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PIECEMEAL REALISM*

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> *No matter how determinedly one flees a country, one is obliged to take along some hand luggage.*
>
> (Salman Rushdie 1983, p. 35.)

Faced with realist-resistant sciences and the no-nonsense attitude of the times realism has moved away from the rather grandiose program that had traditionally been characteristic of its school. The objective of the shift seems to be to protect some doctrine still worthy of the "realist" name. The strategy is to relocate the school to where conditions seem optimal for its defense, and then to insinuate that the case for such a " piecemeal realism" could be made elsewhere too, were there but world enough and time. The burden of this paper is to examine this piecemeal approach and to show why, despite the relocation, it cannot escape the difficulties of its grander cousins. For that purpose I begin with some brief historical reminders, and with a quick review of the state of the argument before realism went to pieces. This will help us see what has been abandoned in realism's flight, and what baggage still remains.

1. Some Prehistory.

Realism, and the issues it raises, go all the way back. I will only pick up the story in the seventeenth century, the era that marks the rise of modern science. The articulation of the method of hypotheses by the great thinkers of that era (Bacon, Descartes, Galileo, Huyghens, Leibniz, Newton, Pascal, etc.) posed questions over the reliability of the method as a source of truth, questions that came to a head in the following centuries side by side with the development of theoretical physics itself. Such questions were at issue in the subsequent discussions of theories of combustion (phlogiston), of heat (caloric), fluid theories of electricity, theories of the ether and light, and finally in treatments of the chemical atom and the kinetic picture of matter. From the beginning, I would say, these discussions had a dual character. On the one hand they were concerned with questions of evidence, in particular with the epistemological question of the extent to which the explanatory and predictive success of a hypothesis (or theory) justified belief in the truth of the hypothesis. On the other hand they were equally concerned with the metaphysical issue of the character of the objects studied by science; i.e., with the nature of heat, electricity, light and matter. Husserl (1970) points to Galileo as the culprit who began a dubious objectification of nature. Dubious or not, Galileo's methods did raise the metaphysical question over the ideality of the objects of scientific thought, precisely the issue which (focused on space, time, and motion) pitted Newton against Leibniz. What was at stake (and, incidentally, well before Kant) was the question of the mind-independence (or objectivity) of the scientific objects. Thus realism has always had a double aspect: epistemological-cum-metaphysical. To put it in modern terms, epistemologically, realism vies with instrumentalism over the reach of evidence and, metaphysically, it contends with various constructivisms over the mind-independent character of the objects of knowledge.

In the early years of this century, especially with the success and acceptance of the kinetic-molecular theory, realism was thought to win out over instrumentalism on the issue of the reliability of the method of hypothesis in leading to truth. The issue of mind-independence (or the objectivity of the "external world"), however, was by no means settled. Indeed in the 1920s, with the interpretive problems set by the quantum theory, that issue became embarrassingly acute for realism. As Niels Bohr, whose views still dominate in this area, described it

> The discovery of the quantum of action shows us, in fact, not only the natural limitation of classical physics, but, by throwing new light upon the old philosophical problem of [the] objective existence of phenomena independently of our observation, confronts us with a situation hitherto unknown in natural science. The limit, ... thus imposed upon us, of the possibility of speaking about phenomena as existing objectively finds its expression, as far as we can judge, just in the formulation of quantum mechanics. (Bohr 1934, p.115.)

In the pre-war period the flourishing of the quantum theory and the antimetaphysical atmosphere set by neo-positivism made discussions of realism unfashionable, at least among philosophers of science. In the post-war period, however, the topic came back, impelled no doubt by the impact of post-war technology on life and culture in general. Wilfred Sellars (1963), J.C.C. Smart (1963) and Hilary Putnam (1975) revived a kind of Moore-type argument in support of realism, one that had been used effectively by realists in the scientific community (like Max Planck and Albert Einstein) in the early years of the century in the debate over the reality of molecules. In outline the argument went like this.

(1) Just assume realism as the obvious attitude toward middle sized objects -- the tables and chairs and rocks of our everyday world. (Surely, only the fool has said in his heart that rocks and chairs are not objectively real!)

(2) Count the increasing success of science, witnessed by its technological applications, as giving a rationale for belief in the truth of the success-generating hypotheses and theories. (How else to comprehend that success ?)

(3) Finally, question whether the theoretical entities posited in the hypotheses and theories ought to be treated as less real, or otherwise different in character, from the middle sized objects with which we, and theorizing, began. (Parity or fairness to unobservables!)

2. The Explanationist Defense.

The line of argument sketched above, or some variation of it, was dubbed the "wouldn't it be a miracle" argument because unless one conceded the grounding of truth in scientific success, as suggested by (2), that success would seem unexplained, and hence "miraculous." Richard Boyd (1981) followed out this line of thought with considerable care and imagination. Taking as his starting point the 1960s Kuhnian conception of science as a highly theory-laden enterprise, Boyd refocused the argument on the success of scientific *methods*, and sharpened the question as follows. Boyd asks why the *theory laden* methods of science yield the high degree of instrumental success that they in fact do. And he answers it by saying that only the truth, or the approximate truth of our scientific theories and hypotheses, explains why a theory driven methodology should succeed as it does. I shall refer to Boyd's version, and variations on that, as the *explanationist defense* of realism.

It will be useful to have an overview of the responses one might make to such a defense.

First, the why question might simply be rejected. For one might not accept the presupposition that the instrumental success of science needs (or "requires") explanation. Indeed once we have reached the age of reason we no longer accept just *any* explanatory challenge and history shows that, often enough, we are right not to do so. So we need to know that this particular request for explanation is a good one to take on. (I call this the Kenny Rogers response: you have to know when to hold them, and know when to fold them.)

Second, we can challenge the explanandum, that science is instrumentally successful. For example we can suggest that each success swims in a large pool of failures. So what requires explaining, if anything does, are why methods that generally fail occasionally also succeed; or so one might suggest.[1]

Third, we can challenge whether any *explanationist* defense of realism is reasonable in the context of a debate over the reliability of the hypothetical method. For the issue under discussion in judging realism in this debate is precisely whether explanatory success provides grounds for belief in the truth of the explanatory story. To use explanatory success to ground belief in realism, as the explanationist defense does, is to employ the very type of argument whose cogency is the question under discussion. In this light the explanationist defense seems a paradigm case of begging the question, involving a circularity so small as to make its viciousness apparent .

Fourth, in the contest with instrumentalism in particular, the explanationist defense seems especially poor. For, assuming that the instrumentalist grants the validity of the question, his general orientation also suggests an obvious reply. If the output of the scientific method is instrumental success, why do we need to suppose that the input has features beyond what is useful for explaining the output ? So the instrumentalist, feeling rather on home ground, may suggest that to explain instrumental success we need only suppose that our hypotheses and theories are instrumentally reliable.[2] After all, while rejecting the policy that uses explanatory success to ground belief in the truth of theories, the instrumentalist is still in a position to offer an alternative. It is that, at best, what explanatory success warrants is belief in the instrumental reliability of the explanatory story. This is an explanation of outcomes by reference to inputs that have the capacity (or "power") to produce such outcomes. It is a typical dispositional explanation, just like Newton's account of the phenomena of gravitation. Although explanations by reference to powers and capacities have been ridiculed by Moliére and others as "occult", in fact they have a very respectable history in the sciences, and are well entrenched in scientific practice today, for example in the probabilistic explanations of quantum phenomena. Thus there is a very specific answer that the instrumentalist can offer to the challenge of why science is successful, an answer that coheres perfectly with an instrumentalist version of inference to the "best" explanation, and with common scientific practice.[3]

The failure of the explanationist defense marks the next-to-last stage in the attempt to find a reasonable basis for realism. We turn now to the last stage.

3. The Fragmentation of Realism.

Realism used to be a global position. It held out a vision for science as a whole, which it pictured as engaged in the task of searching for truth. Truth was understood as some sort of external world correspondence, the sort needed to insure the mind-independence of the scientific objects. Realism also made promises. It committed science not only to the search for truth but also to the possibility (at least) of achieving considerable success at finding it, although sometimes reservations might be expressed as to whether science would get both the ingredients and the structure of the world just exactly right. This vision was certainly a noble and romantic one, and deliberately so, for realism was used to defend the autonomy of science, and to attract political and economic support for it (not to mention practitioners too). It still is.

As we have seen, however, this global realism turns out to have no viable argument in its support; nor does it wear well with time. For by now we have witnessed too many scientific theories come and go, and a whole menagerie of entities with them, to feel strong commitments to any high theory or any current ontology. Further, the most important areas of contemporary physics are governed by quantal theories that still seem highly resistant to realist construals. I think that all of this has eroded confidence in the realist vision. But there is even more. The enormous authority and elitism of the scientific institutions in contemporary society, and their complicity in what many feel are a whole range of reprehensible activities (from the abuse of animals in appallingly stupid and cruel experiments to the evil and mindless engagement in the development of mega-destructive weaponry of all kinds), has also undermined the nobility of the realist vision of science as seeking objective truth, making that vision seem rather contrived, perhaps even conspiratorial. Thus realism seems to have outlived its usefulness. At any rate, these various factors have contributed to the current breakup of the doctrine, out of which I propose to concentrate on two related fragments. The first is entity realism.

The guiding idea behind entity realism is to take an instrumental attitude toward theories and a realist attitude toward entities. So we are enjoined to believe that electrons exist, but not to believe in the truth (or even the approximate truth) of any of the theories of electrons. Thus we abandon the principle that explanatory success warrants belief in the truth of the explanatory theory. But why then believe in electrons? The answer is that we are supposed to be able to get a handle on electrons, independently of any high theory, in terms of a number of low level, seat-of-the-pants principles. These are the kinds of principles that make it possible for us to use electrons to build new tools (like "electron guns") for the further exploration of nature. Thus we get Ian Hacking's (1983) memorable technological advertisement for entity realism: if we can use the thing to build an instrument for manipulating and exploring nature, it must be real.

What the advertisement trades on are the low level principles, which as the talk of building and manipulating makes plain, are generally causal in character. Thus what grounds entity realism is a *causalism* that picks out causes from among the bag of theoretical entities, and holds them up as special. Their special character comes out this way. Although in general we reject the thesis according to which we are justified in believing that acceptable explanatory stories are true, we adopt that thesis in the special case where the explanation is a causal one. Thus entity realism relies on the following principle: *a necessary condition for the acceptance of a causal explanation is that the causes are real.*

Nancy Cartwright (1983) suggests that this principle is part of everyday wisdom, what we might call folk epistemology, with examples like this. If the leaves on our lemon tree turn yellow and sickly, we might be told that the trouble is due to the fact that the roots are water logged. This is a causal account, with the water at the roots functioning as the cause, and if we accept it we would expect to find moisture there when we check the roots. Indeed were there no moisture at the roots we could not accept the explanation at all. Just so, says Cartwright, if we accept the explanation that the vapor trail in a cloud chamber is due to the presence of a certain charged particle we must accept the reality of the particle. Or if we accept that the rate of fall of a tiny ball in a magnetic field is affected by spraying it with electrons, we had better believe in electrons. But had we?

The issue here is the epistemological one that divides realism from instrumentalism. So the question is whether entity realism, causalism, is successful against the general line instrumentalism takes toward belief in explanatory stories. It is often supposed that the instrumental line involves different epistemological strategies for observables and unobservables. The emphasis placed on the distinction between observables and unobservables by some instrumentalists (even when naturalized in the manner of van Fraassen 1980) certainly feeds this supposition. Nevertheless I do not think it is a good or accurate one. It is better to think of instrumentalism as involving a uniform reconceptualization of inquiry along pragmatic lines, without any special regard for observables or unobservables. After all, instrumentalism is the brand of pragmatism associated with the so-called "Chicago School of Thought." The term itself was coined by John Dewey, who sometimes referred to it as "experimentalism," and summarized the position this way.

> Instrumentalism is an attempt to constitute a precise logical theory of concepts, of judgments and inferences in their various forms, by considering primarily how thought functions in the experimental determination of future consequences. (Dewey 1943, p.463.)

It hardly seems likely that a position designed to make sense of experimental practice and, as we see from Dewey, in a causal context, would be put under much stress by a rather traditional form of realist argument confined to experiments and causes. So it turns out. For according to instrumentalism what we want from our theories, posits, ideas etc. in all the various contexts of inquiry is instrumental reliability. That is, we want them to be useful in getting things to work for the practical and theoretical purposes for which we might put them to use. This is the guiding pragmatic idea of instrumentalism, and it treats all entities (observable or not) perfectly on par. Accordingly, an explanation is acceptable if it works. What we believe when we accept an explanation is that it can be relied on now and in further inquiry (the "future consequences" to which Dewey refers.) So when I accept the explanation that my lemon tree is sick because its roots are water logged, I believe it reliable and so, discounting for the usual untidiness of practice (see note 3), I do indeed expect to be able to rely on that story if I proceed to investigate the roots. I expect, that is, to find excessive moisture. In exactly the same way, if I accept the story according to which the vapor trail was produced by the charged particle, then I believe this too is an account I can rely on, for example, in conducting further experiments, and in interpreting their results, or in building new machines. But, like any other, a causal story can be reliable without being true, hence without the cause being real, and in accepting the story I only commit myself to its reliability; i.e., to its being a good and useful guide to thought and action. Of course if the cause happens to be observable, then the reliability of the story leads me to expect to observe it (other things being equal). If I make the observation, I then have independent grounds for thinking the cause to be real. If I do not make the observation or if the cause is not observable, then my commitment is just to the reliability of the causal story, and not to the reality of the cause. Moreover, as the pragmatist sees it, inquiry would not be furthered by making any stronger commitment.

The realist and the instrumentalist differ in their reconstructions of everyday epistemology in the realm of the observable. The realist sees truth as the aim of the game there and finds reliability to be a nice bonus that comes with the acceptance of a causal story as true; so the realist projects truth and not reliability to unobservables as the appropriate necessary condition on acceptance. The instrumentalist sees reliability as the goal, and truth (i.e., the reality of the cause) as a nice bonus that comes by way of observation with the acceptance of an explanatory story as reliable; so he projects reliability and not truth as appropriate for unobservables. Acceptance is nicely ambiguous, allowing for various specifications (accept as true, as useful, as expedient, for the nonce, for a *reductio* , etc.) For just that reason why should one expect that either of the blanket epistemological reconstructions of realism or instrumentalism apply to the diverse practices of inquiry across the board? Indeed, why expect that that any further specification of the modes of acceptance is a part of practice in general, or required for its interpretation at all? Entity realism is joined to instrumentalism in the supposition that there are (must be!) perfectly general goals in the light of which inquiry can be uniformly reinterpreted, or to use Dewey's famous term "reconstructed." We do not have to join them in this supposition. But we should also recognize that the shape of the dialectic does not enable realism, even when confined to the causal inference to entities, to subvert the general instrumentalist strategy. In short, entity realism has gained no advantage over realism *tout court* in the contest with instrumentalism, which is to say that realism thus far has no support at all.

The second fragment in the breakup of realism is a position without yet a name. I shall call it *contextual realism* . It is the sort of position long espoused by Ernan McMullin (1984), with regard to structure and on explanationist grounds, and it has been picked up more recently by Richard Miller (1987) and William Newton-Smith (1989). I will look at Miller's version, set out with great originality and force, in his *Fact and method* . The central idea of contextual realism is to specialize. So we are told, by one author or another, to be realists about geology (plate techtonics and continental drift), or biology (DNA molecules, viruses, bacteria) or physical chemistry (the old atoms and molecules). And so on. On the other hand maybe we may not have to be realists about depth psychology (ego and id and superego), or maybe even features of general relativity (gravitational waves, or -- if we believe Stephen Hawking-- black holes). The failure of realism in the quantum theory, if we grant that failure, is just another example of where it is not required. Thus realism goes contextual and artless; promising nothing and merely yielding modest realist dividends where it can. Or so it says. The advantage to specialization, we are told, is that when we narrow down the context we also narrow the room for reasonable doubt. So the idea is to show how in various special contexts it would be unreasonable to doubt the existence of certain unobservable entities.[4] (I would just note that here, as in entity realism, the metaphysical aspect has dropped out, along with the promise and grandeur of the traditional synthesis. One might wonder whether this is really a "realism" worth fighting for -- and why ?)

The strategy that Miller devises is an interesting deepening and refinement of the causal one invoked in the case of entity realism. What Miller argues is that, in the absence of specific doubt, belief in the reality of certain causal agents is necessitated not by the acceptance of a causal explanation but rather by the admission that the phenomena in question *require* explanation involving belief in certain causal agents (or kinds of agents). The job of requiring or setting the need for these belief-generating explanations is accomplished by certain low level principles of rationality related to what Miller calls *topic-specific truisms*.

It is best to show what is going on here by discussing one of the cases that Miller treats in detail, the reality of molecules. (He only treats one other case in detail, that of microbes, but suggests that most cases in science will turn out the same realist way if closely examined -- again, "were there world enough and time.") The phenomenon here is Brownian motion, the random zig zags of small particles of pollen (or ink, etc.) when suspended in a clear liquid, a motion that depends sensitively on the temperature and viscosity of the liquid, and for which Einstein devised a beautiful theoretical account in 1905, deriving the diffusion coefficient by using (actually re-inventing) techniques of statistical mechanics applied to the molecules of the liquid. Einstein's causal explanation of the motion of the Brownian particles involves their being banged about by these molecules. What sets the need for explanation, however, is a more primitive concept, the topic specific truism for this particular case: *Non-living matter does not jump about erratically unless something external is moving it about* .

This principle might seem to us rather more Aristotelian than Einsteinian. Nevertheless Miller takes it as a *prima facie* truth and uses it to sanction the following inference. *If a non-living thing is in constant erratic motion, that is a reason to believe it is constantly being moved to and fro by an external agent.* Thus Brownian motion is held to require a causal explanation, where we *must* believe in the cause. In fact, Miller's analysis of this case has a general logic, which goes like this.

TRUISMS $\rightarrow$ A REASON TO BELIEVE
\+
NO SPECIFIC DOUBT $\rightarrow$ UNREASONABLE NOT TO BELIEVE.

The truisms give one a reason to believe (say, in molecules). But the conclusion the realist is after is that it would be unreasonable (i.e., irrational) not to believe (say, in molecules), for only this will defeat the instrumentalist. It is the absence of specific doubt that moves one from having a positive reason to believe to drawing the strong conclusion of its being unreasonable to withhold belief. Thus Miller's analysis of the case invokes the following perfectly general principle of rationality:

If one has grounds for belief, and no specific doubts on the matter, then it would be irrational not to believe.

Others have found this principle compelling too; for example, it seems to underlie Dudley Shapere's (1985) account of the internalization of standards of scientific rationality, and it may relate to Jerry Fodor's (1975) tenet that a bad theory is better than no theory. Still, it is surprising in Miller. For Miller is a particularist, and the passion for this sort of generality in our treatment of a topic like rationality is among the things that Miller identifies as the legacy of logical positivism, a legacy his particularism promised to rid us of. Indeed, in the very context in which he applies this principle to molecules and Brownian motion he repeats his conviction that "there are no valid principles that describe what belief should be in the light of data." (Miller 1987, p. 486) Here, I think, we see Miller caught in the generalist (and positivist) web that he is trying to dismantle.[5] I think that what makes him vulnerable is his need for realism, which overrides his particularism and dictates that the antirealist doubt *must* be ruled out. For what could provide the desired force except a strong (and general) principle of rationality? But I think that something else has gone wrong here, as well, and with regard to how one conceives of belief and its relation to reasons.

Those who have been drawn by this principle of rationality seem to have found the following picture compelling: conditions of rationality (i.e., what one ought to believe) supervene on the field of reasons. Thus where the balance of reasons *for* belief and reasons *not to* come out just right, one ought (or ought not) to believe. I think, however, that this is a skewed and erroneous picture of rationality, one that drastically overplays reasons to the exclusion of all else. A more balanced picture would allow for the fact that beliefs are caused, and only in part by reasons. Hence conditions of rationality have to respect the causal nexus, at least to the extent of allowing that it is not unreasonable not to believe where, causally speaking, one cannot believe. Thus we do not charge our friends with irrationality in the denial stage of mourning. To the contrary, we find their denial perfectly appropriate and reasonable. Similarly, despite some moments of temptation, in our better moments we do not consider our students irrational just because the excellent reasons we present, eliminating all their specific doubts along the way, still fail to persuade. Believing is a species of doing, although not an altogether voluntary one, and coming to believe quite reasonably requires not just good reasons (*qua* causes) and the absence of specific doubt (*qua* absence of interfering conditions) but it also requires just the right causal mix in all the surrounding conditions. There are no general rules to say when the mix is right. So particularism seems the proper way to treat rationality, even if that makes one's realist heart go thump.

The emphasis on specific doubt is by way of countering a general skepticism or agnosticism, a way of holding Descartes' demon at bay. It is an excellent tool that can help us be serious about our reflections. But it can also be overused, and worries about skepticism should not lead us to make up principles that block reasonable doubt and compel belief too hastily, as I think the preceding principle does. In the context of a particular scientific debate, in a field and subject matter not yet well explored and understood, a doubting attitude is generally among the attitudes sanctioned by the community of investigators. In its evolutionary wisdom, various degrees of doubt and belief are encouraged in these circumstances, and there are no set rules of closure for these cases. Thus even allowing the truism according to which the phenomena of Brownian motion gives us some positive reason to believe in molecular movers of the Brownian particles, and even allowing the absence of specific doubt, in the decade 1905-15 it would not have been unreasonable for a serious scientist to hedge commitment to molecules, and to take a "wait and see" stance. Any reconstruction of the historical argument that makes such behavior irrational just flies in the face of scientific practice, now and then. It goes counter to the wisdom of the community.

Of course one should not allow the truism to stand unchallenged, nor should one concede the absence of specific doubt in the actual history of the case at hand. Indeed, the focus on specific doubt in working through the details of genuine cases is useful in directing our attention to the enormous diversity of opinion that exists in science when we really get down to looking for it. In view of this diversity it is very unlikely that real cases of scientific dispute would ever show the required absence of specific doubt. In the case of Brownian motion, two sources of specific doubt stood in the way, historically, of granting the applicability of the truism, even *prima facie* . The first has to do with the electo-magnetic view of matter, long the dominant view, and arguably so in the period in question. It was, for example, the view of Lorentz, who was Einstein's scientific patron saint in 1905 and even much later. This view would lead one to look not for external movers banging the Brownian particles about, but for the interplay of electrostatic forces among the particles themselves, in conjunction with exchange forces with the medium. This idea had a good deal of life to it, and until it was fully played out Miller's truism would not have had any special pull with scientists of the time. In 1905-15, it didn't. That period, moreover, especially in German science, marked the beginning of the decline of the classical causal world view. By 1913, as Miller notes, Bohr was free to introduce his atomic model with its uncaused orbit-jumping electrons. But before then the scientific air was full of the idea that maybe causality had had its day. Thus it would by no means have seemed unreasonable to wonder whether Brownian phenomena weren't just the sort of thing that might succumb to an analysis involving a fundamental randomness in the behavior of material objects. A scientist proposing to work on such a project in 1905, or holding out for a program that would involve such work, might not have been in the mainstream of physics, but he would not have been considered on the lunatic fringe either, which is where Miller would place him. Thus Miller's truism, which in the light of the quantum theory is not true, also was not considered to be true in physics, even *prima facie*, during the period of concern. There was, that is, plenty of room for specific doubt.

So far I have urged both a general and a specific criticism of the case for molecules, as Miller construes it. That case involves a general principle of rationality that overestimates the role of reasons and that misuses the tool of specific doubt. It also involves a specific truism whose historical credentials are questionable, and factual assumptions about the actual range of doubts that simply seem incorrect. But there is an even larger scale problem with the strategy that Miller employs to support realism; namely, that it is no better than the others we have looked at in undermining the standard approach of instrumentalism. For suppose we bracket the objections raised above, then we still need to ask why the truism is used to ground the necessity for belief in the *truth* of the causal covering story and hence in the reality of the causes. Were an instrumentalist to go Miller's way he would surely urge that the truism merely grounds belief in the reliability of the causal account, just the view explored above in connection with entity realism. As the instrumentalist sees it, the phenomena of Brownian motion give one reason to believe that a good (i.e., useful) way of thinking about the matter is that the Brownian particles are being banged about by "molecules" of the liquid. Of course one is not compelled to model things this way, but if you do it pays dividends, for one gets a very good way of calculating the diffusion coefficient and, in turn, an excellent way of estimating Avogadro's number. (Subsequent experimental measurements by Perrin, as Miller notes in his realist idiom, show the utility and reliability of this model.) There is, then, an instrumentalist reconstruction of the Brownian motion and molecules episode exactly on par with the realist one offered by Miller, and no rationale has emerged that would enable us to choose between them.

4. Concluding Thoughts.

When realism was grand, no support could be found for it vis-à-vis its instrumentalist and constructivist rivals. So realism went to causes and contexts, giving up general metaphysical pretensions, and resting as best it could only on its epistemological leg. But that hasn't helped. Despite the apparatus of causal explanations, aided by little principles of rationality and topic-specificity, we still find realism begging the question of the reach of evidence against its instrumentalist rival.

The piecemeal approach looks to specific, contextual features to settle the question of what it is reasonable to believe, or to believe in; and what not; that is, the question as to how to specify what acceptance amounts to. Piecemeal *realism* expresses the conviction that for the most part it will turn out that reasonable acceptance amounts to reasonable belief in the truth of certain hypotheses, or in the existence of certain theoretical entities; *and* that contextual features will make it unreasonable to adopt any less strong belief. But piecemeal realism has no argument for this conviction, not only no argument that this is how things will turn out for the most part, but not even any argument that this is how things do turn out in paradigm, carefully chosen cases. There do not seem to be any non-question-begging principles out of which to fashion the general argument that the realist wants, nor even the argument in specific cases. By the same token, there are no such principles, and hence arguments, available for the instrumentalist either.

Does this mean we should just take our pick as temperament or whim dispose? It certainly means that we are free to do so, provided we are honest and acknowledge these external factors (or others) as our grounds. But I think we might pause to wonder whether it is really important to declare our allegiance in this matter either to realism or to instrumentalism, either in general or in particular. If the character of acceptance in science is not governed by principles, even in the small, is it really so important to worry about that character? After all, there is no reason to think that science as a whole, or inquiry more generally, has suffered because the special issue, addressed by realism or instrumentalism, of disambiguating scientific acceptance has gone unanswered. To put it differently, the ambiguity over the character of acceptance in science that results from not raising the realism/instrumentalism question seems to be an ambiguity we can quite well live with. Maybe the moral of this final stage of the realism debate is to drive that message home so that we can acknowledge it in good conscience, and get on with other things. That, at any rate, is the advice of my friend NOA (Fine 1986a, 1986b).

Both realism and instrumentalism are basically pro attitudes toward science. They tend to regard what science accepts as reasonable. They differ as to the character of that acceptance. Like science they favor principled judgment. But if we are persuaded that principles do not determine the character of the judgment, might we not draw the conclusion (or at least consider it ?) that we may have been mistaken in thinking that a more determinate character is required. The point is that we build models and theories ("frame hypotheses"), and act on them without necessarily settling or even addressing the interpretive questions that realism or instrumentalism raises. The reason we cannot seem to find any principled resolution to these questions, even contextually, is that the usual criteria for theory choice, and the techniques and rules of evidence seem to reach only to the fact of acceptance, and not to its character. With theories that stay around, questions about the character of acceptance (do molecules "really" exist ?) frequently drop out of the scientific discussion, assuming they were ever there to begin with. It does not follow that the questions have been answered; e.g., that practitioners now believe in the truth (or approximate truth) of the theory, much less that they would have adequate grounds for doing so and for ruling out any lesser commitment. It only follows that such *questions* are not alive scientifically.

NOA is a pro science attitude as well. It too is inclined to find what science accepts reasonable. In this regard it may be said to assent to what realism and instrumentalism hold in common about acceptance in science. But it differs from realism and instrumentalism in not pushing the issue of the specific character of scientific acceptance farther than the reach of ordinary scientific procedures and common reflective thought allow. So NOA suggests that in general we can get along without regard to the question of what accepting the theory amounts to, unless that question is scientifically relevant. NOA's practice would not alter the fate of science, whether or not the science has staying power. It seems, indeed, to be the practice of science itself, on the whole, despite the history of realist and instrumentalist attempts to reconstruct that practice in their own special terms. NOA thinks we do perfectly well without those reconstructions.

Experienced methodological counsel emphasizes the primacy of action over interpretation in just this way. The noted relativist, Robert Geroch, tells us

> What is needed [for theory choice] is the right mix of caution and daring. One must at times be rash, accepting (perhaps temporarily) ideas with very little observational basis; one must at other times be ultra cautious, examining "obvious" notions with care. The art (and it is an art) consists of making judicious choices of what is to be in the first category and what in the second. (Geroch 1978, p.67.)

Wisely, Geroch refrains from saying what accepting the ideas amounts to, whether to truth or to something else. Very wisely !

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NOTES

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I agree here with Michael Devitt (1984) in his emphasis on the metaphysical side of realism, although Devitt does not give enough weight to the epistemological side. Much of the recent discussion follows Bas van Fraassen (1980) in skewing things exactly the other way round.

[1] This point is nicely illustrated by the recent episode over cold fusion which, with its numerous reports of failed and inconclusive experiments, and inconsistent explanations, provides a glimpse of real science in action.

[2] In using the term "reliability" here I do not mean to suggest that the application of theories is predictable, straightforward or neat. To the contrary, as emphasized in the second point above, in science (as in homecare) things are messy, often require new tricks each time, and are always liable to go wrong. Hence being reliable amounts to no more than being useful for getting things to work (or work out). These points come out in the discussion in section 3, below.

[3] Cartwright's (1989) elegant defense of capacities and powers does not extend to this kind of instrumentalist response (pp.160-61) since, in her view, the range of a theory (and hence its reliability) can be worked out only by using the theory and the theoretical

concepts in question. Surely she is right, but the issue still remains as to whether this use of the theories and concepts commits one to realism about them. Cartwight's unargued assumption that it does, at this point, simply begs the question over instrumentalism one more time.

[4] It is important that the issue be formulated in this way. For if, in context, it would be reasonable to believe in unobservables and also reasonable not to (e.g., to remain agnostic) then no advantage would have been shown for realism. One might try to reduce this burden on realism by going quantitative, and arguing that the weight of reasons is on the realist side. But then the principle that we ought to believe what is most reasonable to believe, brings us back to the original formulation in terms of its being unreasonable to doubt.

[5] Perhaps the principle that Miller has in mind is not the one highlighted above, but rather the one represented by the full schema; i.e., the principle that *if one has grounds for believe that are supported by a topic specific truism, then if there are no specific doubts on the matter it would be irrational not to believe*. Because Miller only discusses two cases in detail, there is not enough material to say for sure just which version is operative. It really does not matter, however, since both principles fall under Miller's own particularist injunction to avoid principles that "describe what belief should be in the light of data."