McMullin's Inference: A Case for Realism?

with Bas C. van Fraassen, "Scientific Realism and the Empiricist Challenge: An Introduction to Ernan McMullin's Aquinas Lecture"; and Ernan McMullin, "The Inference that Makes Science"

SCIENTIFIC REALISM AND THE EMPIRICIST CHALLENGE: AN INTRODUCTION TO ERNAN MCMULLIN'S AQUINAS LECTURE

by Bas C. van Fraassen

Abstract. In *The Inference That Makes Science*, Ernan McMullin recounts the clear historical progress he saw toward a vision of the sciences as conclusions reached rationally on the basis of empirical evidence. Distinctive of this vision was his view of science as driven by a specific form of inference, *retroduction.* To understand this properly, we need to disentangle the description of retroductive inference from the claims made on its behalf. To end I will suggest that the real rival to McMullin's vision of science is not the methodologies he criticizes so successfully but a more radical empiricist alternative in epistemology.

Keywords: abduction; empiricism; induction; Ernan McMullin; retroduction; scientific realism

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In *The Inference That Makes Science*, Ernan McMullin takes us on a fabulous journey through the history of philosophy of science, displaying clear progress toward a vision of the sciences as conclusions reached rationally on the basis of empirical evidence.[1] This is McMullin's vision, distinctively his, though in large outlines shared by the twentieth-century philosophers to whom he refers, in the last few pages, as scientific realists. And surely, in large outlines, though with characteristic qualifications, it is also shared by those to whose contrasting points of view he refers there as instrumentalist. For on all hands, the empirical sciences are accepted as a paradigm of rational inquiry into what our world is like.

But the title itself announces what is distinctive of McMullin's view: the sciences are driven by a specific form of inference that accounts for

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Unless otherwise noted, page references will be to the text of *The Inference That Makes Science* (originally McMullin 1992) which is reprinted in this issue of *Zygon: Journal of Religion and Science*.

[*Zygon*, vol. 48, no. 1 (March 2013)]
 ISSN 0591-2385 *www.zygonjournal.org*

their success, and is indeed the hallmark of the scientific approach to any subject. As the history unfolds we see the attempts, one after the other found wanting, to identify that form of inference, until its final articulation as a process of (as McMullin decides to call it) *retroductive inference.*

Aristotle, Grosseteste, Aquinas, Galileo, Zabarella, the late medieval nominalists and Francis Bacon, Isaac Newton's methodological vacillations, . . . the story reads as well and as fluently as a mystery novel, and is as engaging. What I shall comment on here is not McMullin's excursions into history, however, though they were certainly for me the most fascinating part. My concern will instead be with McMullin's project, the project to characterize the sciences as, in essence, a practice identified by a form of inference.

Contrasting McMullin's Vision with Rival Epistemologies

McMullin does enough to discredit some alternative projects with a similar aim, such as attempts to define induction as a method for science. Today other projects of that sort exist as well, drawing in one way or another on the concept and theories of probability, notably varieties of Bayesianism or a more liberal probabilism. It would be of interest to ask how, or to what extent, such alternatives could do justice to the insights that support McMullin's concept of retroduction as the crucial or central form of scientific inference. I will leave that aside as well. The more interesting question, for me, is rather whether scientific practice, the enterprise of science, is best characterized in that sort of form at all.

McMullin does not have an overriding ambition in this project. He emphasizes that it was "not intended to furnish a criterion of demarcation between science and non science [. . . .] retroductive inference makes use of ingredients that are commonplace in human reason generally" (144). In good human reasoning to be sure; McMullin mentions approvingly the detective and the journalist. But retroduction is easily discerned in not so good human reasoning as well, when conspiracy theorists are retroductively inferring from the facts in evidence to their weird or wonderful causal explanation. So it seems at least at first blush as if the hallmark of scientific inquiry will not be that the form of inference is different, but rather how well it is employed:

> What is distinctive about the way in which explanatory theories are constructed and tested in natural science is the precision, as well as the explicitness, with which retroductive inference is deployed. (146)

But that is too modest. It is not just a matter of doing it better, not just a matter of greater precision and explicitness, because McMullin emphasized features of the practice that are not captured by such earlier accounts as

were focused on deduction, induction, or even Peircean abduction. The details emerge for McMullin after a long scrutiny of errors and insights accumulating through some twenty centuries of reflection on the matter, and they are not simple or neat, let alone algorithmic.

As a process of inference, retroduction "is not rule-governed as deduction is, nor regulated by technique as induction is" (183). McMullin elaborates on this elsewhere, indicating a strong difference from another rival that was much in the limelight in the closing decades of the twentieth century:

> retroduction [. . .] is not a strict form of rule-governed reasoning, or at least, it is not as long as it isn't equated with the easily-criticized "inference to best explanation." [. . . .] The vulnerability of such an inference need hardly be emphasized. (McMullin 2007, 175)

These are important differences, and it is a characteristically twentieth-century insight that rational change in view is not a matter of rule following, that rules of right reason cannot be dictates, only guidelines. But something is needed beyond this negative point.

McMullin's Account of Retroductive Inference

It is in fact not easy to disentangle the points that allow us to recognize a process of retroductive inference from the claims McMullin makes concerning this sort of inference. We must concentrate on the definitive account that McMullin provides in the last 5 pages (in the reprint that follows) of *The Inference That Makes Science,* but it may help to look first at a formulation McMullin provided in a later publication, as a short summary:

> Retroduction, argument from observed data to an explanatory causal structure which may itself be unobserved though not necessarily unobservable is of its essence tentative. It terminates in likelihood (in the everyday sense of that term, not the sense given it in probability theory). It allows for the *gradual* mounting of evidence of all sorts: increasingly troublesome anomalies eliminated, ambiguities resolved, new evidence successfully incorporated, and the rest. Above all, under certain circumstances it encourages more and more persistent questioning of the assumption that the paradigm in possession is beyond challenge or that a potential rival is, on the face of it, absurd. There is a lot of room here between strict reason and *credo quia absurdum*, the room afforded by an ever-increasing likelihood that may begin from a very low level indeed. (McMullin 2007, 176)

To what extent is this a *description*, such as a neutral observer of scientific practices might give, and to what extent does it involve *claims* about the adequacy or rationality or truth-conduciveness of this form of inference?

First: that in such an inference we are "led backwards" from effect to cause, for example, we can read as merely describing the form (from premises about what happens to conclusions about what causes them). But we can also read it as a *claim* that what happens is always in fact an

effect, that is, an event that has causes, and in addition that these causes are discovered by retroductive inference. That this composite claim is in fact part of what McMullin maintains becomes quickly evident toward the end of his Aquinas lecture.

That McMullin is making a strong claim on behalf of this form appears also earlier in his critique of Newton, whom he describes as having been misled by the "quasi-demonstrative" form of his own writings, and as having had a distorting influence on eighteenth century methodological reflection, which

> was to have negative repercussions for decades to come, until the atoms and ether-vibrations of the early nineteenth century once and for all showed causal inference to underlying structure to be indispensable to the work of the physical scientist. (180)

Second: one feature McMullin lists, which clearly distinguishes this retroductive inference, is the creation of new concepts. We can imagine a situation in which all attempts at explanation fail, within the conceptual framework that has been actualized so far. In that case—and surely there are famous historical cases of this sort—a smaller or larger conceptual revolution is the only way forward. As a distinguishing mark of retroductive inference, though, it has its limits; for this feature is one that *may* be present, and certainly is not always involved.

Once again, Newton furnishes the bad example of a misdirected empiricism. The need for new concepts and new language appears to be ruled out by Newton's Third Rule of Reasoning which postulated that the relevant properties of all bodies would be those accessible to the human senses. And this was not incidental, Newton "needed this restriction . . . in order that induction might be, as he claimed, the all-sufficient method of natural science" (185).

Third: that the product of retroduction is a theory which presents a causal explanation, distinct from the sort of empirical law that registers a regularity, is crucial. We can perhaps typically see the feature of causal "explanatoriness" at a glance, and if so it can serve as a hallmark to recognize retroduction. But even here a claim of adequacy or efficacy, not just something offered as description, is entangled with the description:

> The language here is, of course, that of scientific realism. It is because the cause is, in some sense however qualified, affirmed *as* real cause, that retroduction functions as a distinct form of inference. (184)

Here, after all, Newton appears as on the side of the angels. For this phrasing echoes Newton's First Rule of Reasoning, the "vera causa" principle. What I will suggest though is that inductivism in the naïve form that Newton may have preached, if not practiced, is in any case not the most important rival to McMullin's view of science.

An Analogy, to Arrive at What May be Distinctively Different

As an analogy, suppose that someone wanted to construct an account not of what science is but of business, commerce. If someone starts a business, he will begin by amassing some capital, acquire a place of business, equipment, inventory, employees, and begin to advertise. As the business gets going he has to look ahead, plan replenishing his stock, have reserve funds for repair and for salary, including his own, when receipts are lagging. What is the inference that makes business?

Certainly inference is involved. Evidence of demand for his goods or services needs to be available before he can set out at all. A record of the expenses and receipts, and the timing of each, forms a growing base of evidence that he needs to consult continually, not simply to assess how well he is doing but to assess what is needed to go on. This assessment is a process of arriving at some conclusion that, though perhaps not logically derivable from that evidence, is at least sufficiently likely to him in the light of that evidence. That process is a process of inference. So yes, inference is involved.

But this we could say of almost any form of intentional activity or practice. In order to characterize business in a way that distinguishes it from other human practices, is looking for a distinctive form of the sort of inference involved the right thing to do? Is business distinguished by a special form of inference? Is engaging in that sort of inference precisely *what it is* to do business?

McMullin's concentration on inference in developing his view of science, in continuation with the tradition he explores, suggests that we should assume science to be distinguished from such other practices as business and commerce in these terms. Science, not business or commerce or the like, is distinguished by a special form of inference. But it takes patience and willingness to look for differences, partly differences of degree and partly of kind, to elucidate what is special about that special form.

We can go back at this point to the early pages of McMullin's Aquinas lecture and remember that the ingredients of retroductive inference, as present in science, are commonplace in human reason generally. That all sorts of rational ways to reach conclusions are involved in business, and that this should be a common feature of business practice and scientific practice, should come as no surprise. It may well be in addition that in business sometimes the way to victory over rivals, to commercial success, can only come through the creation of new, novel concepts. A new invention, conceptually novel, may open an opportunity for a business to take on a whole new form. The concept of the computer was with us since Pascal, but not the concepts involved in electronic information storage; that may be a case in point.

But then, if the differences are to be discerned about much that is in common, all the weight of McMullin's project comes to rest on some specific *claims* he makes, even if implicitly in the main, that we need to disentangle from whatever counts as a neutral description of the form.

McMullin's Claims on Behalf of Retroduction

Those claims I introduced briefly above; let me quote the entire passage now:

> retroduction is not an atemporal application of rule as deduction is. It is extended in time, and logically very complex. It *is* properly inference, since it enables one to move in thought from the observation of an effect to the affirmation, with greater or lesser degree of confidence, of the action of a cause of a (partially) expressed sort.
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> The language here is, of course, that of scientific realism. It is because the cause is, in some sense however qualified, affirmed *as* real cause, that retroduction functions as a distinct form of inference. (184)

McMullin goes on to indicate clearly the position in epistemology that provides the epistemic side of this scientific realism:

> It is a far cry from the demonstrations of Aristotle to the retroductions of modern theoretical science. Where they differ is, first, that retroduction makes no claim of necessity, and it settles for less, much less, than definitive truth. It can, under favorable conditions, when theories are well-established, yield practical certainty. (185)

What we see here, I think, is that by "inference" McMullin means something more than something that "enables one to move in thought from the observation of an effect to the affirmation, with greater or lesser degree of confidence, of the action of a cause of a (partially) expressed sort" (184). Even adding, for example, "rationally" after the word "move" would not be enough to complete what he means. Logically speaking, there is still a gap between the statement that this is a rational sort of move from evidence to an affirmed conclusion and the claim that this sort of move is "truth-conducive," that it is likely to lead to true conclusions. When McMullin speaks here of a degree of confidence, and of practical certainty, he is surely not just describing a subjective state of mind of the person engaged in retroduction. He is claiming that retroductive inference, to the unobserved causes of observed events classified as effects, leads to truth.

McMullin's Sophisticated Scientific Realism

McMullin did not much like the use of "true" and "truth" in this context. In his well-known "A Case for Scientific Realism," he ends by apparently eschewing claims to truth altogether:

> I do not think that acceptance of a scientific theory involves the belief that it is true. Science aims at fruitful metaphor and at ever more detailed structure. To suppose that a theory is literally true would imply, among other things, that no further anomaly could, in principle, arise from any quarter in regard to it. [. . .] Scientists are very uncomfortable at this use of the word "true," because it suggests that the theory is definitive in its formulation . . . .
>
> The realist would not use the term "true" to describe a good theory. He would suppose that the structures of the theory give some insight into the structures of the world. But he could not, in general, say how good the insight is. He has no independent access to the world, as the antirealist constantly reminds him. His assurance that there is a fit, however rough, between the structures of the theory and the structures of the world comes not from a comparison between them but from the sort of argument I sketched above, which concludes that only this sort of reasoning would explain certain contingent features of the history of recent science. (McMullin 1984, 35)

These passages make it clear, however, that the discomfort with "true" signals only an implicature: use of "true," in ordinary contexts, will convey a possibly quite unwarranted certainty. That contextual import is not typical in philosophical debate. Indeed, it was the scientific realists of the 1950s and 1960s who convinced the philosophical community to sever all logical connection between certainty or verification (on the side of the subject or speaker) and truth or reference. Nor does McMullin shrink back from language that we are entirely used to connecting with knowledge, and thereby to truth and reference. To have some insight into X is to know something true about X, and implies the reality of X; to have assurance of a fit between model and world is to have assurance that there is in fact a fit between the two. These points are not affected by the degree of caution with which any of this should be asserted.

But McMullin's caution here is also alerting us to significant differences between his scientific realism and that of other realists he mentions, such as Hilary Putnam (circa. 1980) or Richard Boyd. For McMullin's historically and philosophically informed view of science is nuanced and sophisticated in ways that much writing of the time was not. A remarkable feature of his view is its emphasis on the metaphorical power of theory, with its implications for intellectual fertility and the adventure of scientific exploration, both experimental and theoretical. Discussion of this aspect of his scientific realism—grounded in a conception of science quite different from that found in naïve realism—would warrant a separate article, and I will leave it aside here. Instead we can look closely at how McMullin characterizes our current topic, the character of retroductive inference in science, in the above-mentioned article that makes his distinctive case for scientific realism. For there too we see a remarkable twist away from the more traditional epistemology's conception of rules of right reason that one might have suspected of McMullin's allegiance.

Here McMullin introduces the claim that retroduction is the inference that makes science as, first of all, a historical conclusion about the sciences, and does immediately enmesh that conclusion in claims about its epistemic efficacy, but only to expose the radical contingency of any such claim.

First, the historical conclusion:

> A third consequence one might draw from the history of the structural sciences is that there is a single form of retroductive inference involved throughout. (McMullin 1984, 29)

Then the claim of a warrant for truth:

> As C. S. Peirce stressed in his discussion of retroduction, it is the degree of success of the retroductive hypothesis that warrants the degree of its acceptance as truth. (1984, 29)

which is classified as a logical point:

> What the history of recent science has taught us is not that retroductive inference yields a plausible knowledge of causes. We already knew this on *logical* grounds. (1984, 29)

But then comes the twist: if that is not what we learned from the history, what precisely was learned is an actual, contingent, empirical fact about retroductive inference:

> What we have learned is that retroductive inference *works* in the world we have and with the senses we have for investigating that world. This is a contingent fact, as far as I can see. [. . .] There could well be a universe in which observable regularities would *not* be explainable in terms of hidden structures, that is, a world in which retroduction would not work. Indeed, until the eighteenth century, there was no strong empirical case to be made against that being our universe. (1984, 29–30)

The sentence I omitted in this last quote is: "This is why realism as I have defined it is in part an empirical thesis." And McMullin emphasizes this in several ways: "The realist seeks an explanation for the regularities he finds in science, just as the scientist seeks an explanation for regularities he finds in the world" (1984, 34), the realist's "assurance that there is a fit, however rough, between the structures of the theory and the structures of the world comes not from a comparison between them but from the sort of argument I sketched above, which concludes that only this sort of reasoning would explain certain contingent features of the history of recent science" (1984, 35).

But at this point, it seems to me, McMullin has severely undermined his own argument. For this means that the argument for the efficacy of retroductive inference, and indeed for the claim that it is the inference that makes science, is an argument that is itself an instance of retroductive inference.

McMullin was quite right to dismiss naïve forms of inference to the best explanation in the current literature as easily criticized (McMullin 2007, 175). But at this point his form of defense of retroduction certainly recalls the more familiar "that the practice of science as inference to the best explanation accounts for the success of science is true, for it is the best explanation of that success."

Not every circle is vicious. To show that one may refer to the soundness and completeness proofs for classical deductive logic, which are indeed rigorous proofs in the sense that they themselves follow the rules of classical logic. We gain real understanding of logic by going through those arguments, and this might be offered as a parallel for the use of a form of inference in the study of that form. But there is a great disanalogy. In the case of logic we do not add, as McMullin did for retroduction, that there could well be a universe in which logical inference would fail to preserve truth.

If the warrant for the claim that retroductive inference accounts for scientific success is itself the conclusion of a retroductive inference, what is the warrant for that? For its warrant, it would have to return to the purported logical connection between such inference and the warrant for belief, indicated by McMullin's rather offhand claim that "retroductive inference yields a plausible knowledge of causes" is something we "already knew . . . on *logical* grounds." And whatever those logical grounds are meant to be, they would have to be something that is very reassuring after all, offsetting the admission of pure contingency, about how assured we can be of having true insight into the causes behind the phenomena. But that is where realist and empiricist part ways.

Empiricist Demurral

In McMullin's description of retroductive inference as the process by which science arrives at theories, we encounter a great liberalization of traditional epistemology's view of the sciences. As McMullin moves to his conclusion, he shows us how much had to be progressively discarded. First, the Aristotelian tradition's pretension that we can arrive at contingent empirical truths with certainty. Then on the other side the classical empiricist's claim that the general truths about nature can be deduced from the phenomena, or arrived at by a straight induction from the evidence. Going even further, scrutinizing his own heroes of epistemology, McMullin takes his distance from William Whewell and Charles Sanders Peirce. The kind of confidence in this sort of inference, expressed by Whewell (1847, vol. 2, 67, 284, 286) with his proud claim that consilience is the mark of truth, and that no truly consilient theory has ever been found false, McMullin eschews.

But McMullin is equally adamant that the kind of rule-following paradigm of such lively contemporaneous movements as formal

epistemology is wide off the mark. Retroductive inference is a creative, innovative, often conceptually revolutionary, risk-taking, at the same time severely self-policing, epistemic enterprise.

All of this must be music to any would-be empiricist's ears today. The emphasis on choice and practical decision in scientific progress that entered early on in Hans Reichenbach's, Rudolf Carnap's, and other logical empiricists' writings, are here just as much evident in McMullin's scientific realist epistemology. So what sets McMullin still clearly opposite to that tradition?

I think we can see the clues first of all in McMullin's brief reference to the first great schism he notes in the history he is retelling:

> Looking at the Middle Ages as a whole, one would obviously have to separate two quite diverse methodological traditions, the Aristotelion and the nominalist [. . . .] The Aristotelians remained faithful on the whole, to the ideal of demonstration set down in the *Posterior Analytics*, while developing some aspects of that doctrine, the distinction between demonstrations *propter quid* and *quia*, for example, much more fully than Aristotle had done. The nominalists began to shape the notion of inductive generalization, entirely rejecting the notion of necessary connection between essence and property on which the older notion of demonstration had been based. (*Inference*, 154)

That nominalist turn was also the first step on a course leading to Hume's critique of a concept of causality involving any sort of necessary connections in nature.

There is no countering McMullin's critique in *The Inference That Makes Science* of Bacon, Hume, Mill and other such figures in the history of the empiricist tradition who tried to raise naïve inductive methods to the status of scientific methodology. But that critique, however well it does in demolishing those attempts, does not end the story for a more radical empiricist view of our epistemic situation. McMullin sets those mistakes aside only to return to the tradition that assures us of *epistemic safety*, of a proper handling of evidence that *we can be assured* will be *likely* to lead to truth about nature's deepest structure.

We cannot be entirely sure, from McMullin's actual text, what all is involved in this. I think we can be sure that the "likely" does not signify mere subjective probability on the side of the scientist, or for that matter, the scientific realist. In addition, we have McMullin's own qualifying comment quoted above: "likelihood (in the everyday sense of that term, not the sense given it in probability theory)" (McMullin 2007, 176). Whatever that sense may be, it is to be understood as involving sufficient objectivity to give bite to the professed realism. The entire tradition recounted by McMullin, including the classical empiricism and the inductivism he associates with it, consists in the unfolding of an ever more desperate search for epistemic safety. If not demonstration then induction, if not evidence from recondite experimental or observational procedures then the sense data of immediate

perception, if not inductive generalization from such data then retroductive inference to the *vera causa* . . . safety for our beliefs about the natural world can be gained.

So in this respect McMullin and the sorts of epistemology that he submits to his severe critique are the same. The real opposition emerges in a rival strand never given such ample critical attention: the vision of our epistemic situation perhaps most clearly found in Blaise Pascal, but always initially evoked in the recurring empiricist reactions to realism, before the temptation to seek safety defeats it again.

That other reaction, the one that I would think proper to what empiricism can be today, is to set aside any such illusory safety (see also van Fraassen 2002). Recall that McMullin characterized retroduction as "properly inference" because it "enables one to move in thought from the observation of an effect to the affirmation, with greater or lesser degree of confidence, of the action of a (partially) expressed sort." From this, we can glean a concept of inference in general, as a practice enabling one to move in thought from given or assumed information to a conclusion. There are applicable criteria of rationality, to ensure that at least consistency, perhaps some stronger standard of coherence, even plausibility, are preserved. But what they can guarantee only is to avoid *inevitable* or *necessary* failure to reach truth. There is in the satisfaction of such criteria no guarantee, however much we would like to have one, of reaching truth, with anything more than subjective certainty or probability.

The real rival to McMullin's vision is not the classical empiricist program of induction as objective road from certainty in the deliverances of sense to certainty going beyond the concrete individual fact. That was certainly a philosophical illusion; McMullin is right about that. When McMullin states, in "A Case for Scientific Realism," that the success of retroductive inference is a contingent matter, he realizes a crucial point, but does not go far enough. That the contingent conditions for the success of retroduction actually obtain could only be inferred by retroductive inference: an insight that sweeps the rug from under any assurance of epistemic safety to be found there.

Rationality and criteria of rationality for our epistemic life will rule out self-sabotage, reject any procedures with built-in failure. Apart from that, there remains just the admission that our procedures will work for us, and give us the wherewithal to live and act in this world, only if the world continues to be hospitable to them—and the hope, or faith, that it shall be so.

Note

1. I am personally as well as academically deeply indebted to Ernan McMullin from whom I learned much over the years; and I treasure especially the copy of *The Inference That Makes Science* that Ernan gave me.

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