On McMullin's Appreciation of Realism Concerning the Sciences*

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Constructive empiricism is indeed set squarely within a common sense realism that was foreign to much of the empiricist tradition. But I do not see this common sense realism, which I take myself to share with many scientific realists, as harboring or leading to scientific realism. That is in part because of the way I separate the opposition between empiricist and realist understanding of science from other issues that divide us in epistemology. This discussion brought to light our quite different conceptions of what is at issue between empiricists and realists in this area. After a response to McMullin's critique, however, I will be especially concerned to respond to his challenging proposal for a shift in the debate over realism concerning the sciences.

1. Introduction. Ernan McMullin appreciates that constructive empiricism is set squarely within a common sense realism that was foreign to much of the empiricist tradition. This common sense realism I take myself to share with most (even if not all) participants in recent debates over scientific realism. I will comment on this common sense realism first of all, and then turn to McMullin's fair and detailed critique—to which this short reply will not do sufficient justice. But beyond this I wish to respond especially to his challenging proposal for a shift in the debate over realism concerning the sciences.

2. What Is Not Stated in a Philosophical View. Every philosophical view takes its basis in a form of discourse that it trusts for that role. That

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discourse purports to be a common basis for participants in the dialogues in which that view is proposed and defended. If that purported common access is missing, the philosopher is indeed engaged in the art of speaking nonsense in language expressly designed for that purpose.

The common basis I assume is language in which reference is unproblematic to trees and mountains, people and books, to lightning and car crashes, as well as to the processes of aging, burning, and flooding. To trust such discourse as our basis does not set it beyond critique. To see philosophy as always conducted in a trusted language-in-use does not imply seeing it as oblivious of the language it lives in. But in this, as elsewhere, we always start from where we are; we can't step out of where we are into a presuppositionless discourse any more than into a view from nowhere.

This trusting start, of us mariners who repair our boat on the high seas, is precisely what is not made explicit when we state our positions. To some extent, the lack of explicitness is inevitable. If we do pay attention to our own language in use, and begin to make its features more explicit, we still do that in a part of this very discourse. Thus we continue to rely on its trustworthiness implicitly in that very act. We can bring only so much of what we are doing to light, for this bringing to light is, after all, a part of what we are doing.

Within all of this that I take for granted, the constructive empiricism I advocate is not a general philosophical position (though I hope to be developing that as well) but an answer only to the question, *What is science?* This question is to be understood as of a piece with questions raised elsewhere in philosophy such as, *What is art? What is law? What is religion? What is mathematics?* The term "science" has several roles in our language; sometimes it denotes an activity, sometimes a product of that activity. As an activity I try to characterize it in terms of its aim, which is identified by its main or basic criterion of success. The product of the activity I take to be mainly theories, which provide models as candidate representations of nature. The criterion of success is of course to be related to the main terms in which that product is to be assessed.[1] I take the bottom line criterion of success in science to be empirical adequacy (see further below), and see support for this in scientific practice as evidenced both in its history of theory replacement and in the contemporary technical journal literature.[2] Description and assessment of both scientific activity and scientific

1. I cannot blithely continue to use "criterion" without noting differences in our usage of this term. I use "criterion" without implication of e.g., human applicability in finitely many steps. More on the terminological differences between us below.

2. That simplistic statement I would of course improve on if the occasion allowed for it. For one direction of improvement that I would wish to take into account here see Suppe 1993.

product, as I characterize them, are carried out within that common sense realist discourse—the same that I designated above as the trusted basic discourse in which constructive empiricism is formulated as well.

All of this is quite clear to McMullin, but as he fairly and rightly points out, the main terms used soon become terms of art. My use of them differs from other uses that may be more familiar in earlier philosophical contexts. He clears up, very effectively, some of the confusions that such differences can engender. I will add some comments to his clarifications. But I do not want this brush clearing to distract us from the main philosophical issues that McMullin raises for us.

3. McMullin's Challenges. The most important part of McMullin's paper, for me, is the constructive suggestion which emerges near the end. That suggestion, if accepted, would shift much of the debate over scientific realism from epistemology to ontology. I will have to approach this slowly, as he does also, through the earlier parts of his paper. But I should say at once that I too avidly seek a shift in these discussions away from epistemology. McMullin shows effectively the impasses we land in through the pursuit of certain traditional epistemological problems in this context. But the specific way I wish to shift is not in the same direction as he does.

The first six sections of McMullin's paper can be viewed as consisting of two parts. The first is a classification of Constructive Empiricism in relation to more realist and less realist views of science. This part brings to light some disturbing disparities both in conception and in terminology. It would be ill-advised for either of us to continue in usage at odds with the other's, unless the differences are clearly spelled at and made salient. (We will both resist, I think, the alternative of simply surrendering crucial terms like "criterion" and "empirical adequacy".) The second part is a sustained and acutely critical challenge to my views on epistemology.

The two issues—views of science and views on epistemology—are connected for both of us, *but very differently connected.* That difference in connection has undoubtedly been a main obstacle in our dialogue. It does not derive simply from differences in terminology but from basic differences in conviction and approach. To put the matter very briefly, I see core realist and anti-realist views of science as answers to "What *is* science?" which are logically independent of any epistemology. In this sense one could have an anti-realist view of science while believing in the complete literal truth of all currently accepted science. And similarly one could have a realist view of science while maintaining that success in the scientific enterprise so far has been mainly illusory and is perhaps forever beyond human reach. While such combinations are logically consistent, there is of course a reason why they sound paradoxical. The reason is that in the presence of certain epistemological claims the debates over scientific re-

alism would lose all relevance and the questions at issue become moot. So despite the logical separation on which I continue to insist, I must take McMullin's challenge in epistemology very seriously.

Besides these two main parts there is McMullin's highly interesting constructive suggestion at the end. In the last part of my response I'll be receptive to his suggestion but in the context of a reconception of ontology as different as are our conceptions of epistemology.

4. Clarification of Terms—and Separation of Issues. Older points of view may be rejected in part because they either lack certain distinctions or obscure them. It is not easy to reject a previous philosophical view while staying entirely in its own language and the ways in which it prefers to present the issues. So I can see the disparity between, for example, my meaning for "accept" and earlier or other more common usage. In most uses, I will certainly grant, the term "accept" may indicate a weaker doxastic attitude than belief that the theory is successful by *any* measure. But what is important is that we should have a typology for the various epistemic, doxastic, and practical alternatives that endorse a theory in one way or other. I classify the attitudes actually encountered as qualified, partial, and tentative forms of belief and of practical commitment, reserving the short words "accept" and "believe" for the unqualified extremes.

So the use I give "accept" is explicitly stipulated: to accept a theory is to believe that it is empirically adequate, while also taking on certain practical commitments concerning its use and authority in further theorizing and application. I contrast this with belief that the theory is true. In the case of what McMullin calls O-theories, acceptance does involve belief that the theory is true, because for an O-theory truth and empirical adequacy coincide, by definition. Even there I see a contrast, for belief does not automatically bring such practical commitments with it. It seems to me that realists often rely on the idea that believing a theory brings along with it those practical commitments. I see no basis for that. In any case, acceptance—in my use of the terms—does always involve belief that the theory is true *in part.*

Before turning to the no less contentious "empirically adequate", I had better address "true". While a word in common use, it has been exposed to much stormy philosophical weather. I insist in this context on a naive, pre-theoretical understanding of "true" and "truth". A theory says that certain entities are real precisely if it implies statements to that effect. The entities in question are real precisely if those statements are true. Nor do I distinguish here between "there are Xs" and "Xs are real". To say what is true is to say, of what is, that it is and, of what is not, that it is not—I add no metaphysical or epistemological burdens to this venerable point.

What of "empirically adequate" then? Now we come not just to a clar-

ification of terms but to a crucial separation of issues if we are to understand each other. Truth and falsity are properties that a theory can have whether we know it or not, and whether we have any inkling or indication or hunch as to which it is—even independently of whether the theory has ever been formulated in human tongue. *So it is with empirical adequacy, as I use that term.* There are in the world, in the full course of its history, past, present and future, many short and long lived objects, events, and processes which together constitute nature.[3] Some of those objects, events, and processes are observable; that depends on what they are like. It depends certainly also on our limitations, but does not depend on whether we have any knowledge of those entities or even any indication of their existence. They constitute the observable part of nature. If that observable part of nature fits into some model which is in accord with a given theory then that theory is *empirically adequate;* and otherwise it is not. With "empirically adequate" understood in my terms, a theory which postulates only observable entities is true if it is empirically adequate. If it postulates also unobservable entities then it can be empirically adequate though false overall.

What I just wrote is a stipulation concerning the phrase "empirically adequate". I need a term for the concept, and this choice seemed the best at the time. The notion of adequacy here is, as McMullin rightly notes, devoid of connections with such topics in epistemology as "belief", "know", "evidence", and the like. There is no logical connection. But this brings us to the much more serious difference between McMullin and myself in what we depict as central and as peripheral to the realist debate.

I regard the main issue of *what science is* as entirely separate from *any* question as to what we are entitled to believe. Hence I strongly disagree with McMullin's "What the two sides dispute is what people in general (and not just scientists) are *entitled* to believe. . . ." And although I agree with his contention that "[s]cientific realism bears primarily on the reality of the theoretical entities postulated by the theory", I am taken aback by seeing the question "Does the success of electron theory warrant a qualified belief in the existence of electrons?" immediately added to that contention. Whether we had best understand science as aiming to provide us with true theories or with empirically adequate theories is a question distinct from whether there is any warrant to believe that it achieves that aim, wholly or partially, in general or in specific cases.

This separation of issues should also strongly condition our response to Sections 3 and 4 of McMullin's paper. There is much here that I want to applaud as greatly clarifying the terms of debate. Theoretical entities

3. Although each of the main terms in this sentence has been subject to metaphysical theorizing I trust that they also have an ordinary common use.

are entities postulated by a theory, in the course of constructing a theory, or for the sake of theory. This term "theoretical entity" does not denote a division in nature, but a distinction relative to our intellectual activity. Many entities postulated in the history of science have been observable, in some cases turn out to have been previously observed, in other cases are observed later, though some are clearly beyond our actual reach.

This should highlight the important difference between what the observable part of nature is in fact and what there is about it to which we have good epistemic access. To observe something does not guarantee the gleaning of any non-trivial amount of information about it. In the dark I trip over a raccoon; I take it to be my neighbor's cat. Certainly I observed the raccoon, but I was quite wrong about what this observed animal was like. I still took it correctly to be an animal, but I might equally well have taken it to be the neighbor's children's teddy bear, or something still further removed from what it was. Priestley, we say, did isolate and observe oxygen, but we give him little credit for he formed no good idea of what it was. Does some thing's being observable imply that under the right conditions we can gather accurate or true information about it by observation? Yes, that is so, for in observation we function as our own measuring instruments. But there is no implication in the mere fact of observability, all by itself, about how much or how precise or accurate the gatherable information will be.

Besides the distinction between what is observable and what is sufficiently accessible to us, there is another distinction that I want to observe. Under what conditions is a theory valuable to us? It might just happen that a certain theory is true, but either so complex that we cannot calculate usable predictions, or pertain to features of the cosmos that we cannot investigate. Such a theory might not have much value to us, as part of science. (It might, though; perhaps it is providing a theoretical bridge between parts that are more directly useful, for example. But it might not.) So 'true' does not imply 'valuable'; neither does 'empirically adequate'. So meeting the basic, bottom line criterion of success does not guarantee that the theory is of value to us—in general, at least, such more anthropocentric virtues as humanly possible testability in a sufficiently short span of time may also be required. I give these other possible requirements as examples only: what is of value to us depends on what values we have, but I think that whatever values we have we can conceive of examples that would make this distinction salient as well.

5. The Challenge in Epistemology. Let us come back now to McMullin's "What the two sides dispute is what people in general (and not just scientists) are *entitled* to believe. . . ." Despite the logical separation of issues on which I have been insisting here, I must agree of course that much

discourse in the realist/anti-realist debates over science has been precisely that. To do justice to the part of McMullin's paper in which he addresses the epistemological topics, including retroduction and the ideas that come under the heading of "inference to the best explanation" might require a paper twice as long as his. What I will do instead, falling short of the ideal, is to focus on the question of whether, how, or to what extent various theses in epistemology—stated in very general terms—could undermine constructive empiricism.

There are two points, however, that have seemed to me to obstruct some of our dialogue, and that I should note in a preliminary way. The first concerns how we relate to traditional epistemology (neither of us being exactly conservative in this area!). The second concerns how we see the epistemological landscape overall.

Informally I constantly use such traditional epistemological terms as "warrant", "reason", and "entitled" in our discussions. But I understand them as pertaining specifically to dialogue and deriving their content and significance from their role in dialogue. So, for example, I do not think of a person's beliefs as arranged in a sort of lattice or hierarchy with "is a reason for" connections between the elements. Instead, I notice that if someone asks me for reasons for, e.g., my belief that something is the case, I will try to offer things (which I do myself believe) that will make a difference to him or her, that will tend to convince or at least be acknowledged as tending to do so. Technically speaking, it would be best if I could find antecedents for conditionals that this other person believes (so s/he could apply modus ponens) or point to high conditional probabilities that are part of his or her opinion (with an invitation to conditionalize). Notice that the status of *reason* does not derive from connections in my own opinion, taken in and by itself, but from my guesses as to my interlocutor's opinion and its relation to mine.

So I bridle at the introduction of "entitle" and "warrant" at certain points in our debate. The issue for me in a debate about what science is can't very well be whether we have warrant, reason, or entitlement to believe the theories that science has been producing for us. For these words import into the discussion a brand of epistemology—which I call "defensive epistemology"—that seems to me to bias the discussion. It is certainly a brand of epistemology deeply at odds with what I called the "new epistemology" in *Laws and Symmetry*.[4] Since I dispute the role of such terms as "entitled" in epistemology, I am not placing myself in that debate by asserting that we are or are not entitled to believe certain things under

4. This *probabilist* epistemology (with a *voluntarist* twist) has both more and less technical aspects, and is certainly by no means as yet in a finished state. See further van Fraassen 2002.

certain conditions. Casting questions about the rational management of our opinion in terms of entitlement, warrant, justification, and the like—a projection of a certain class of moral categories onto the epistemological terrain—seems to me to be a mistake.

Certainly I think of epistemology as mainly concerned with *normative* questions—or at least with questions about normative questions pertaining to opinion and belief. How shall we understand assessment of the extent to which given opinion is in accord with the facts? Or assessment of how coherent this opinion is? Or assessment of policies for *rational management* of our opinion in response to experience and to new ideas and new critique? But in all this I equate rationality not with what we *ought to believe* but what we are *rationally permitted* to believe. Forming a belief which does not land us in incoherence, in a fashion that does not amount to self-sabotage by our own lights, I consider rational, no matter how audacious or far-fetched it may be by standards of conventional wisdom.

For now I wish only to insist on this separation of issues. This is still only a preliminary to the response required by McMullin's challenge in epistemology. The second preliminary pertains to the way McMullin views the landscape in which to locate constructive empiricism, which comes to light in his general discussion of anti-realism and skepticism.

The *global anti-realism* that McMullin describes is connected with a certain view of science, and perhaps includes that view as part, but is mainly a skeptical position with respect to even the accumulation of empirical information to which science lays claim. I won't dispute McMullin's attributions of (versions of) such global anti-realism. Others may defend themselves. As he characterizes that sort of position, its mainstay is what seems to me a clearly sophistical argument. In any case, as McMullin indicates, I subscribe to no such global anti-realism nor, for that matter, to the sort of skepticism it can bring along.

Nevertheless I understand why McMullin would use this as a foil against which to compare and contrast my position. For my position entails that the defining criterion of success is empirical adequacy, and that acceptance of a theory (as successful) involves no belief that the theory is more than empirically adequate. Hence such acceptance does not involve belief in the reality of any postulated unobservable entity. That can certainly be classified as a skeptical position; but it is not anywhere near the sort of debilitating skepticism whose specter tends still to hover over traditional courses and anthologies in the theory of knowledge.[5]

5. It seems to me one of the virtues of the 'new epistemology' is that it delivers us from such skeptical threats (see van Fraassen 1989, ch. 7, section 6).

6. How Could Positions in Epistemology Undermine Our Debate? So far then I have laid out, for my own part, a view of what science is, and of what acceptance is. By itself it implies nothing about reasons for, rationality of, or risk taken, in either acceptance or belief. (It certainly does not entail that we *ought* to be agnostic about the truth of our favorite scientific theories, even those which go farthest beyond the observable, nor that it would be *irrational* for an empiricist to believe such a theory.)[6] To dispute this view of science one must either argue that:

> **I.** Science has other or additional internal independent criteria of success (that is, not just criteria that concern means to the end of success with respect to empirical adequacy),

or else argue that:

> **II.** To accept a scientific theory involves belief going beyond belief that it is successful by scientific criteria alone.

It is quite clear from various parts of the article that McMullin does want to maintain Thesis I. But in view of how he raises his challenge in Section 4, it seems best here to scrutinize carefully how epistemological questions are both connected with and yet separable from Theses I and II.

There would be simply no point in arguing about these two Theses if certain epistemological points could be established. Examples would be:

(1) Belief that a theory is empirically adequate cannot be rationally combined with doubt that it is true.
(2) It is irrational
  (2a) to believe more than is logically implied by one's evidence, or
  (2b) to believe anything unless it is something that one definitely ought to believe.

While (1) is clearly to be accepted for what McMullin calls O-theories (for which empirical adequacy and truth coincide), constructive empiricism would become pretty well an empty gesture if (1) held for theories in general. Arguments for (1) existed in the literature at a time when empirical adequacy was habitually equated, Carnap and Hempel style, with the truth of those theorems that can be expressed in "observation" vocabulary. The idea was that if two theories differ in any way at all, regardless of how far afield they go beyond the observable, they will have some differences in consequences which can be expressed in that vocabulary. However that may be, and whatever we think of those long-ago arguments, with the sense that "empirical adequacy" has for me, point (1) is simply implausible.

6. This is made very clear by Teller 2001.

Of course, if (1) were accepted, there would be no point in arguing over constructive empiricism—that view would not be incoherent, but it would collapse into a merely academic distinction.

I don't suppose that any of us—since W. K. Clifford—would subscribe to (2a), but there may be some temptation to hold (2b). If (2b) were accepted, the center of gravity of the discussion would certainly be in epistemology. For then the crucial question would simply be:

> *Are there certain theories that we ought to believe although they go beyond all the empirical evidence we can have?*

Note well: since I do not hold (2b), this question is not to the point.[7] However, that does not let me off the hook with respect to a somewhat weaker putative point:

(3) There are *strong* forms of reasoning or theory assessment leading to consequences that we ought to believe but go beyond the empirical evidence; and in some cases those further beliefs, that we ought to have, include belief in the reality of unobservable entities.[8]

If this point is accepted, then does constructive empiricism have the rug pulled from under it, in the way that acceptance of point (1) would do?

Actually no, not necessarily, or at least not obviously so.[9] Much would depend on how much those forms of reasoning or theory assessment could give us. Whatever they be, we could define e.g. "extended empirical adequacy" in some such way as this:

> A theory has *extended empirical adequacy* if and only if it is empirically adequate and in addition if all consequences (drawn by those strong

7. Suppose the answer to this question were to be conclusively shown to be *Yes.* Then a constructive empiricist would not need to give up his or her view of science—but would have to conclude that what we ought to believe in this case is more than that the science in question is successful. To see this point in context one should imagine various cases here: not only the case in which the existence of, say, phlogiston or the neutrino is demonstrated, but ones in which the reality of universals or of physical probability is shown. See further the discussion of 'extended empirical adequacy' below.

8. As McMullin notes, I have argued that 'strong' forms of reasoning which purport to play this role lead to incoherence, if formulated with sufficient precision (van Fraassen 1989, ch. 7, section 4). There is clearly a great deal more to discuss with respect to McMullin's conception of retroductive and other non-deductive forms of reasoning in the sciences. I would find it hard to do so without including a much larger exposition of the differences between our approaches to epistemology. In addition to ch. 7 of van Fraassen 1989, see van Fraassen 2000.

9. I do not accept (3), but the point I wish to make is that it is considerably weaker than (1) or (2b), and would still allow for an amended constructive empiricism, equally opposed to scientific realism.

forms of reasoning) of the statement that it is empirically adequate are true.

This leaves open that there is still a line to be drawn between this extended empirical adequacy and truth overall. Thus, provided this extended empirical adequacy is still logically compatible with falsity of the theory as a whole, we will automatically have a *modified constructive empiricism* which views science as aiming at theories with extended empirical adequacy. The main lines of debate might not be shifted very far at all, unless those additional forms of reasoning were very powerful. More to the point: the form of the debate would not change; the arguments between empiricists and realists would take the same form, but refer to a different dividing line.[10]

7. A Happy Ending? Under this heading, at the conclusion of his paper, McMullin makes a positive suggestion for a quite differently modified empiricist position. "Things that we can see and touch belong to intuitively comfortable categories, like 'substance', 'individual', 'corpuscle', in earlier ontologies" he suggests. Agnosticism with respect to certain entities postulated for the sake of theory may be based on the impossibility of fitting them into such familiar ontological categories. Unobservability is a secondary concern, though—surely not coincidentally—such ontologically difficult-to-place entities provide the paradigm example of the in-principle unobservable.

This suggestion would shift the discussion resolutely from strife within epistemology to ontology. The modified empiricism suggested here would presumably view science as aiming to be adequate to those parts of the world that fit the categories in question. Models might have many elements which demonstrably could not correspond to those parts, and acceptance of the theory would permit withholding belief that for those parts there are (using Einstein's phrase) any corresponding elements of reality. The cost, from an empiricist point of view, would be to grant legitimacy to ontology, that is, to a sort of philosophical enterprise that falls squarely within traditional metaphysics.

What immediate motivation could one have for adopting this modification? I can imagine the following circumstances. Suppose that someone had embraced an empiricist or other non-realist view concerning science *on the basis of* the epistemic inaccessibility of the unobservable. Suppose in addition that realist arguments had convinced that person that this basis is a shaky one, perhaps because there are rationally compelling or strongly

10. This point is of course similar to one that has been made a number of times in response to suggestions that the notion of observable be broadened to include e.g. detectability by means of an optical (or electron) microscope.

inclining forms of inference that make some of the unobservable epistemically accessible after all. Such an adherent to constructive empiricism would certainly have reason to welcome McMullin's suggestion. But as should be clear by now, I do not advocate constructive empiricism on the basis of the epistemic inaccessibility of the unobservable. I do not see the controversy between empiricist and realist in the philosophy of science in the first instance as a dispute over how much to believe. To repeat—I hope not once too often—I place that controversy instead in the wider context of answering such questions as "What is science?", "What is art?", "What is religion?", "What is law?" and the most salient *pièce de résistance* is the problem of whether the answer must or need not require recourse to metaphysics.

But I do not want to respond to this positive, constructive suggestion with mere negatives. McMullin's happy ending scenario is attractive exactly because it offers an area of collaboration for empiricist and realist. Both must after all confront the fragile or even dubious intelligibility of what McMullin calls "U-theories"—of modern and contemporary sciences—which depict a world unimaginably different from the manifest image displayed in experience.

Here is what I would like to propose in response. My proposal intends to acknowledge, gratefully, how much common work is already being done by realists and empiricists in effect. Realists are engaged in ontology: do the categories of substance, accident, essence, haecceity, causality, determinism, and determinacy apply to certain (putative) sectors of reality? Do the concepts introduced in the course of theorizing, and the instantiation of those concepts postulated by theory, align well with these and like categories? In reply, they analyze the theories in question while also submitting those categories to analytical scrutiny and even revision. As examples I would mention the ingress of possible worlds in many-world interpretations, but also analyses of causality and violations of the causal order, as well as the role of chance which is apparently ineliminable from modern physics. Further examples relating to haecceity and individuation have been ubiquitous in the interpretation of quantum mechanics and quantum field theory.

While all that fits in well with traditional ontology, and empiricists cannot (as I see it) in good conscience participate in it, there is *an empiricist take on the same philosophical labor.* Science is an activity of representation, and its intellectual products are representations of (parts of) nature. The conclusion we must reach from modern science and its mathematical turn is that our means of representation go far beyond the reach of imagination. Representations are images, if you like—if you are willing to count Hilbert spaces, non-Abelian groups, and differentiable manifolds as images—but they are not within the scope of what imagination can hold.

McMullin emphasizes this as well in his concluding section, pointing to Bohr's model of the atom as already making use "of entities we cannot even imagine, in the usual sense of 'imagine'". Things that we can imagine "belong to intuitively comfortable categories" or near enough—but not so what is ostensibly represented in scientific models in general. Indeed, McMullin has himself emphasized this challenge to the philosophical appreciation of physics, for realists and for empiricists alike, in his historical studies as well as in his papers on scientific realism, pointing out that this break with the imaginable is at its clearest in mechanics, from Newton's to Bohr's and Heisenberg's (McMullin 1984, 1987, 1994).

How does the empiricist respond to this challenge? I submit that the empiricist response in fact opens up an area of philosophical inquiry where realists and empiricists can work in a collaboration equally valuable to both sides. For when we analyze the scientific image *as representation* all those questions posed in ontology do arise—such as questions about substance, individuation, causality, haecceity—but in a new key. They are not just *verbally* the same questions, for the concepts used belong to the same conceptual framework drawn from metaphysics. But on empiricist lips they are questions *not about nature, but about our representation of nature.* Empiricist philosophy of science revamps and relocates those metaphysical questions, giving them a distinctly different (though structurally similar) content, namely as questions about *nature as represented,* not about nature.

This is however sufficient for a fruitful feedback between realist and empiricist investigations of physical theory; and of that we have ample evidence already in the philosophy of physics. But there is more to this. All the participants in these sorts of studies have in recent decades given increasing importance to the pragmatic aspects of scientific representation.[11] Looking back to McMullin's well known "Case for Scientific Realism", we can see him as insisting on this pragmatic aspect at a very early stage in the recent debates, at the same time initiating the trend (much more evident now than then) to connect scientific with artistic representation:

> the model functions somewhat as a metaphor does in language. The poet uses a metaphor . . . as a means of expressing a complex thought. A good metaphor has its own sort of precision, as any poet will tell you. . . . The poet who is developing a metaphor is led by suggestion, not by implication; the reader of the poem queries the metaphor and

11. As a telling example of work on scientific representation that provides a meeting ground for empiricists and realists in the philosophy of science I can point, for example, to the papers by Steven French, Ron Giere, Mary Morgan, Mauricio Suarez, Andrea Woody, and myself at PSA 2002.

searches among its many resonances for the ones that seem best to bear insight.

The good model has something of this metaphoric power. (McMullin 1984, 31–32)[12]

So perhaps McMullin and I can agree on the following: it is time to shunt the traditional debates over scientific realism to a back corner, and to concentrate on those common (though differently viewed) problems that vex the understanding of specific scientific theories. Indeed this may be just the practical side of McMullin's friendly suggestion. For the scientific realist confronts conceptual challenges in recent U-theories as much, and in much the same way, as does the empiricist.

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12. For a detailed elaboration on this metaphorical (hence, pragmatic) aspect of scientific representation with reference to the Bohr model of the atom see McMullin 1968.