On Not Being a Pragmatist: Eight Reasons and a Cause

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Nothing is easier than to familiarise one's self with the mammalian brain. Get a sheep's head, a small saw, chisel, scalpel and forceps (all three can best be had from a surgical instrument maker), and unravel its parts either by the aid of a human-dissecting book, such as Holdén's 'Manual of Anatomy,' or by the specific directions ad hoc given in such books as Foster and Langley's 'Practical Psychology' (Macmillan) or Morrell's 'Comparative Anatomy and Dissection of Mammalia' (Longman's).

James 1890: i. 11 n.

Of course pragmatism is true: the trouble is that it doesn’t work.1

One of the few domains in which I am a consistent pragmatist is pragmatism itself: use it when it is useful, but don’t when it isn’t.

Hacking 1998: 93

Our editor Cheryl Misak has long been asking me to contribute to this collection, which is to illustrate the extent to which contemporary philosophers have become, or simply are, pragmatists. I have long been resisting, because I do not think of myself as a pragmatist, or identify my ways of doing philosophy with pragmatist ways.

1 'A famous and deep joke attributed to Sidney Morgenbesser' (Williams 2002: n. 14).

0 THE CAUSAL OR HISTORICAL EXPLANATION

I identify myself as an analytic philosopher, knowing full well that I practise analysis in my own idiosyncratic ways. When in public debate someone wants to pin some other label on me, I may get annoyed and
say I am Leibnizian. On two occasions, both a very long time ago, being asked to describe my philosophical position in French company, I said: 'I am both a positivist and an anarchist at the same time.' But I have never called myself a pragmatist. The closest I got was in the third epigraph above.

Things might have been different had I begun my formal study of philosophy in the United States. Instead, I began at Cambridge University, where I did a two-year B.A. in moral sciences after studying mathematics and physics in Canada. America would have been a different experience. But not because I would have learned about pragmatism: on the contrary, because I would have been educated in the shadow of logical positivism. Hence I would have discovered pragmatism as rebellious liberation, or if I did not do so myself, I would have been liberated by Rorty's lovely book *Philosophy and the Mirror of Nature*.

I had a wholly eccentric education centred on Frege, Moore, and Russell. Wittgenstein had recently died. One absorbed a good deal of the man, and had the incomparable advantage of never attending a lecture in which his name was mentioned. Instead, he was internalised, and remains one of the two most profound influences on the ways in which I do philosophy.

The most original philosopher in Britain at the time was J. L. Austin. I did go to Oxford to attend his last course of lectures. I acquired a rather ascetic sense of moral philosophy not from the usual run of writers on ethics, but indirectly from F. R. Leavis. I never went to his lectures, but I spent late nights hanging out with Leavites. That was the most absolutist ethics imaginable, derived from an intense reading of novels in what Leavis called *The Great Tradition*. Pragmatism, not. It did encourage a strong taste for what Philippa Foot and Bernard Williams were later to call 'thick' moral concepts, 'rude' over 'bad', 'treachery' over lying. Although Leavis's classic book *The Great Tradition* was conspicuously non-pluralist, it did create the sense that morality is historically situated, without inviting simplistic relativism. For me (but not for Leavites in general) it fostered a way of thinking that Bernard Williams was to articulate in *Ethics and the Limits of Philosophy*, a work resolutely opposed to neo-pragmatism. I acquired something like what Williams (2005) later called 'Left Wittgensteinism'.

I read Carnap's *Logical Syntax of Language* in my first term as an undergraduate in moral science, largely to show off. Yes, I could read things that were (in the context of that time and place) totally out of the way and bizarre. That is still one of my character defects. In an evening discussion group run by a young research fellow (post-doc), I pompously declared that we had to distinguish syntax from semantics. 'And what exactly do you mean by semantics?', I was asked. With an education like that, who needs pragmatism? Not pragmatism as prophylactic, anyway. When Rorty's *Mirror* was published, I was asked to be one of the commentators on the book at the Eastern meeting of the American Philosophical Association. It is obvious from what I said, even that late in the day, that I had no idea why so many young American philosophers found the book so exciting (1980a).

1 ON NOT NEEDING PRAGMATISM FOR FALLIBILISM

One thesis now strongly associated with pragmatism is that knowledge has no foundations, and that all beliefs, no matter how strongly held, are fallible. I am of that party. If I needed a twentieth-century philosopher to choose as role model here, it would be Karl Popper. He had little use for pragmatism. Once in a while I tauntingly declare that I am the last living Popperian. I would never, ever, call myself Peircean, living or dead.

I absorbed Popper rather late, largely thanks to another *émigré* from *Mitteleuropa*: namely, Imre Lakatos. I got to know him as soon as he arrived in Cambridge. I needed only Lakatos's name, 'fallibilist', to become one, and that was years before discovering that Peirce had taught a similar lesson, and that both men claimed that methods of the sciences had the unusual virtue of being intrinsically self-correcting. Authors whom I really did admire, most notably F. P. Ramsey, taught the virtues of pragmatism for their generation, but they did not speak to me or mine in the same way.

It never occurred to me that all knowledge needed foundations, so I did not well understand what Popper opposed. I did indeed know pretty much by heart Frege's *Foundations of Arithmetic*, as translated by J. L. Austin. But I did not think of it as providing 'foundations' in any literal sense. It offered a brilliant analysis of the concept of number. From the start I never thought of it as discovering what the numbers 'really are'. Instead, it brought into being a whole new way of comprehending the numbers, which calibrated well with, and made sense out of, less sharp perceptions.

Frege had a dream of understanding a pre-given truth that made arithmetic certain, but I never caught the dream. Russell, once my hero,
really did seek certainty in mathematical logic. When I was a student, the search for certainty seemed as dated as Edwardian clothing, soon to be favoured by Teddy boys. The ambitions of Frege and Russell were picked up and continued in a noble way by some members of the Vienna and Berlin Circles. They were then transported wholesale to the United States—like so many other of the best aspects of German-language culture, civilization, and learning. Philosophy had been in the doldrums in the United States, and in my opinion the imports had wholly salutary effects. They passed me by. I was educated in another milieu, which was in many ways vastly inferior. Nevertheless, it served me, personally, well.

When I became interested in probability, I of course learned Carnap’s confirmation theory, but it had lost all connection with human reasoning. It was not just sterile; it was dead. The attempts by J. M. Keynes and Harold Jeffreys to develop a logic of probable reasoning had been full of life, but already in 1926 F. P. Ramsey enunciated their death knell in the simplest and truest words imaginable: ‘But let us now return to a more fundamental criticism of Mr. Keynes’ views, which is the obvious one that there really do not seem to be any such things as the probability relations he describes’ (1926: 161). Carnap’s confirmation functions were intended as the formal logic of Keynes’s probability relations, and these functions do exist; but they are irrelevant to the probable inference that is the guide in life. Hence I turned away from Keynes and learned some thick statistical theory, and wrote Logic of Statistical Inference (1965). Note, not the logic. People asked me why not, and even then I could say that there is no such thing as the logic of anything.

2 TAKING A LOOK IS NO MORE CHARACTERISTIC OF NEO-PRAGMATISM THAN IT IS OF ANY OTHER CONTEMPORARY STYLE OF PHILOSOPHIZING

Thus I took a look at statistical inference, as it was understood in the 1960s. Ever since writing that first book, nearly all my work has turned to real life, real knowledge, real expertise. I have come to call that taking a look. For an illustration, let’s use the most trivial example. For a note titled, ‘Was there ever a Radical Mistranslation?’ (1981). I looked up every example in the literature, of an alleged real radical mistranslation. I started with ‘kangaroo’, which I had myself used as an example of a radical maltranslation until Jack Smart put me right. I ended with indri, a Malagasy word for a type of lemur that is alas now very rare but once was common in Madagascar. In the course of a discussion with several other philosophers about naming, Quine had said that ‘indri’ was native for ‘there he goes’ as the lemur scooted up a tree (Discussion 1974: 500). I gave good (but not conclusive) reason to think, contrary to the OED, that a word much like indri was an old word for a very specific type of lemur, in a very specific Malagasy dialect. This shows nothing about the a priori doctrine of indeterminacy of translation, but it suggests that one should, as I now like to put it, always ‘Take a look’. It also encourages a reality check on more abstract doctrines about translation. At the very least, it forces one to acknowledge the immense breach between actual, rare-if-ever indeterminacy of translation, and logically possible indeterminacy, which is ubiquitous (and thereby boring!).

It would be mere equivocation to say that my invariable need to take a look at real-life examples or real-life expertise proves that I am at heart a pragmatist. Perhaps mine is an attitude that could, by a stretch, be called pragmatism in sense 2 of The American Heritage Dictionary: ‘A practical, matter-of-fact way of approaching or assessing situations or of solving problems’. But it has nothing to do with sense 1: ‘Philosophy. A movement consisting of varying but associated theories, originally developed by Charles S. Peirce and William James and distinguished by the doctrine that the meaning of an idea or a proposition lies in its observable practical consequences’.

Neo-pragmatists are no more given to asking for real-life examples or for mastering the rudiments of a branch of useful knowledge than so-called Wittgensteinians or those who admire Heidegger. Undoubtedly the first philosopher whom I came across who fostered this productivity to take a look was J. L. Austin. I still regard it as a great misfortune that his Sense and Sensibility is no longer read, while How to do Things with Words has been used and abused for all sorts of purposes. The former is a masterpiece of reflection on what is actually said in a certain sub-community of English-speaking people. It was combined with an inspired sensibility to detect what sounds right to those of us who inhabit that community. Austin may have been a pragmatist sense 2, but was definitely not a pragmatist sense 1.

The original Oxford school of ordinary language philosophy shared Austin’s careful attention to linguistic detail. Even after Peter Hacker’s
demolition of the confusion, there is still a tendency abroad to speak of some 'linguistic philosophy' that covers both Wittgenstein and Oxford. Aside from the fact that the two detested and despised each other, their instincts with respect to reality are quite different. Wittgenstein did not care a fig what we say in ordinary literate Viennese or English, and built a philosophy around imaginary possibilities. I regard Wittgenstein as a primary source for my ways of thinking, but not for my superficial if pervasive habit of dwelling on thick descriptions of real cases.

It hardly needs saying that my later enthusiasm for the writing of Michel Foucault wholly reinforced my obsessive taking of a look. Foucault is admired by no well-established pragmatist today, and is regarded by some of them as a deplorably evil influence. Foucault was a fact-lover me too. I said above that Wittgenstein remains one of the two most profound influences on the ways in which I do philosophy—the second, of course, is Foucault.

3  ON DISAGREEING ABOUT TRUTH

Truth, in the saying so often attributed to William James, is what works. A coherence theory of truth is commonly taken to be a core doctrine of pragmatism. If I had to subscribe to a named theory of truth, it would be the redundancy theory introduced by F. P. Ramsey, with the rider that an affirmation of the form 'it is true that p' is used to say something about the assertion, conjecture, proposition, hypothesis, or whatever p, usually one already introduced into the discussion or conversation. Straight J. L. Austin, in short (1961: 94). But I do not wholeheartedly agree even with that. When it is said that the concept of truth is necessary for any human communication, I reply that if so, it is a purely formal concept—which is a way to understand Tarski. Tarski did say that his semantic approach was consistent with any substantive theory of truth, which is part way to saying that it is about a formal rather than a substantive concept.

I have recently been deeply influenced by Bernard Williams's last book, **Truth and Truthfulness: An Essay in Genealogy** (2002). This has received a pretty lukewarm reception, like Colin McGinn's (2003). If we may take Richard Rorty's review (2002) as expressing the neo-pragmatist reaction, then this book shares almost nothing with pragmatism. In contrast, my public admiration (2004b and 2005) appears to know almost no bounds. Williams takes *truth* to be timeless, to have no history, to be part of the structure necessary for human linguistic communication. That, I say, is truth as formal concept. In contrast, Williams takes *truthfulness* about a subject matter to have a history and to have a beginning. He dates the beginning of historical truthfulness with Thucydides. He locates the possibility of authentic truth-telling about oneself in the eighteenth century. I used to think of the truth about a subject matter as coming into being, but I now see it as the possibility of truthfulness. I had the good luck to express the idea correctly in 1982: 'although whichever propositions are true depends on data, the fact that they are candidates for being true is a consequence of historical fact' (2002: 167).

This line of thought has to do with the evolution of standards of objectivity, of what feels like an inevitable, 'timeless canon of objectivity, a standard or model of what it is to be reasonable about this or that type of subject matter' (2002: 188). We discovered how to make proofs in mathematics, and brought into being new ways of being truthful. Human beings discovered the laboratory style of reasoning, in which one manufactures apparatus to purify and even to create phenomena, and thereby generates new ways to be truthful about unobservable structures that underlie experience. The fact, that the methods of argument we now regard as canonical have a history, and once did not exist even for the wisest of the ancients, does not make them any the less the objective standards.

These ideas of truth, truthfulness, and objectivity are foreign to neo-pragmatism. I (1996: 73) have even used the idea of scientific styles of reasoning to object to Richard Rorty's criticism of Bernard Williams's insistence on a distinction between scientific and moral reasoning, although that thought needs further development. Some will trace back to Hegel the notion that standards of objectivity evolve in history. Peirce grew up as something of a Hegelianist, but so far as he was concerned, there were only three rather archistyle methods of reasoning: deduction, induction, and the method of hypothesis (abduction). He did not reflect on the historicity of mathematical proof. He did not see

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3 Austin is kinder to pragmatist theories of truth than one might expect (1961: 98 n. 1). He was not a 'correspondence theorist', as casual readers often imagine. He favoured a particular and detailed version of a 'correspondence' theory for **simple sentences**.

4 The invention of the deductive methods used in Euclidean geometry is elegantly investigated by Reviel Netz (1999).
that the method of hypothesis is not enough; the natural sciences require a rather late European invention: the laboratory and its apparatus for creating phenomena to study and measure.5

Our editor, in her Introduction, argues that a historical attitude to objectivity is part of the most recent pragmatist thinking. Excellent: this shows that the new pragmatism shares more than might have been expected with other contemporary strains of thought. It does not show that the current historicisms are pragmatist in nature, but rather that pragmatism has recently bought into some blend of historicism.

4 ON AGREING, TO SOME EXTENT, ABOUT WHAT'S REAL

My Representing and Intervening (1983) used the then current debates about scientific realism as a wedge to introduce the serious study of experiment into the philosophy of the sciences, which at that time was wholly dominated by theory. The Representing part of the book (Part A: 21–128) is an introductory review of some debates, expressed in a way that I hoped would interest undergraduates, educate them, and induce a healthy scepticism. Page 2 warned that I did not care much about scientific realism. (Today I care still less.) Are questions about realism important, I asked? 'I doubt it,' I replied, and continued by saying that I was 'a realist on only the most pragmatic of grounds.' Not pragmatist grounds, but pragmatic ones. The first discussion of reality (pp. 32–3) recycles J. L. Austin's brilliant examination of uses of the word 'real', and thereby implies that 'realisms' of any philosophical sort are pretty numbing.

Representing has a long chapter (17 pages) on positivism—not the logical sort but the kind urged by Auguste Comte and Bas van Fraassen. That was the scientific anti-realism current in 1983. It is followed by a short chapter (6 pages) on pragmatism. It admires Peirce, and quotes at length his youthful statement about the real being what, sooner or later, information and reasoning would finally result in. I have never agreed with that, but have always been impressed by what he said in the same paragraph: that 'the very origin of the conception of reality involves

5 One way to read Simon Schaffer and Steven Shapin (1985) is as the establishment of the laboratory style of reasoning. See my review of the book (1991).

the notion of a community without definite limits, and capable of a definite increase in knowledge' (quoted in Hacking 1983: 58).6

I picked up from Dewey the scathing phrase 'spectator theory of knowledge', which seemed to me to characterize most of the general philosophy of the sciences that was then being read and written (Popper, Carnap, Kuhn, Lakatos, Putnam, van Fraassen, the lot). 'My own view, that realism is more a matter of intervention in the world, than of representing it in words and thought, surely owes much to Dewey' (Hacking 1983: 62). Obst. Not in the sense that Dewey incited me to think that way, but in the sense that, when I looked back over the history of philosophy, I recognised that Dewey had been there before me. How did I get there? By talking to my scientific friends, especially the two I single out in the preface Melissa Franklin, then of the Stanford Linear Accelerator, and Francis Everitt, of Gravity Probe B. In August 2005 I revisited Stanford on day 485 of the actual probe in space, watching and listening in the control room as the experimental observations were coming to an end. The liquid helium in the satellite is almost exhausted, and the next bit of fun, the data analysis, begins. By April 2007 we may have some results. 'In my opinion', I wrote in 1983, 'the right track in Dewey is the attempt to destroy the conception of knowledge and reality as a matter of thought and of representation. He should have turned the minds of philosophers to experimental science, but instead [Rorty's] philosophers praise talk' (1983: 63). (I had just unkindly said that 'Rorty's version of pragmatism is yet another language-based philosophy, which regards all our life as a matter of conversation'.)

In Intervening (Part B: 149–275) I tried to turn the minds of philosophers to experimental science such as is exemplified today by Gravity probe B. By a coincidence far better than anything I could have hoped for, similar trends were evolving in the history and in the sociology of the sciences. So an interest in experiment has taken root, but not as deeply as I would like. At the end of Intervening there is a brief return to the numbskull topic of scientific realism. 'Experimental work provides the strongest evidence for scientific realism. This is [ . . . ] because entities that in principle cannot be "observed" are regularly manipulated to produce new phenomena and to investigate other aspects of nature. They are tools, not for thinking but for doing' (1983: 262). That is surely in the spirit of pragmatism, but I know of no

6 From Peirce's contribution to the 1868 Journal of Speculative Philosophy, 'Some Consequences of Four Incapacities'; repr. in Peirce 1968.
professed pragmatist who has even agreed with my criterion, let alone asserted it.

I am not 'against' theory, whatever that might mean; I wanted only to restore a Baconian enthusiasm for experiment. My current work in the philosophy of physics is about the intimate dynamics of interaction between theoretical modelling and the experimental creation of phenomena. My example is Bose–Einstein condensation, which had been on the theoretical drawing board since 1924, but was not realized until 1995.\(^7\) That is a field in which, at present, no theoretical advance is possible without experiment, and no experimental inquiry is possible without theory. At the leading laboratories I have visited (which are all down in the basement to avoid vibration), they say: We are so lucky to have so-and-so (who leads a theory group on the fourth floor) who actually cares about what we are doing and can help us think about what to look for.

This example of ultra cold research—we are talking less than a nanokelvin above absolute zero—illustrates many aspects of physics. Soon after I came to Toronto, I organised a conference called 'Table-Top Experiments'.\(^8\) The aim was to re-emphasize small experiments over large ones. So I am delighted that much of my new hobby really is conducted on table-tops, the laser table, and that the typical BEC lab consists of six people, a director, one or two post-docs, two or three grad students, and a lucky undergraduate or two.

I care about theory, but I am not, and was not, much interested in the reality of non-observable theoretical entities, which around 1980 was the philosophy-of-science flavour-of-the-year. Perhaps I have, and had, a pragmatist meta-sentiment, that it does not make the slightest difference to physics whether its theoretical entities are in general called real or not.

Perhaps it does matter to the funding of physics: It was once alleged in the journal Nature that the fallibilism or anti-realism of Popper, Kuhn, Lakatos, and Feynman caused Mrs Thatcher to put a spoke in the wheel of British physics. Actually it was her kind of pragmatism (sense 2 above, not pragmatism sense 1, the philosophy) that made Thatcher, the chemist turned Prime Minister, try to kill off fundamental physics, once a glory of the United Kingdom. She wanted cash value and saleable results.

\(^7\) The manuscript of Einstein's 1925 paper turned up in Leiden just the other day, in August 2005. Bose–Einstein condensates were not produced until 1995, first by Karl Wieman and Eric Cornell in Boulder, and then by Wolfgang Ketterle at MIT.

\(^8\) The papers were published much later in Buchwald 1995.

Of course it matters whether particular conjectured entities or quantities exist or not. Is there a positive electric dipole moment on the electron? Perhaps the fate of super-symmetry hangs in the balance. It certainly matters, in a very different way, whether proposed mental disorders are called real. Health insurance. Moral opprobrium or not. Self-esteem. I do not think 'real' is the most helpful word to use, but I was obliged to discuss such matters in Rewriting the Soul (1995) and Mad Travelers (1998). I did invoke Peirce at the last stages of my argument, but in an oddly non-pragmatist way (1998: 93).

5 ON READING PEIRCE, BUT NOT FOR PRAGMATISM

Charles Sanders Peirce was the greatest philosopher of probability whom we have known. One example: He was the first philosopher to understand one of the two viable approaches to learning from experience using probabilities. The ideas had been intuited by Jacques Bernoulli and Laplace, but Peirce was the first to state the general mode of reasoning, and perhaps I myself finally caught on thanks to Peirce, though there were certainly others from whom I took similar ideas (Hacking 19806). Learning from him about probability did not make me a pragmatist.

Hilary Putnam (1994: 160–9) has drawn attention to Peirce's proposal that probable reasoning requires, among other things, altruism, in order to be sound. He rightly asserts that 'the problem Peirce raised [about probable inference] is so deep [. . . ]', and he also drew attention to a letter from Peirce to James (Putnam 1994: 180 n. 36) emphasizing Buddhist virtues over banal charity. But even Putnam avoided detail. Advocating not only altruism as a logical foundation for science, Peirce invoked John the Evangelist's three cardinal virtues of faith, hope, and charity. 'Charity' here not the charitable donations to the poor that Peirce mocked, but the classic rendering of *caritas*, often translated as disinterested love. I used John's and Peirce's very profound trio as early as 1965 (p. 47), and in 2001 used the same passages to end my introductory text on probability and induction (pp. 265 f). In a forthcoming book, 'The Tradition of Natural Kinds', I spend a little time explicating Peirce's use of the words of the Evangelist, as understood by Peirce and as understood by St John, in a way that might discomfit your average twenty-first century pragmatist, but not, I hope, Putnam.
It is an important fact that each of the three classical pragmatists was involved in practical endeavours. Peirce worked for years in the US Coast and Geodetic Survey, which is where he learned what the curve of error means. Amusingly, his classic example of the use of the method of hypothesis is taken from an experience when he went with the American expedition to Turkey to measure the transit of Venus. It involves a strange Turk arriving with four men carrying a canopy to shelter him from the sun: he must be the governor of the province. I can hardly accuse Peirce of not taking a look, both on and off the job!

I used Peirce as a ‘witness’ throughout The Taming of Chance (1990), as I had used Leibniz throughout The Emergence of Probability (1975). A long chapter on Peirce ended Taming, and I still hold it to be an important reading of Peirce. It emphasized his experience as a scientist, and gave some of the details which, curiously, few pragmatist commentators do. I also offered textual evidence (1990: 213) that Peirce well knew that his own account of truth in terms of the long run is circular. I know of no Peirce scholar who has paid any heed to the fact that Peirce blithely admitted (at least once) the circularity of his account of truth.

I said in section 0 above that, when annoyed, I call myself Leibnizian. I could never call myself Peircian. The two philosophers are similar, and both resemble a hardware store that I used when I first moved to Toronto. The shop had a vast inventory of all the objects that had been put into the century-old houses in my neighbourhood. So when some mysterious bit of plumbing or electricity or whatever went awry, I could take it to Luigi, and he would tell me exactly what it was and produce from one of his thousands of drawers a duplicate or surrogate. The shop finally went out of business from a surfeit of inventory. Just like Peirce and Leibniz — though their drawers were filled with the future, not the past. ‘We should see [Peirce] as a wild man, one of the handful who understood the philosophical events of his century and set out to cast his stamp upon them. He did not succeed. He finished almost nothing, but he began almost everything’ (Hacking 1983: 61).

Peirce had, in one respect, a character fundamentally different from that of Leibniz. Leibniz was content with almost any popularization or even bowdlerization of his ideas. Peirce thought there was one right exposition. Not Leibniz: he would have loved to have had a William James.

6 ON READING WILLIAM JAMES

I love reading James. When I was an honorary Leavite, I had to read all of his brother Henry. I am glad I did my duty, but I am a little relieved that I may never read much of him again. William, in contrast, is always a delight. He was a great stylist, with an enormous heart. In my forthcoming The Tradition of Natural Kinds, there is a chapter called ‘The Joy of Kinds’ which begins by quoting him, in part: ‘Once we know that whatever is of a kind is also of that kind’s kind, we can travel through the universe as if with seven-league boots’ (1907: 179). How much more insightful than the turgid stuff about natural kinds one reads nowadays.

As I said, each classical pragmatist spent some time working in the real world. James’s Principles of Psychology (1890) is a wonderful introduction to the psyche and its brain. I love that first footnote to the Principles, quoted as my first epigraph above. There is a philosopher who knew how to take a look!

7 ON NOT READING JOHN DEWEY

I once tried valiantly to read John Dewey, but it did not click. He goes on and on. He was a very important public figure in his day. Patrick Suppes told me that his seminars at Columbia were masterpieces of precision and concision, in great contrast to his books. My failed attempt at reading Dewey did, however, lead to that fine term of abuse ‘spectator theory of knowledge’. What a splendid way to characterize all that is wrong with traditional epistemology! I always invoke it with admiration, but I have never given a page reference because I could not bring myself to actually reread some texts of Dewey to relocate the place he said it.

For me, Dewey does not mean New York but Chicago. The practical attempts at education, which meshed with the ambitions of the St Louis Hegelians: the Lab School. The emergence of successive Chicago Schools of sociology. For me, Erving Goffman was the greatest of sociologists (Hacking 2004a). Was he a product of pragmatist-oriented Chicago sociology? Of course. But I like the idea of his biographer, that the whole methodology of face-to-face interaction was something Goffman learned in his summer job while an undergraduate. He worked
with the National Film Board of Canada at a time when it was making extraordinary face-to-face documentaries in sparse communities across the country (See Winkler 1999). Primed for pragmatist sociology, you might say, and he went off and wrote a Ph.D. doing face-to-face on an island in the Hebrides. He was taking a look, unlike so many sociologists who are content to crunch numbers.

8 ON LEARNING FROM NELSON GOODMAN

The name of Nelson Goodman is not one of the first to roll off the tongue when someone starts mentioning pragmatists or neo-pragmatists. He had the good fortune to be born too late to be a founding pragmatist, and too soon to be neo. He was a great nominalist, too much so, I think, to serve as a good role model for future pragmatists. I do think that his self-moniker, irrational, is one that ought to enter the pragmatist lexicon.

I spent a long time worrying about the tradition of natural kinds from William Whewell and John Stuart Mill to now. In the end I had to conclude that for all its insights and inspirations over 160 years, the tradition has self-destructed (Hacking, forthcoming). Goodman got it right before anyone else saw the looming shipwreck. We should not, he argued, speak of natural kinds, but only of relevant kinds.

I say 'relevant' rather than 'natural' for two reasons: first, 'natural' is an inapt term to cover not only biological species but such artificial kinds as musical works, psychological experiments, and types of machinery; and second, 'natural' suggests some absolute categorical or psychological priority, while the kinds in question are rather habitual or traditional or devised for a new purpose. (1978: 10)

Relevant to whom? To us, or to you, or to them, to those who group items together, for this or that purpose. Relevance is all there is to be said, in general. Some philosophies take Goodman's relevance to be an expression of nihilism. It may, on the contrary, be close to bedrock, a word used more often in talk about Wittgenstein than Goodman.

Goodman is the only pragmatist from whom I ever learnt anything—that is, the only one who led me to think new thoughts or rethink old ones. Sidney Morgenbesser gets an assist, yes, the Sidney of all jokes, including the second epigraph above. The very first paper that I read in public, and which was later printed (1964), was a talk to the Aristotelian Society in 1962. There was this man in the back row looking wildly inappropriate for a covey of English dons of that time. He put up his hand, and out came the New York: 'Yeah, but what about grue?' Exactly. In Logic of Statistical Inference (1965) I wrote, alas without thanking Morgenbesser, that Goodman's riddles 'combine precision of statement, generality of application, and difficulty of solution to a degree greater than any other philosophical problem broached in this century' (p. 40).

That endeared me to Nelson Goodman, who ever after ascribed greater philosophical insight to me than I merit. I have gawned away at grue ever since. Not because I want to solve the 'new riddle of induction', but because I think it is a deep fact. A bedrock fact. There is a 178-page bibliography of writing about 'grue'; the authors cited are of many persuasions, but card-carrying pragmatists are few among them (Stalker 1994: 280–457).

Partly thanks to his activities as an art dealer and patron, and partly thanks to his unusual contributions to aesthetics, Goodman is better known in quite a few American intellectual circles than any of his philosophical contemporaries, including Quine. It is characteristic that Mary Douglas, the great English cultural anthropologist, together with her then colleague at Northwestern, the pre-eminent philosopher of biology David Hull, should have put together a volume of essays (1992) inspired by and offered to Nelson Goodman. The title is How Classification Works, which well might serve as a title for a book in which to view pragmatism at work. Yet I would not readily identify any of its authors as a pragmatist. My own contribution, 'World-Making by Kind-Making' is an attempt to analyse a real case of kind-making—the making and moulding of the concept of child abuse. The evolving practices, value judgements, institutions, and laws connected with that appalling subject seem to me an exemplary case of world-making.

I don't know that Goodman much liked the paper. One piece of mine that he did recommend to many people was about Bruno Latour's Laboratory Life (Hacking 1988). That was written some time before people were taking Latour really seriously, either as potentially ally or as ogre, the end of reason as we know it. My version of early Latour—not a faithful one, but a useful one—seemed to Goodman to be a right version of much scientific activity. That is a good point at which to end, by echoing a sentence from my second paragraph above: Some of the theses favoured by pragmatists, neo-pragmatists, or new pragmatists, arise from and seem natural in many other contemporary

* See Hacking 1994, 1993a, and 1993c. This material and more appears in one volume (Hacking 1995b).
perspectives: mine for example, or Bruno Latour’s. But those selfsame perspectives do not owe much to pragmatism, and do not define one as a pragmatist unless one so chooses.

BIBLIOGRAPHY


On Not Being a Pragmatist


