnian “categories,” accordingly, are practice-embedded and may vary, depending on the changes our (scientific) practices undergo. This, if anything, is a pragmatic way of viewing the world as a human construction in the Kantian sense. Both Kuhn’s “structured lexicon” and Kant’s “apriori” (in an historically relativized sense) are “constitutive of possible experience of the world,” without dictating what that experience must be (ibid., 245), i.e., without telling us which experiential objects and events we in fact encounter in the world. Thus, in an interview in 1995, shortly before his death, Kuhn perceptively described himself as a Kantian with “movable categories” (ibid., 264). In an analogous way, in my view, the pragmatists are also such historicized Kantians (Pihlström 2003). What Kuhn rejects in Kant is, unsurprisingly, the Ding an sich (see Kuhn 2000, 207; cf. also Conant and Haugeland 2000, 8), although he admits that underlying the processes of change (of lexical categories) there

39. Nickles 2003, 5, in his “Introduction” to a volume of essays on Kuhn, also describes Kuhn’s view as a “historical Kantian relativism” and employs a Kantian expression when claiming that, for Kuhn, (shifting) scientific traditions “constitute the basis for intelligibility” (7).
must be “something permanent, fixed, and stable” which is as ineffable as the thing in itself (Kuhn 2000, 104; cf. Andersen 2001, 85). This comes close to Putnam’s admission that “perhaps Kant was right” in postulating a noumenal ground of experience, after all (see Putnam 1983, 226)—even though Putnam, no less than Kuhn, has always been suspicious of the very intelligibility of the notion of a thing in itself (cf. Pihlström 1996, chap. 4.6–4.7). Indeed, while Kuhn’s position is often described as “non-realist” (e.g., by Andersen 2001, 60), Kuhn seems to think that we should somehow be able to combine realism and Kantianism (see Sharrock and Read 2002, 178). As Kant himself was not only a transcendental idealist but also an empirical realist, this should not be an impossible task. In any case, Kuhn is engaged in the same task of mediating between realism and idealism (constructivism, relativism) that the classical pragmatists from Peirce onward already carried on, following Kant.40

As Paul Hoyningen-Huene (1993, especially 35 ff., 267–271), one of Kuhn’s best commentators, lucidly explains, the notion of a world-in-itself does play a significant role in Kuhn’s conception of scientific inquiry. Science is concerned with a Kantian-like humanly constituted “phenomenal world,” whereas the world-in-itself remains unknowable (cf. also Sharrock and Read 2002, 52 ff., 173, 179).41 Our phenomenal worlds are, however, “reshapings” of the world-in-itself, “substantial” and independent of human subjectivity (Hoyningen-Huene 1993, 268), and the world is, in this sense, resistant to our activities. This is inevitable, given Kuhn’s account of anomalies as formative factors of scientific crises and revolutions (ibid., 269–270). If anomalies are to occur, the structure of the world cannot be simply up to us. This view might be construed along the lines of the “one world” (two aspects) interpretation of Kant’s transcendental idealism (cf. Pihlström 2003). Obviously, however, this is no place to engage in Kant exegesis, even though it can hardly be denied that Kant is the most important background figure of both pragmatism and the realism dispute in the philosophy of science.

It is not easy to interpret the key Kantian notions, as used by Kuhn, in any strictly non-metaphorical manner. It remains unclear what Kuhn’s—or Putnam’s—commitment to a noumenal reality in effect amounts to.42

40. See Pihlström 1996 and 2004 on this mediating task in the pragmatist tradition, interpreted in a Kantian manner.
41. “Peircean realism” is one of the positions Hoyningen-Huene 1993, 56 ff. contrasts with Kuhn’s views.
42. For both, the issues of realism and truth, theory and observation, etc., were in any case profound and significant, genuine ones: “It’s always been clear to me [. . .] that the two people I was sure were taking the problems I was looking at seriously were me and Hilary.” (Kuhn 2000, 312; interview in 1995.)
But his views, as interpreted by Hoyningen-Huene in particular, indicate how difficult it is to avoid employing the traditional Kantian vocabulary, as soon as one admits (as pragmatists tend to do) that the world as we know it is in some sense a human (transcendental) construction, or dependent on the constitutive activities of our subjectivity (e.g., our scientific paradigms, practices, or practice-embedded lexicons), instead of being anything objectively ready-made or pre-categorized, or straightforwardly and unproblematically given.

Kuhn’s attachment to something like a thing in itself has also led to the rather non-standard charge that he after all seems to maintain, in an *a priori* manner, an invariant ultimate structure of nature (viz., something like the mind- and lexicon-independent world of the metaphysical realist) and thus precludes any epistemically realist (or, for that matter, idealist) view of the *availability* of that structure, given his historicist conception of science (Margolis 1993, 72–80, especially 74). There may be a correct insight in this argument, but on the other hand it is an open issue whether any even partially Kantian approach in the realism issue can avoid some commitment to the *Ding(e) an sich*. Among Kuhn’s interpreters, Wes Sharrock and Rupert Read have perceived the central problem most clearly: while Kuhn admits that something (some sort of a “fixed nature” or “the world out there”) can be said to retain “a certain constancy” in scientific revolutions, “there is no way of saying *what it is that remains constant throughout*” (Sharrock and Read 2002, 57). That is, there is no paradigm- or lexicon-independent, neutral point of reference that could be appealed to in saying something more specific about the constancy at issue.

The problems underlying the interpretation of phrases like “world changes” are, then, primarily semantic rather than metaphysical; they are problems concerning what we can meaningfully say in our language and what we cannot. But this does not lead us out of the Kantian predicament; it merely transforms the Kantian concern with the *limits of knowledge* (and thus with the unknowability of the *Ding an sich*) into a concern with the *limits of language*, of the sayable—and thus we are brought, through Kuhnian philosophy of science, to a largely Wittgensteinian territory (cf. ibid., 165, 169–170, 200 ff.). Semantically speaking, it is also possible

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44. This broader issue must obviously be set aside here, as I am not engaging in Kant scholarship in this paper. For an attempt to develop a pragmatic interpretation of the significance of the *Ding an sich*, see Pihlström 1996, chap. 4.7.

45. The influence of Wittgenstein’s 1953, 1969 later philosophy on both neopragmatism and the developments in the philosophy of science in and after the 1960s would obviously require another study.
to explicate the constitutive role that paradigms and/or lexicons play in making scientific knowledge and its objects possible by saying that it is only against the background provided by a paradigm or a lexicon that scientific statements can receive a truthvalue, or are possible truth-candidates (and falsity-candidates) (cf. Friedman 2001, 74–75). Functioning as such constitutive presuppositions, paradigms are necessary for the empirical possibilities postulated in a given scientific field (see ibid., 94, 114), i.e., not just presuppositions of the truth of the statements scientists make. They create a “logical space” for such possibilities to be taken seriously in scientific investigations (ibid., 95). Something essentially similar could again be said, in pragmatist terms, about scientific practices. In Kuhn’s (2000, 249) own words, it is effability instead of truth that is to be relativized to “worlds” and scientific practices; indeed, a “pluralism of practices” (ibid.), an unambiguously pragmatist—above all Jamesian— notion, captures Kuhn’s ideas nicely.

Using phrases like this, the Kuhnian pragmatic realist is, obviously, embedded in deeply Kantian commitments. However, once we are prepared to relativize the Kantian conditions for the possibility of experience or cognition, we can, according to Friedman (2001, 2002, 2003), see Kuhn as vindicating crucial parts of Carnapian logical empiricism, precisely because of his profoundly Kantian approach, which he shares with Carnap and other logical empiricists. This relativization of Kant’s transcendental conditions or principles is, in fact, a common theme in the early history of twentieth century philosophy of science: in addition to Carnap and Hans Reichenbach, a relativized notion of the a priori was defended by C. I. Lewis (1923), among others. Through Lewis, we can, again, somewhat more explicitly build a bridge to the pragmatist tradition, whose interesting encounters with logical empiricism in mid-century America are thus not restricted to Quine’s reaction to Carnap. These connections have, unfortunately, remained more or less neglected by Kuhn’s commentators, including Friedman, although his defense of the relativized a priori is readily applicable, mutatis mutandis, in pragmatism, too.

This is not, however, the right place to examine in any more detail to

46. Also, the pragmatist aspects of Reichenbach’s Experience and Prediction 1938 have been emphasized—already by Ernest Nagel in his review of the book in the year of its appearance (see Shook 1998, 503; see also Siitonen 1997). It may be noted that Reichenbach 1939 contributed to the exchange between pragmatism and logical empiricism by criticizing Dewey’s reasons for denying the reality of scientific objects. (Ilkka Niiniluoto has informed me that Reichenbach, who developed a well-known frequency interpretation of probability, had not known about Peirce’s related views on probability, thus surprising the Deweyan pragmatist Sidney Hook who had mentioned Peirce to him in a conversation.)
what extent pragmatism can accommodate insights drawn from Kantian transcendental philosophy and transcendental idealism, or how a pragmatically established form of (scientific) empirical realism can be reconciled with transcendental idealism (see further Pihlström 2003). The topic is important, nevertheless, because it has become clear that Kuhn and Putnam can both be interpreted as leading neopragmatists in the philosophy of science and in the general realism discussion, and both, precisely because of their pragmatism, have developed neo-Kantian views about the practice-laden (transcendental) constitution of empirical reality.

5. Scientific practice and pragmatic naturalism

5.1. Unsurprisingly, Kuhn’s true novelty in the philosophy of science has been claimed to lie in his emphasis on science as a practice, and his rejection of the traditional epistemological questions of justifying scientific beliefs (see Rouse 2003). The interpretation of Kuhn as a kind of a Kantian thinker (or even a transcendental idealist), as developed above, is compatible with this suggestion, provided that (1) the view of “science as a practice” amounts to a form of pragmatism and (2) pragmatism can receive a Kantian reinterpretation (as briefly proposed above, and at some more length in Pihlström 2003). Joseph Rouse’s reading of Kuhn is helpful, however, in bringing out nicely what a truly pragmatist reading should not look like: after correctly noting that Kuhn leads us toward a description of science as an activity (rather than as the product of an activity), Rouse says that paradigms should be understood as “exemplary ways of conceptualizing and intervening in particular situations,” instead of being understood “as beliefs (even tacit beliefs) agreed upon by community members” (Rouse 2003, 107). Now, a pragmatist should construe beliefs themselves as habits of action, as famously done by Peirce and the other classical pragmatists; it is precisely in virtue of being ways of conceptualizing and intervening that paradigms are also, ev i ps o, beliefs, or perhaps

47. The obvious pragmatic turn in Kuhn’s account of science does not prevent one of the most influential contemporary pragmatism scholars, Murray G. Murphey 2003, from describing his view as “one of the most bizarre theories of science ever propounded” (294). He says that Kuhn leaves it unexplained why scientists after the revolution come to believe in one new theory (or paradigm), instead of a chaotic proliferation of alternatives (ibid.). This may be a problem in Kuhn, but for our purposes of placing Kuhn’s work in the pragmatist tradition, Murphey’s criticism is not an obstacle. Certainly the pragmatist interpretation is not, as such, sufficient for rendering Kuhn’s ideas fully acceptable or unproblematic.

rather networks or Quinean “webs” of beliefs. It is in our activities and practices themselves that our ontological construals of the way(s) the world is are to be found. Thus, Rouse’s dichotomies between “practitioners” and “believers” (ibid., 109) and between beliefs and forms of life (ibid., 112) are far too unpragmatist to be helpful in what might be labeled a “transcendently pragmatist” rereading of Kuhn as a philosopher of scientific practice.

Rouse, however, comes close to a transcendental interpretation when proposing that Kuhn should be seen as a critic of scientific realism, as someone who denies that science aims at correct representations of a concept- and practice-independent world (ibid., 117), and especially when he points out that “[t]he realist can posit a world ‘beyond’ language and culture only by mistakenly thinking that we can have a definite language and culture distinct from how we engage the world” (ibid.). What he misses is only that a form of scientific realism (just like Kantian empirical realism) can be maintained within the overall position of transcendental pragmatism. Of course, Rouse is free to offer a non-pragmatist account of Kuhn as a “philosopher of scientific practice”; it just needs to be pointed out that a pragmatist way of putting the matter is also available and might strengthen Rouse’s point by connecting it with the transcendental concerns raised by Friedman and others. Certainly we should not read Kuhn as an opponent of scientific realism, if the issue is the existence of unobservable theoretical entities.

If, as I suggest, we approach Kuhn as a pragmatist (or pragmatic realist), should we also acknowledge Feyerabend (1993 [1975])—the anarchist and arch-relativist—as a pragmatist, in some sense? Probably we should, inasmuch as we also acknowledge Rorty’s entitlement to the word “pragmatism.” Feyerabend avoids committing himself to the pragmatist (or any other) tradition, but he does speak about “pragmatic philosophy” when referring to people or groups “participating in the interaction of traditions”; such pragmatic philosophy is required for the “open exchange” between people and traditions that is, in turn, needed to transcend simple relativism. (Cf. ibid., 217–218, 226–228.) In any case, Feyerabend’s pluralism, antiessentialism, and antifoundationalism are clearly close to pragmatism, especially James’s pragmatism.

Here our survey, arriving closer to our own days, must end up with acknowledging a plurality of quite different neopragmatist philosophies of science. It will suffice to mention a couple of examples in order to give an impression of the richness and variety in the field. A very important pragmatically oriented philosopher of science—very different from Kuhn and

49. Cf. again Pihlström 2003 for a more comprehensive elaboration on this idea.
Feyerabend—is Ian Hacking, whose emphasis on experimentation, “intervening” instead of representation, has partly set the tone for later empirically informed work in science and technology studies (Hacking 1983). While Hacking agrees with Dewey’s (1960 [1929]) rejection of the spectator theory of knowledge (Hacking 1983, 130), the chapter he devotes to pragmatism (ibid., chap. 4) is located in the context of his treatment of the philosophies of science oriented to representing rather than intervening. 

Furthermore, other late-twentieth-century philosophers of science in some ways close to pragmatism include, for example, Mary Hesse, who also wishes to steer a middle course between realism and relativism, and Larry Laudan, a leading critic of “convergent realism” (see Laudan 1981), who has come to favor a version of pragmatism in the “four-cornered debate” he sets among pragmatists, realists, relativists, and positivists (see Laudan 1990).

Laudan’s “pragmatist” joins Kuhn in, for instance, rejecting the (naively) realist view of scientific progress as cumulative (ibid., 17), and in characterizing science as a problem-solving activity (ibid., 28), with the pragmatist qualification that we should be primarily interested in “whether a theory will be a reliable guide to the future” (ibid.). Moreover, Laudan’s pragmatist joins Quinean naturalists in asserting that “philosophy can, and should, make use of any of the forms of reasoning appropriate to scientific research” (ibid., 99), and in urging that explaining the empirical success of science requires paying attention to our interaction with nature (ibid., 165–166). In a Deweyan experimentalist fashion, the rules for theory-choice should be accepted or rejected on a “trial and error” basis:

[T]here is a fact of the matter which our rules reflect, namely, that inquiry in this particular world works better using these rules [viz., the ones that have produced successful theories about the natural

50. It is worth noting that Hacking was one of those who relatively early recognized Putnam’s 1981 connections with pragmatism (see Hacking 1983, chap. 7); it was only later that Putnam himself (1990, 1994, 1995, 2002) started to elaborate on these connections.

51. Hesse’s 1980, xviii–xix, 190–201 “pragmatic criterion” for empirical science is the requirement to exhibit “increasingly successful prediction” and thus the possession of “instrumental control” of external reality. This is clearly not unique to pragmatism. But as Hesse admits that the pragmatic criterion allows for a “permanent plurality of conceptual frameworks” (xxiv), she comes close to Jamesian pluralist pragmatism, though not going as far as Feyerabend.

52. Already Laudan’s earlier work 1984 contains some references to Peirce and Dewey, but in his 1990 book, Science and Relativism, he more explicitly affirms his pragmatism. For criticisms of Laudan’s attempt to avoid the concept of truth in his theory of scientific change and progress, see Niiniluoto 1999.
world] than it does using a host of rival rules which humanity has devised for getting a valuable knowledge. (Ibid., 103.)

[T]he justification for our theories about the world and for our methods (which, for me, are simply theories about inquiry) are precisely the same: our theories are worthy of acceptance precisely because they work; our rules are worthy of acceptance because they have shown themselves able consistently to pick out theories which work with a pretty high degree of reliability. It’s all a matter of finding the right tools for the job. (Ibid., 105–106.)

Thus, as Laudan already argued in his 1984 book, theories, methodological rules, and the values or aims of science ought to be in harmony with one another. None of these is more fundamental than the others. As far as I can see, a moderate realism—even about truth, pragmatically understood—is compatible with this view, as clearly as it is with Dewey’s or Kuhn’s conceptions of science.

5.2.
For Kuhn and Putnam, as we have seen, the world as investigated by science is an elaborate human construction, not absolutely independent of paradigms, theories, descriptions, conceptualizations, or (scientific) practices and traditions. In this sense, but only in this sense, we “construct” the world. This neopragmatist and neo-Kantian view, which bears close resemblance to the constitution of empirical reality (or phenomena) Kant himself defended, is easily visible in what the classics of pragmatism, especially Dewey, said about scientific objects (cf. section 2 above). For example, Dewey remarked that thinking—or, more specifically, inquiry—does not set out with the object of knowledge already in place but ends up with it: the object is “an achieved triumph,” “something which the processes of inquiry and testing, that constitute thinking, themselves produce,” something essentially practical, as “it depends upon a specific kind of practice for its existence—for its existence as an object of knowledge” (Dewey 1916, 334).53 Furthermore, while the classifications of things we construct for our pragmatic purposes can in some sense be objectively evaluated, “a classification is not a bare transcript or duplicate of some finished and done-for arrangement pre-existing in nature,” but instead “a repertory of weapons for attack upon the future and the unknown,” classi-

53. For the development of Dewey’s “idealistic” view of the objects of knowledge as dependent on inquiry, see Shook 2000. See also the discussion in section 2 above.
fying things so as to promote “successful action for ends” (Dewey 1957 [1948], 153–154).

Similar formulations that today remind us of Kuhn’s or Laudan’s pragmatism can be found in James (1975 [1907], especially chap. 7), who repeatedly affirmed that the world is not ready-made but still “in the making,” constantly made and remade by us, by inquiring humans acting in the world. However, while Dewey’s pragmatist articulation of the way in which scientific objects are constructed in and through inquiry, or emerge out of inquiry, can usefully be compared with later accounts—especially within science and technology studies—about the “constructed” nature of scientific objects and facts, James was not as much interested in the scientific construction of reality. He focused, rather, on human beings’ individual constructions of meaningful objects, situations, and experiences through their moral and religious lives.

Insofar as a pragmatic realism is a possible outcome in Kuhn’s and Putnam’s case, it should be possible also in Dewey’s (and the other classical pragmatists’) case. But we cannot get rid of the tension I have analyzed. Recalling Quine’s pragmatist tension—that is, the fact that his “robust realism” is clearly a realistic account of mind-independent nature, yet his philosophy of science has often been criticized as antirealist, both in the sense of relativism and in the sense of instrumentalism—we ought to take seriously the claim, made recently by Rouse, that even Quine himself is actually guilty of “first-philosophical” metaphysical assumptions:

Quine argued that naturalizing epistemology abolished any sense of “first philosophy” that would take priority over the ongoing course of inquiry. [Instead] Quine offered an alternative candidate for first philosophy: an implicit metaphysics of nature as the already determined object of what natural science can be about, and the already unified subject of knowledge in the physical bodies of individual human organisms. What physical bodies, the physical world, and their causal interactions are like is to be determined by ongoing scientific inquiry. That these are physical bodies, interacting causally, and that the “subject” of knowledge is the individual organism to which the rest of the physical world is “external” have nevertheless been specified in advance as the prior conditions for epistemology having been naturalized. (Rouse 2002, 133.)

While I was a moment ago critical of Rouse’s reading of Kuhn, I fully approve of his criticism of Quine. The pragmatist (or the true naturalist), according to both Rouse and Putnam, should not engage in the kind of postulation of prior metaphysical conditions that Quine seems to engage
in. S/he should, rather, let the (scientific) practices through which we encounter the world we live in themselves determine the proper conditions and postulations that are needed for those encounters to make sense.\textsuperscript{54}

Drawing close to the conclusion of my survey, I must acknowledge the obvious similarities between Rouse’s (ibid.) views and the ones I am sketching here, without being able to do justice to Rouse’s extremely rich presentation, and understanding that Rouse may have his own reasons for not wanting to label his position “pragmatist” at all. It is, however, natural to end this survey by emphasizing the importance of Rouse’s work as one of the most recent manifestations of a full-blown pragmatic turn in the philosophy science. Indeed, a pragmatic attitude seems to be so obvious for him that he need not explicitly connect his views with the pragmatist tradition—which, given his obvious Deweyan overtones, is something he might have done already in his earlier, somewhat more “postmodern,” book (Rouse 1996; cf. Pihlström 1998, chap. 9).\textsuperscript{55}

Rouse argues for a naturalism that focuses on the notion of a scientific practice (instead of, say, representation or theory), and invokes scientific practices as “the basis for understanding normativity,” yet he understands practices as “not meaningful, socially and historically situated actions as

\textsuperscript{54} Other—to some extent pragmatist—variations of the view that scientific practices themselves, in all their multifariousness, should be taken “at face value” and that meta-level philosophical problematizations and interpretations of them should be avoided, include, e.g., Feyerabend 1993 [1975] and Fine 1996 [1986], 2004. Note that Fine, who hardly ever refers to pragmatism but who does subscribe to both a Deweyan “experimental point of view” (Fine 2004, 121) and a Feyerabendian “openness to many methods” (118), is Rorty’s 2004, 132 ff. favorite philosopher of science, which is hardly surprising. Just as Rorty 1991, 1998 opts for solidarity instead of truth, and for democracy instead of philosophy, Fine 2004, 127 redefines objectivity as “that which in the process of inquiry makes for trust in the outcome of inquiry,” thus characterizing objectivity as “trust-making” rather than “real-making.” Both Fine and Rorty clearly attempt to transcend the philosophical controversy of realism vs. antirealism altogether, arguing that it is a remnant of old-fashioned representationalism, which has not learned its Deweyan, Wittgensteinian, and Davidsonian lessons. Rorty 2004, 136–139 deplores the fact that Fine still speaks about ontological commitment (when saying that, according to his “natural ontological attitude,” or NOA, we should take the ontological commitments of science at face value), and suggests that he should endorse his critics’ claim that NOA amounts to a “preemption” of philosophy. See, however, Kukla 1994; Leplin 1997; Niiniluoto 1999, 18–20; Pihlström 1998, chap. 3; as well as Pihlström and Koskinen 2001, for some critical reflections on Fine. On one interpretation, Fine is just a realist in a relatively ordinary sense of the term—after all, his NOA urges us to accept the theoretical postulations of science at face value, without problematizing them philosophically.

\textsuperscript{55} For views in some ways close to Rouse’s antireductionist naturalism, see Westphal 2006. Westphal also defends, though with a Hegelian twist, a form of pragmatic realism, attacking, e.g., such empiricists as Quine and van Fraassen (see also his contributions to Westphal 1998).
opposed to inexorable natural processes” although “identifiable by their normative accountability rather than by any performative or dispositional regularity,” allowing us “to understand the modality of causal processes on the basis of the normativity of scientific practices rather than the reverse” (Rouse 2002, 12). He further writes: “Understanding discursive practices as causally intra-active [. . .] denies that one can intelligibly do metaphysics in the sense of saying what the world as a whole is like” (ibid., 292). This is well in line with the pragmatists’ anti-scientistic, yet (non-reductively) naturalist, argumentation. According to Rouse, our naturalism should not share the dreams of “supposedly naturalistic metaphysics (such as physicalism)” about “the possibility of representing the world in a language that is not itself Constitutively part of the world represented” (ibid.). This is because “discursively articulated scientifc understanding always intra-actively belongs to the world (‘how things are’) and cannot magically represent the world from outside” (ibid., 293). All of this, or nearly all of this, I find congenial to the kind of pragmatic naturalism that emerges from classical pragmatism, particularly from Dewey’s work.56

56. In a Deweyan tone of voice, Rouse 2002, 360 concludes: “[N]aturalism as I have articulated it does not appeal to an already established, determinate concept of nature or of science in order to settle supposedly philosophical questions scientifcally. It rather locates us in the midst of the scientifc and technological practices that continue to reshape what it is to be nature, and how we can understand ourselves and our possibilities as natural beings.” Most antinaturalists, Rouse tells us, join metaphysical naturalists in presuming that nature is anormative and in accepting the legitimacy and intelligibility of questions about what the source or ground of normative authority and force is, while they antinaturalistically deny that these questions can be adequately answered in naturalist terms, concluding that normativity must have a source in a non-naturalizable reason, social contract, etc. (351). Against such views, we must reject the presupposition that nature is anormative and the “demand for a fnal accounting of the source and ground of normative authority”; further, we should reject these assumptions “for fundamentally naturalistic reasons,” because they are arbitrary impositions on science (ibid.). In brief: “The effort to ‘naturalize’ semantifc, epistemic, or practical/political norms [. . .] illegitimately presumes that nature has already been ‘naturalized’ in the same way. To allow the naturalistic legacy to claim ns appropriately, I am arguing, we should recognize instead the normativity of nature, manifest in the possibilities expressible in part through scientifc practices” (27). This is not, of course, the proper place for a substantial investigation of Rouse’s excitingly reinterpreted naturalism, but his lengthy and detailed discussion of the “constitutive normativity” of materialized scientifc practices (cf. 13) and the implications this has for what naturalism is and ought to be obviously do deserve further scrutiny, especially from those working within a Deweyan pragmatic naturalism. Rouse further notes that “[t]he proponents of an unremitting naturalism and of a thoroughly humanist social constructivism can each plausibly claim to have left nothing out of their accounts—nothing, that is, except the entire world conceived in the opposing terms” (97). Thus, strong metaphysical naturalism (such as, perhaps, Quine’s) and “humanist social constructivism,” according to which even the physical world depends on our normative practices (which might be close to Kuhn’s and Putnam’s view, interpreted as “transcendental pragmatism” in section 4 above), are both unintelligi-
Recent Deweyan philosophy of science, as developed by John Shook (2002, 2003b), in particular, agrees in some interesting ways with Rouse’s naturalism. Where Shook differs from Rouse is his requirement that pragmatist philosophy of science must, above all, be empiricist: the object of scientific knowledge must be experienceable in some way, though not necessarily directly. However, like Rouse’s “scientific practice” philosophy, Shook’s Deweyan pragmatism is a naturalist position (though perhaps not exactly in Rouse’s sense), claiming that scientific knowledge is directed toward natural objects. Furthermore, Shook’s pragmatism is “productionist,” viewing those objects as “technologically created.” Yet, they are real objects; hence, pragmatism is a form of scientific realism. (Shook 2003b, 331.) What such a pragmatic realism denies is, first, “transcendent realism,” according to which scientific knowledge aims to describe a totally experience-transcendent reality; secondly, “scientific idealism,” which does not view scientific knowledge as a relation to naturally existing objects; and thirdly, “objectivism,” which claims science to be aiming at an understanding of reality in the absence of human intervention (ibid., 332). In these respects, Shook’s Deweyan realism is similar to Rouse’s but very different from, say, Leplin’s (1997) or Niiniluoto’s (1999) favorite forms of scientific realism, although Shook is obviously right to insist that his realism is more realistic than positivist instrumentalism or van Fraassen’s (1980) constructive empiricism, which too restrictively limit experimental evidence to directly observable infallible data (Shook 2003b, 342). As Shook summarizes his view,

[. . .] pragmatism offers an empirically naturalistic and moderately realistic philosophy of science. Pragmatism’s expansive concept of interactive evidence harmoniously complements a naturalism wary of the transcendent. Philosophy of science should embrace pragmatism’s view that the proper object of scientific knowledge is the technologically created natural object in human experience. Pragmatism offers the most realistic interpretation of the growth of
scientific knowledge that is consistent with both actual scientific practice and experimentally confirmed results. (Ibid., 343.)

The emphasis on technological creation or production should bring pragmatism into a natural dialogue with recent science and technology studies (see, e.g., Sismondo 2004), in which such a “productionist” attitude is often present without explicit pragmatist influences—and without detailed attention directed to the realism issue. Thus, “Deweyan” philosophy of science should have its future in the growing interdisciplinary work on science as embedded in its social and technological surroundings.

What slightly worries me, however, is not so much Shook’s demand that scientific objects be (indirectly) observable but his being relatively silent about what Rouse discusses at considerable length, namely, the normativity of scientific practices. By being (rightly) wary of the transcendent, Shook’s Deweyan pragmatic realism may end up rejecting the legitimate transcendental concern about the practice-embedded conditions for the possibility of science (of scientific knowledge and experience) that Kuhn—and, in his own way, Rouse—investigates. The pragmatist tradition, when carried forward in the philosophy of science, should definitely not forget such transcendental concerns (cf. Pihlström and Siitonen 2005), and this is one of the reasons why I have found both Kuhn’s and Putnam’s neo-Kantian forms of pragmatism highly promising, though not unproblematic (see section 4 above).

It remains a further task for pragmatist philosophers of science to investigate what, say, Rouse’s conception of scientific practices and the normativity of nature might ultimately come down to in explicitly or implicitly pragmatist terms, and how this kind of normativity could be invoked to constrain the “production” of scientific objects Shook regards as essential to a pragmatist account of science. And it is still an open issue exactly in what sense a form of scientific realism can be based on these ideas. The only thing that is clear is that pragmatism, as such, is no enemy of (moderate) scientific realism.

6. Conclusion
It is through the kind of complex developments I have highly selectively reviewed that we ought to, again and again, take a fresh look at the pragmatist classics—especially Dewey—in order to evaluate their role as the (largely implicit) background figures of today’s “pragmatist philosophy of science.” It was Kuhn in particular, I suggest, who smuggled pragmatist ideas into the philosophy of science, making possible a (Deweyan-like) historicist pragmatic naturalism, a view in the end much more pragmatist than Quine’s. In the field today, thinkers like Rouse carry this task for-
ward, usually without explicitly linking their views with the pragmatist tradition. This makes Rouse’s take on Kuhn somewhat unpragmatist (as we saw), which is unfortunate, given his otherwise strong entitlement to the label of pragmatism, indeed his strong potential for continuing the pragmatist tradition in contemporary philosophy of science. Others, like Laudan and of course Shook, are more explicitly pragmatists, though in Laudan’s case the possibility of defending a pragmatic realism may in the end be sacrificed (cf. Niiniluoto 1999)—depending on how his concept of “problem-solving” is interpreted.

The fact that the American tradition of pragmatism suffered a period of decline, roughly between the 1930s and the 1960s, as logical empiricism arrived and was established in the United States, being soon overshadowed by scientific realism and by Kuhnian constructivism, might be explained not only by reasons internal to philosophy but also by external, social ones: perhaps the fact that pragmatism, Dewey’s in particular, was a form of “social philosophy” made it suspicious in the post-World-War-II strongly anticommunist America (cf. Giere 1996, 346–348). The new rise of pragmatism since the 1970s might, conversely, be explained by the fact that the academic community has become increasingly dissatisfied with the applications of strictly scientific modes of thought and principles of rationality in human life at large. Pragmatists have traditionally argued that we can never have enough rational, critical discussion (both within and out of science) of not only the means by which to pursue our ends but of the ends themselves, and this seems to be precisely what is urgently needed in the current situation, natural and political.

The pragmatist tradition in the philosophy of science, I conclude, provides an example of the complexities involved in any attempt to write the history of this discipline, a case in which several issues are intertwined. These include the issues of (i) realism vs. antirealism (that is, in their various forms, instrumentalism, relativism, constructivism, idealism, etc.); (ii) logical vs. socio-historical and practice-oriented approaches to science in general and to theory-choice and scientific change in particular (as manifested, for instance, in the opposition between traditional scientific realists and their Kuhnian opponents); and (iii) “hard” vs. “soft” naturalism (as epitomized in the conflicting accounts of naturalism we find in Quine and Rouse). A reconsideration of pragmatism as an identifiable, albeit somewhat indeterminate and inevitably open, tradition in the philosophy of science may thus deepen our understanding of the historical transformations of these—and many other—issues. There is no reason why the historian of the philosophy of science should treat this (or any) tradition as fixed and closed once and for all; keeping the tradition open for constant reevaluation and redescription is itself a most pragmatic attitude.
So how to write the history of pragmatist philosophy of science? It is easier to say, on the basis of the somewhat scattered and lengthy reflections I have offered, how it should not be written. It should not be assumed that pragmatism is, on the whole or inevitably, committed to some particular standard views, such as instrumentalism—or realism, for that matter. The pragmatists’ problems and tensions surrounding the realism issue are much more complicated; none of the pragmatists, classical or modern, can be said to have simply defined their positions in terms of traditional oppositions, such as realism vs. antirealism. But precisely for this reason tensions do remain, and it should not be assumed that pragmatism can avoid tackling the disputes between realism and its alternatives, or the various metaphysical and epistemological problems connected with these disputes. These classical issues are, then, well and alive in pragmatism, despite some self-styled pragmatists’ (e.g., Rorty 1991, 1998, 2004; Fine 1996 [1986], 2004) claims to have overcome the entire realism debate. If anything, I hope the present essay has contributed to our need to see, pace Rorty and Fine, the realism issue as a genuinely pragmatic, and even pragmatist, one.

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